**The collected scientific papers of John James Waterston / edited, with a biography, by J.S. Haldane.**


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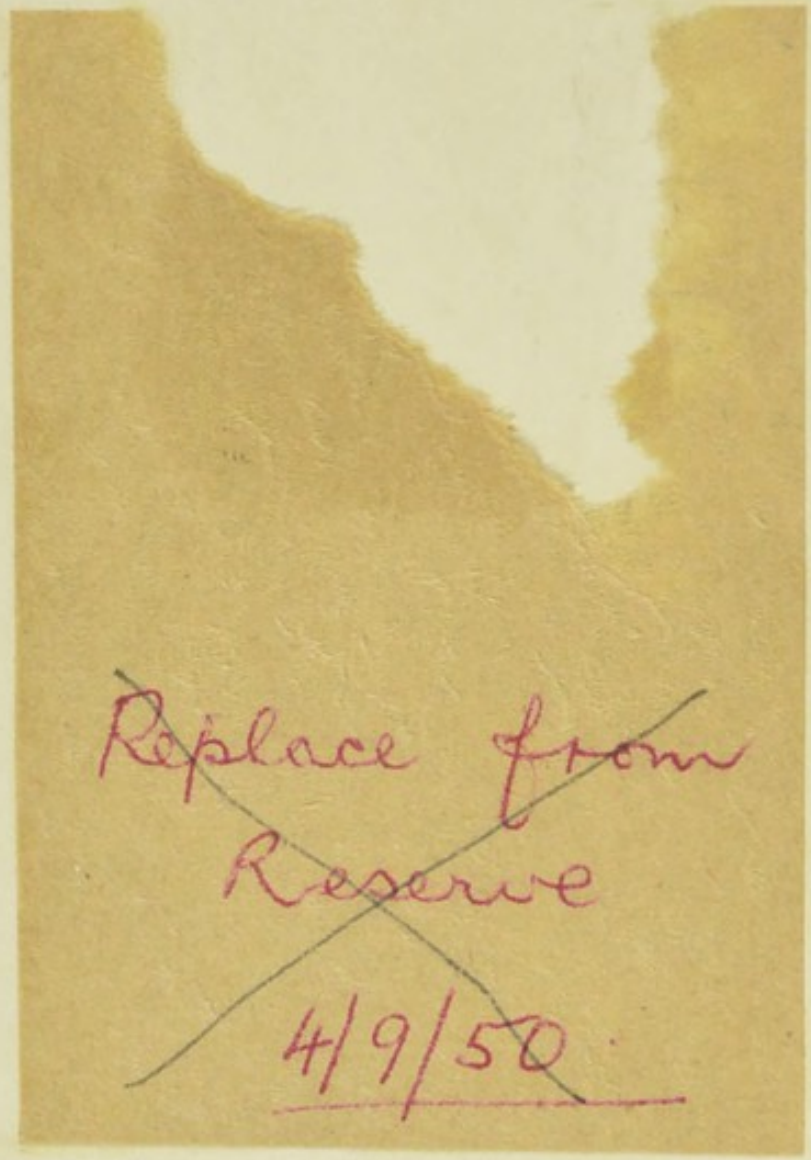


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# The Collected Scientific Papers of John James Waterston

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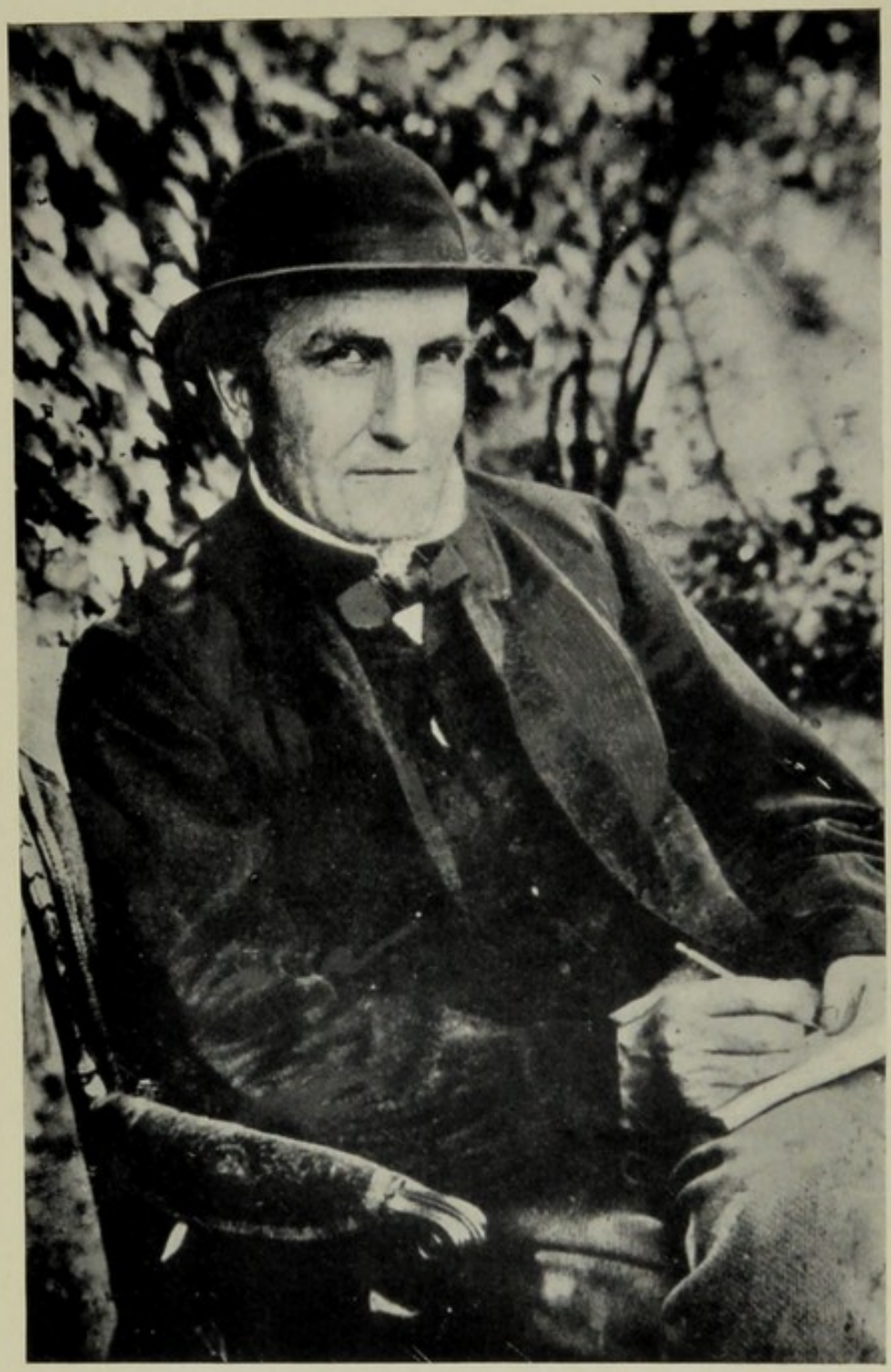

John James Waterston

J. J. Waterston, aged 46

*Frontispiece*

# The Collected Scientific Papers of John James Waterston

EDITED, WITH A BIOGRAPHY, BY

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# PREFACE

Owing to the fact that the late Lord Rayleigh discovered in the manuscript Archives of the Royal Society, and published in its Philosophical Transactions of 1892, with an introduction and very valuable notes, the paper by J. J. Waterston in which he formulated in 1845 the dynamical theory of gases and of heat and temperature, Waterston's name is known to many physicists. His other papers are, however, still practically unknown, and have never been collected together till now. It was only a stray reference in Landolt and Börnstein's *Physikalisch-Chemische Tabellen* that directed my own attention to his fundamental work on the physical chemistry of liquids, and I afterwards found that his earliest and in some respects most interesting work was on physiological psychology.

The present volume is published with the support of the Carnegie Scottish Universities Trust and of members of his family, and contains unpublished as well as published papers. Many of these papers are not merely of historical interest, but throw what is still new and, as it seems to me, very important light on the scientific theories and problems of to-day. As in the cases of a number of other great men of science, Waterston's ideas were far ahead of those current in his time. In consequence of this, and of his retiring disposition, his work, whether actually published or only submitted in manuscript, was almost completely neglected during his lifetime, and its importance was only partially realised through the publication, after his death, of his paper of 1845.

As nothing is generally known about Waterston's life, I have added to the Collection a biography. The only biographical notices of him that have yet appeared are a very short one, entitled "The Strange Story of John James Waterston," by his nephew, the late Mr George Waterston, in *The Clan*, a privately

printed series of records of the Sandeman family, edited by the late Colonel Glas Sandeman, and another very short one by Professor Turner in the recently published *History of the Royal Astronomical Society*. For many further details I am indebted to members of his family, and particularly his nieces, Miss Margaret Waterston, Dr Jane Waterston of Cape Town, and Miss Georgina Waterston, and his nephews, Mr Charles Waterston and the late Mr J. W. Waterston, who had preserved in affectionate remembrance a number of his letters and other papers, together with several photographs of him. To Mr Charles Waterston, who has given me much assistance, and who prepared manuscript notes of his uncle's life, he left his papers.

The scientific papers are arranged, not merely in chronological order, but also according to their subjects, as this arrangement will make it more easy for the reader to follow the development of the reasoning and experimental work. I have also tried to indicate in the biography the general course of this development, the sweep of the reasoning, and its bearings on scientific theories still current. A considerable amount of overlapping in certain of the papers is due to the fact that the two most important of his earlier papers had not been published. Papers of minor importance have, for the sake of completeness, been included. In the case of a great man of science, as Waterston undoubtedly was, this seemed only fitting.

To the Council and Officers of the Royal Society I am indebted for their cordially given permission to publish in this volume the whole of the papers, whether already published or not, which Waterston submitted to the Society.

J. S. H.

# CONTENTS


PAGE

PREFACE . . . . . vii

BIOGRAPHY . . . . . xiii

I. PHYSIOLOGICAL PSYCHOLOGY.

Thoughts on the Mental Functions, 1843 . . . . . 3

II. GASES AND THE THEORY OF HEAT.

Note on Molecularity, 1843 . . . . . 167

Note on The Physical Constitution of Gaseous Fluids and a Theory of Heat, 1843 . . . . . 183

The Physics of Media that are composed of Free and perfectly Elastic Molecules in a State of Motion (with the late Lord Rayleigh's Introduction and Notes, and an abstract of a paper on A General Theory of Gases, submitted to the British Association in 1851). Submitted to the Royal Society 1845. Published 1892 . . . . . 207

An Account of a Mathematical Theory of Gases, 1845 . . 320

The Deviation from the Primary Laws of Elastic Fluids, 1857 . 332

The Theory of Sound, 1858 . . . . . 345

III. LIQUIDS AND THEIR VAPOURS.

A General Law of Density in Saturated Vapours, 1851 . . 365

A General Law of Density in Saturated Vapours, illustrated by Chart No. II., 1852 . . . . . 371

Capillarity and its relation to Latent Heat, 1858 . . . 407

Evidence of a Graduated Difference between the Thermometers of Air and Mercury below 100° C., 1858 . . . . 429

A Law of Liquid Expansion, 1861 . . . . . 450

The Expansion of Water at High Temperatures, 1863 . . 468

Liquid Expansion, 1864 . . . . . 490

Certain Thermomolecular Relations of Liquids and their Saturated Vapours, 1868 . . . . . 498

PAGE

IV. Miscellaneous Physical and Chemical Subjects.

A New Dynamico-Chemical Principle, 1831 . . . . 531

The Integral of Gravitation, and its Consequents with regard to the Measure and Transfer, or Communication of Force, 1858 . 545

Chemical Notation in conformity with the Dynamical Theory of Heat and Gases, 1863 . . . . . 565

Chemical Notation, 1863 . . . . . 579

The Application of the Vapour-Density System of Notation to express Chemical Reactions, 1864 . . . . . 588

Some Electrical Experiments and Inductions, 1865 . . . 598

Some Electrical Experiments, 1865 . . . . . 642

The Thermal Action in the Wire of a Voltaic Circuit . . 647

Electric Conduction, 1865 . . . . . 650

V. Astronomical Subjects.

Finding the Latitude, Time, and Azimuth, 1842 . . . 657

A Graphical Method of predicting Occultations, 1844 . . 665

Remarks on the Comet, 1845 . . . . . 667

A Direct Method of determining the Distance of a Comet by Three Observations, 1845 . . . . . 670

A Graphical Method of computing the Excentric Anomaly, 1850 . 673

Thoughts on the Formation of the Tail of a Comet, 1859 . . 674

Observations on Solar Radiation, 1861 . . . . . 676

The Change that would take place in the Elements of the Earth's Orbit by a Sudden Accession to the Sun's Mass, 1866 . . 693

The Change that would be induced in an Elliptic Orbit if the Intensity of the Force of Gravity were influenced by the Centripetal Velocity of the Orbital Body, 1867 . . . 698

The Heat of the Stars, 1878 . . . . . 705

A Solar Thermometer Couple to measure the Heat of Daylight, 1878 708

# LIST OF ILLUSTRATIONS TO BIOGRAPHY


FACE PAGE

1. J. J. Waterston, aged 46, with his signature. From a photograph taken in the garden of the house at St John's Hill, Edinburgh, and developed by himself. (Photograph in possession of Miss Margaret Waterston). . *Frontispiece*

2. Catherine Sandeman (Mrs William Waterston, and later Mrs Robert Ferrier). From an oil portrait in possession of Miss Margaret Waterston . . . . . xv

3. The Waterston House and Sealing-Wax Factory at St John's Hill, Edinburgh . . . . . xvi

4. Photograph of J. J. Waterston, aged about 60 . . lvi

5. Lithograph of J. J. Waterston, drawn from memory at the time of his death, aged 72. (Lithograph is at Warriston works) lxv

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# MEMOIR OF J. J. WATERSTON

JOHN JAMES WATERSTON was born at Edinburgh in 1811. His father was George Waterston, a well-known manufacturer of sealing-wax and other stationery requisites. It is evident that the family traditions and intellectual atmosphere in which young Waterston grew up accounted for a great deal in his scientific career.

The Waterston firm (now a Limited Company, but still owned and directed by members of the family) was founded by William Waterston, grandfather of J. J. Waterston, in 1752. He was evidently a man of ability and thorough integrity. He was also a good musician, and wrote, or else composed the music of, a number of hymns. Originally a schoolmaster in East Lothian, he established a business in Edinburgh as a "wax chandler," and particularly for the manufacture and supply of sealing-wax and the flambeaus or "links" used in the days before public lighting of the streets. These flambeaus were made mainly of resin, and when they were no longer needed it was a natural step to develop the business in sealing-wax and wafers, for which there was a rapidly growing demand, as in those days letters were either sealed or closed by means of wafers. It is related that a man connected with Holland, who had taken sanctuary at Edinburgh on account of debt, and whose debts William Waterston paid, imparted to him information of considerable value in the manufacture of the highest qualities of sealing-wax, which had hitherto been imported from Holland. The making of sealing-wax and wafers led, in later generations, to developments in many other directions connected with stationery, printing, engraving, lithography, and the publication of illustrated books, so that the business is now a large one, with widespread connections in this country and abroad.

William Waterston married Catherine Sandeman, daughter of a Perth merchant, and a woman of great character and ability, who after her husband's death carried on the business vigorously, and took her second husband, Robert Ferrier, and then her son George Waterston, into partnership. She was a niece of Robert Sandeman (1718-1771), a well-known religious leader, and with her there came into the family, not only an additional tradition of culture, but also the religious teaching of Robert Sandeman and his father-in-law John Glas (1695-1773). Glas and Sandeman were the originators of the religious body known as Glasites or Sandemanians. Glas was a Forfarshire Presbyterian minister who objected to State interference with the Church, and was therefore deposed, but continued to act on his convictions, together with members of his congregation and others who held similar opinions. His son-in-law and other members of the Sandeman family were prominent in spreading these opinions in Great Britain and America.

The main principles laid down by Glas and Sandeman were that though a Church must have its own internal discipline and organisation through solemnly chosen bishops or elders, deacons and deaconesses, it is subject to no external human authority such as that of the State: that though salvation is by faith alone true faith is only that which shows itself in works; and that all services to the Church must be given free, as from love to the truth. The Glasite principles were thus associated with strict integrity in all matters of conduct, and with an idealism which was constantly expresting itseelf in practical affairs. Owing to the fact in addition to the usual communion service of bread and wine they took together an ordinary meal which usually consisted of kail and beef, the Scottish Glasites were often nicknamed the "Kail Kirk."

One of the English Sandemanians was Michael Faraday's blacksmith father, and Faraday himself remained throughout his life an active member of the Sandemanian Church in London. The principles of Glas and Sandeman were closely akin to those requisite in a real leader in pure science, and it is significant that two such men as Michael Faraday and John James Waterston were connected in their upbringing with this

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Catherine Sandeman

very small religious body. Neither Faraday nor Waterston ever turned away for the sake of other considerations from their work in pure science.

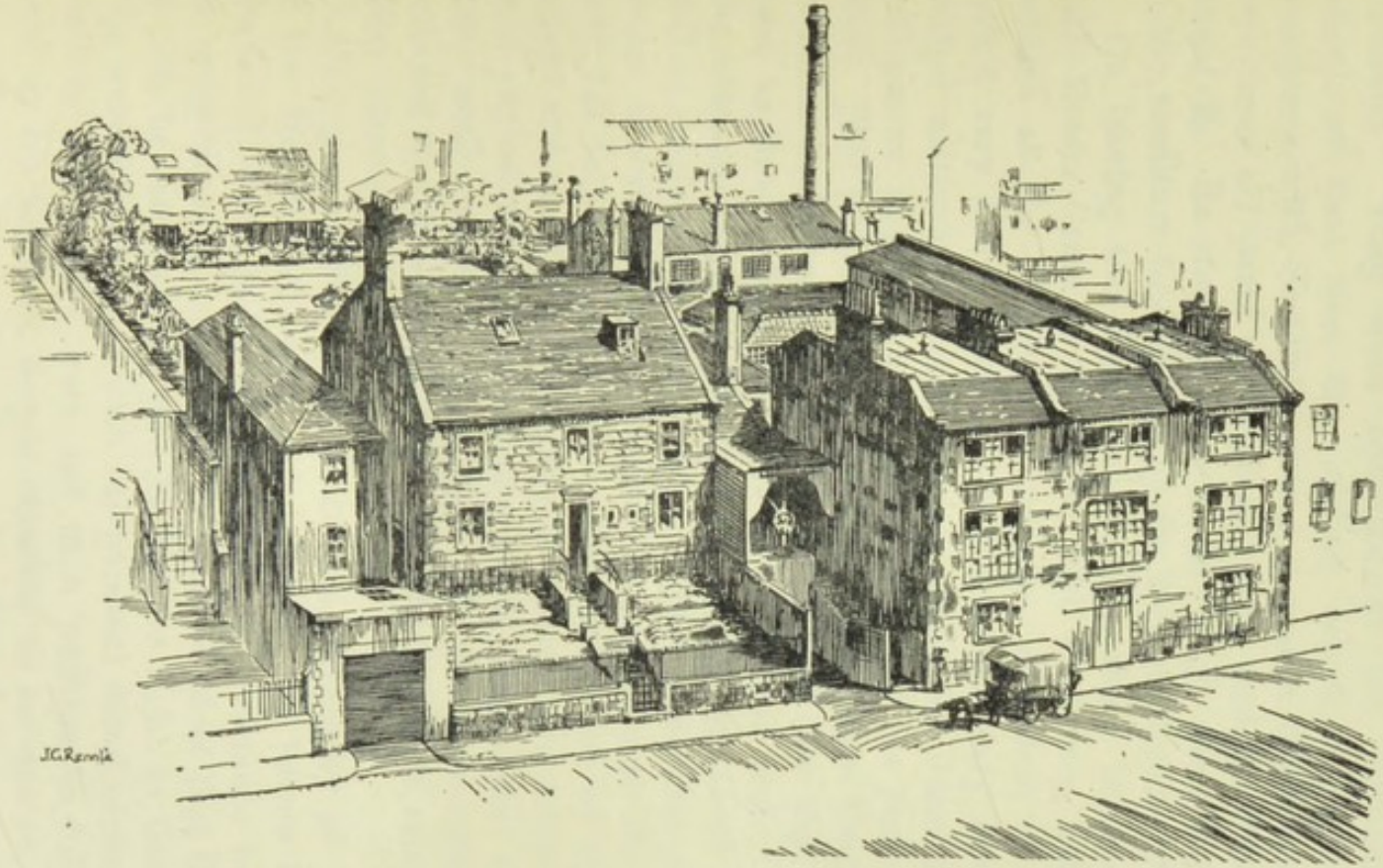
Mrs William Waterston was a sister of George Sandeman, founder of the well-known London firm of port wine merchants. The manner in which she pushed the business is illustrated by a rather quaint advertisement in the *Caledonian Mercury* of 1782, preserved as a curiosity by the family. It appears that Mrs Waterston, like her brother, thought that she might as well have some share in the wine trade. Hence her advertisement refers not only to sealing-wax, etc., but also to a remedy supplied to "the best inns in England" for the relief of persons suffering from the effects of "a mixture of liquors." J. J. Waterston's impression of his grandmother, who lived till 1831, appears clearly in a letter in which he describes her London brother as a man "not only willing to help, but of unmistakable power and influence," and adds that "his character is very like my grandmother's in many points, and his manners, though extremely quiet and reserved, cover feelings both warm and violent." The grandmother was a first-rate hostess, with various literary friends, some of whom she seems to have helped very substantially. Her long and prominent nose was characteristic of her family, to judge from portraits of her brother and sister which are in the possession of the present senior members of the Sandeman firm.

There can be little doubt that the strain of genius which showed itself in J. J. Waterston's scientific work came to him through his Sandeman grandmother. Genius, if we define it as a capacity for seeing what others have not yet seen, has shown itself in many other descendants of the same family who have become prominent in various professions—as merchants, bankers, soldiers, sailors, lawyers, doctors, journalists, etc. Only one other member of the family became at all known as a scientific man, however. This was Professor Archibald Sandeman, author of various writings, including a book published at Cambridge in 1868 on *Pelicotetics, or the Science of Numbers*, but perhaps better known for his large educational benefactions to Perth. The family genius usually found its practical outlet in administration and finance, but

often also in other directions: for instance in J. J. Waterston's insight into molecular behaviour, or his cousin, General Sir Robert Sandeman's insight into the behaviour of the turbulent inhabitants of Baluchistan, whom he converted into peaceful, prosperous, and loyal citizens of the British Empire. An interesting account of members of the Sandeman family up to the end of last century is contained in the numbers of *The Clan*, a privately printed magazine, edited by the late Colonel Glas Sandeman, who also edited a pedigree of the family, published by the Waterston firm. It appears from these and other family records that Catherine Sandeman's father had a prosperous business at Perth as a builder and wood merchant, while her mother, Jean Duncan, was descended from the same family as Admiral Duncan, the victor of Camperdown, and also from a branch of the Scottish Royal family of Bruce.

George Waterston, J. J. Waterston's father, was greatly interested in literature, science, and music. His family thus grew up in an atmosphere of culture, and often came into contact with young literary men such as John Hill Burton, the future historian, De Quincey, and George Combe, the apostle of phrenology. The family lived in a house which then stood by itself, except for the adjoining sealing-wax factory, on St John's Hill, a piece of high ground near the Pleasance, on the outskirts of the Old Town, not far from Holyrood. This house, which was bought by the grandmother after her first husband's death, was built for, and originally occupied by, Professor Robison, a well-known eighteenth - century physicist and a friend of James Watt. The ground had been bought from Dr Hutton, the famous geologist, who also lived on St John's Hill. From the garden and windows there is a view across the intervening valley towards Salisbury Crags, over a scene which Scott made famous in *The Heart of Midlothian*. Mrs George Waterston, whose maiden name was Jane Blair, and who belonged to Dunkeld, was devoted to her husband and children, and their home was a very happy one.

John James was the sixth of a family of nine, who remained deeply attached to one another throughout their lives. Of the other brothers, William, the eldest, became a well-known banker in London, and was the author of a




WATERSTON HOUSE AND SEALING-WAX FACTORY AT ST JOHN'S HILL

widely read Cyclopædia of Commerce; George developed the family business with great vigour and success; Charles became a banker in Inverness and director of several companies; and Robert, a merchant in Rio de Janeiro and later in Auckland. They were educated at the Edinburgh High School, at that time the leading school in Scotland. In accordance with Scottish custom it was a day-school, though boys from all parts of Scotland were sent to Edinburgh to attend it. Its standard was a very high one, largely owing to the tradition established by its former Rector, Dr Adam.

On leaving the High School, Waterston became a pupil with Messrs Grainger & Miller, civil engineers, but attended lectures at the University, and also, as letters of his show, took a very active part in a students' Literary Society. He studied mathematics and physics under Sir John Leslie, the well-known physicist, and was medallist of his year in Leslie's class. This perhaps meant more in those days than it would now: for Leslie seems to have taken special care that his prizes went to those whom he himself considered best. Waterston also attended lectures on anatomy under the third Monro, as well as on chemistry and surgery. This somewhat remarkable choice of subjects was doubtless connected with the fact that both his father and he were much influenced by the physiological and educational teaching of George and Andrew Combe. In a cupboard in the old house on St John's Hill there were still, until recently, a number of gruesome-looking casts collected by Waterston's father, of the heads of criminals and other persons.

One great difficulty in writing a biography of Waterston is the fact that though a number of his letters have been preserved, there is hardly any detailed reference in them to his scientific work and ideas. It is also evident that throughout his life he felt strongly that he had no right to trouble those who were near to him with his personal disappointments over matters which they were not in a position to follow. Not even to his brothers, deeply attached as he was to them, did he endeavour to explain the main aims of his scientific work. He also carefully concealed from them the fact that what he considered his greatest papers had been rejected by the

Royal Society, while his other scientific writings had fallen absolutely flat, or else been refused publication. He knew that it would only cause them pain if he mentioned his own deep disappointments. It thus comes about that only through his scientific writings can the real story of his life be interpreted. It is clear, however, that he was well aware of the great scientific importance of his work. In a letter of 1858 to his brother William he writes: "I send you my papers, but I don't expect you to read them. The next will be more intelligible perhaps. There are plenty more budding, the fruits of my Butcher's Island studies, which of course will change the aspect of physical science. At least there is no harm in fancying this so as to obtain stimulus for work that is wanting in the ordinary inducements for labour."

His first published paper was written in his student days when he was 19 years old, and appeared in the *Philosophical Magazine*. This paper (page 531 of this book) is nothing less than an attempt at a mechanical explanation of gravitation, and is interesting, not only because of its evident relations to the kinetic theory of gases, but also because it reveals clearly how he accepted without question the mechanical conception which Galileo and Newton had formulated of physical reality. On this conception action at a distance is unintelligible, and Waterston, like many other physicists, was groping for a means of dispensing altogether with action at a distance. The part played by energy of rotation in his suggestions on the cause of gravitation is remarkable, as, owing to circumstances referred to below, he thought it justifiable to ignore energy of rotation in connection with gases.

In his case the seeds of Leslie's teaching had evidently fallen on very fertile ground; but for many years Waterston made no further contribution to physics, and it seems clear that what diverted him was the study of anatomy, physiology, and psychology. There is nothing, however, to show the direction of his work till the publication, in 1843, of the book which will be referred to below.

At the age of 21 Waterston went up to London, with a view to prosecuting his intended career as a civil engineer.

He was received with great kindness by his grand-uncle, Mr George Sandeman, in whose town house in St Swithin's Lane, close to what is now Cannon Street Station, and on the site of the present central offices of the Sandeman firm, he lived for a short time. Among the letters of Waterston's which have been preserved is the following, written from his grand-uncle's house, to his brother George, describing his journey and arrival in London. It gives a good idea of his bright, humorous, kindly character, and throws light on his deep admiration for Faraday, and indirectly on his own very high scientific ideals.

St Swithin's Lane,
London, 11*th July* 1832.

My dear George,—I take the opportunity of Mr Rose, whom you are acquainted with, to answer yours of the ——. From my father's letter you will have had some notion of my adventures since I arrived here, so I must not recapitulate. You enquire about my voyage, but there is so little to relate that it is hardly worth putting it down. You remember that it was a very bleak and misty evening when we set out, and it continued the same all that night and half the next day, getting gradually very boisterous till about the middle of the night, when, as a passenger who went on deck told me, the waves washed over the ship. My state cabin, as they called it, was also so near the engine that I dreamt I was in the infernal regions.

The only meal we had that evening was tea or coffee, and we had just time to finish it before the see-saw motion took effect on the stomachs of those who were not salt-water gentlemen, and soon cleared the cabin. Even Sir John's feelings were overcome, and he retired at a quick march—as fast as he could waddle.* He had been boasting that he was an excellent sailor, having been across the Atlantic, etc. Next day, however, and ever afterwards, he kept up and misbehaved no more. That evening he commenced a conversation with me about the weather—he knew who I was quite well. The subject was so pathetically handled by him that I felt quite overcome, and the ship being rather unsteady at the time, I rushed from his presence and gave vent to my feelings in a

* The reference is to Sir John Leslie. He was knighted in 1832, and was probably on his way to receive his knighthood. He died in the autumn of the same year.

peace-offering to the troubled ocean. Sir John sympathised with my delicacy, and retired considerably agitated by the moving spectacle. I found it best to go to couch, but before I could adjust my raiment for the posture my agitated feelings several times got the better of my more manly resolution, and my contortions were truly agonising. Whenever I got into bed, however, my painful sensations ceased and a general calm crept over my troubled spirit. Sleep, however, was yet far off, and I remained like a child in a cradle, rocked backwards and forwards with the tempest. All of a sudden I started at the entrance of a pale being, who seemed the paltry remnant of a sea-sick passenger who had given forth as a sacrifice to Neptune his grosser substance! This ghastly being turned out afterwards to be an itinerant missionary from Glasgow. He was my cabin-mate, but for that night only, as the two next I slept very comfortably on one of the long sofas in the saloon, where several of the gentlemen had likewise resorted who were too long for the short cabin beds. There was among this company a Mr Campbell, a huge Highland laird from the Western Isles, seven feet high. He had his carriage alongst with him, his wife, two children, servants, horses, etc.

Next morning I felt myself much better, and was no more troubled by sea-sickness. Sir John was really, and is really, a dreadful glutton. What do you think? I actually counted six eggs and the same number of cups of coffee that he gobbled up. The slices of bread were quite innumerable. He was a perfect dragon of Wantley at table. There was general astonishment amongst those who did not know him. A young wood-merchant from Leith, who was upon a fortnight's visit to London (and who, I think, was at the High School with you), could not keep his eyes off him.

This said wood-merchant had been very much troubled with his eyes, and wore preserves. He had been confined to a dark room for a month, and got them leeched. It was a kind of inflammatory weakness, he said, that he was subject to. Whenever it came on he immediately put himself under the care of Dr Wishart. I hope William's eyes are better, but would not advise Dr W. to be called in, if a month's confinement in a dark room was to be the result. The wood-merchant said he was actually tired of existence before he got out of the doctor's hands: not being able to read or do anything else. I pray you don't let William read this if he is not quite better. I met the young man afterwards in the Zoological Gardens and accompanied him to Colosseum, etc.

I was much amused with a new-married couple, neither man nor wife under 40, and both very stupid, the lady especially very silly, and always running about and saying, "I wonder where my husband has gone to," "Did you see my husband anywhere?" She would then hurry upon deck, and bring him down by force of arms. "My husband," however, took the first opportunity to escape, and the same scene was acted o'er and o'er to the great amusement of the observing passengers. Their cabin was not far from mine, and as I was awake most of the first night as aforesaid, I heard the lady in great alarm for the safety of the ship. She was crying out to her husband, "Do you think there is any danger?" At last she made the poor wretch get out of bed to find the Captain, who came to pay her a visit and quiet her alarm. She was pacified for a while, but gave her husband no peace. About half an hour afterwards the thundering noise of the waves and the engine, combined with rolling of the ship again excited her fears. Again her husband had to seek the Captain; but the C. had gone to bed. The lady was so horrified at this that she got out of her bed and started along the passage to get to deck. As she went along the ship gave a great lurch and she came tumbling against my door with a horrible noise. I declare I was so angry at the disturbance she and her husband were making that I felt inclined to get out of bed and take a stick to them. The morning was good, but foggy, and as I paced the deck before breakfast the said gentleman came and joined me. I hoped, of course, he had rested well. He looked most woe-begone at me and answered he had not got any rest at all: he had enough to do quieting the fears of "that lady below" (so he always denominated his wife).

The next character of any notoriety was the missionary. He wished to convert me to become a member of what he called the Verse Society—perhaps you know about it. The members, in whatever part of the world they may be, read every morning the same verse of the Bible, and think upon it for about a quarter of an hour: 365 verses are printed in a small book every year, and given to the members—about 30,000 in number, he said. His sanctified looks and affected language quite disgusted me. He had been lately travelling all over the United States, establishing missions, etc. He showed me a whole portfolio of letters from various pious and distinguished males and females. Every letter he came to, he introduced it to my notice by saying, "This is from, etc., much distinguished by his (or her) writings in the great cause of our Master Jesus.

You will probably have heard much of him (or her)." I was obliged to confess my ignorance of all without exception, and could scarcely refrain from telling him that the letters appeared to me, as they really did, canting abominations.

I have given you a sketch of the worthy Professor as he sat on a sofa reading the *Pomonia* or *Hortus Britannicus*, a description of British fruits.* His appearance was much more ridiculous than in the sketch. Indeed he was so like Punch, as Cruikshank makes him, that I and a fellow-passenger involuntarily burst out laughing to think of him with fool's cap and baton. I had several conversations with him upon his travels, etc. There was one characteristic observation he made which showed no respect for the Bible. He was talking of the Zoological Gardens as the finest sight in London, and concluded by remarking that "he doubted much whether friend Noah had such a fine collection in his Ark, or had them so well taken care of," grinning in my face with such a look of intelligence and comicality that (you must excuse me) I could not but join in his mirth.

We did not see the coast till we came to the headland south of the Wash, when I had a view of the coast through an excellent telescope. Towns, villages, windmills, and bathing-coaches all passed the object glass as we scudded along, and so distinct that it had quite the effect of a panorama. We dropped anchor at the mouth of the Thames on Thursday evening, so that we had an excellent view of the banks as we sailed up in the morning. They much surpassed my expectations, though there was a terrible want of hills. Sir John, wood-merchant, missionary, another gentleman, and myself went up from Greenwich to London Bridge in a barge. You know the rest. When I came in sight of the Tower, Custom House, St Paul's, and the bridges, I thought from the views I had formerly seen of them that I recognised old acquaintances. I may say that I never saw any prospect more grand than the view of St Paul's, Thames, shipping, and bridges, from the middle of the new London Bridge in a clear moonlight night. There was little or no smoke, and the clear sky, spangled with stars, contrasted in such a remarkable manner with the magnificent city and river, lighted up by the moon while the utmost stillness prevailed, that the effect was quite overpowering. I should like much to be on the top of St Paul's on such an evening.

* In an inset of the letter there is an amusing pen and ink sketch of Leslie.

Well, I must conclude. I hope next time you write (and let it be soon) you will give me all the Edinburgh gossip. I see the *Scotsman* regularly, which is one comfort. I observe the cholera is very bad in Edinburgh just now; it is even worse here, I believe, though the papers say nothing about it. They dismissed all the prisoners from the jail the other day, it was so bad among them. There are six or seven confined with it next lane to us, but we are proof against it from drinking plenty of port wine.

I heard Mr ——'s third sermon. I cannot give you any very flattering account of his qualifications. He is by no means eloquent, but as his head is a perfect Cruden's *Concordance* he is very clever at the parallel passages. Michael and Robert Faraday were made deacons the other week. I spent an evening with Henry Bell, who gives the most extraordinary accounts of the former gentleman. If all is true, I shall certainly revere him, not only as the first chemical philosopher in the world, but, likewise, as one who is quite unmoved by all the honours that have been heaped upon him. Davy behaved very meanly towards him in opposing his entrance into the Royal Society when President. His behaviour was generally scouted by the members. Faraday has gone from town just now, so I will not be able to cultivate his acquaintance for some time.

Have you had any word of my books, etc., in Kirkintilloch? I feel the want of them much here. I hope they are forthcoming. You will perhaps see Martin. Pray cabbage all the news you can from him. I hope Aunt Shaw is quite better. Aunt Leighton is always anxious to hear about them. So pray follow my example and fill your sheet when you write. Yours most affectionately, J. J. WATERSTON.

Soon after his arrival in London Waterston became, through an introduction from his grand-uncle, a pupil in the office in Westminster of Mr James Walker, F.R.S., a leading civil engineer and President of the Institution of Civil Engineers. For two or three years he was employed in drawing and surveying work in connection with the rapidly developing railway system of England, and with docks, lighthouses, etc. He became an Associate of the Institution of Civil Engineers, and contributed a useful technical paper to the first volume of the *Transactions* of that body on a graphical method of estimating the earthwork in embankments and cuttings. But

he seems to have realised very soon that his heart was in pure science, and that if he continued as a civil engineer all his time would be taken up with professional work. He therefore, to the disappointment of Mr Walker, applied for, and ultimately obtained, what was for him a very easy post in the Hydrographer's Department of the Admiralty. The pay was modest, but enough for him to live on; and the easy work left him the leisure he was seeking for, and of which he seems to have taken full advantage in connection with scientific libraries to which he had access.

The head of the Department was Captain (afterwards Admiral) Beaufort, F.R.S., who formed a high opinion of Waterston's abilities, and sympathised with his scientific ambitions. On Beaufort's suggestion, and with his backing, Waterston applied, in 1839, for the post of Naval Instructor at Bombay to the East India Company's Cadets. What attracted him to this post was that while the salary was far more than he could ever hope for at the Admiralty, the work would be quite easy, and would leave him plenty of time to develop his ideas. He presented testimonials from scientific men, and was then summoned before a Board, the members of which began to ask him questions about navigation, of which at the time he knew nothing. As things were becoming awkward he finally said, "Gentlemen, I did not come here to be examined. I refer you to my testimonials." He was appointed, and, in the long voyage in a sailing ship round the Cape, had ample time to make himself familiar with the details of what he had to teach.

At first he lived on board a ship in Bombay Harbour, and later on Butcher's Island in the Harbour, along with Captain (afterwards Admiral) Jenkins, with whom he was on terms of close friendship. His life there appears to have been a very happy one, and gave him the leisure he wanted. He was also able to save a good deal of money (about £400 a year according to one of his letters). In a letter, in 1856, to his brother William he said: "My appointment has answered well for me in giving me leisure, and no anxiety, care, or responsibility, at the same time enabling me to lay up a little for my old age. I fitted the situation well, and

it has answered my purpose." From the library of the Grant College, Bombay, he could obtain scientific books and journals; and he mentions the help given him by Dr Haines, the Professor of Chemistry. His study was in a guarded building where ammunition was stored, and which was surrounded by an old moat infested by dangerous snakes. He thus had plenty of peace. Of the snakes he writes very cheerily to his brother Charles in 1849: "As yet, the snakes have had the worst of it. We have bruised many of their heads, but no heels have as yet been damaged."

All the cadets passed through his hands for instruction on the theoretical sides of navigation, gunnery, etc. The story is told of him that once when he was passing a battery used for practice, the cadets, with whom he was very popular, asked him to lay a gun. He did so, and, by good luck, hit and sank the target. He turned round and said: "Now you see what science does"; but he took care never to lay a gun again!

The only break in his service in India was caused by a newly-appointed senior officer ordering him to read daily prayers to the cadets, at the same time perhaps offering him the emoluments of this office. Reading prayers was no part of his duty, and he refused. He was a most scrupulous and honourable man, whom nothing would have induced to appear to be identifying himself with what he did not believe. The refusal to read prayers was followed by various acts of petty retaliation on the part of his superior officer, who seems to have been lacking in the qualifications required for his post. This led to Waterston's resignation. He returned home, but was very soon re-appointed, after which he had no further trouble.

The influences of his early studies as a civil engineer, and of his familiarity with the class of calculations needed in navigation and gunnery, are evident in many of Waterston's papers. For instance, he makes free use of graphical methods, and he compares molecules with projectiles. His mathematics are also more of a practical than of an academic type.

During his stay in India he forwarded home the manuscript of a short book and several scientific papers. The book must have been occupying his attention for many years, and was

doubtless the outcome of the anatomical and physiological studies which he began in Edinburgh. It seems pretty certain that during the time he spent in London he was mainly thinking and reading about the physiological and psychological problems discussed in the book. This probably accounts in part for his not having come into closer personal touch with Faraday and other physicists.

The book bore the somewhat uninviting title of *Thoughts on the Mental Functions*, and was anonymous. It was published in Edinburgh by Oliver & Boyd in 1843. So far as can be ascertained it fell absolutely flat and soon became unobtainable. The year 1843 was the date of the "Disruption" of the Scottish Presbyterian Church, and probably this was unfortunate for the book, which might otherwise have at least attracted attention on account of its apparently materialistic tendencies. The body of the book is reprinted at the beginning of this collection; but as two remarkable "Notes" in it contain the first sketch of the dynamical theory of gases and corresponding theory of temperature and heat, they have been placed along with the other papers relating to gases.

The book is a very acute essay, far ahead of its time, on what would now be called physiological psychology, or the physiology of the central nervous system. The general philosophical standpoint adopted appears clearly in the Preface and Introduction, and in the sub-title, "An attempt to found Metaphysics as a branch of the Physiology of the Central Nervous System." Waterston, like nearly all the scientific men of his century, started from the assumption that the physical world, as described in terms of the fundamental principles laid down by Galileo and Newton, forms part of reality. This was the "Common Sense" view of Thomas Reid and the dominant "Scottish philosophy" of Waterston's university days; and what he attempted was to work out the implications of this scientific and philosophical assumption when it is applied to human behaviour. The idea which guided him was that human behaviour can only express itself in material changes, which must, in so far as they are intelligible, be dependent on previous material changes.

Waterston defined truth as "the consistent relations of

things." Clearness or consistency was to him, as it was to Descartes, and as it ultimately is to everyone, the test of truth. He assumed, with Reid, that our conception of physical reality, as handed down from Newton, is clear and consistent with the rest of our experience; and in the course of the book he extends this conception, as far as possible, to human behaviour. "Truth," he remarks, "is equally lovely, equally estimable, equally to be sought after, whether it is concealed under the grossness of animalism or interwoven with the intellectualism of genius. Let us not shrink from conclusions because they do not flatter: once satisfied of their consistency let us embrace them as approximations to the true course of nature—to reveal which is the proper aim of all abstract disquisition, and the great end to which the aspirations of the philosopher ought to be directed." In this passage we recognise the spirit which has animated all the greatest men of science, including, for example, Charles Darwin. The test of consistency is one which necessitates thinking, and it is his greatness as a scientific thinker that makes Waterston's career so interesting.

What may at first strike a reader of the essay is the imperfection of the conceptions current in Waterston's time of the structure of the central nervous system as later revealed by the microscope and by experiments on cerebral and spinal localisation. He also speaks with considerable respect of phrenology, which at that time had a great popular vogue, comparable to that of the "physiological" psychology which is so popular at present. His reasoning, however, is affected neither by the meagreness of existing anatomical knowledge, nor by the extravagant conclusions of phrenology. He simply attempts, as indeed Descartes in his *De Homine* had already done, to see what kind of mechanism the human nervous system can be, though the attempt led him to realise far more clearly than Descartes did the fundamental difficulty of the problem as thus stated.

After preliminary matter Waterston enters in Chapter III. on a discussion of the possible physical basis of Association, which Hume and other psychologists had used as a cement in building up their psychology. The chapter, which is not easy reading, is a struggle with this problem; and the difference

between Waterston's treatment of it and that of others who, before and after him, have approached the same problem from the same standpoint, is that he thoroughly realised what he had set out to explain in physical terms. At the end of the chapter he found himself forced to write: "The faculty of association has been elaborately treated of in the preceding chapter, and reduced by analysis to the single principle of simultaneousness of excitement. Every exertion was made to penetrate this apparently simple law, but without success. It involves a power of registering successive action, which, as there is nothing in physics the least analogous, must be viewed as a primary organic principle—a characteristic of organisation." He had just pointed out that similar difficulty is met with in the endeavour to form a physical conception of hereditary transmission, and he remarks that "we are led to expect that if molecular philosophy is ever destined to advance into the region of organisation the phenomena of perceptive consciousness will admit of being applied to illustrate the physical aspect of the elementary powers of matter." These words are almost prophetic of relativity physics.

Waterston seems to have had no suspicion that the difficulties of the association theory might be due to an error in Hume's assumption that conscious experience consists essentially of separable units of sensation which require some external process of association to bring them together. Though Kant had been dead for forty years his work was still almost unknown in this country. The idea that the physical world, however wonderful its molecular "powers" might turn out to be, could be anything essentially different from what corresponded to the physical philosophy of Galileo and Newton was also far from Waterston's thoughts; and he never suspected that the current physical conceptions of matter and motion, and even of time and space themselves, would before very long be proved by actual physical experiment, and regardless of biological or psychological phenomena, to involve difficulties as great as those involved in the attempted application of current physical principles to the physiology of generation, or of the central nervous system.

In his next chapter Waterston discusses the nature of what, viewed from the physical standpoint, must be happening in the

central nervous system during voluntary action. He does not fob us off in the usual manner with tedious anatomical descriptions and irrelevant mechanical analogies, but goes straight to the main facts, and points out that a voluntary act implies an exact co-ordination, varying indefinitely in character according to the positions from and to which a bodily part is moved, of an immense variety of motor and sensory nervous impulses, proceeding from and to many parts of the body. The motor influences are continuously guided and determined by innumerable afferent nervous influences, of which the nature is determined by relation to all the other afferent influences of touch and the other senses. The mysterious "principle of association" extends, therefore, to volition as well as to mere perception; and the physical aspect of this association is as difficult to interpret in terms of current physical conceptions as its psychological aspect. He dismisses with contempt "the old system of explaining organic phenomena by mechanical arrangements—the miserable shifts of ignorance, and only to be excused in an infant state of the science." Men still speak glibly of the mechanism of nervous action or of heredity; but what meaning can be attached to the word "mechanism" in these connections? Waterston realised the significance of this question.

The remaining chapters on "Hearing and the Voice," "Sympathy and Expression," and "The Organology of Dr Gall," are full of acute constructive criticism, and contain a number of original observations on the physiology of the senses.

Waterston saw clearly that the problems of life and conscious behaviour are bound up in those of molecular or atomic behaviour. We may thus say that they are problems of biochemistry, to use a recently coined word. But he also saw that this conclusion by itself takes us very little further, as the behaviour of the molecules and atoms of biochemistry carries us into what seems a quite different world from that of ordinary physics and chemistry.

A physiologist cannot but feel regret that Waterston never followed up this remarkable essay, which indicated clearly the real facts that must be faced in any adequate physiological treatment of the central nervous system, or psychological

account of conscious behaviour. What diverted him appears clearly in the Notes appended to the book, and now reprinted as the first of his papers relating to heat and the kinetic theory of gases. Near the beginning of the first Note he writes: "We derive from the physiology of the sensori-motor phenomena of the animal system, more perhaps than from any other branch of science, high ideas of the importance and refinement of molecular powers and arrangements. In whatever way we examine the phenomena of nature, molecularity is the grand terminus to which all our investigations lead—to which all efforts and discoveries necessarily tend, and upon which they are now becoming more rapidly concentrated. If we were to contemplate only the extraordinary complication and refinement of molecular arrangement in organised structure we might have reason to despair of ever arriving at a knowledge of their essential nature; but on the other hand the symmetry of the laws of chemical combination—of the habitudes of elementary substances, with heat, light, and electricity—encourages the hope that all may yet be brought under the dominion of the exact sciences. Although the attempts that have been made in the several departments of molecular philosophy have not as yet been successful in throwing down the barriers that separate them from one another, yet they so far demonstrate that the principle of dynamical sufficiency pervades all, and show the possibility of uniting them in one scheme of mechanism."

We may well agree with Waterston that if we fully understood the nature of what we call a molecule, we should have understood the nature of the whole of our mysterious universe. In reality, however, he had recoiled, baffled, from this deeper problem as it meets us in physiology and psychology. His efforts were now directed to phenomena which seemed to him far easier to attack on ordinary mechanical lines than the phenomena of life, and he never went back to physiology or psychology in any of his later writings, but devoted himself almost entirely to what he rightly called molecular physics, though it is now called physical chemistry and thermodynamics. He found that in this domain he could apply with extraordinary practical success the Newtonian principles which he

had inherited through the Gregorys and Leslie. The kinetic theory of gases, and corresponding theory of temperature and heat, as formulated by him, constituted a new and striking triumph for these principles.

The two Notes show the genesis in his mind of the theory of gases and of heat which was set forth much more fully and perfectly in the famous paper which he sent to the Royal Society in 1845. He was aiming at establishing on fundamental mechanical principles, a theory of the behaviour of gases and of heat, and his later work was mainly devoted to attempts to extend the theory to liquids. It is interesting to note that Waterston's book appeared in the very year (1843) in which Joule read his first paper on the mechanical equivalent of heat.

The theory current at the time, and almost universally accepted, was that there is a distinct "element," caloric, which is "imponderable" or without sensible mass, and may exist not only in the "manifest" form as sensible heat, but also in the "latent" form. Manifest caloric was assumed to possess the peculiar property of being attracted by ordinary matter, but repelled by itself in inverse proportion to distance (not the square of distance). Hence the molecules of a gas or other substance containing manifest caloric repel one another in direct proportion to their concentration, in accordance with Boyle's law, while they also repel one another in proportion to the amount of manifest caloric which they contain, in accordance with Charles's law. Absolute zero, on this theory, would represent a state in which there was no manifest caloric in the molecules; and since a given volume of gas was found to lose (according to the existing data) one 274th of its pressure at 0° C. for each degree of fall in temperature, the absolute zero of temperature is presumably −274° C. or −461° F. The existence of latent caloric was held to be proved by Black's experiments on latent heat; and it was further assumed that when rise of temperature is produced by friction or compression this is due to the "liberation" of latent caloric, while the cooling which accompanies expansion and other processes is due to caloric becoming latent. It is quite a mistake to suppose that experiments on the "liberation" of heat by friction, etc., or even Joule's experiments, had upset the caloric theory.

The caloric theory had often been attacked—for instance by Rumford, by Davy in his earlier days, and by Herapath, but had successfully held its ground, and was a main basis of the mathematical physics of the time. The theory that heat is molecular motion had not hitherto been successful in giving any connected account of the behaviour of gases. The hold which the caloric theory possessed is well illustrated by the fact that Thomson (Lord Kelvin), who had recently been appointed Professor of Physics at Glasgow, and was generally regarded as one of the most progressive of the younger physicists, wrote as follows of the caloric theory in 1848, five years after Waterston's book was published, and in full knowledge of Joule's experiments, which had meanwhile appeared.* "The conversion of heat (or caloric) into mechanical effect is probably impossible, certainly undiscovered. In actual engines for obtaining mechanical effect through the agency of heat, we must consequently look for the source of power, not in any absorption and conversion, but merely in a transmission of heat." It was not till four years more had elapsed that Kelvin was converted to the dynamical conception of heat; and, as we shall see, even then, and to the end of his life, he remained partly under the influence of the caloric theory.

The expressions "imponderable" and "latent" were very convenient and soothing, like many other expressions which come into currency among scientific men. The caloric theory also lent itself admirably to mathematical treatment; and this of course helped to make it one of the soft pillows on which scientific progress so often comes to rest.

The great advance which Waterston made in the two Notes and his subsequent paper submitted to the Royal Society in 1845, two years later, was in pointing out that as regards ideally perfect gases, we can dispense entirely with mechanically unintelligible assumptions (imponderability and action at a distance) in connection with the theory of gases and temperature. We have only to assume that a gas consists of extremely minute, free and perfectly elastic molecules of negligibly small volume, travelling with enormous velocity and constantly colliding at all possible angles with one another and the

* Thomson, *Proc. Cambridge Philosophical Society*, 1848.

similarly elastic sides of any containing vessel. It then follows on purely mechanical principles that the pressure or "elasticity" of the gas depends upon the translational *vis viva*, or in more modern language kinetic energy, of the molecules, and on their concentration. Thus we not only obtain a simple mechanical explanation of Boyle's law that at constant temperature the pressure of a gas varies directly as its concentration, but also see that, if we regard heat as molecular *vis viva*, and absolute zero as the temperature at which molecules have no relative motion, the pressure of a given mass of gas varies as its absolute temperature; and since *vis viva* varies as the square of velocity, the absolute temperature varies as the mean square of the velocities of the gas molecules. In this way the empirical law of Charles, that the pressure of a gas at constant volume varies directly as its temperature, follows on mechanical principles.

From the known laws of impact of elastic bodies it follows further, as Waterston argued, that in equal volumes of gas, at the same pressure and temperature, there are (always assuming that the volume occupied by the substance of the molecules themselves is so small as to be negligible) equal numbers of molecules, whether these molecules are relatively heavy or light, large or small. The heavy molecules must move so much more slowly than the light molecules that the mean *vis viva* (half the mass multiplied by the square of the velocity) of either sort of molecule is the same, and the same number produce the same pressure in the same volume at the same temperature. In this way Avogadro's law follows from the theory, and Dulong and Petit's law of atomic specific heat becomes mechanically intelligible. It also follows that the mean velocities of the molecules of different kinds of gas vary inversely as the square roots of their molecular weights. This corresponds to Thomas Graham's empirically discovered law of the velocities of diffusion in different gases.

The statement of the theory in the Notes contains a somewhat serious error, corrected in the Royal Society paper. Somehow or other Waterston had made the mistake of inferring that molecules of different weights travel, on an average, at rates inversely proportional to their weights, instead

of, as his theory demanded, to the square roots of their weights. He could hardly have made this mistake had he been aware of Graham's experiments on the rates of diffusion of gases. These experiments were published in the *Transactions of the Royal Society of Edinburgh for* 1834, and Waterston does not seem to have yet known of them when he completed his book in India. He had perhaps been too deeply engaged in physiological and anatomical studies. In the course of the two Notes, of which the second was written some time after the first, we witness the gradual development of his general conception of molecular physics in Waterston's mind. Among many other things the Notes contain the first approximate estimates of the actual mean velocities of molecules.

From Waterston's earlier physiological writings we can appreciate the significance of the title of the paper he wrote for the Royal Society. The title was "The Physics of Media composed of free and perfectly elastic molecules in a state of motion." No one knew better than he did that actual molecules, as shown by their association in liquids, solids, chemical compounds, and, more particularly, living organisms, do not seem to behave as if they were free from one another's constant influence; but in permanent gases the molecules seem to behave, in the main, as if they were nearly free. He therefore proceeded to consider how they would behave if they were actually quite free, as well as perfectly elastic. His "media" were thus ideal media, and it was expressly of these ideal media that his paper treated. As soon as he had completed it he went on to attack the less simple problem presented by "imperfect" gases and the liquid state.

Not only did Waterston's book pass unnoticed, but the very carefully written paper which he sent to the Royal Society, through his old friend Captain Beaufort, was rejected as not worthy of publication by the two referees appointed to examine it. The story of its rejection is told by the late Lord Rayleigh in his Introduction, reprinted at page 207 of the present volume. In accordance with the rules of the Society the manuscript became its property, and was preserved in the Archives, where Lord Rayleigh, who was then Secretary, discovered it nearly

fifty years later, and at once had it published in the *Philosophical Transactions*, along with a very appreciative introduction and careful notes by himself. The very short abstract published in the *Proceedings of* 1846, and republished by Lord Rayleigh, was probably not written by Waterston himself; and his name is even misspelt, as Lord Rayleigh notes.

In view of the long previously expressed contentions of Rumford and Davy that heat is a "mode of motion," and considering that Joule had already, in 1843, published his first paper on the mechanical equivalent of heat, the rejection of Waterston's paper may seem to us very surprising, and even suggestive of great carelessness on the part of the referees. But if we feel inclined to throw stones at the referees we should do well to consider that we ourselves, to judge from all past experience, live in glass-houses. The caloric theory was based on careful and striking experiments and calculations, including those of Newton, Black, and Laplace. It was also interwoven with the whole framework of the accepted mathematical physics of the time. To judge from Lord Rayleigh's account of their reports, the referees, or at any rate one of them, had performed their duty carefully and conscientiously. They were not merely acting as private individuals, but as representatives of a Society which would certainly have rejected by an overwhelming majority Waterston's revolutionary reasoning. He sent in the paper after he knew that his book had been a failure; but doubtless he remembered that it was the Royal Society which brought about the publication of even such a revolutionary book as the *Principia*. The Royal Society was now, however, a highly organised body, and its psychology had become to a large extent that of a crowd, albeit an educated though highly specialised crowd. The position of the referees was as difficult as that of the inquisitors who had to deal with Galileo's writings; but their action was far more effective than that of the inquisitors.

Waterston attempted to draw attention to the paper by printing, and probably circulating privately, a much shorter abstract of it. This abstract, which escaped Lord Rayleigh's notice, although there was a copy of it, presented by Waterston, in the library of the Royal Society, is reprinted at page 320 of

this book. It is not dated on the title page, but the date entered in the Catalogue is 1845, so it must have been sent along with the manuscript of the paper, and as an abstract of it.

Another paper was sent to the British Association in 1851, and of this Lord Rayleigh reprinted a short abstract, which appeared in the *Association Report* (page 318 of this book). This abstract does not seem to have been written by Waterston himself, as he could hardly have made the mistake of placing the absolute zero of temperature at "491° below the zero of Fahrenheit's thermometer." It was impossible for him to publish the original paper as, by the statutes of the Royal Society, it had become, when it was sent in, the property of the Society.

In various of his subsequent papers, published after his return from India in the *Philosophical Magazine*, he again attempted to draw attention to this first Royal Society paper, as well as to a further very important one on saturated vapours, rejected by the Society in 1852. The manuscripts were open for consultation in the library of the Society, in accordance with the statutes. To refer to them was, however, in practice only to inform the world that they had been examined by very competent and impartial persons, and found to be of little or no value. Waterston was thus fatally handicapped in the task of making his discoveries known. But for the fact that he had become so deeply immersed in the endeavour to extend to liquids the ideas which he had so successfully applied to gases and heat, he would doubtless have written a book on gases and the theory of heat and temperature. He was, however, pressing forwards towards a more complete development of molecular physics, or physical chemistry and thermodynamics.

Within about fifteen years of the rejection of his first Royal Society paper, the reasoning which had led Waterston to the dynamical explanation of the gas laws had been independently rediscovered piecemeal by Krönig, Clausius, and Clerk Maxwell, the rediscovery beginning with a paper by Krönig in 1856; and long before the end of last century the kinetic theory of gases and a dynamical conception of heat and temperature had become a commonplace in ordinary text-books.

Various difficulties in connection with Waterston's reasoning

have been cleared up. One of these is specially referred to by Lord Rayleigh, who pointed out that through an arithmetical mistake Waterston had calculated the ratio of the specific heat of a gas at constant pressure to that at constant volume as 1·33 (which was close to some imperfect existing determinations) instead of 1·67. This mistake led him to think that energy of rotation could be neglected in the kinetic theory of gases. The actual ratio for air as determined experimentally is, however, 1·405. When Lord Rayleigh wrote there was no explanation of why air should give this ratio. A partial explanation was, however, given by Boltzmann in 1895. Boltzmann, who was led to his conclusion by Willard Gibbs's reasoning about degrees of freedom of movement, pointed out that in diatomic gases, such as air, the observed specific heats show that the gas absorbs heat-energy in the form of molecular rotation in only two of the degrees of freedom of rotational movement; also that some energy is absorbed in respect of intramolecular disturbance. We can thus explain the apparently anomalous ratio of the specific heats in air, and at the same time bring the specific heats of gases into definite relation with the atomic specific heats of solids. In monatomic gases the ratio of the two specific heats is 1·67; and Waterston had treated gases as if they were all similar in properties to monatomic gases, such as argon, helium, or mercury vapour. But that monatomic gases do not take up energy of rotation in proportion to rise of temperature, and diatomic gases do so in respect of only two degrees of freedom of rotation, remained still anomalous until the advent of the quantum theory and corresponding recent revolution in fundamental physical conceptions.

Waterston's mistake about the two specific heats for air led him also to miscalculate the mechanical equivalent of heat. His own calculation of what he took to be the two theoretical specific heats gave him a value about a seventh lower than Joule's experimental result, and he thought that Joule's experimental value might be too high (p. 241). It was the basis of his own calculation that was wrong, though the principle of it was correct; and it gives Joule's result almost exactly when 1·405 is taken as the correct ratio. The same error confused his arguments, repeated in a subsequent paper, as to the relation of

the kinetic theory to the velocity of sound. The striking fact remains, however, that he showed how to deduce the mechanical equivalent of heat from the kinetic theory, although the deduction could not be made absolutely correct without taking into account the subsequent determination by Amagat of the deviation of air from the gas laws.

Before the rediscovery of the kinetic theory scientific ideas about heat and its relation to mechanical work done by the agency of heat had developed along different lines from those indicated by Waterston's discoveries. It is true that within a few years of the date of Waterston's paper the caloric theory was generally abandoned; and, under the influence of Joule, Mayer, Helmholtz, and others, a dynamical conception of heat came to be accepted, heat being regarded as a form of energy, interchangeable with other forms of energy. As regards, however, the conversion of heat into mechanical energy, scientific doctrine, under the lead of Kelvin and Clausius, developed along lines which had first been laid down by Sadi Carnot in 1824, and were only so far modified as to adapt, in appearance, his reasoning to a dynamical conception of heat. In this modified form Carnot's reasoning became a generally accepted basis of thermodynamics and the theory of absolute temperature.

Carnot's afterwards famous book was published in 1824, and was entitled *Réflexions sur la puissance motrice du feu.* It is short and admirably clear, its object being to give a scientific account of the action of heat-engines in the light of the then accepted caloric theory. He pointed out the fundamental fact that in the working of a heat-engine there is always a working substance which alternately expands and is recompressed to its original volume; but as the temperature of the working substance is, owing to the application of heat, higher during expansion than during recompression, the pressure which can be overcome is greater, and more work is done during expansion than during recompression, so that there is a gain in work done, or motive power, at the end of each repetition of the process. To keep the temperature down during recompression a condenser is necessary, so that heat is always lost during recompression; and this is true even if the working

substance is thrown away at the end of each expansion, as in the noncondensing steam-engine or the modern internal combustion-engine. In these cases the atmosphere acts, wholly or partially, as the condenser, and receives the wasted heat.

On the caloric theory, as adopted in Carnot's book, heat becomes "latent" during the expansion, so that the heat communicated originally to the working substance disappears from view. During recompression this heat again becomes "manifest," but owing to the presence of the condenser cannot raise the temperature, and is thrown away in the condenser, so that no heat disappears during the working of the engine. Heat at a high temperature is converted into heat at a low temperature during the working of the engine, although at the end of each cycle there is as much manifest heat present as before the cycle. Thus the work done by the engine is, on the caloric theory, due to the letting down of heat from a high to a low temperature. and not to any loss of heat.

Carnot proceeded to argue that if a heat-engine, working between a temperature communicated to the working substance and the temperature of the condenser, can be worked backwards by the same expenditure of motive power as was gained in its forward working, no more efficient engine is possible. For if there were a more efficient engine—one, that is to say, which furnished more motive power in the forward working than the reversible engine, then by allowing the engine to work forward by the more efficient method, and afterwards reversing it, we should each time gain motive power, and thus have a continuous source of new power without net expenditure or alteration of anything. This would be contrary to all experience, and to what is now known as the principle of conservation of energy. Up to this point there is certainly no flaw in Carnot's reasoning, which is simply an indirect way of stating the principle of conservation of energy.

He then proceeded to describe what he believed to be a reversible engine working between two temperatures, with air as the working substance. In this engine, which consists of an ordinary cylinder and piston driving the engine, the air is first heated by compression to the temperature of the source of heat: the piston is then allowed to expand in contact with

the source of the heat, so that the temperature of the air remains constant during the expansion: the source of heat is then removed, and the air allowed to expand further until its temperature falls to what it was at first. A condenser at this temperature is then applied, and the air compressed, remaining at the same temperature, until it reaches its original volume. It is assumed that during the cycle no heat is either lost or gained except to the condenser or from the source of heat, so that apart from this loss or gain the working of the engine is what Rankine afterwards called "adiabatic." This is the famous Carnot cycle, and upon Carnot's reasoning with regard to it an imposing structure of thermodynamical reasoning was afterwards erected, as we shall see.

When he wrote his Royal Society paper of 1845 Waterston had not seen Carnot's book; but he had seen Clapeyron's graphic representation of a Carnot cycle, and in a note to the paper (page 275 of this book) expressed his approval of this mode of representation. At the same time he pointed out (five years before Clausius did so) that the assumption that no heat disappears in the cycle of a heat-engine doing work is inconsistent with the dynamical conception of heat. More heat must be taken up from the source than is thrown away into the condenser. He also redrew the Carnot cycle, since the dynamical conception of heat enabled him to draw curves of adiabatic expansion and compression, whereas Clapeyron did not know how to draw them. The contrast between the correct graphic representation of such a cycle as Carnot imagined and the crude representations of Clapeyron and of various text-books at present current is a very striking one.

In 1848 Kelvin, who was of course quite ignorant of Waterston's paper and published book of 1843, or of how to draw the adiabatic curves, published a famous paper in the *Transactions of the Cambridge Philosophical Society*. In this paper, though he knew of Joule's papers on the mechanical equivalent of heat, and referred to them, he completely accepted Carnot's reasoning, including the caloric theory, and proceeded to develop mathematically the consequences of this reasoning. He argued that since the doing of work by a heat-engine depends upon a letting down of

temperature and not a disappearance of heat, and since, during the stage of isothermal compression in the Carnot cycle, the amount of motive power wasted depends upon the temperature of the condenser, the efficiency of a Carnot engine must reach 100 per cent. when the air cools itself by adiabatic expansion to absolute zero of temperature and the condenser is at this temperature. There can then be no throwing away of motive power during the isothermal compression. The percentage efficiency of a Carnot engine thus gives us a scale of absolute temperature. If T is the absolute temperature at which heat is supplied to the engine during isothermal expansion, and $T_1$ the absolute temperature of the condenser, the efficiency of the engine is $\frac{T-T_1}{T}$, so that $T_1=0°$ when the efficiency is 100 per cent.

The next step in the development of this reasoning was taken by Clausius in 1850, and a few months later by Kelvin himself independently. The experiments of Joule demonstrating a "mechanical equivalent" of heat, and the writings of Mayer, and later Helmholtz, on the conservation of energy, had begun to penetrate the mathematical screen behind which the defects of the caloric theory were concealed. Both Clausius and Kelvin adopted the view that heat is a form of energy, though they were as yet quite ignorant of the dynamical theory of gases. They still adhered to Carnot's reasoning about his reversible engine; but a dynamical theory of heat carried with it, as they now pointed out, the conclusion that when a heat-engine does work an amount of heat disappears which is equivalent to the net work done in the cycle, so that less heat passes into the condenser than is communicated to the working substance. If, however, as they believed, Carnot's reasoning about his reversible engine was still correct, apart from his mistaken interpretation on the caloric theory, it followed that the efficiency of a Carnot engine is proportional to the relative letting down of heat from a higher to a lower absolute temperature, and that Kelvin's previous conception of absolute temperature as being defined by the percentage efficiency of a Carnot reversible engine still holds good. With the temperature of the condenser at absolute zero no work

would, according to Carnot's conception, be done during isothermal compression in a Carnot engine; and as all the heat-energy communicated from outside to the engine would at absolute zero have been converted into mechanical energy the thermal efficiency of the engine would be 100 per cent.

Waterston had defined absolute zero as the temperature at which the molecules of a substance are at rest relatively to one another. It is significant that in his later papers he ceases to speak of absolute temperature, and uses the expression "gas-temperature" or "G-temperature," since the variations in pressure of a fixed volume of perfect gas, consisting of "free and perfectly elastic molecules," would indicate degrees of this temperature. He probably wished to distinguish clearly his conception of absolute temperature from that of Kelvin, not believing in either the latter's scale of absolute temperature or his thermodynamical reasoning.

There is no published paper or surviving manuscript of Waterston's to show what he thought of Carnot's and Kelvin's thermodynamical reasoning, but there can, I think, be no doubt that he considered this reasoning quite unsound. In the short biography of Waterston, written for *The Clan* by his nephew, George Waterston, there occur the following passages: "J. J. W. often conversed on scientific subjects, but any mention of the Royal Society led to very strong and contemptuous language, which we thought a little eccentric;" and "he showed a restlessness and dislike at the mention of scientific men, except Faraday, and he used very strong language in respect to some who bulk largely in public estimation."

When his paper of 1845 was discovered and published by Lord Rayleigh, it was very naturally supposed by his relatives that his grievance against the Royal Society was due to the rejection of his early papers. But it was evidently a present, and not a past, grievance that was irritating him. He cared singularly little about priority, and in this he was true to his Glasite upbringing. It was of Kelvin and those who followed him, including Clausius, Clerk Maxwell, Tait, and almost all the leading physicists of the time, that he was critical; and they had nothing to do with the rejection of

his paper, and had accepted with one accord the kinetic theory of gases. All of them would also, like Lord Rayleigh, have hastened to give him full credit had they known of his paper.

I think there can be little doubt that what he did not believe in was their thermodynamical reasoning. If he did not believe in this he would certainly have written on the subject as soon as he had completed the papers, which will be mentioned below, on liquids. But his published papers suddenly ceased about 1868. In his will he mentioned specially a manuscript which could not be found after his death. This manuscript was probably on thermodynamics, and it is also probable that it had been refused publication in a scientific journal. As his views were at variance with those which had been accepted by all the younger physicists of the time, and the rejection by the Royal Society of some of his papers was known, he had probably been set down as an old Anglo-Indian "crank," possibly suffering, as was actually the case, from the effects of sunstroke. This would be very irritating to him, and would explain the somewhat violent language which he used.

It remains to consider what were Waterston's probable objections to the thermodynamical conceptions of Kelvin and his followers. We can put Carnot's original argument into the form of a syllogism in the following manner. No heat-engine can work more efficiently than when it works reversibly: the Carnot heat-engine is reversible: therefore there can be no more efficient engine working between two temperatures than a Carnot engine. If we accept this reasoning, which assumes rightly that a gas-engine does not work in proportion as the temperature of the gas is lower during compression, it follows from the description of the Carnot cycle that the efficiency is proportional to the relative difference in absolute temperature between isothermal expansion and compression, so that the amount of work done by a given amount of heat supplied to the engine varies with the relative drop in absolute temperature in the cycle of the engine.

Let us examine the syllogism in the light of the dynamical theory of heat, and at the same time of temperature, as

formulated by Waterston in his paper of 1845. For the reasons given by Carnot, and reinforced by the general formulation of the law of conservation of energy, the first premise cannot be attacked; but it would be preferable to put it into the form that if there is such a thing as a reversible heat-engine there can be no more efficient engine.

As regards the second premise the position is very different. On the dynamical theory of heat and temperature as formulated by Waterston the doing of work by the Carnot engine during the expansion stage is due to the impact of innumerable perfectly elastic molecules on the perfectly elastic surface of the piston. Each molecule returns from its impact against the moving piston with less kinetic energy than it possessed before the impact. Hence the layer of working substance next to the piston is cooled, and the cooling extends to a constantly diminishing extent to the source of heat. During isothermal expansion the temperature of the gas cannot be even. It is only the mean temperature of the gas which is even, and which determines the pressure of the gas with any given mean concentration of its molecules. But this temperature must be lower than that of the source, otherwise no heat would pass, since where an innumerable number of molecules in chaotic movement are concerned the chances are practically infinite that no measurable heat-energy would, on a balance, pass.

It was long ago recognised, particularly by Clerk Maxwell, that a Carnot engine cannot, strictly speaking, be reversible; but it was urged, at the same time, that if the engine was made to work slowly enough, as could be effected by attaching to it a very heavy flywheel revolving very slowly, the engine could be made *practically* speaking reversible, so that its thermal efficiency would practically speaking be $\frac{T - T_1}{T}$. There is, however, a factor which has been entirely neglected in this reasoning. In the working of the engine heat is taken up and given off, not merely by the gas, but also by the cylinder walls, since they, like the gas, consist of molecules. The result of this is that in part of what would otherwise have been adiabatic compression and all of isothermal expansion heat is being taken up by the walls of the engine, while during most of what would other-

wise have been adiabatic expansion and the whole of what would otherwise have been isothermal compression heat is being given off by the walls. The temperature of the gas, and consequently the work done, during expansion is thus diminished, and the temperature and work done during compression is increased. Hence with a slow enough motion of the piston and a fixed temperature of the source of heat and condenser large quantities of heat would be absorbed uselessly by the walls, and passed on to the condenser, so that there would be no limit to the thermal inefficiency of such an engine, even if it worked without friction and was so effectively lagged that no appreciable quantity of heat escaped to the outside. So far from the thermal efficiency of the engine being increased by making it work very slowly, the thermal efficiency would be greatly diminished, and the engine would become still further from being reversible.

On the other hand it is equally evident that the thermal efficiency would be indefinitely diminished by letting the engine work very fast, as when it had an insufficient load; for the temperature of compression would approximate to that of expansion, so that in this case also a very large proportion of the heat drawn from the source would be passing uselessly into the condenser. It is clear that in order to get the greatest thermal efficiency out of the engine the piston-rate must be such that as little time as possible is given for exchange of heat between gas and cylinder walls without the rate becoming so great that diminution of efficiency owing to increase of rate outbalances gain in efficiency owing to diminished exchange of heat with the cylinder walls in each cycle.

It is thus perfectly evident that the Carnot syllogism has broken down completely. Neither the Carnot engine nor any other heat-engine of any sort or kind is reversible: nor does its efficiency vary merely with the relative difference in absolute temperature between source of heat and condenser: nor can the efficiency be increased indefinitely by increasing this relative difference in absolute temperature. It may be added that the maximum efficiency of a steam-engine, in which we have to take account, not merely of the kinetic energy present in the steam as heat, but also of potential energy, does not vary with temperature in the same way as in a gas-engine.

Quantitative inferences as regards the maximum thermal efficiency attainable in heat-engines with various differences in temperature between source of heat and condenser, and various working substances, are discussed in the writer's book on *Gases and Liquids*, published along with this book.

When we analyse the reason why a heat-engine is not reversible we find that the reason is that heat-energy is chaotic energy, as Waterston pointed out. If it were not chaotic we could have reversible heat-engines; and heat from a body at a lower temperature might even pass directly into a body at a higher temperature, if, indeed, we could speak of temperature with non-chaotic molecular energy. The dynamical conception of heat is an extension to molecules of Newton's mechanical conception of the physical world; and a proper consideration of the possible thermal efficiency of a heat-engine enables us to realise that this mechanical conception implies that the energy of the physical world is not merely chaotic, but must be constantly running down towards heat at a uniform temperature. There is no need, however, to ascribe this inference to the operation of a mysterious empirical principle under the name of the "second law of thermodynamics."

Carnot's very ingenious conception of a reversible heat-engine has continued to mislead both physicists and mechanical engineers for a century; and apparently the one man who saw through the fallacy of this conception was Waterston, though unfortunately we have no definite record of his criticisms, which seem to have been effectively suppressed during his lifetime. It is strange that not even Clerk Maxwell saw the fallacy: and perhaps nothing is a better illustration of the overbearing influence which authority often exercises in scientific questions. Kelvin's scientific authority appears to have been unduly enhanced by his early appointment to an important Chair, and by the various honours which were subsequently conferred upon him.

Enough has been said to show what Waterston probably thought of the generally accepted thermo-dynamical teaching of the latter part of his life. He was of course wrong, however, in specially identifying this teaching with the Royal Society, instead of with various of its prominent Fellows and

many others not connected with it. He probably regarded the so-called Second Law of Thermodynamics as nothing but a lame attempt to neutralise, by means of an unintelligible empirical principle, the error of the Carnot reasoning. This law, as formulated by Clausius, in 1851, was that "it is impossible for a self-acting machine, unaided by external agency, to convey heat from one body to another at a higher temperature." It follows from the dynamical theory of heat and temperature, as formulated by Waterston, that heat-energy cannot communicate itself from one body to another at an *equal* or higher temperature except by doing work upon it, when this is rendered possible by work previously done, as when previously compressed air communicates its heat-energy to air at the same or a higher temperature by compressing it.

Except in connection with his analysis of physiological and conscious behaviour, Waterston was a thorough Newtonian. His work in physics was simply an extension of Newtonian principles to the laws of behaviour of molecules. In his theory of heat, temperature, and the behaviour of a perfect gas he had laid the foundations of Newtonian molecular physics, and he had no patience with thermodynamical teaching which diverged, and quite unnecessarily as he thought, from Newtonian conceptions. His wrath against contemporary physicists seems to have been like that of a Hebrew prophet against those who had given themselves up to the worship of graven images. His faith was the Newtonian faith: the Carnot Cycle and the "Second Law" of thermodynamics were the graven images.

The question apparently at issue between Waterston and his contemporaries is not only of the greatest scientific importance, but on opinions with regard to it there depends the practical design of heat-engines, and decisions as to the comparative thermal efficiencies and other comparative merits of steam-engines and internal combustion engines. One of the reasons for publishing Waterston's collected papers is their fundamental bearing on these questions.

On examining Waterston's various discussions of the possible nature of gravitation, it will be seen that in his mind the dynamical theory of gases and heat realised only a part of

his ideals for the extension of the Galilean and Newtonian conception of physical reality. In his theory of gases and heat he had got rid of substances without mass, and partly got rid of action at a distance and of what had hitherto been called latent heat; but he was still, like Newton himself, searching after a mechanical explanation of the apparent action at a distance in the cases of gravitational, molecular, chemical, and electrical "attraction." He did not realise that in assuming the existence of perfectly elastic molecules he was in reality assuming the existence of action at a distance.

In 1842, a year before the publication of Waterston's book, Mayer, who, as is well known, was a German country doctor, published, after difficulties which remind us of those met with by Waterston, his first paper on the conservation of energy. In this paper, which at the time passed almost unnoticed, Mayer showed how, by assuming the existence of what was later called potential energy, the scholastic maxim *Causa æquat effectum* could be converted into a quantitative scientific theory of extraordinary power in helping us to predict phenomena. In none of his later papers does Waterston discuss specifically the conception of potential energy. From his earliest writings he speaks only of the conservation of *vis viva;* and he evidently regarded both potential energy and the "latent heat" of Black's experiments as only make-shift conceptions, involving the unintelligible assumption of action at a distance, and to be replaced later by the application to the "luminiferous ether" of a theory on the same lines as the kinetic theory of gases. He was nevertheless unable to dispense with the provisional conception of action at a distance, and had to employ it in the discussion of such phenomena as the liquefaction of gases, latent heat of melting and evaporation, and the failure of many gaseous substances to conform fully with the ideal gas laws. That action at a distance and potential energy remained for him only provisional conceptions appears clearly in his paper of 1858 on "The Integral of Gravitation." Unlike the majority of his scientific colleagues, he could not rest happily on these convenient soft pillows.

The momentous experiments of Michelson and Morley on the velocity of light were published in 1887, four years after

Waterston's death. To him these experiments, if he had accepted their results, would probably have seemed like an end to the "philosophy" of which he so often speaks. They were, in truth, an unmistakable indication of the breakdown, now clear enough in various other directions, of the "philosophical" part of the Galilean and Newtonian physics, and of the "common sense" realism which, in spite of Hume and Kant, had lasted so long in popular esteem, and that of scientific men in particular. Even in Waterston's time, however, there were indications of this breakdown. The chief one was the difficulty in applying mechanical conceptions to life and conscious behaviour; another was the known anomaly in the specific heats of carbon and various other solids.

It is clear that after Waterston had completed his first paper for the Royal Society he was satisfied for the time with his treatment of gases and heat. But he was well aware that the kinetic or *vis viva* theory of perfect gases was only a beginning. The further problems of "molecular physics" were still before him; and his previous struggles with the "law of association," together with his wide knowledge of actual physical and chemical phenomena, impelled him to investigate next the molecular physics or physical chemistry of liquids.

His first actually published paper in this direction was on the calculated relative densities (in more modern language the molecular or molar concentrations) of vapours which are saturated in presence of their liquids at different temperatures. Like van der Waals, twenty years later, he was searching for a quantitative law connecting the liquid with the gaseous state, and this motive led him to investigate the relative densities or molar concentrations of saturated vapours and their liquids at different absolute temperatures, rather than mere vapour pressures. At the critical temperature (called by Waterston "the point of transition") the densities of a liquid and its saturated vapour become equal. At comparatively low temperatures the pressure of a saturated vapour is an almost exact index of its density when absolute temperature and the molecular weight of the substance are taken into consideration. In other words, the vapour, unless it condenses,

conforms with Boyle's, Charles's, and Avogadro's laws. But at high temperatures this is no longer the case, since the density or molar concentration of the saturated vapour increases out of all proportion to the gas laws, and finally becomes the same as that of the liquid itself.

The paper, as he mentions in a later paper (page 461), was sent first to the British Association (which did not publish it), and then to the Royal Society in 1851. It appeared in the *Philosophical Transactions* of that date. In this paper Waterston shows that when the sixth roots of the densities of a vapour saturated at different temperatures are plotted against the square roots of the absolute or "gas" temperatures the graph is a straight line over a considerable range of temperature. He quickly followed up with another longer paper (in 1852) in which the sixth roots of the densities were simply plotted against absolute temperature. This gave a much more exact result, since with the previous method of plotting the lines turned out to be curved when the range of temperature was extended.

It will be seen, on examining the graphs, that if the straight lines which they represent are extended downwards they indicate zeros of vapour pressure, denoted by $g$ in Waterston's equation, at temperatures far above absolute zero. With Waterston's conception of temperature this remained quite unintelligible. There would, presumably, be plenty of heat-energy left in the molecules at the indicated zero vapour pressure, so that some of them would continue to fly off as vapour. He was of course quite unaware of the manner in which the specific heats of solid and other substances, and presumably, therefore, the energy which would otherwise be available for external molecular movements, fall off at low temperature. The low atomic heat of carbon and several other solid elements at any ordinary temperature was, of course, known, but only as a very anomalous phenomenon; and the general existence of the low temperature phenomena to which the quantum theory has been applied in recent times was quite unknown.

The discoveries embodied in this paper, and the equation, with its mysterious $g$, which embodies them, are undoubtedly of

very great importance, and seem to have passed unnoticed until now. They are evidently connected with the facts embodied at a later time in van der Waals's famous equation, and with the latter's endeavour to interpret these facts. Any attempt at an adequate discussion would, however, lie outside the scope of the present biography. This and other subjects connected with Waterston's discoveries are treated of very fully in the book by the writer on *Gases and Liquids.*

Included in the paper are a number of experimental results of Waterston's own, the vapour densities being determined by a quite new method, which is not affected by the fact that at high temperatures the saturated vapours do not conform with the gas laws of Boyle and Charles, and which gives the densities of both vapour and liquid. The new method is described fully, and further very important results obtained by its means are given in several subsequent papers. The use, now so familiar, but then novel, which Waterston made of graphical projection, is remarkable.

This very important paper sustained the same misfortune as the paper on the kinetic theory: for it was not published but referred to the Archives, where, so far as can be ascertained, it has remained unnoticed until the writer found it. Lord Rayleigh does not seem to have discovered it, or else did not realise its importance—perhaps because he himself had not yet become interested in the physical chemistry of liquids. It is probable that the difficulty and expense of reproducing the unwieldy graph belonging to the paper were largely responsible for its rejection.

It was to this paper that Waterston refers as follows in a letter written in 1852 from Bombay to his brother William: "The Royal Society will, I think, print a paper I have lately sent them. They ought also to give me one of their medals if kissing does not go by favour: not that I value their favours, but others do, and it might be melted down to pay the expenses of experiments."

Waterston's next paper was one describing experiments carried, as he believed, up to the "point of transition," or temperature (discovered originally by Cagniard de la Tour in 1821) at which liquid and vapour become indistinguishable. He

doubtless did not realise that his method must break down at a temperature a little below what we now call the true critical temperature, but he got determinations at temperatures very close to it, and found that the law which he had discovered held good at the highest temperatures which would give results. Despairing, probably, of the Royal Society, he sent the results to the British Association; but they were not published, though they are given in another paper, published in 1868 after his return from India. The well-known work of Andrews on the critical temperature and pressure of carbon dioxide was not published till 1869, after Waterston's papers had ceased to appear. The discoveries of Andrews would have helped him greatly.

Following up the same lines of thought with regard to the physical chemistry of liquids, Waterston carried out at Bombay a series of experiments on the relation between capillarity or surface tension and latent heat of evaporation of liquids at different temperatures; but he did not attempt to get the results published till after his return home.

He was evidently much hampered in India by difficulty in obtaining suitable glass and thermometers for his experiments, and this is referred to in his unpublished Royal Society paper. He had to rely on a Hindoo blacksmith and Parsee carpenter for assistance in the construction of apparatus; and the thermometers which he ordered from home, and which, owing to the long sea voyage, took a very long time in reaching him, were usually more or less wrongly made.

He also worked at Bombay on solar radiation (p. 676), and at certain astronomical subjects (pp. 657 to 674). During an experiment on solar radiation he had a severe attack of heat-stroke, and his native servant, not knowing what to do, left him for some time unconscious in the sun. From the effects of this accident he suffered more or less during the rest of his life. He was subject to sudden attacks of giddiness, particularly in crowded rooms and on railway journeys. This probably accounts in part for the fact that after his return from India he saw so little of fellow scientific men, and did not go to scientific meetings. There is every probability that, as will be related below, his death was indirectly caused by one of these attacks.

His teaching work at Bombay brought him into contact with astronomy, and from 1852 onwards he was a life member of the Royal Astronomical Society. His first astronomical paper appeared in 1842, and this brought him into communication with Adams and Challis. In a letter of 1850 to his brother William he writes: "I am preparing experiments and a paper [his first one on vapour density, published ultimately by the Royal Society in 1851] for the British Association on a subject which I think of some scientific importance. I may be disappointed in expecting it to be understood by them, but have been encouraged by the complimentary notice of the astronomers Adams and Challis of Cambridge on a paper of mine printed some years ago by the Astronomical Society. The seed may chance again to bear fruit, as it has in the case of Challis, who has embodied my discovery in his paper with the most liberal acknowledgements."

In their account of Waterston in the *History of the Royal Astronomical Society* (1923) Professor Turner and Dr Dreyer refer to his contribution in June 1844 of "a short note on a graphical method by which, with ten to fifteen minutes' work, an occultation could be predicted within one minute, and in the January following some good observations of the comet." "In 1850 he published a paper on 'A Graphical Mode of computing the Excentric Anomaly' (*Monthly Notes*, **10**, 169). About fourteen years later the method was rediscovered by Dubois (*Astronomical Notes*, 1404) and has been called by his name. The injustice was first noticed by T. J. J. See (*Monthly Notes*, **56**, 54)." Professor Turner, who had already referred to Waterston's misfortune with the Royal Society in connection with the kinetic theory of gases, adds the following paragraph:

"The figure of Waterston is so tragically interesting that a brief reference to his later papers may be excused. In 1858 he communicated to us some 'Thoughts on the Formation of the Tail of a Comet,' which are surprisingly suggestive of modern views of light pressure. He shows himself aware of the difficulties in converting a vibratory movement into a translatory, but he is impressed with the fact that *if* the whole heating power of the sun's rays could be converted into a centrifugal force, the acceleration would be 800 miles per sec., per sec., and he is tempted to think that even a fraction of this

might serve. He draws careful distinction between the case of a large body like the earth and a 'single and free molecule.' In the following year he considered the development of heat in the sun from bombardment by meteors, but had not good enough data to make suggestions of modern value. But he set about improving the data for radiation by experiments of his own, though they still led him to a potential temperature of about 12 million degrees for the solar surface.

"There our printed records about him practically stop. But, alas! there are others in our archives and minutes. At the end of the Council Minutes for 1879, May 9, 'a letter was read from Mr J. J. Waterston, asking that his name might be removed from the list of Fellows of the Society. His request was acceded to.' Seeing that he had compounded for his annual subscriptions (1852, January 9) the step is a remarkable one and led to further search. In 1878, May and June, two papers were received from Waterston, and are duly recorded in the printed lists (*Monthly Notes*, **39**, 298-9). 'On the Heat of the Stars, J. J. Waterston,' and 'On a Solar Thermometer Couple to measure the Radiant Force of Daylight, J. J. Waterston.' But they were not allowed to be read or printed. They even appear to have been returned to the author, for they are the only absentees from the bundle of that year in our archives; and the sequel and undoubted consequence was the above withdrawal."

The manuscripts of these two papers were found, together with correspondence relating to them, among the papers preserved by Mr Charles Waterston; and they are printed in the present volume. They were submitted to Professor Turner, who kindly wrote as follows with regard to them:—

"On page 28 of the *History of the Royal Astronomical Society* (1923) reference is made to two papers presented by Waterston to the Society in 1878 (May and June) 'On the Heat of the Stars,' and 'On a Solar Thermometric Couple to measure the Radiant Force of Daylight.' These papers were never printed or even read, and it was presumed that they had been returned to the author, as they were missing from the MS. bundle of papers for the year 1878 ('the only absentees'). Waterston subsequently withdrew from the Society, although he had 'compounded' for his annual subscriptions in 1852.

"It now appears that they were returned to the author on July 8, 1878, as 'scarcely suitable for publication by the

Society.' A draft reply in pencil inquires whether the Council would 'permit them to be stitched up with the *Monthly Notices* if [the author] printed them at his own expense.' No date was on this draft, but it was apparently sent, and the answer to it is dated August 8, stating that the letter would be brought before the Council at their next meeting in November, but that the matter of expense did not influence the decision of the Council. There is again a draft reply in pencil (dated Oct. 21) recognising the futility of further action, and accordingly withdrawing the former letter, but expressing regret that 'the solar thermometer couple is not acceptable, as I thought it was a great desideratum. It ought to have been included in my paper of observations on Solar Radiation, but I did not recognise the principle until lately.'

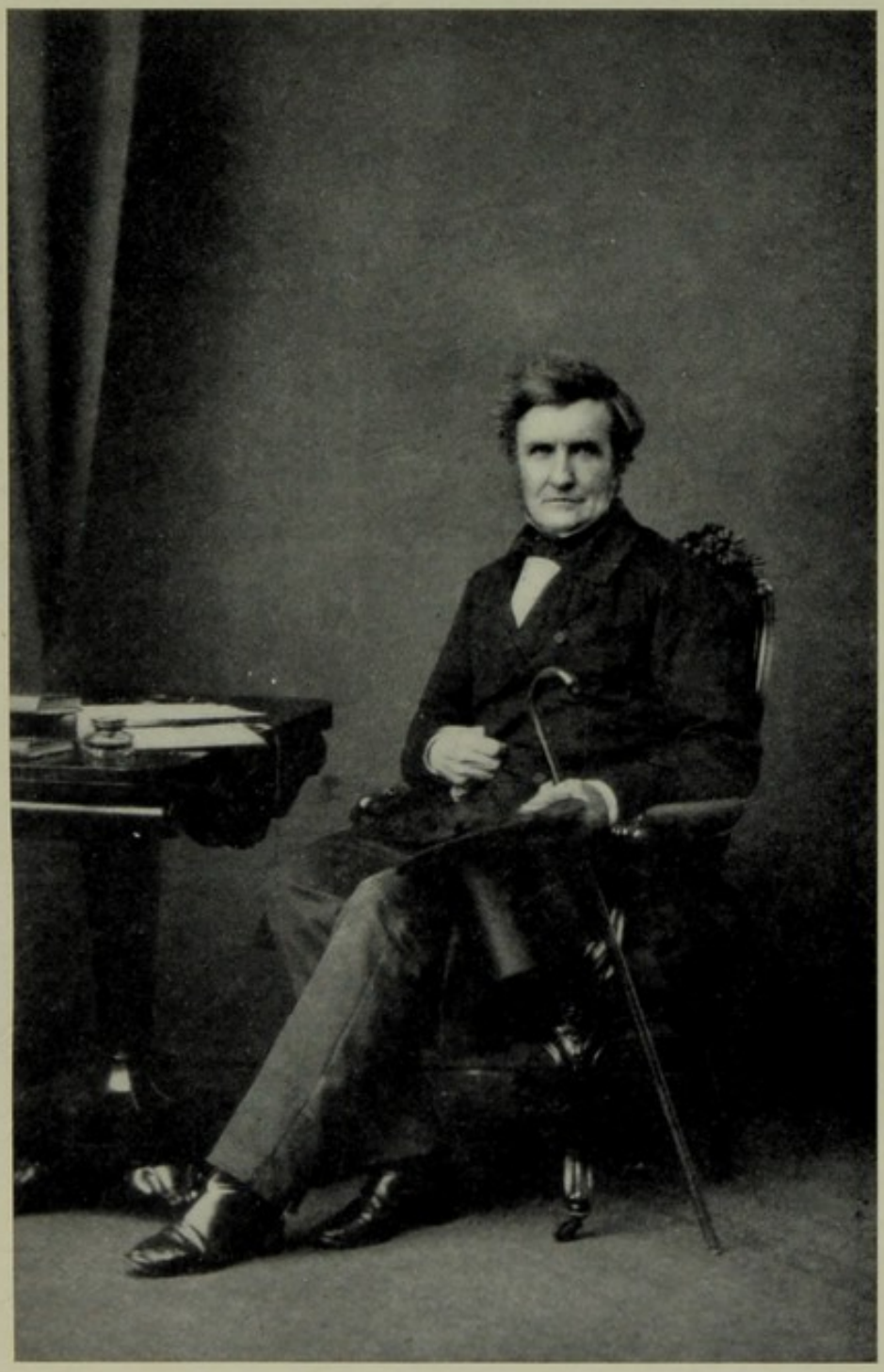
"Both papers are now printed in this volume, and readers can judge of their merits for themselves. The one specially regretted by the author is very brief, and makes a practical suggestion of a simple kind which might have started useful work on solar radiation. Perhaps the Council thought it rather meteorological than astronomical. But work on stellar radiant heat had been already attempted in 1870 by Mr Stone at Greenwich, and the other paper is a criticism of his results, suggesting that they were far too large to represent the probable truth. In this contention Waterston was essentially right, though the form of his argument is now antiquated. When Mr C. V. Boys attempted in 1890 to measure the heat of the stars with the radiomicrometer (*Proc. Royal Society*, **47**, p. 480) he found it far too small to justify Stone's results. This is not the only piece of Stone's work which has failed to stand the test of time, but in 1878 Stone was at the height of his reputation. After being Chief Assistant at Greenwich he had been appointed (in 1870, soon after his paper on the Heat of the Stars, which he was thus unable to follow up) H.M. Astronomer at the Cape, and was known to have shown great energy and skill in constructing a catalogue of stars for the Southern Hemisphere. His determination of the radiant heat had been accepted as a piece of careful and successful observing, and was so accepted by Mr Boys in 1890 when he prepared with some confidence to confirm it with his own radiomicrometer. His paper shows clearly that to find Stone's results spurious (due to an instrumental defect) was a complete surprise to him.

"We can thus see that the R.A.S. Council might well hesitate in 1878 to accept a destructive criticism of the kind offered by Waterston. Indeed they did not hesitate at all;

they rejected the paper out of hand, and the other along with it. Thus poor Waterston got another rebuff, which he apparently felt deeply. He did not for several months (May 1879) take the step of withdrawing from the Society; but this deliberation suggests a deeper wound than would immediate action in a fit of pain or anger."

In 1857 Waterston resigned his appointment at Bombay, and returned to Scotland. A main reason for his resignation was probably the difficulty in getting his scientific work published, and in carrying it on, while he remained in India. He was only 46; but he had saved a good deal of money. In one of his letters he alludes to his chances of obtaining a professorship—possibly at the Addiscombe College of the East India Company. In another he inquires about the possibility of an Irish professorship. But any such chances were destroyed by the non-publication of his papers or their failure to attract attention. He first returned to Edinburgh, and remained there for about eight years. He then took a house in Inverness near his brother Charles, who was a banker there. Afterwards he removed to Dunkeld, where his mother's people lived; but he soon returned to Edinburgh and remained there till his death in 1883. The frontispiece, from a photograph, taken in the garden of the old St John's Hill house just after his return from India, and developed by himself in the early days of photography, gives a good idea of his appearance at that time. The accompanying photograph was taken at a later date, after his final return to Edinburgh.

When he returned to Scotland he again began experimental work on liquids, and in the course of the next few years succeeded in publishing a series of papers—mainly in the *Philosophical Magazine*. What scientific friends, if any, he had in Scotland is not known, though probably Sir David Brewster, who was one of the editors of the *Philosophical Magazine*, was at least an acquaintance, and helped him in the matter of publication. He does not seem to have gone to meetings of the Royal Society of Edinburgh, or to have made friends with either Kelvin or Tait, who were the mainstays of the Society during the latter part of his life. It appears that he was very critical of some, at least, of the scientific work of



J. J. Waterston, aged about 60

Kelvin and his associates. This was probably because he considered Kelvin's thermodynamical reasoning and conception of the meaning of absolute temperature to be quite unsound.

He was also critical of Kelvin owing to what he considered the latter's too close connection with commercial applications of science. Waterston, like Faraday, carried Glasite principles into science, and held that scientific men should support themselves in other ways—for instance by teaching—than by the practical applications of their scientific investigations. Faraday acted on these principles, and Waterston referred to him often as an example, and had the greatest admiration for his work and character. It is doubtful, however, whether Waterston ever came into very close personal connection with Faraday. Their ideas took them in different directions; and while Waterston was in London he seems to have been interested mainly in physiology and psychology. It is probable also that Faraday's orthodox and very sincere religious beliefs created something of a bar between the two men. Waterston had, through his connections with biology and psychology as well as physics and chemistry, a wider intellectual outlook than Faraday or perhaps almost any of the prominent scientific men of his time.

As Lord Rayleigh remarks in his Introduction to Waterston's first Royal Society paper, it was a great pity that he never got into touch with Clerk Maxwell, who would have seen at once the importance of the paper, and drawn attention to it. But by the time Clerk Maxwell began to work at gases Waterston had already left this subject, and was deeply immersed in the physical chemistry of liquids. Thomas Graham was working at the latter subject, but had left Scotland, and was now Master of the Mint; while Andrews, who was also working in this direction, had gone to Belfast.

One of Waterston's first pieces of experimental work after his return was to follow vapour and liquid densities up to very high pressures and temperatures. The experiments, which were done in a sitting-room, involved heating the liquid in a sealed glass tube by means of a Bunsen flame in a special apparatus, and must have been very risky owing to the enormous pressures, and the fact that he had to watch the tubes closely through a

lens. He seems, however, to have had no serious accident, though he mentions that a glass tube containing water burst at a temperature of 330° C. owing to corrosion of the glass.

The following extract from a letter of April 1862 to his sister-in-law, Mrs William Waterston, gives a picture of his domestic affairs:—

"This is the season for beating carpets. My rare and radiant maiden is very much disappointed in not being allowed to take up the carpets and have a ploy in the cleaning way. I shall pack her off and board her with her friends in Kelso.

"I have reckoned without my host. She has just been in and insisted on having the house cleaned before she goes. She has also spoken of invading my sanctum sanctorum or apparatus room, but that of course won't do unless I am present. I see it is quite a malady of the mind, and she would lose her peace of mind if the cleaning did not take place. She is a United Presbyterian, and subscribes to a foreign mission, indeed assists in supporting a church in Caraboo, for awaking and rescuing the benighted heathen from the wrath to come. Where Caraboo is, I can't find out, and she does not *exactly* know."

Apart from his deep disappointments owing to his scientific papers not being understood, Waterston's life after his return seems to have been a very happy one. He was held in affection and the utmost regard by members of his own family and a circle of friends, from whom he concealed his own troubles, but who saw easily how high his scientific ideals were, and respected him accordingly. Until Lord Rayleigh revealed the fact, none of them knew of the rejection of any of his papers by the Royal Society, or of any other rejected papers, though they did know that he was strangely contemptuous of the Royal Society and some of his well-known scientific contemporaries. He lived the life of a very popular bachelor, fond of the best music, billiards, a good cigar, or chess, and the idol of the children who knew him. His rooms in old-fashioned Gayfield Square, off Leith Walk in Edinburgh, were the scene of many merry gatherings of young people. Perhaps the sense of mystery inspired by his scientific instruments and the manuscripts and calculations which were constantly occupying him added to his popularity.

Although he was living at Edinburgh in the midst of a

Glasite community he was never a member of the Glasite Church, either there or in London, nor did he attend its services. This, however, caused no real estrangement between him and the members of his family. He avoided theological discussions, but in all practical matters remained true to his Glasite upbringing. In nothing, indeed, was this more clearly shown than in the patience with which he bore the misfortunes to his scientific papers, and the care which he took to conceal his own disappointments from those to whom they would have caused pain. His concern over poverty or distress of any kind is also evident in letters of his which have been preserved.

His first paper published after his return from India was on deviations from the gas laws, as demonstrated by Regnault's recent experiments, and as related to Thomson and Joule's well-known porous plug experiments. The latter experiments had shown that when certain gases are compressed the heat produced is greater than corresponds to the work done in compression. Waterston, who criticises Thomson and Joule's mode of stating their results, points out that the external work done is not the whole of the work, and shows how the whole work may be estimated from the data, and brought into accordance with the conception of a perfectly definite mechanical equivalent of heat.

Three months later he describes experiments which he had made in India on the relation between capillarity or surface tension and latent heat of evaporation of liquids at varying temperatures. These experiments, as he says in introducing the paper, were "suggested by the modern views of the dynamical or *work*-value of molecular force. The argument kept in view is that "if the capillarity of a liquid is the exhibition of part of the cohesive force of the superficial stratum of its molecules, numerical relations with the latent heat of its vapour ought to be demonstrable if latent heat is the measure of liquid cohesion." The conception and definite measurement with different liquids of what is now called surface energy are first treated of, and his experiments on its diminution with rise of temperature described. There follow a number of interesting calculations, which include an estimate, for different liquids, of their molecular volumes. He then discusses

observations by himself and others on capillary phenomena at temperatures close to what is now called the critical temperature, and concludes that both surface tension and latent heat of evaporation must diminish rapidly as the neighbourhood of the critical temperature is approached. Waterston's dynamical treatment of the whole subject is far ahead of his time.

Two months later came a paper on a method of correcting the indications of a mercury thermometer between the fixed points of 0° C. and 100° C. As was already well known, a perfectly constructed mercury thermometer which is correct at 0° and 100° is not perfectly correct at intermediate points. To introduce the necessary corrections Waterston proposed to make use of the law, for which he had given the evidence in his second unpublished Royal Society paper, that the sixth root of the density of a saturated vapour varies as the absolute temperature minus a constant. As this evidence was unpublished, and only a summary is given, the method could hardly produce any conviction. The real interest of the paper lies in its restatement of the new and very important conclusions in his unpublished paper of 1852.

His next paper is a discussion of gravitation. At the end of it he again expresses in the following words his fundamental belief in mechanical principles: "It would be taking too narrow a view if we limited the function of the luminiferous ether to the conveying of physical pulses only. The atmosphere also conveys physical pulses, but that is the least important of its functions in the economy of nature. There is nothing that should hinder us attributing to the media concerned in the radiation of light and heat the higher functions of electric polarity and gravitation. The special dynamic arrangements by which this is effected may ever elude research, but as there is no limit to the *vis viva* which such media may conserve in their minutest parts, so there is no physical impossibility in that *vis viva* being suddenly transferred to the molecules of ordinary matter in the proportions and sequence required to carry out the order and system of nature. The fundamental principle of action in such media must be in accordance with elastic impact, for upon that the dynamical theory of heat and conservation of force rests as a foundation."

This paper is followed by one on the theory of sound. He puts clearly and succinctly his objections to the theory of Newton and Laplace, while incidentally he attacks Kelvin for apparently accepting that theory. He is of course much handicapped in his argument by the fact that his first Royal Society paper, to which he refers, had not been published; but the paper is interesting on account of his approving reference to Krönig and Clausius, who had by this time begun to rediscover the kinetic theory; also because of his full references to Herapath's attempts to establish a kinetic theory of heat. The remarks already made as to the wrong value which Waterston had taken for the ratio between the two specific heats of air must here, however, be recalled. Kelvin had at least taken the right value and the only one consistent with the result of Joule's determination of the mechanical equivalent of heat, though the theory of Newton and Laplace was quite untenable in the form which they had given to it.

The next paper was the first of four published from 1861 to 1868 on liquid expansion. In his experiments on liquid expansion at high temperatures, he used the method described fully in his second unpublished Royal Society paper, and it was a serious handicap to have to refer to this rejected paper: for scientific men, like other men, are only too apt to behave as a flock of sheep in following one another.

In the first of the four papers he analyses mathematically the data as to liquid expansion, including his own data for very high temperatures up to about the critical temperatures of the substances employed. The analysis leads him to the conclusion that a definite quantitative relationship exists between the increase of density of saturated vapour and expansion of the liquid as the absolute temperature rises. In the third paper he points out that his first statement of the relationship was imperfect, and substitutes a more definite statement. For further discussion of these fundamental discoveries I must refer to my book on *Gases and Liquids*.

The expansion of water at temperatures up to 200° C. had been found by him to constitute an exception to the relationship existing for various other liquids, and the third paper is

devoted to the expansion of water. He carried the observations on water up to a temperature of 330° C., and describes the method employed, and the trouble he experienced owing to corrosion and bursting of the glass tubes required for the work. His results showed that above about 190° C. the expansion began to follow the same law as in the case of other liquids. As he points out, the abnormal expansion of water at ordinary temperatures has been a stumbling-block for molecular physics. Later observers have been quite as well aware of this fact, which is presumably due to the presence in the water of fleeting molecular aggregates occupying more space than the constituent molecules after the aggregates have been partially dissociated at higher temperatures. Above 190° C., however, the abnormal behaviour of water had disappeared, to Waterston's great satisfaction.

In the fourth paper, a very interesting one published in 1868, he sums up his previous work on the subject of liquids and their saturated vapours, and points out definite quantitative relations which exist between vapour density, liquid expansion, molecular volume, surface tension, and latent heat of evaporation.

This is his last published paper, but does not read as if it were the last paper he was ready to write on the physical chemistry of liquids. Brewster died in the same year, 1868; and it seems probable that after Brewster's death Waterston was in some way given to understand that the *Philosophical Magazine* was no longer open to his papers. He had been publishing papers in the *Philosophical Magazine* pretty regularly up to 1868, when their publication suddenly ceases. It seems evident that none of his scientific contemporaries had followed his reasoning, or realised that he was the original author of the kinetic theory of gases, which by that time was becoming known in the form given to it by Krönig, Clausius, and Maxwell. As the two unpublished papers, which he so often had to refer to, had been refused by the Royal Society, he was probably regarded as more or less of a crank, whose further papers it was not desirable to publish. As shown, moreover, by his later correspondence, already alluded to above, with the Secretary of the Astronomical Society, he had become very sensitive as regards refusal of his papers.

The fact that he occupied no position as a teacher, and that he had kept so much aloof from other scientific men, may have further increased his difficulties in making his results known. In any case it has come about that much of his work on the physical chemistry of liquids is just about as new now as it was sixty or seventy years ago.

In 1863 and 1864 Waterston published three papers on chemical notation. These papers are referred to by Professor M'Leod in Lord Rayleigh's Introduction to the paper on the kinetic theory. Waterston was so impressed with the importance of molecular physics or physical chemistry, that he wished to base chemical notation on the molecule rather than the atom. This involved a fractional notation. For instance, the formula for water would be $HO\frac{1}{2}$ instead of $H_2O$, because half a molecule of oxygen is combined with a whole molecule of hydrogen to form one molecule of water. Waterston actually uses this form of notation in his papers, with the result that the substances to which he refers are at first somewhat difficult to identify. From the standpoint of physical chemistry there is of course much to be said for Waterston's proposal; but the complications and inconveniences would have been endless, and at the time when he wrote no one but himself had any clear conception of the far-reaching importance of molecular physics or physical chemistry. Even when Professor M'Leod wrote, this importance was only beginning to be realised generally, in consequence of the writings of Raoult, van't Hoff, Arrhenius, and others.

Between 1857 and 1868 Waterston published twenty papers in all in the *Philosophical Magazine*, including four on electrical subjects in 1865, and three on astronomical subjects. His special astronomical papers have been alluded to above. They are of course of minor importance, and by far his greatest contribution to astronomy was his work on gases and on heat.

In 1868 he had reached the age of 57, and it is clear that he had entirely failed in awakening the interest of the scientific men of his time in molecular physics or physical chemistry as he understood the subject. Probably he was weary, as one gets weary of struggling with a telephone when there is no response from the other end. It seems a pity

that when he returned from Inverness to Edinburgh he did not attempt to write a book summarising his investigations on molecular physics and heat so far as he had got, and dealing also with the thermodynamical side of the subject. There was, however, no encouragement to him in such an undertaking, and owing to the after-effects of his attack of sunstroke in India his health was somewhat precarious and his energy no longer what it was.

He took great interest in all the members of his family, and particularly in his nephews and nieces, to whom he used to teach mathematics and other subjects in which they showed interest. He was also keenly interested in contemporary literature and politics, and was strongly liberal in his political sympathies, though his liability to fits of giddiness kept him away from public meetings. One of his chief pleasures was in listening to good music, particularly that of Beethoven. His sympathies with poverty or suffering in any form were constantly appearing in acts of kindness. To those around him he showed no sign of being a disappointed man: he was always cheery, genial, and full of humour. To the end of his life he also took great interest in banking, railway, and other enterprises, particularly those in which his brothers were concerned.

On 18th June 1883, he went out one morning from his lodgings in Gayfield Square, apparently for one of his ordinary walks. As he had borrowed some pennies from his landlady he probably went down by tramway to Leith, where he was fond of walking on a new breakwater in course of construction. He did not return, and though the utmost efforts were made to discover what had become of him, they were completely unsuccessful. It seems practically certain that he fell into the water and that his body was carried out to sea by the tide, which was high at the time. He had read in the newspaper that morning an account of a terrible accident at Sunderland. A building in which a children's entertainment was being held had caught fire, and in the panic which ensued a very large number of them had been suffocated. The writer can well remember the impression which the news of this accident produced. On a man who was as fond of children

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J. J. Waterston, aged 72

as Waterston was the impression would be a very deep one, and may well have precipitated one of the attacks of giddiness or loss of consciousness to which he was liable, and thus caused a fall into the water.

All his affairs were in perfect order, and a will which he had made showed his usual kindly thoughtfulness for the needs of others. In the will he also mentioned an unpublished manuscript; but this could not be found, and it had probably met its end in a spring cleaning. We are reminded of how Carlyle's manuscript perished in this way at John Stuart Mill's house. The probable subject of Waterston's manuscript has already been discussed.

The accompanying illustration, drawn from memory at the time of his disappearance, as an aid to tracing his movements, gives a good impression of his general appearance during the last years of his life.

Waterston was one of a brilliant group of physicists turned out in the early half of last century by the Scottish Universities. This group included Brewster, Forbes, Thomas Graham, Andrews, Rankine, Kelvin, Clerk Maxwell, Tait, and Balfour Stewart, as well as Waterston. All of them except Waterston became well known during their life-times, though the work of some of them, as in the cases of Clerk Maxwell and Thomas Graham, has become better known since their deaths. It seems likely that as regards sheer genius and strength as well as breadth of scientific grasp posterity will rank Waterston highest in the group.

It is probable that in the long and honourable history of the Royal Society no mistake more disastrous in its actual consequences for the progress of science and the reputation of British science than the rejection of Waterston's papers was ever made. The papers were foundation-stones of a new branch of scientific knowledge, molecular physics, as Waterston called it, or physical chemistry and thermodynamics as it is now called. There is every reason for believing that had the papers been published physical chemistry and thermodynamics would have developed mainly in this country, and along much simpler, more correct, and more intelligible lines than those of their actual development.

In the case of a man of less retiring and more combative

disposition than Waterston, the mistake of the Royal Society Referees would have mattered very little; but by the time Waterston, away in India, realised the significance of the rejection, he was deeply immersed in the physical chemistry of liquids, and he had neither inclination nor time for controversy about his paper on gases. When his paper on vapour densities was also rejected he seems to have given the Royal Society up in despair. One cannot help regretting that in Waterston's pedigree there had not been some admixture of the fiery Highland blood which made the Gregory family, including Thomas Reid, whose mother was a Gregory, such effective apostles of Newtonian principles in the Scottish Universities. As is well known in Scotland, the real family name of the Gregorys was the proscribed one of Macgregor, and they were closely related to Rob Roy.

In one of his papers (p. 348 of this book) Waterston calls attention to Newton's distinction, when he is discussing the properties of gases, between "philosophical" and merely "mathematical" principles. In his theory of perfect gases and of heat Waterston substituted what Newton would have called "philosophical" for the merely "mathematical" principles of the caloric theory and of Newton's own account of gases. Waterston's molecular physics constituted a brilliant triumph for Newtonian "philosophy." He had, it is true, completely failed in a determined attempt to apply the mechanical conceptions of Newton's philosophy to biological and psychological phenomena, and he had not found any clear way to dispensing with the conceptions of potential energy or of action at a distance in connection with gravitation, molecular and chemical attraction, etc.; but he held firmly to Newton's "philosophical" conceptions, and he was a truer and at the same time far more progressive representative of those conceptions than any physicist of his century. Thermodynamics, under the lead of Kelvin and Clausius, developed actually along what Newton would only have called "mathematical" lines; and in later times physical chemistry, led by van't Hoff, developed also along what were only "mathematical" lines. To put this more directly, thermodynamics as a science became more or less centred round the

"second law of thermodynamics," and physical chemistry round van't Hoff's supposed law of osmotic pressure, neither of which laws had any intelligible relation to mechanical principles. Had Waterston's papers been published this could hardly have happened, and we should probably by this time have had far clearer conceptions in molecular physics, and been in a much better position to appraise the magnitude and importance of the new developments of physical conceptions since about the beginning of the present century.

Even to Waterston himself the "medium" which he treated of in his first Royal Society paper was not an actual gas, but an ideal medium, to the properties of which many actual gases seemed to approximate at ordinary temperatures and pressures. It follows also that his absolute temperatures were only ideal absolute temperatures; and with our present knowledge of the manner in which the specific heats of all sorts of solid or solidified substances fall off at very low temperatures this has become much more evident. But the whole framework of the physical conception of Galileo and Newton has been falling to pieces rapidly in recent years. Their conception was only an ideal conception, though it has been, and still is, extraordinarily useful for prediction in connection with what we distinguish as inorganic phenomena.

The physical "realism" to which the modern world has been for so long accustomed is thus turning out to be an inherently impossible form of idealism. To understand this new position, however, we must fully grasp the implications of the traditional physical realism; and here Waterston's contributions are essential. The Newtonian philosophy must "dree its weird" before the new ideas can mature fully. We have seen above, for instance, how the apparent facts which are embodied in the second law of thermodynamics or the law of constantly increasing entropy follow from Waterston's extensions of the Newtonian philosophy to molecular physics. Not only many biologists, but also many physicists, among whom we must include Waterston, have refused to regard the Newtonian mechanical philosophy as applicable to biological and psychological phenomena. A wider justification for their refusal is now present.

When Lord Rayleigh wrote his Introduction to Waterston's first Royal Society paper it seemed as if the rejection of this paper had only delayed scientific progress as regards a theory of gases for about fifteen years, and given to other physicists credit which was rightly due to Waterston. With a fuller knowledge of the sweep of Waterston's conceptions in molecular physics we cannot take such a view now. It is not as any mere act of historical justice, but because science has still need of them, that Waterston's papers have been collected together; and among them there is perhaps none of which it has more need than his early essay on physiological psychology.

The story of the persistent manner in which he developed the principles handed on to him through generations of Scottish physicists from Galileo and Newton, and under the inspiration of religious traditions which had come to him under the same form as to Faraday, is also of lasting significance. Science is no mere tool for the acquisition of material advantage: its meaning for mankind is a far deeper one.

# I

# PHYSIOLOGICAL PSYCHOLOGY

# THOUGHTS ON THE MENTAL FUNCTIONS*

*Being an attempt to treat Metaphysics as a branch of the Physiology of the Nervous System*

## PREFACE

THE following attempt to study Metaphysics as a branch of the Physiology of the Nervous System, is based on the great discoveries in this department of science that have distinguished the present age. The division of the nerves into motor and sensitive involves a principle that is all-important in tracing the primary relations of the mental phenomena. Combined with association, it seems capable of resolving those difficulties in the doctrines of sensation which Dr Reid has so ably pointed out in his "Inquiry," and which are certainly among the most formidable that beset the subject.

The organology of Dr Gall is another splendid contribution to psychical science, the offspring of the highest genius; but, from being too hastily reduced to the popular standard, its value has been unfortunately obscured.

The usefulness of phrenology in drawing attention to the dependence of the moral on the physical has perhaps more than compensated for its crudeness as a science. But while its doctrines are peculiarly adapted for *exoteric* and *esoteric* treatment, it is to be regretted that the latter has been so totally neglected.

The slightest application of analysis is sufficient to make us aware of the difficulties that attend the rigid prosecution of the subject, and to show us that all metaphysical discussions are

* Originally published by Oliver and Boyd, 1843.

at present but *conjectures*, more or less probable as they are consistent with what is known of physiology.

In the same rank of importance with the discoveries of Bell and Gall ought to be placed those of Mr Knight, on the habits peculiar to certain breeds of animals, alluded to in Chap. III. § 31. That gentleman's researches have *demonstrated* the remarkable fact, that a sensi-motor train of impressions may be established in the organization, and transmitted as an innate susceptibility to the offspring. It is manifest that this principle admits of the most important applications to the human mind, and some of these are noticed in the following pages; but in the further analysis of the faculties it will be found to harmonize and simplify them in a surprising degree.

## INTRODUCTION

Metaphysical discussions are obviously more uncertain in their results than almost any other branch of human research. The most successful inquirer must therefore be prepared to find that, in so far as he oversteps the verge of inductive science, he risks wandering into the snares of erroneous hypothesis.

The history of physical discovery testifies this by many examples, and points out the rocks and shoals upon which illegitimate speculation has so often been wrecked. The history of mental philosophy, on the other hand, more than sufficiently abounds with instances of self-delusion—of profound writers imposing upon themselves in their efforts to explain the refined abstractions of intellect. And no very critical knowledge is required to discover this; for, as Hume wittily remarks, even the rabble without doors may judge from the noise that all goes not well within.

These fallacies seem to arise more from an excess than a deficiency of mental power. Their works have become standard monuments of excellence, perhaps unrivalled in any other branch of literature for power, compass, and depth: they have seized and embodied in elegant language the most refined, subtle, and evanescent of the perceptions of intellect; but they evah despised the study of organization—it was too gross for so

elevated a subject. They were carried away by too lofty views of human nature, and flattered themselves and their readers with ideal images of moral perfection that are altogether remote from every-day life, and hold no place either in the minds or actions of the bulk of mankind. It may certainly be gratifying to the pride of all that our common nature is capable of such comprehensive power as Newton or Shakspeare has manifested, and that it is generally equal to the conception of elevated thoughts that soar above ordinary events, however seldom it is possible to realize them. But it must not be forgotten, that man has to subscribe to the same conditions of existence as the lower animals; and as those conditions are obviously most intimately connected with the higher faculties that distinguish him from them, they deserve to have the first place in the attention of the mental inquirer, instead of being considered below his notice.

We must accustom ourselves to consider truth, or the consistent relations of things, as equally lovely, equally estimable, equally to be sought after, whether it is concealed under the grossness of animalism or interwoven with the intellectualism of genius. Let us not shrink from conclusions because they do not flatter: once satisfied of their consistency, let us embrace them as approximations to the true course of nature—to reveal which is the proper aim of all abstract disquisition, and the great end to which the aspirations of the philosopher ought to be directed.

## CHAPTER I

### ON THE ANALYSIS OF PHENOMENA

In this study we have to find out (1) What is common to all phenomena; (2) In what respects phenomena differ from each other. The term Phenomena, or appearances of nature, designates every thing that can be the object of human knowledge; their relations, all that can form the subject of scientific inquiry.

The relations of form and size constitute the science of Geometry; the relations of number, Arithmetic, including Algebra and the higher calculus.

The relations of form may be transposed into relations of magnitude, and the latter into numerical relations. Thus geometry may be transformed into arithmetic.

The abstract relations of quantity present an infinite variety of curious results, which the mind delights in contemplating, by its innate adaptation to this class of perceptions, which arise from the original and necessary order of the material creation. But these results are abstract, and, although impressed with the vivid impulse of truth, are not represented directly to the senses by the spontaneous workings of nature.

It is otherwise when matter and motion come to be viewed in their relations. The stupendous machinery of the heavens respond to the deductions of the philosopher, and, while they confirm in their silent course the principles of his inevitable belief, connect the organism of his mind with the most remote members of the material universe. The grand relations of matter and motion, which are perceived and necessarily acknowledged as truths by the mind, must be represented to the inward sense of perception by forms of matter and motion, which, although wholly different from the external objects, maintain and are connected by similar relations.

All change, all effects, all existence, all appearance, when perceived or reflected upon by the mind, are connected with motion,—motion in the agent that conveys the impressions, and motion in the recipient or mental organism that is impressed and retains the impulse.

Analysis, in respect to phenomena, may be defined as the reduction of particular appearances into the more simple and general, and as classifying them according to their natural order of dependence or relation. If we seek to discover what are the essentials of all phenomena, we shall find that there can be no appearance, phenomena, or perception without matter in a state of activity. Matter and motion may thus be viewed as the elements of which all things are composed; and the difference between phenomena, as limited to a modification of the properties of these elements. One phenomenon can

differ from another only in so far as the essential properties of matter and motion admit of change in kind or degree. Matter may be in large or small quantity; it admits of change in quantity, also in figure. Impenetrability, or resistance to motion—which, being analysed, includes mobility—is the characteristic essential property of matter, body, or substance; quantity, or extension and figure, is also essential to it, but common to space likewise. The idea of the latter is evidently derived from the former, conjoined with non-resistance to motion.

Thus matter is known to us by its resistance to our sense of motion, by its quantity, and by its figure; space by its non-resistance to our sense of motion, and by measuring both matter and velocity,—that is, being their standard of comparison, their common measure.

The term velocity, being used when comparing two motions of the same body, is the representative sign of that which, admitting of change in degree, is the characteristic essential of motion, and distinguishes a body in motion from the same body at rest. The perception of velocity is simple, and cannot be analysed; it is the consequence of the moving body exciting through the senses a simultaneous action in the organism of the mind.

The analysis of phenomena may thus be considered as the analysis of motions. But all motions may be said to be under the dominion of the science of form and number, of geometry and arithmetic; their analysis therefore must be limited to ascertaining their mathematical elements and relations: it can proceed no further.

The analysis of phenomena has therefore reached its extreme limit, when it has resolved them into specific variations, in kind and degree, of the essential distinguishing properties of matter and motion.

The term analysis is therefore inapplicable to those essential properties. To extend causation to their existence is in fact absurd, yet it is an absurdity which is generally overlooked. Our sympathies extend to a final or first cause; but the conviction of this existence which is implanted in the human mind—the most enlightened as well as the most illiterate—is due to feeling, not to strict causation.

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All our perceptions, whether of simple impressions or their relations, are intimately associated by sympathy with the motives, actions, and demeanour of our fellow-creatures generally, including the whole animal creation; and our state of existence renders it necessary that we should both act upon and be acted upon by them.

The perception of the relation between the acting and the acted upon seems to be owing to an original power of the mind, which has an essential reference to the actions of our fellow-men and animals generally. This power, like others of a different character, originates the desire to view every phenomenon or impression as both the acting and the acted upon; and when one part of the relation only is obvious to the senses, the other is suggested by the imagination: it is conceived to exist.

In the chain of events and successive phenomena, the double relation exists in harmony with the mental organism that takes cognizance of it; and in fulfilling the ordinary business of life, this principle of the mind is sufficiently occupied with common events, in ascertaining their true relation to self. When so fastidious as to reject such ordinary fare, and seek to regale itself with philosophical contemplations, it stretches beyond its natural province, and the fruit of its exertions is but too often vague and useless. The voice of nature speaks alike to all, and the elements that are at work around us are instinctively and irresistibly viewed as the acted upon as well as the acting. The uneducated mind here stops, and rests contented in the belief of an unknown agent; or, if he proceeds further, it is to personify it in his imagination, so that it may become the object of instinctive reverence and idolatry. (It seems consistent with all experience and history, that such feelings can in no other way be excited than by the personification of their object.) The educated and enlightened proceed further. Phenomena are analysed and classed, and they are found to be governed by certain fixed laws. The chemical elements are discovered; their specific and general or common properties are developed; and the fact (as yet ultimate) ascertained, that in certain circumstances they become *impressed* with a greater or less degree of motion,

which they can communicate or transfer to neighbouring bodies (*vid.* gunpowder), but *how* impressed, science has not yet shown.

Are we at this stage to rest contented in the ignorant belief that thus far and no farther can our faculties carry us! Most assuredly we are not justified in holding such an opinion; and, notwithstanding the prodigious range of modern discovery and observation, the innate desire of knowing the why and wherefore is as unsatisfied as ever. Will it always be so? Does the connexion and dependence of phenomena not admit of being viewed in a way that will satisfy the understanding? What is it that will satisfy the understanding? We see but a link of the chain of causation;—whence is its origin! Where does it lead to? How shall it terminate? The analogies that are derived from every living thing that surrounds us, people our imagination, and, looking back through the vista of past ages, we behold a creation: looking forward to the future, we predict a final catastrophe; we feel assured that the death and dissolution of the material world draws near.

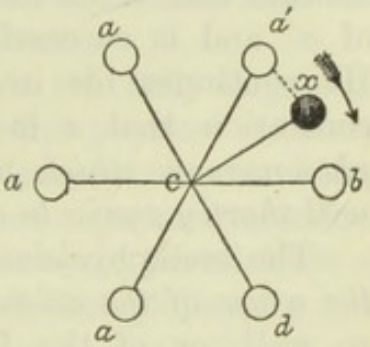

Fig. 1.

Let us not confound final causes with the hidden sources of activity that sustain the phenomena of nature. Let us not class together appearances that are endowed with activity, and those which are divested of every active quality; those which, after having stript off motion—the very essential element of all change—we inquire of, from whence comest thou? how wert thou begotten?

Conceive $x$ $b$ $d$ $a$ $a$, etc., to be a series of balls perfectly elastic, equal in size and devoid of gravity, ranged in a circle, and connected by rigid lines with its centre, $c$. Let a certain velocity be given to $x$, in the direction $x$ $b$; when it arrives at $b$, it will impinge on the ball at that point, and wholly transfer its motion to it, in accordance with the laws of elastic collision; * $b$ will then act in the same way upon $d$, and $d$ upon $a$, etc., until $a'$ acts upon $x$. This circle of motions will go

* That have been determined experimentally by Wren and Wallis.

on without ceasing,—each ball being in its turn the acted upon, and the acting or agent.

The ball *x, which alone we may suppose visible,* will complete a revolution by a series of intermittent motions. Let it, during an interval of rest, be the subject of attention. The metaphysician may inquire the *cause of its existence,* as a solitary and passive or inert phenomenon. The natural philosopher, having previously observed it in a different situation, will inquire the cause of its change of place,—what active phenomenon has happened,—the event or occurrence,—and the cause of it. After some inquiry, he discovers that it has been acted on by the ball *a'*; but after moving to its present place, it has lost that motion, by coming against another ball, *b.* He then inquires into the cause of the motion of *a'*, and is successful in tracing it to a prior phenomenon. He continues his investigations, and at last arrives at the conclusion, that *x must have been in action previous to the phenomenon which first engaged his attention, and that it will shortly again be acted upon in a similar way.*

The metaphysician may say this does not bring us nearer *the cause of the existence* of these balls, that play their part so well, or of the force which successively animates them. If you tell me that such questions are absurd, because nothing can be conceived prior to matter and motion, you must likewise inform me how the so-called absurdity is not viewed as such generally by the world, although deemed to involve an inscrutable mystery.

To this, the answer that has already been proposed can only be repeated: the mental organism which analyses, views everything indiscriminately that presents itself to our senses or imagination as the acted upon; the agent is thus for ever sought, and when found, is instantly viewed in its turn as the acted upon. The faculty seeks only to become acquainted with *prior action*; not matter alone, but matter having velocity; which is synonymous with motion, force, energy, and is implied in the use of the active verb.

The abortive longing for the cause of abstract existence does not seem incongruous, nor does the possibility of the creation of matter out of nothing. Our abstract ideas being derived

from sensible impressions, these are familiar with a multitude of like appearances, such as the growth of plants and animals, where there is an obvious increase without visible addition,—an assimilation of materials from no apparent source. Hence the young imagination delights in the marvellous, and tales of enchantment seem not inconsistent with realities. The relish for such narratives is not spoiled by a matured acquaintance with the true relations of events. It is evident, then, that when our reasonings proceed beyond the relations of common occurrences and sensible objects to those of abstract subjects, our ideas ought to be divested of all the mobile colours which they receive from the feelings, sympathies, and superficial analogies, and be held down to a standard inflexible and independent. Is it not to experimental induction that we owe the true standard of sufficient reason?

It is to be feared, that if the mind is not fully impressed with the imperious necessity of applying this physical standard to all phenomena, whether moral or physical,—if the conviction is not matured and wholly derived from that experimental induction which has revealed the primary and most simple relations of matter and motion,—it is in vain to expect that philosophy will progress towards truth.

## CHAPTER II

### ON MENTAL ANALYSIS

There are four heads under which the analysis of a mental organism may be contemplated:—

(1) The analysis of its *external* exciting causes.

(2) The analysis of its *internal* exciting causes,—those, namely, that are dependent on its organic constitution, and its relations to the other parts of the nervous system.

(3) The analysis of the internal effects of its activity, or the influence which it exerts when active on other parts of the nervous system.

(4) The external effects, or personal manifestations connected with the motor system and with external objects.

If we join with phrenologists in viewing the mind as composed of a congeries of distinct functions, performed by means of specific organs, it is evident that each of such organs must be adapted to something external to itself, by which it is acted upon or excited. Adopting their nomenclature, it may be said that Combativeness requires or is adapted to something external to contend with; Acquisitiveness, something external to hoard. The external object or quality must be distinguished by a specific peculiarity, that enables it to act on the particular organ, and the internal adaptation of the organ must have reference to and be adjusted to the specific quality of the external object. To discover the specific quality is one great step towards determining the type of the mental function.

The second and third heads involve the analysis of the structure of the organism (assuming it to be material and quickened by vital influence) and its physiology, or the functions it performs while being acted upon by external influence, and acting upon the motor elements.

The fourth head involves the analysis of the personal manifestations: not general views of conduct and action, such as moral writers are accustomed to dwell upon, but a minute scrutiny of the individual acts,—the muscular exhibition, in short, which accompanies the gratification of the organism,—the mode by which the active physical powers are influenced by it,—the analysis of these actions, and their relations to all external objects and influences.

If this mode of discussion were to be successful in eliminating the primary and most simple forms of action belonging to the mental principles, we should be enabled to enter into the study of the relations that subsist between them with some chance of success. Of these, some are apparent without elaborate analysis, especially among animals in their natural wild state. Thus, Combativeness and the Sexual Instinct. Males fight for the possession of the female, not in deadly conflict, but to try their strength,—thus giving the predominance to the most perfect organization, and securing a vigorous offspring. Hunger and Destructiveness, with carnivorous animals. The latter is evidently subservient to the

former. With mankind this also appears to be the case. Hence probably the source of the sportsman's delights. The general propensity to *retaliation* that is found in all classes and conditions of men, as well as some of the higher animals, seems to be a very powerful ebullition of Destructiveness or Combativeness. The excitory motives being various, shows that this principle (if it is entitled to the name) is liable to be excited by all others when they are painfully affected. It may therefore be justly doubted, whether it is to be considered as a primary source of action, or only as a state induced by other mental principles, when excited in a manner discordant to their natural constitution. The practice of cannibalism gratifies at once the hunger and revenge of the savage. Instances are given of New Zealanders having sucked the blood of their conquered enemies before putting them to death. The fiery torture of the North American Indians, and the atrocities of religious persecution in Europe, are other exhibitions of destructiveness, where the spirit of malignant retaliation seems to have revelled in excessive indulgence. In all cases, this spirit seems to be gratified by extorting personal manifestations of suffering; and if a similar spirit animates the opposite party, his strength of mind is engaged to the uttermost in withholding all such exhibitions.

Among gregarious animals, individuals maimed or weakened by disease or age are invariably put to death by their companions. This is reported to be the custom among the inhabitants of Socotra; also among certain South American tribes. Such instinct has apparently for its object, both the relief of the individual from pain, and to prevent it injuring the breed. It has been observed among the wild cattle of Chillingham Park: the weakened animal is gored to death by the herd. This is a special effort of destructive energy for a special purpose, distinct from hunger or retaliation.

An important relation seems to subsist between the love of approbation, or sympathy of expression, and the sexual instinct, for the purpose apparently of establishing or inducing the state of contemporaneous excitement between the sexes, which is necessary to ensure conception. Among the lower animals a grosser influence seems sufficient for this purpose.

Most of the passions and bonds of human society appear to have their simple type among the higher classes of animals, although presenting in man a much more complicated form under the influence of cultivation, and a more extensive range of action. Thus the primary sources of action have—

(1) Relations to food: hence, in man, the hoarding and hunting propensities; in animals, the particular habits depending on the nature of their food.

(2) Gregarious or social relations: hence the division of society into tribes, clans, parties; hence the artificial distinctions of rank, by which superiors and inferiors are recognised.

(3) Defensive relations: hence animals protect themselves against their natural enemies.

(4) The relations of sex and progeny.

The Cautiousness of phrenologists seems to be the major element of the third class of relations. It appears to stand alone in function and importance, and instead of acting a part along with others, it commands them all, influences their activity, and watches over their manifestation. Ever wakeful to guard against bodily injury, pain, or discomfort, it continually suggests the idea of danger and summons up the faculties to aid in escaping from or avoiding it. Phrenologists class it with the propensities; but it is evidently unique in all its powers and relations. The rule, that a propensity desires gratification with a degree of energy proportionate to the size of its organ, cannot be applied to it. Its function is to avoid every kind of pain, not to enjoy a specific gratification.

There is also a broad line of distinction between the two appetites, hunger and lust, and the other mental principles. The cravings of the former recur periodically, and are more directly dependent on internal stimulus than the latter. When unsatisfied, their effects on the constitution are also much more striking. The concatenation of motions which consummate their function, has a degree of prominence which is wanting in others, and terminates in both with involuntary acts. Respiration has in this respect some singular analogies with the appetites, which almost warrants it in being classed alongst with them. Although no excitement accompanies the act, it is both semi-voluntary and rhythmical in its character,

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and highly concatenated: the craving, or *besoin de respirer*, also is urgent in the extreme, when delayed beyond its accustomed period.

In all the three—respiratory craving, hunger, and lust—when circumstances or voluntary effort delays gratification, the concatenated movements become less under the influence of volition, and actions that usually seem quite voluntary become almost automatical. This is most distinctly the case in respiration. In restraining the act of breathing, the muscles become less and less under control. In hunger and lust a similar analogy exists, although the mind is more violently affected, and the involuntary character of the muscular exhibitions is lost sight of, when viewing them as the results of a delirious or depraved state of the mind.

The cardinal vices, common to all classes and conditions of men, being abuse of the appetites and gambling, demonstrate the conspicuous place which the natural functions of these principles occupy in the psychical organism. The higher parts of the moral fabric, instead of being equal, or, as some would have it, superior in importance, seem to be as much subordinate to them in man as among the lower animals.* This opinion degrades the human character perhaps too much to be cordially assented to by any one: by the majority it would be scouted as false and impious; yet is it not well supported by analogy? and is it not consistent with the life and habits of nine-tenths of *the human race?*

Analysis ought to discriminate the exact features of those relations which have been cursorily alluded to here, that attention might be directed to the manifest omission of phrenological writers, in overlooking their vast importance. They have compared the mind with its plurality of organs to a musical instrument; but in discriminating the different tones, they seem to have paid too little attention to the natural scale upon which their harmony depends. The individual and the relative functions of the different organs of mind may, with greater apparent accuracy, be compared to the different parts of the animal system. The digestive, sanguiferous, and

* Cabanis, in his *Rapports*, appears to admit this. He is perhaps the only philosophical writer who has expressly done so.

respiratory organs, act very distinct parts in the animal economy, but all are connected by their common relation to the process of nutrition. So the elements of mind may differ

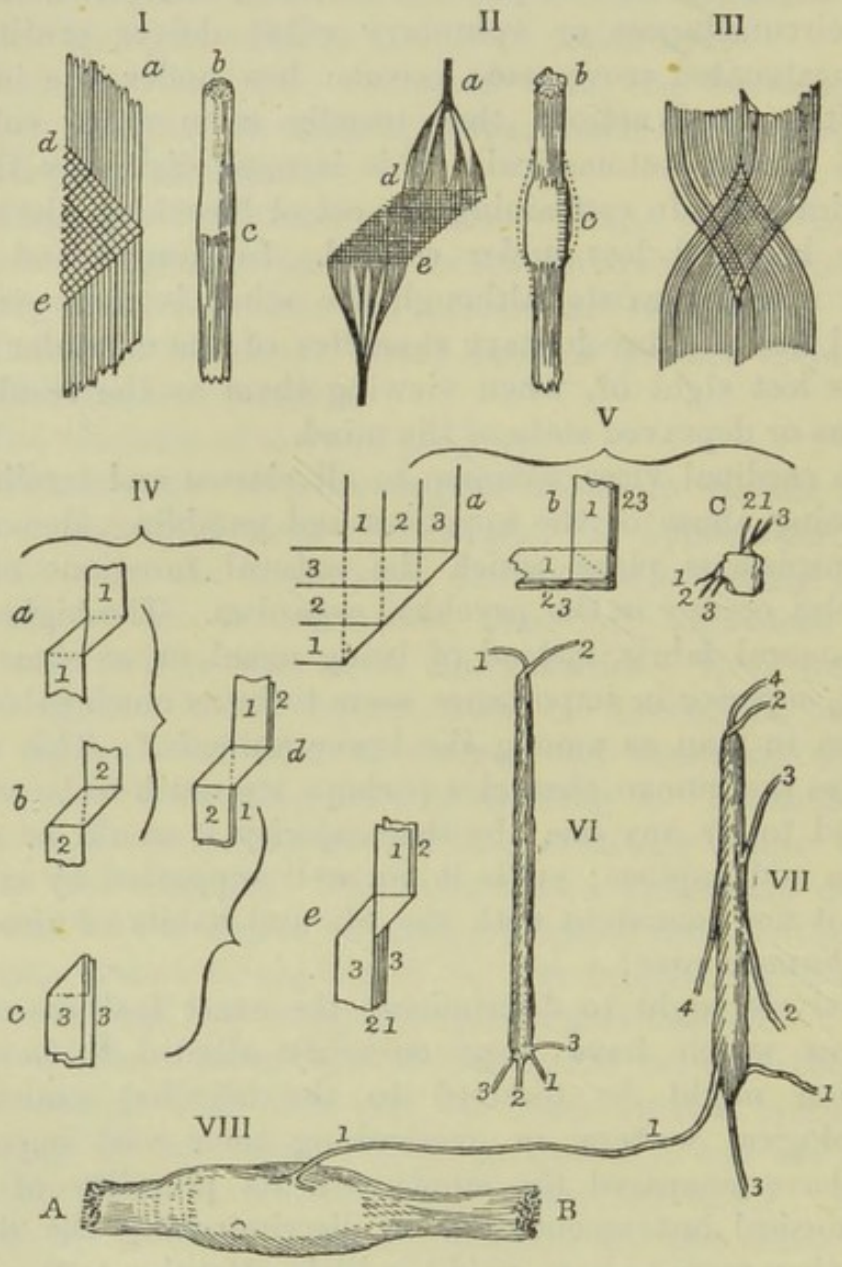


Fig. 2.

as much in their primary mode of action, and likewise in their relative place in that chain of connexion which binds all together in ministering to the welfare of the individual and conservation of the species.

## CHAPTER III

### ON ASSOCIATION

1. Continuity, resemblance, causation, are the three principles of association, according to Hume. Contrast has been added by later authors. The two first appear to be the most evident; the two latter will probably be found to belong to specific faculties, not general and universal properties of sensation and volition.

2. Continuity may be more analytically expressed as the juxtaposition of ideas or impressions in point of time. Something pleasant or offensive happens to me at a certain place, at a given time, and the place and the event become connected afterwards in the mind: the one suggests the other. Hume's example,—"the mention of one apartment in a building naturally introduces an inquiry or discourse concerning the others,"—is included in this definition, if the building was previously an object of contemplation and attention.

3. The term contiguity would appear to refer ideas to a relation of contiguity of place or situation in the objects, without reference to time; but the latter is evidently included in the former, and extends to a wider range of phenomena.

4. Language, in its most extensive signification, is connected with the particular meaning by this principle of association. The word *horse* must be associated in the mind of a child with the impression of the animal, before it can be understood; and the connexion that subsists afterwards between the name and the idea of the animal, is evidently derived from both impressions having taken place at the *same time*. They have thus a juxtaposition in regard to time.

5. Such a connecting principle cannot account for the faculty of language. Animals have the former, but not the latter. Association is the *modus operandi* of all the mental powers, but is not in itself an impelling principle. It is the channel through which all mental activity must be exerted, but is not of itself a source of original action.

6. The philosophy of language presents us with many

examples of the influence of association, in suggesting the form of the characters, as with the Chinese,—or the sound of words, as in the rudiments of our own language, and probably most others. An effort of imitation characterizes both; in the one case, suggesting the original form of the characters from the form of the object signified; in the other, the sound of the words from the sound of the act. The poverty of the Chinese spoken language, contrasted with the richness of its written character, would show a greater disposition in the people to express ideas by form than by sound. The reverse is the case with the languages of Europe. The written character is the extreme of simplicity, while the words are flowing and harmonious. It would seem as if the musical character of a people influenced the melody of their language. Thus Italian, the language of song, may be contrasted with the monosyllables of the Chinese, the abrupt terminations of which give a singular and ludicrous effect to their conversation.

7. In proceeding with the analysis of this principle, we must keep in view that the mind must be adapted to receive impressions before these can excite ideas; and some kinds of impressions are more calculated than others to do so. Thus the mind must respond in its action to the external influence of the sensitive impression; and in proportion to the intensity of this action, so are the ideas more strongly attached to each other, as well as more durably impressed.

8. The transformation of an impression into an idea may be said to depend upon mental power, combined of passion and intellect. It evidently does not depend on the magnitude or intensity of the impression: a glance or whisper, addressed to the passions, will be remembered for years, whilst the most gaudy spectacles make but a fleeting impression. According to the natural constitution of the mind, so will be the complexion of the store of ideas,—the most prominent being those which are continually sustained by the most powerful of our feelings or faculties. Such ideas are most difficult to efface, as they are the most liable to be excited by trains of association.

9. This specific attraction, in regard to time, indicates a specific structure of the mental organism. The passions or faculties, acting and acted upon by the simultaneous impressions,

may be totally different and separate, yet they are permanently connected by this simultaneous activity of two parts of the brain.

10. *Instance of suggestion.* The remembrance of a conversation that has passed in the presence of an individual, who cannot at first be brought to recollection; but, as the train of what was spoken passes through the mind, a feeling is re-excited by it, which was at the time caused by the subject of conversation having a relation to his profession. His image and name are instantly summoned before the mind's eye. In this instance, it appeared that if the feeling had not been excited in addition to the connexion of ideas between the subject of conversation and his profession, his image would not have been recalled. The re-excitement of the feeling gave instantly excitory force to the impression.

11. The organic recipient of impressions is capable of a vast variety of different motions, but these are caused from without. There is also an internal cause. What is it? The sustained watchfulness of the brain? A perception of continued effort accompanies sensations or impressions. Now what is this? Is it not something in the mind that measures time? Take an extreme instance—strong muscular exertion. This fixes itself in the mind, associated with the objects acted upon, and afterwards they recur together. We are most conscious of effort when exertion approaches to exhaustion. Volition, which may be called the consummation of mental activity, is difficult to analyze, further than as it appears the simple consequence of a preponderating impulse of the mind. Passion, which is an extreme case of voluntary activity, sometimes incites so strongly to action as to be almost beyond control. Passion is an eagerness for action with a specific object in view.

12. Association acts with greater force according to the mental excitement that accompanies it. But mental excitement is, as aforesaid, an irritation of the elements of action. Thus we come to the conclusion, that ideas are strongly imprinted and associated when accompanied with a powerful excitement of the elements of action.

13. The impression from without is adapted to excite the element of action within, and the recurrence of the latter

recalls the former. Thus the impression A excites the elements of action X, and B excites Y; the result is, that the strength with which A and B are connected by contiguous association depends on the power of X and Y. The attractive power subsisting between consecutive motions, A and B, depends on the intensity of X and Y.

14. The material elements of thought, by means of association, are agitated in a peculiar way, with a consecutive train of motions, by external influences. The agitation affects the inward springs of action. The consecutive train may again be brought into action by the excitement of any one part, and the springs of action are simultaneously awakened. Now, which is the antecedent motion? An external object, that has formed part of an associated train, awakens the whole; but is not this in consequence of the springs of action being first influenced, and then acting upon the images of things that are associated with them?

15. Muscular action obeys the same law of association—speaking, writing, musical performance, handicraft trades. These, analyzed, resolve themselves into a confirmed connexion between the elements of sensation or perception and motion. The connexion of ideas is probably accompanied with a similar excitement of the motor elements, without muscular activity.

16. These instances indicate a peculiar mental adaptation; still they do not appear to lead us nearer to an explanation of the fundamental characteristic—that one motion in the organ of thought induces another, if they have previously acted in contiguity of time.

17. There is a property of the mind which deserves to be thought of previous to this analysis: it is memory—the power which a mental organism appears to have of retaining ideas corresponding to previous impressions from external objects. The idea bears no comparison with the impression in point of vivid distinctness, yet it is the true representation of it in all its qualities of form, magnitude, colour, etc. If it is the same organic substance that retains an impress of all those faint images with which the mind is stored, how is it that they are not confused by interference? It is evident, from what has been suggested above, that the impression from without,

assisted by a power from within, effects a change of some kind in the organic structure.

18. The sexual passion is excited by the personal appearances of the opposite sex; hence we may conclude, generally, that the change which takes place in the organic structure, corresponding to such impressions, influences personal demeanour and action, and irritates the elements of specific action. We may thus adopt, as an approximate deduction, that perception is a certain change in organic structure, accompanied with an irritative influence over certain specific elements of action, and that these may exist either in a latent or active condition.

19. To explain association, it is necessary to determine the essential circumstances that connect two motions that are nowise related except in the order of time.

20. While the organic particles are impressed in a consecutive order by external impulse, there must be an intermediate influence pass between them, that attaches them in a new relation to each other, without the external influence being repeated. Now, what is this intermediate influence? The only way by which one motion can be connected with another, as cause to effect, is, either by communicating the specific impulse, as the hand giving motion to the pen, or by directing power in a particular line of action, by removing the necessary obstacles, or forming the necessary connexion, as putting machinery into gear. Since impressions from external objects are not dependent upon one another when entering the mind, but are evidently so when recalled, there must be a putting in gear of some kind.

21. When an impression is recalled, it seems as if it were the effect of the previous external impulse; but all the impetus or power with which it acts is derived from an internal source that is subject to decay, if not refreshed from time to time by the vital energy being directed towards it.

22. There is something in the mind that registers time, and when the same subject has been recalled repeatedly, it acquires an addition of some kind, which leaves an impression that it has been so recalled. May this not be simply explained by its having been connected with a new line of contiguous association?

23. Keep in view that this related excitability is subject

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to decay. The impression is certainly a disturbance in the elements of organic structure, which they are adapted to enter into as soon as the external influence is applied. In this case, the external impression *directs* the living or cerebral energy towards the seat of this organic change, and the next towards another: in recalling the impression, the cerebral energy tends to flow in the same direction. Now the great point to be explained is the *cause* of this tendency.

24. The cerebral energy vivifies each impression as it passes through it. It must find a conductor from one seat of action to another while the original impressions are succeeding each other. The connexion is stronger in proportion to the emotion excited by the idea. The two excitabilities, in proportion to their original energy, are more strongly connected; but this energy depends on the action of the mental organism that is adapted to the impressions, which thus determines the strength of the connexion of contiguity.

25. Consciousness moves from one to another. Is not the seat of consciousness in the related excitability of the motor and sensitive elements?

26. The sequence of the impression must establish a connexion between the matter moved in the two contiguous impressions. This will account at once for the liability of impressions to be excited by the motions of those adjacent, and their individual distinctness is preserved by the original specific nature of the cerebral substance.

27. The connexion between perceptions that have any quality in common is easily accounted for, by the part *holding* the said quality being common to both, and therefore in so far they are identical.

28. How are the parts of any specific and extensive impression, that is simple and presented to the mind at once, registered in the cerebral apparatus? It must be so, that when another impression, containing a part similar to a part of the first, awakens that part, the whole of the first is excited. It appears to be a similar property that connects the train of impressions, when they succeed each other continuously: there appears to be a relation of apposition as well as of consecutive time. Can we arrive at an explanation, by classing both as effects

of the same cause? Every impression leaves a specific sensibility in the organic part affected, which causes it to resume its original action when any other part is excited that was *contemporaneously* affected before.

29. The grand principles of association are *simultaneousness* and *similarity*. The agitation caused by an idea or impression lasts for a perceptible instant; and while another contiguous one is acting, the latter part of the first may be simultaneous with the first of the second. Similarity ought not perhaps to be reckoned an original quality of association; because, when two impressions are similar, but not identical in all their parts, the comparison and recognition of the similarity are distinguished by their respective simultaneous parts being associated, and marking their difference in all particulars. It does not require any specific principle of association to cause like impressions to excite like feelings or ideas. That the mind should run from one train of thought to another, both having a common point of resemblance, is accounted for at once by the principle of simultaneousness, if we reflect that the point of resemblance is actually an element of thought identical in both; and when it is excited in the first train, the principle of simultaneousness is awakened in the second. It is therefore active in both, and will carry the mind in the direction where the ideas have been most vividly impressed (27).

30. Let the motive apparatus, C and D, be excited by the respective simultaneous impressions, $\alpha$ and $\beta$. Let us further suppose all impressions to be so related to the motor elements, that although A only responds to the impression $\alpha$ by organic adaptation, yet when it happens to be excited simultaneously with another, $\beta$, the latter acquires the same influence over it that $\alpha$ has by natural adaptation.

31. This is a point that deserves attention: it seems to show, that if such a transverse connexion, as $\beta$ to $\alpha$, were kept strongly excited for several generations, it at last may become as established as that of $\alpha$ to A; and become communicable to the offspring as a fixed element of the organization. Mr Knight has proved such to have taken place in certain breeds of spaniels. (*Phil. Trans.*). This discovery is of the highest psychical interest; it opens a wide field of research, but

unfortunately so difficult and tedious as to be in the power of very few to cultivate.

32. In the act of associating there are two sets of fibres in motion at the same time; and these become so connected, that on one being afterwards excited, the other spontaneously resumes its activity.

33. In what consists the predisposition to action in the parts not immediately acted upon? Is it not the very essential quality of this simultaneous associating principle? Is it not this that binds together the parts of the same impression, as well as those that are different, but still simultaneous?

34. The brain supplies to the associated idea the same kind of impulse as the original impression. Does not one impulse give rise to both?

35. The impression of an object appears to be consciousness which the mind has of the state of the apparatus of sense. The idea is the action of the impression on all the mental *organs* of perception. The feeling of pleasure or pain, or disposition to action, that accompanies the idea, is the result of the natural adaptation that subsists between the idea and the organism. Other ideas, that are intimately associated with the first, obtain an equal excitory power; but this connexion is established after birth,—that of the first is *ab initio* part of the organization.

36. The train of mental phenomena may thus be described:— The organs of perception seize upon the different qualities of each impression, and give them forth again to form the idea. The next part of the mental process is the excitation of the propensities, by the ideas adapted to them lodged in the perceptive faculties, and the reciprocal influence which the propensities possess over the ideas. Indeed, all the parts of the train of influences may be said to be reciprocal, acting in both ways, and, being simultaneous in their original excitation, are associated in the closest manner.

37. The peculiar organized connexion of part and function that establishes this relation is yet unknown, but may be considered as an original type in the structure of the brain, that pervades it throughout. Now, something of a similar connexion must influence, or be the cause of all simultaneous

excitation of the brain, not only of those excitabilities that exist prior to external perceptions, but those that are connected by simultaneousness only. If both are of the same kind, they may be distinguished,—the one as permanently existing, the other as requiring to be supported by occasional re-excitement. And yet, perhaps, this is but a difference of degree.

38. It appears to be one act of consciousness that perceives two simultaneous impressions, although their locality in the brain is no doubt very distinct. Thus, auditory and visual impressions are attached to distinct parts of the brain. The same part cannot be assigned to both, as their function is totally distinct; yet when simultaneous they are instantly connected together, so as to be recalled by the same impulse. As this phenomenon takes place between all parts, the act of simultaneous excitement must determine the peculiar connexion that influences their after-appearance (9).

39. Is the internal power of excitement of the different organisms derived from one seat of power? or is it eliminated from the blood at the respective localities, as in the case of muscular motion? There is an analogy: the medullary fibres may conduct a specific influence, that determines the evolution of power in the cerebral organ, as it does in the muscle.

40. The column of the spinal cord, which gives out the motor nerves, has probably a closer connexion with the organisms of the propensities than with those of perception. Each propensity appears to have a specific relation to certain impressions on the one hand, and to be disposed to give a specific direction to muscular activity on the other. Their highest state of activity and enjoyment is when they are acted upon by all their adapted impressions, and acting upon the motor elements in their specific manner.

41. Impressions act upon the organisms of the propensities by means of and through the perceptive faculties. There must therefore be a class of conductors between the perceptive organs and the propensities. The influence which they transmit is probably similar to that conveyed through the motor nerves; and if we carry out this comparison, each filament is probably a conductor of this influence, and isolated

from those adjacent. A very large proportion of the cerebral mass is probably composed of such conducting filaments.

42. The association of muscular contractions is a phenomenon probably similar in kind to the association of mental conditions. In the exertion of muscular power, there appears to be an influence transmitted *to* as well as *from* the brain. There is also a mental state prior to muscular action—the *directive effort* of the mind. Without attempting to define what this is, it is evident there must be a something going on within, in correspondence with and prior to that which is taking effect externally. If the inward directive efforts are associated, so will be the motions. (See Chap. IV.)

43. Viewing these associated motions as the simultaneous excitement of various muscles, in various degrees of intensity, and selecting some example of this kind of association to keep before the mind's eye, it appears that we can scarcely trace the exertion of a directive effort to every point of a quick succession of motions, but that there is a very lively consciousness of the muscular exertion, its extent, degree, and direction; and this distinct perception of one motion is the means of directing the due performance of the next.

44. There appears to be a similarity between the mental phenomena attending voluntary motion and those attending thought or the exercise of the perceptive powers; both are influenced directly by the passions or propensities; but the one acts upon external objects, while the other acts on the images or sensitive impressions of things. There appears to be very little doubt that the law of association is the same in both, and that the influence communicated, and the nervous fibres that communicate it, are also similar.

45. The two prominent points of the mental economy thus appear to be, *perceptive consciousness* and *directive effort*. The passions, or organisms of the propensities, are the great sources of activity, which are acted upon by the former, and act upon the latter.

46. In motor association, the effort is simultaneous with the degree and direction of the preceding muscular exertion. Is not a similar conjunction of phenomena necessary in all cases of association? And may it not be defined, as the simultaneous-

ness of an influence that proceeds from the seat of consciousness with another that arrives at it?

47. Does this apply to ordinary trains of thought? If so, what is the influence that proceeds from the seat of consciousness? Is it not the mental force, arising from the excitement of the faculties of thought and propulsion,—the same, in short, that gives life to the impressions on the one hand, and excitement to the muscles on the other? The influence that arrives at the seat of consciousness, consequent to muscular action, is from the muscles (?) and sensitive extremities moved, which bear an impress from the external objects involved in the motion,—that is, they must be directly influenced by the latter. The influence that arrives at the seat of consciousness, in ordinary trains of thought, is the perception of ideas which manifestly bear the impress of external objects. Thus there appears to be a relation of physical resemblance between strictly mental action and muscular exertion. There are distinct impressions in both, analogous in their centripetal and centrifugal relations to the centre of consciousness.

In this we have a glimpse of a feature that seems common to all the mental functions, and which may prove serviceable when prosecuting their separate analysis. (See Chap. IV.)

48. Does the fibral excitability, corresponding to an idea, when not the object of consciousness—when not present to the mind—retain the same condition as when it forms part of a train of thought? Is it latent merely because it is not in connexion with the seat of consciousness—because it is not felt? or is the consciousness of it a re-excitement? the non-consciousness of it an absolute cessation of activity in the seat of the idea, retaining the capacity of being re-excited?

49. Each idea is connected with many trains of thought, but the attention is always carried to those which have the strongest connecting influence.

Suppose ideas traverse cerebral fibres, these must be connected with each other in different degrees of force, which are subject to continual change, both from external and internal causes. Whatever may be the organism that retains the idea, and connects it with others, there must necessarily be an

intermedium of connexion, if it is at all to be treated as a subject of human comprehension.

50. The mental power which constitutes attention is not carried along from one idea to any of the others indifferently, with which it happens to be associated, but it is confined to that one whose excitability is greatest. The mysterious connexion of association thus depends for its absolute existence on the contemporaneousness of ideas, and for its strength, or conducting power, on the relative intensity of such ideas.

51. It has been remarked, that the intensity of ideas depends upon a specific cerebral reaction, as well as an external influence. Their disposition to associate with other contemporaneous ideas is thus dependent upon specific cerebral reaction. The exciting influence of consciousness passes easier from one to another when the reaction is strongest. There is less obstruction to it in such circumstances.

52. If the medullary fibres communicate with each other only by means of the cineritious part of the brain and nervous system (an opinion favoured by Müller and other eminent physiologists), the connexion of association is probably consummated in such matter, which is most abundantly distributed upon the external parts of the hemispheres; the medullary or white portion being supposed to consist of insulated conducting fibres only. On this hypothesis, it is in such parts of the brain that the specific reaction takes place on impressions, which bestows on them a greater or less degree of prominence in the memory,—in other words, that establishes their power of connexion, their relative excitability.

53. Does the simultaneous excitement of two different parts of the cerebral surface modify the intervening cineritious matter, so as to establish a lasting communication between them? The difficulties in this hypothesis are, *firstly*, The double function attributed to the grey matter, of not only originating power, but also conducting it; *secondly*, The interference that the multitude of crossing lines of communication is likely to occasion; *thirdly*, Every single point must be capable of communicating with every other point. Each possible line of communication cannot have a distinct fibre appropriated to it; one part of one line must therefore form

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a part of another, and occasionally of an indefinite number. Thus a single elementary particle must have an indefinite number of conducting influences passing through it at different points and at different times, which must not interfere. Now, is such non-interference to be attributed to impressions acting in different lines or at different times? Difference in the time of excitement has certainly a power of insulating one impression from another, as identity in the time has the power of connecting them by association. With all this, we have not advanced in reducing the associating principle to a simpler form.

54. Another mode of explanation occurs, which presents a more favourable aspect. Every impression on the senses must have free access to *all parts* of the brain, or to the several localities of the specific action and intellectual powers of the mind. When impressions of sight and hearing are simultaneously active, they are conducted at once to the same parts, but excite them probably in different degrees.

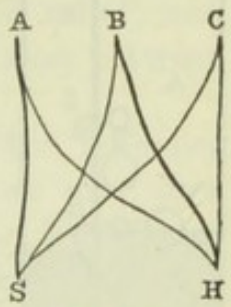

FIG. 3.

Thus, let S be the visual impression, and H the auditory, and A B C, etc., the several localities of the brain where specific powers are excitable; S will be conducted to all and H to all; but it may happen that S is able to excite A only, and H, B only, to *conscious* action. If we suppose that H, while conducted to A, receives from it a reactive connecting influence, during the excitation of the latter by its proper stimulant S, an associating power would be established between S and H.

55. Certain analogies favour this view; but there are difficulties. Let us attend to the latter. The same conducting filament serves for a variety of impressions: Why should the particular impression H be excited in the auditory conductor by S through A, in preference to any other with which it may have previously been affected, and which exists in the filament in a latent form? It is impossible to answer this, in ignorance of the primitive nervous organism and influence, but there is nothing contradictory in such hypothesis. The specific excitation of A and H are established, in a relation differing from that of

others, by means of the circle of conscious and unconscious action, S A H B S.

56. A mechanical simile may be useful here. Let us suppose a vessel, A, to have three valves, *b a c*, fixed in one side, at different heights, communicating with another vessel, H. If the water in A is made to rise to *c*, the valve *c* will be opened by the attached float, while *a* and *b* remain shut. If it is made to rise to *a*, *c* will be shut, *a* will be opened, and *b* remain shut. If the water is made to rise farther to *b*, the valve *b* will be opened, and *a* and *c* will be shut. Let the difference in the height of the water in A and H represent a difference in the time of excitement of the organism A, and of the nervous conductors H, and let their simultaneousness be represented by equal heights. We must conceive, further, that when the water in each is at the same level, a valve is fixed at that level. We thus have an arrangement, by which a relation is established between the level of the water in the two vessels, similar to that between simultaneous impressions: the one induces the other, if they have been contemporaneously in this condition before, not otherwise.

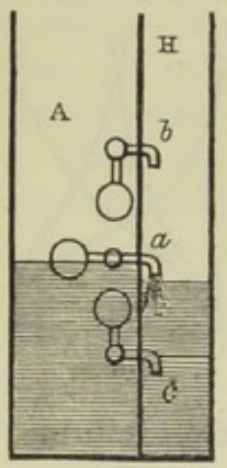

Fig. 4.

57. It would be easy to modify the arrangement of the valves, so as to represent the intensity of the excitement in each fibre by the height of the fluid in each vessel, and that the supply of fluid (of vital force) to all the vessels (all the fibres) should be drawn from one reservoir (the blood). The simultaneous action of the valves binds them together. The characteristic of association consists in the related action of the valves. That which answers to it, between the nervous fibres, is a mystery of molecular action, probably far beyond the present ken of science.

58. In these speculations, it is taken for granted that the conducting fibres have the power of assuming a variety of conditions at different times, and retaining an excitability for each; but is not such also the case with the nerves of sense? After the simultaneous excitation of S, A, and H (§ 54), the excitement of H is associated with A in a way similar to S. The connexion of the latter is an essential part of the organ-

ization of the brain—a special adaptation of structure; that of the former is incidental, depending on its simultaneous excitement with A. Nevertheless, this sympathy between (H and A) powers or influences naturally incongruous, shows distinctly that, on such hypothesis, the contact of an excited conductor with an excited organ (to use the phrenological term) determines for the future a mutual excitability between them.

59. If such views are correct, they develop a property or function of nervous tissue of a primary kind, upon which all mental operations are carried on, and which, by analogy, may be supposed to influence the organic functions of the animal system, and their manifold relations to the phenomena of consciousness. The analysis of individual elements of mind will bring out this primary function in higher relief, and develop more fully its influence over the connexion between the motor and sensitive elements.

60. Whatever may be the essential nature of the influence that passes so rapidly through the nervous fibre, we may view it as a molecular change of some kind, similar throughout its whole extent, whether it passes in a centrifugal or centripetal direction. The action in one part may be viewed as the *effect* of the action in the part adjacent. If the fibre should separate into several branches, it may be inferred that the same action is possibly conveyed through them all (54).

61. Suppose two fibres, each transmitting a different influence, to lie adjacent at a part of their track, and to be in such close contiguity that the molecular disturbance that exists in one may affect the other also,—what difference is there in the causal relation of the adjacent parts of the same and of the different fibres? If the organization, at the point of junction, in the one case, was similar to that in the other, there would appear to be no obstacle to the same influence passing from one to the other. What will be the consequence of a less intimate union, when the molecular influence is not allowed to pass over but interfere with each other partially?

62. It is necessary here to keep in view, that the strength of the association depends on internal influence—a reaction from an internal organism; that ideas, or the traces that are left by impressions, are quite distinct in their nature, and

depend for their existence on the activity of an internal organism; that the same fibre conveys an influence that not only fluctuates in intensity but in kind. The optic fibres acquaint us with the colour as well as intensity of light; and so with all the senses.

63. When impressions become ideas, is the excitement of the sensitive fibres transferred to a specific apparatus, in which the associating principle is developed?—an emanation from the source of vital energy, that gives to the idea its power? The impression or idea has a property, to which an established adaptation of a cerebral organism responds. It has the property of opening the valve that admits vital force, and *simultaneous ideas become endowed with the same power.*

64. We must not forget, that the excitement of one fibre has not such power; but it is the combination of a great multitude, and that individuals of the multitude are bound together by association. How do excited fibres come to have excitable properties in the aggregate which they do not individually possess, or out of a certain order? Their combined action is the key that opens the source of vital influence. A number of partitions pierced at a single point will not admit light to pass, unless they are arranged so that all the holes are in a straight line. One out of place obstructs the light.

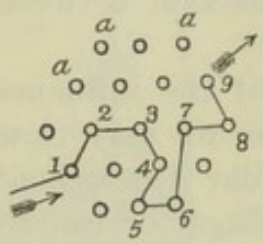

Fig. 5.

In a multitude of conductors, *a a*, etc., a current of electricity may be made to take a determinate route through a select number, 1 2 3 4, etc., if these are closer together than the others, or if the medium is a better conductor between them. Let us fancy such an arrangement, and, further, that each is provided with an insulating cover, that prevents the passage of electricity. If the conductors, 1 2 3 4, etc., are uncovered, a current of electricity will pass through the group, and act upon any apparatus on the other side. If any other set of conductors were uncovered in like manner, it is plain that no current could pass. It is by the pre-arrangement of the particular set that the electricity is allowed to pass to the other side, and act upon any other set in any specific way that may be arranged. We may compare the uncovered con-

ductors to nervous fibres in a state of excitement; those in the route, 1 2 3, etc., to the elements of an exciting impression, which vital energy pervades, and acts upon motor elements on the other side. To complete this simile, it will be necessary to suppose, that the simultaneous excitement of adjacent conductors, and the passage of the current through the interposed medium, deprive it of its insulating qualities, render it a better conductor, and that a long cessation of such excitement restores its original non-conducting properties.

65. When the adaptation is responded to, vital force is immediately directed to the part, and perhaps, in the act of passing through the termination of all the excited fibres, connects their excitabilities together. When the order of excitability from without agrees with that within, an immediate evolution of vital force ensues, which seems to be the cause of establishing the relative excitability of association. The action of memory—the force expended in recalling an associated idea —comes from the organism affected. As this organism cannot be excited to action by isolated fibres, we may perhaps conclude that it cannot *react* upon isolated fibres. Thus, if one part of a perception is excited by an external cause, and it is sufficient to awaken the organism to which it is adapted, the awakened action necessarily excites the remaining part.

66. The great difficulty is in conceiving how the many specific kinds of excitement are re-developed in the fibres by any single impulse. The external exciting cause of an impression is a direct action of the medium of the sense, which varies in intensity and quality; but it would seem that the internal re-exciting impulse must be simple. How then can a simple internal impulse excite such various perceptions?

67. If we supposed the impression conveyed by a single fibre to be decomposed at its termination, and each simple element stored in a separate reservoir, as it were, which could only be excited in one degree, the establishment of a relative excitability might be explained, if they all crossed each other, and at the point of contact were exposed in a greater or less degree to each other's action, according to the conducting qualities of the point of contact (§ 64), which we must further suppose to improve the more frequently they are simultaneously excited.

Let the fibres *a b c* (see Fig.) only be re-excited in a perception composed of *a b c d*: the action of *a b c* instantly excites *d*, and the perception is completed. It is evident that the influence of *a b c* must have easier access to *d* than to any other fibres, *e f g*, etc., although it may be, that between *a* and *g* there is a stronger connexion than between *a* and *d*, and between *b* and *h* than between *b* and *d*. The direction of the excitable influence must be determined by the state of conductibility at *all* the points.

68. As electricity and nervous influence have analogies, we may inquire if electric conductors, arranged in a similar way to the nervous fibres *a b c d*, etc., would have similar effects. It is known that all the electricity would pass through the point of best conduction where there was least resistance to its motion; but is it known how far the force existing at the other points may assist its passage? In arranging such an experiment, the points that connect *d* with *a*, *b*, and *c*, must be more or less imperfect conductors, and the current so regulated as not to pass through any of the points while one only is applied. Under such circumstances, if the current passes through any one when *all* are applied, the experiment will show conclusively that there is a resemblance between the action of electricity and of the nervous influence in the act of association.

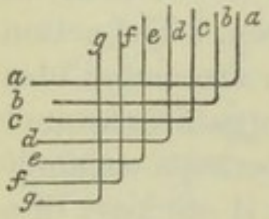


Fig. 6.

69. Such analogies are apt to lead us prematurely to expect some discovery of consequence. The discussion which Dr W. Phillips' experiments on the influence of the *par vagum* on digestion have given rise to, and the conclusive experiments of Müller, seem to show the futility of expecting much advantage at present from these analogies. Electricity in the nerves is attached to animate matter; it cannot therefore be expected to respond to the same tests as electricity attached to inanimate matter, although occasionally perhaps evolved in a free state.

70. If the molecular impulse is unlimited in the degree or species of the disturbance, and adjacent contemporaneous states of activity become mutually excitable, what law of

contiguous action does this involve? In the act of such a state of mutual excitability taking place, the parts of the two fibres that lie adjacent are molecularly affected in a specific way. We must conclude, that the temporary active qualities impressed upon the organic elements of the fibres generate an attractive force between them; of such a nature, that the same action being renewed in one, the molecular change, which may be said to be the cause of such action, induces in the adjacent elements of the contiguous fibre the specific molecular change with which it was previously contemporaneous. Thus, in two filaments, A and B, let the contiguous elements be distinguished by *a* and *b*, and the contemporaneous impressions by *m*, *n*. Now, the facts that have been ascertained regarding the excitability of the nerves seem to prove, that in the conducting of impressions they are not passive, but that they are organically arranged to respond to the action of the medium by which they are excited, within certain limits. The *modus operandi* by which they do so must involve a change in the relative position of the elements, however minute; nor is it going too far perhaps to say, that upon the relative position of the ultimate organic elements depends the action conveyed; so that if the position corresponding to any impression could by any means be induced in a fibre, that very impression would be transmitted.

A small galvanic circle being completed in the mouth, causes a flash of fire to appear before the eyes. Pressure on the shut eye makes luminous spots appear. In these instances the molecular disturbance is the same as that caused by the action of its ordinary stimulus, light.

Let the arrangement of *a* and *b*, corresponding to *m* and *n*, be represented by *a m*, *b n*. It is necessary that these should be mutually excitable, after having been several times contemporaneously excited; by this is meant, when *a* is made to enter into the arrangement *a m*, it tends to propel *b* into the order *b n*. Thus, when a perception is internally excited it is because the organic elements are first forced into the peculiar arrangement by a specific attraction: when externally excited it is because they are forced into the arrangement by the specific action of the medium on the peripheral expansion of the sense.

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71. It is of advantage to have a mechanical simile of this mode of action; but it is extremely difficult to suggest an appropriate one, as its nature is probably quite different from any thing that can at present be suggested. The meaning that is intended to be conveyed in the preceding discussion is illustrated by the following, which is certainly clumsy and artificial:—

Conceive each fibre to be represented by a bundle of tubes, differing in bore, and conveying specific fluids, and the motion of such fluids through the tubes to represent a specific excitement of a fibre. This motion is regulated by valves placed at the extremities, which are supposed to lie adjacent. We must further imagine, that the valves admit of being opened in a degree proportionate to the pressure applied at the further extremity of the tubes, which may represent the external impression. By this arrangement it will be observed, that the same motion of the fluids may be caused either by external pressure or internally by opening the valves. Now, while a specific movement of the fluids is going on in each set of tubes, and the valves of each are open in their respective degrees, the latter must be connected together, each one to all the others, either by mechanism or an attractive force. In this way, we may conceive how the opening of one set of valves, and consequent motion of the fluids in one specific manner may determine the opening of another set, and consequent motion in a different specific manner.

72. By such reasoning, then, are we led to the conclusion, that the fundamental law of cerebral action approximates closely to what has been suggested in §§ 67 and 70. The hypothetical properties attributed to the nervous fibres are analogous to what is already known of their functions. A law of mutual disturbance between the organized elements of different fibres certainly exists: we have endeavoured to suggest a simple one, which seems consistent with the laws of association, and holds out a prospect of materially aiding us in the analysis of the principles of cerebral activity. The contemporaneous excitement of *contiguous* nervous elements is supposed to facilitate the transmission of influence between them; they are at such times endowed with specific powers of mutual attraction, which every repetition of simultaneous

excitement strengthens. In speaking or reading, we are scarcely sensible of the effort; but in studying a foreign language, we become aware of the multitude of associations that have to be established before any progress is made—of what thousands of repeated irritations of the same fibres are necessary, before consciousness can travel from one to another without effort.

Such effects of repeated irritation, although subject to fade, must be looked upon as essentially a modification of the organic relation of the parts, so long as they last. The observations of Mr Knight on the habits of a breed of spaniels (§ 31), favours the probability of such effects becoming an essential part of the organization, capable of being transmitted to the offspring. These facts are of the highest psychical interest, but the time has not yet come for appreciating their full value.

73. These views of association seem capable of being applied to simplify the consideration of almost all the phenomena of the nervous system, and bring them under one principle of action. The abstract results of mental action,—the efforts of volition,—semi-voluntary, rhythmical, and automatic movements, all seem capable of being embraced by this law. The influence of the nerves on secretion, the sympathy of tissues, and pathological phenomena, show such analogies as might almost lead us to suspect that a similar principle of action pervaded organization to its core—to where chemical is translated into vital action.*

* The vital principle that sustains and endows the organic elements with extraordinary affinities and powers of combination, is it not in itself supported by nervous action?—that endowment of organic matter which permits it to transmit and concentrate distant influences upon one point, and thus communicate to an organic element molecular powers that could not otherwise act in combination or exist together? Thus the antagonism to chemical action, which organization displays, is perhaps due to the concentration of different molecular influences. This might perhaps be imitated by galvanic conductors, but in a very limited manner, if possible.

In the hands of Sir Humphry Davy, we have already seen what singular perversions of chemical affinity may be engendered; and the more recent experiments of M. Bequeril, Mr Cross, and others, seem to carry this a step further. Galvanism will most likely be the great agent of future discovery in this subject. It is to be feared, however, that the experiments (if any are possible) that would lead to successful results involve too complicated an arrangement to be imitated.

74. In the first place, as regards purely mental action, there is little doubt that an adapted excitability exists, for the purpose of organizing as it were the impressions of sense. Our perceptions come to us through an associated medium. A higher adaptation of the elements of neurine presents us with the abstract qualities of the perceptions, and so on to an extent that is well calculated to fill us with astonishment and admiration. These adaptations are to be viewed, not only as the means of directing the order in which the elements of perceptions shall present themselves to consciousness, but also of directing upon those elements the stream of vital force, and thence transmitting it to the elements of muscular activity. One person has greater facility in learning languages than another:—Does this not arise from the peculiar associations being established with greater facility in one brain than in another? and hence does not the faculty of language altogether depend on conducting powers of greater or less degree being *organically* established between certain elements of neurine, so as not only to facilitate the association of perceptions with symbols or vocal sounds, but to *incite* to such acts of association. The same may be said of the other active principles of the mind.

Such considerations lead us to hope, that mental analysis may be prosecuted with some degree of success, although ignorant of the recondite phenomena of organic molecular action.

75. Volition, in so far as it is the direct effect of mental action, is included in the preceding remark; but inasmuch as muscles are associated together in their action, a distinct principle would seem to operate in associating them. Is not the establishment of conducting properties between certain fibres sufficient to accomplish this, and first to draw from the blood—the source of vital energy—a specific impulse of any required intensity, and to distribute it in any required manner over any number of motor elements? It may be remarked, that often-repeated efforts of volition are capable of gradually modifying, and even changing, the organized arrangement of excitability. This is a proof that it is the same principle of association that operates—that the connexion

established by reason of adjacent fibres being simultaneously in a state of excitement, is similar in kind to that which exists as an original part of the organization.

76. Involuntary acts, such as coughing, sneezing, and vomiting, are concatenated movements for a specific purpose;—a conducting power has been given to certain motors in a certain order, and that influence, from the peripheral surface to which the arrangement has been adapted, is conveyed to those motors, and performs its function of directing more or less vital force upon them. The researches of Dr Hall demonstrate, that an organized connexion between the elements of centripetal and centrifugal nervous action takes place in the spinal marrow, and that extensive associations are there established, which, if properly analyzed, may lead to important inferences, applicable to the higher functions of the brain. As an example, the concatenation of action that causes locomotion appears to be completed there; but an influence, proceeding from the brain in a normal condition, controls and regulates it with the most perfect precision; and, as we might fancy, similar to the agency of the engineer, who directs the motion of a steam-vessel backwards or forwards, with greater or less speed, by acting upon the valves of the engine.

Now, the type of such action involves a duplex associated connexion:—one (A), between the spinal centripetal and centrifugal nervous elements; the other (B), between (A) and certain fibres that must ascend and descend from the brain.

There are many reasons for believing that cerebral activity conforms to this type, and probably involves a triplex, or even higher, system of associated action.

77. Perceptions, after being elaborated or shaped by one organism, act and are acted upon by others. An excited propensity prompts to certain actions which afford sensations that tend to its gratification. These may either be simple or elaborated by other organisms. This, expressed analytically, means, that after certain ideas have been acted upon by one organ, they become capable of acting upon others. They must thus acquire new properties, as in the following example:—An individual, after making certain arithmetical calculations, finds that he is richer or poorer than he anticipated. Here the

calculating faculty is employed about certain impressions, whose relations it develops, and establishes as an elaborated idea, which has the property of exciting the hoarding propensity, or Acquisitiveness of phrenologists.

78. The hypothesis leads to the question,—Do all the primary filaments of nerves come into contact with each other? and if so, what is the simplest arrangement by which this may be effected? The diagrams in Fig. 2 (p. 16) are given as an attempt to answer this.—They are interesting, from the analogies they bear to the minute anatomy of some parts of the nervous system, especially the ganglions.

The inferences which it may be useful to keep in view are as follow:—

(1) There will be a protuberance at the part where each single filament of a bundle crosses all the others.

(2) The length and shape of the protuberance, caused by the mutual crossing of all the fibres of a given nerve, may be various, according to the manner of folding; but the quantity of nervous matter required cannot be less than what would constitute a nerve of the same breadth, and of a length equal to the sum of the breadths of all its primary filaments. Thus the length of a nerve $\frac{1}{40}$ inch in diameter, taken up by the mutual crossing of all its primary filaments (supposing these $\frac{1}{2000}$ inch in diameter), is $1\frac{1}{4}$ inch. The same of a $\frac{1}{20}$ inch nerve is 5 inches,—of $\frac{1}{10}$ inch nerve, 20 inches,—increasing as the square of the diameter.

(3) The ganglions of the sympathetic are of such size and form as might be the consequence of all the filaments of the nerves that enter them mutually intersecting each other symmetrically, and having additional nervous matter interposed.

(4) The ganglion at the root of a spinal nerve is nearly similar to the protuberance that would be caused by the primary fibres of a branch of the sympathetic, $\frac{1}{40}$ or $\frac{1}{60}$ inch in diameter, crossing all the primary fibres of the spinal nerve.

(5) It is not possible that all the primary filaments of the spinal nerves can cross each other, either in the nerves themselves, or in any prolongations of them that may be supposed to exist in the spinal cord and brain.

(6) Such a mutual crossing appears to characterize the grey fibres of the ganglionic nerves only.

(7) Most of the phenomena of association are probably due to the cineritious matter of the spinal cord and brain; and it may be conjectured at the same time to eliminate a power from the blood, impelling to specific modes of action (54).

79. *Explanation of the Diagrams in Fig.* 2.—I, *a*, represents a nerve $\frac{1}{90}$ inch in diameter spread out into a lamina of the thickness of a single filament ($\frac{1}{2000}$ inch). The fibres, by turning first to the left and then to the right, all cross each other, and resume their original direction. When folded up into a thread, the bulging caused by the crossing is so small in comparison to its length as to be imperceptible: one hundred such nerves, in one bundle, are represented by *b*. If all the filaments in *b* crossed each other in this manner, the greatest thickness would be the same as at *c*, but the length would be 100 times *d e*, or 4 feet 2 inches. If the nerve were $\frac{1}{2}$ inch thick, *d e* would be 81 feet.

II, *a*, represents the same nerve as last figure, the filaments spread out and crossing each other twice: *d e* becomes shortened one-half, and the thickness at *c* = $1\frac{4}{10}$, the ordinary thickness of the nerve.

III represents the filaments of two bundles crossing each other: the bulging is exactly the same as in I.

IV. Suppose three nerves pass through a ganglion, it is proposed to determine how all their constituent filaments may be made to cross each other, so that when folded up they may occupy the least possible bulk. Take three slips of paper, *a b c*, representing the fibres of the nerves spread out into laminæ; draw a longitudinal line on both sides of each slip, to show the course of the fibres; then fold them at right angles, as shown by *a b c*. Insert *b* into *a*, as shown by *d*, and put *c* over *d*, as shown by *e*. If the folded part of the slips are rolled together, and the outer parts, 1 2 3, rolled separate, a ganglion will be represented, in which all the filaments of the nerves that pass through it mutually cross each other with the least expenditure of nervous matter—viz. double that required to pass through the length of the ganglion without crossing.

VI represents a ganglion formed in this manner, by the interlacement of three nerves, each $\frac{1}{40}$ inch in diameter, and containing 2500 primary filaments. 1 1, are the extremities of the first nerve, entering and leaving the ganglion; 2 2, of the second, etc. If the nervous laminæ had been folded over each other, as shown by V, *a* and *b*, previous to being rolled up, the nervous matter of the ganglion would be increased one-half; but the arrangement is the more simple of the two.

V, *a b c*, represents three laminæ laid crossing each other, and then folded up in two directions, at right angles to each other, forming a flat irregular shaped ganglion, as at *c*.

VII represents a ganglion consisting of four nerves, similar to those in VI. The diameters of such ganglions, when cylindrical, increase in the simple ratio of the number of their constituent nerves. Thus the diameter of a ganglion of six equal nerves is three times that of one consisting of the mutual intersection of two only. A smaller nerve, passing through a ganglion and crossing all its fibres, will cause a partial but considerable thickening at one part.

VIII. In this figure a branch, $\frac{1}{40}$ inch in diameter, from the ganglion (VII) is supposed to join a nerve (A B) $\frac{1}{8}$ of an inch in diameter, and the constituent filaments of the one to cross all the constituent filaments of the other. If A B were spread out into a lamina $\frac{1}{2000}$ of an inch thick, it would extend to the enormous breadth of 6 feet 8 inches. While in this condition, the branch 1 is supposed to be spread into two elementary laminæ, each $\frac{5}{8}$ inches broad, and one applied above A B and the other below. The three being then rolled up together, will present an appearance similar to the ganglions at the roots of the spinal nerves. The thickness of the protuberance will be $1\frac{3}{4}$ times that of the nerve, if all the primary filaments are of equal size. If additional grey matter is interposed, it will of course increase the proportionate thickness of the ganglion.

### NOTE.—*On Secretion.*

1. In organization, the cause is often too subtle to be traced, so that effects appear like magic, being determined with a precision that may be called geometrically perfect,

without any directing agent being apparent. This is peculiarly the case with regard to the secretions. The first gland to analyze is the nerve, which assimilates its own matter, and then secretes from the blood the peculiar energy and excitability that it transmits, and which evidently determines all other secretions. It may be said that each tissue has the power of nourishing itself from the blood; but as the additional power which the tissue of a gland possesses is notoriously subject to the influence of the nerves, so it may be considered as wholly dependent thereupon. We know as a fact, that different nerves have excitabilities specifically different (as the nerves of sense), and these are unchangeable in their quality, and have each its peculiar effect on the sensorium.

2. Shaping our ideas by analogy, the nerves that govern secretion may have specifically adapted powers in relation to the blood and to the peculiar secretion; so that that active phenomenon which takes place amongst the molecules of the blood during the separation of the secreted fluid, exists organized in the nerve, so that any kind of excitement applied to it stimulates the secretion, in the same manner as any stimulus applied to the optic nerve excites luminous impressions; in the auditory nerve, sonorous impressions.

The force that draws the secretion from the blood is one of selection, in which complicated affinities are shown. The nature of this, if it does not embrace the whole range of action that divides the chemical elements from the organized fluids, must include all that divides the blood from the secretion.

3. The seminal secretion must be homogeneous, each particle endowed with similar properties. Hence influences proceeding from every part of the system must be concentrated upon it,—an impress from each process of assimilation and of secretion. This can only be conceived to take place through the system. Is part of the sympathetic or ganglionic system devoted to this? The secretion acts powerfully on the nervous system of consciousness, and is in its turn liable to be influenced through it. Salacious ideas accelerate the secreting process, and an accumulation of the secretion excites a salacious condition of the system. Here is action and reaction in the same line as it were; from the blood and all parts of the system through

the ganglionic nerves to the testes, from which an exciting or irritative influence is reflected upon the nervous system, by which an impulse is given to the sensi-motor train that consummates the function. This train is infinitely more complicated than that which regulates the discharge or efference of any other secretion; yet it may be of advantage to consider the type of all as identical, inasmuch as the end to be accomplished by all is similar,—namely, to give out or eject the organized fluid at the proper time for fulfilling its intended functions. The efferent ducts of the common glands are placed so as to convey their secretions at once to the parts where their functions are fulfilled. This is not the case with the testes; and the astonishing mechanism of the sexual passion, to which all the energies of the system contribute, has no other purpose to serve. The principle, however, upon which the sexual distinction of individuals depends, is a primary necessity of organic action, by which the germinal elements require to be elaborated in separate individuals; and the importance of this separation is manifested by the various contrivances and arrangements of nature to secure it, both in the animal and vegetable kingdoms. The distinction of sex, the sexual passion, and all the phenomena of consciousness and volition that accompany it, are thus subordinate to this necessity, and as much so as any of the unconscious organic phenomena that contribute to generation.

4. The germinal elements may be viewed as the representatives of the respective organizations of the sexes. What is the part taken by each in establishing the circle of independent vitality? Does the possibility of retaining the stamp of an assimilative or organic process—of a train of molecular action in nervous tissue—afford any light of explanation? In the growth of the embryo, of the fœtus, and of the individual, there is a gradual evolution of structure to a certain determinate limit. All the parts of the perfect individual do not appear in the embryo. In what form do the undeveloped parts exist in the organism? There is a radiation of assimilative influences from a centre, that follow each other in a certain order. How are the limits of development determined? The process goes on until some specific powers are neutralised. The great circle

of organic action is divided and subdivided into many minor circles, in which the assimilative influences operate in different directions. As the union of powers derived from the two sexes were necessary to quicken the embryo, these powers may probably be still distinguished in the fœtus as necessary to each effort of assimilation. In whatever way we may choose to view the subject, the impress of a vast variety of organic processes must exist originally in very small compass.

5. The argument that is put forward here has but a faint outline at present, but it may chance to be an attempt made in the right direction.

The impress which the offspring frequently exhibits of certain perceptions of sense which the parents have been impressed with during coition, and the various arguments by which the power of the nerves over secretion has been demonstrated, are considered sufficient evidence of a power resident in them, which as it is evidently capable of modifying assimilation and secretion, probably wholly determines these phenomena. Each process may therefore be capable of reflecting on the testes a powerful impress, which afterwards, through a different medium, may reproduce the same assimilative effects. The mental phenomena being undoubtedly represented in the nervous organism, shows that the latter is capable of preserving the impress of a multitude of incident actions from without, superimposed as it were upon each other. The non-interference of these, and the gradual evolution of them in accordance with the law of association, may admit of being applied, although in a shadowy manner, to elucidate the process of conception.

6. The prodigious concentration of influences on the germinal element can only be compared to the state of a small portion of the ethereal fluid while transmitting luminous vibrations, according to the undulatory theory of light. If we suppose the active condition of the fluid articles in this portion to be arrested—to be solidified—they may be supposed to bear the impress of the indefinite number of pulses that were passing through it. From the importance of vision, and the optic impress that accompanies almost all our ideas, it is evident that light acts an important part in that molecular action of

the brain that is the subject of consciousness. The rapidity of nervous action, and of the succession of thoughts in a reverie or dream, is like that of light or electricity, which are perhaps modifications of the powers of the same ether. In many diseases, the real visual impressions of absent objects are suggested by nervous action; in some, the patient is haunted with spectral illusions. In fevers, in dreams, all optic motor *ideas* even testify clearly that luminous undulations are solidified (so to speak) in the brain: the cerebral organism has the power of *arresting* them and *retaining* them, and, lastly, of *transmitting* them to the germinal element. We are of course profoundly ignorant of how this is done, and probably will ever remain so; but we know that it *is* effected, and such a characteristic of cerebral activity is well worthy of being kept in mind when attempting the analysis of the perceptive faculties. It leads us to expect, that if molecular philosophy is ever destined to advance into the regions of organization, the phenomena of perceptive consciousness will admit of being applied to illustrate the physical aspect of the elementary powers of matter.

7. The faculty of association has been elaborately treated of in the preceding chapter, and reduced by analysis to the single principle of simultaneousness of excitement: every exertion was made to penetrate this apparently simple law of dependent excitability, but without success. It involves a power of registering successive action, which, as there is nothing in physics the least analogous, must be viewed as a primary organic principle—a characteristic of organization. The importance of this, in reference to the nervous system generally, has been already referred to.

If the organic processes of assimilation and secretion are represented by impressions on the nervous system similar to ideas, and capable of generating the same processes, an indefinite number may thus be associated together in a train, upon an organic nucleus or matrix, and evolved in a train from this as a centre, which may thus be supposed to germinate in a predetermined manner.

There must be an essential difference in the germs of the two sexes, which makes it impossible for both to be eliminated

from the same system. Their mutually exciting and neutralizing powers have a great likeness to the two electricities; and this analogy has been carried out at great length by a modern speculative writer, but with less advantage than might have been expected, showing that such attempts are still premature.

8. Thus it must be admitted that the laws of cerebral excitability and of association, inasmuch as they show the susceptible powers of the nervous tissue, and therefore of organized matter, deserve to be applied as far as possible to the physiology of generation. They do throw some light upon it, although it must be confessed it is of the faintest. But it is of a positive kind, which, in this the most obscure of all that is obscure in nature, it is pleasing to contemplate.

## CHAPTER IV

### ON SENSATION AND VOLITION

*Including the Sensi-Motor Phenomena that Constitute the Optic Tangi-Motor System*

1. In every instance of volition, there is an effort of which we are conscious, that must involve an internal action amongst the elements of the nervous system corresponding to the external, and they must bear the most exact relation to each other. Two impressions must be engaged in the internal action, viz.—(1) Of the part of the body to be moved; and, (2) That of the place to which it is to be moved. The action in the brain, or nervous system, to which the motion of the body conforms, is *moving the first of these impressions to coincide with the latter.* The cerebral impression of the member has to be moved and made to coincide with the cerebral impression of the place. This is performed by the mental effort, and the simultaneous motion ensues. This is not put forward as representing the actual mode of cerebral action, but the consciousness we have of it, expressed in an analytic form.

2. It is not always that such an effort of the attention is required in regulating our movements as to leave any

recollection behind. Thus we can walk and avoid obstacles with thoughts engaged upon other matters;—we can read aloud with proper emphasis, sometimes a whole page, without being conscious of uttering a syllable, if the passage is familiar and totally void of interest. Thus, the more strongly external impressions are associated with specific motions, the less effort becomes necessary, although they may be very complex. The same relation of position undoubtedly exists between them at last as at first, when a powerful effort of the attention was required; but the relation upon which mutual excitability depends must have undergone a change.

3. It seems impossible to analyze this relation into a more simple form. A most complicated affection of the muscles takes place at the most simple effort; yet we are conscious only of the impressions of the part moved, and the direction or position moved to, occupying our attention. The mind may contemplate one of these, or both, and fancy the motion to take place: the image of the action may pass before the mind without the action itself taking place—without the motor energy being directed to the member which is the object of attention.

4. What is the difference that can be traced by consciousness between the idea of the action and the action itself? In the first case, it is the idea (or image left by the impression of the member) that is moved; in the latter, it is the actual impression that seems to be moved (for when the nerves are not paralysed or benumbed, we have at all times an impression of the existence of the parts supplied by them). Here then appears a manifest distinction between the *impression* and *idea* of the members. The latter may be moved to coincide with the idea of the place, without any motion ensuing; but the instant that the impression is thus operated upon, the muscles are affected.

5. If we view both ideas and impressions as actions taking place in the brain, it appears that the latter are more closely connected with the motor system than the former. The actual difference that subsists between ideas and impressions is an interesting subject of speculation. Whatever it may be essentially, this difference in their relation to the motor

elements is evidently a point of great importance, and one which there will be frequent occasion to illustrate.

6. In the simplest effort of volition, there can be no doubt that a vast number of the primary motor filaments must be excited at their cerebral or spinal extremities in different degrees, to correspond with the different degrees of tension and relaxation of the antagonist muscles. Volition may thus be defined, as the act of communicating different degrees of excitement to the cortical (?) terminations of the motor fibres. The nature of this excitement, its mode of communication and transmission, is unknown. But one quality it possesses in common with physical power,—it varies in degree of *intensity* or *quantity*.

7. The conscious operation that accompanies muscular contraction is, as above stated, the effort to bring the impression of the member to coincide with the impression of the place. The change that takes place in the muscular fibre, and therefore in the motor filament, is not a sudden transition from one state to another, but a gradual alteration, the degree of which probably differs in each fibre, or at least in those that supply different muscles, but is regulated in all by the cerebral action, which, although simple to consciousness, must be as complex in reality as the variety of muscular changes that are caused by it.

8. It is not necessary that the parts should be seen. With eyes shut, the tip of a finger of the right hand may be directed to the tip of any other finger of the left hand with the greatest precision, and at once, without any intermediate groping. Any other two sensitive points may be brought together in this way, if the motions of the body permit; but there is a remarkable difference in the degree of exactness with which this can be done. The points of the fingers can be brought more exactly to the tip of the tongue than the knuckles or any other point of the hand; and the fingers more exactly on each other than on the knuckles, or than the latter on each other. The result of such subjective experiments is, that the accuracy with which voluntary motions can be directed, bears a relation to the distinctness of the perception of the part moved, and part moved to, according to the sensibility of the two points.

This power of direction does not increase when the points are irritated, *but it is affected by a change of position.* Thus, with shut eyes, the fingers cannot be brought together behind with the same precision as in front, neither can they at one side, with the arms twisted in an unusual way.

9. Now, in what respect does the tactile *unexcited* impression of one peripheral point differ from that of another? It is plain that there is no difference, except the difference of position to which they are referred. How are we cognizant of relative position? It manifestly depends on the motion of the part: the change is in the state of the motor system. But this affords no explanation of our sense of relative position. It is intuitive, for those born with imperfect limbs have the same internal sensations as if the limbs were perfect. This is also demonstrated by various phenomena attending surgical operations. If the flap of the ear is pressed forward with a quill, and the finger of the other hand directed to it, it will be found that volition aims not at the position of the part at the time, but to the position that it naturally occupies.

Young birds will avoid obstacles in their first attempts at flying. A chicken has been seen to catch at a fly while the shell stuck to its tail.

10. Thus everything favours the conclusion, that knowledge of position arises from an innate adaptation. This it is of the greatest importance to analyze, although at first view it seems almost inexplicable. The impressions from all the senses, except taste and smell, inform us of the position as well as of the other qualities of external objects. If we examine further, we shall find that almost all our thoughts carry along with them the idea of relative position; and there is little doubt that the abstract notions of extension, motion, and many others, are derived from this principle. Indeed, without it, it is impossible that mere sensation could connect our minds with the external world (13).

Here, then, we have a subject for analysis, which strikes at the root of our mental constitution, and promises, if explained, to throw a flood of light upon the nature of all the mental functions.

11. The question, then, which we have to examine is,—

*How do we become acquainted with relative position?* It is sufficiently evident that we are instinctively impelled to refer all positions to that of our own person, and that any two are compared together and judged of by their relation to self. It is also clear, that a variety of other perceptions, involved in the primary one of position, such as extension and motion, are more distinctly appreciated the nearer they approach the standard of those qualities that is manifested in our person, and which assumes an involuntary part in all our thoughts and actions.

12. If we examine the subject closer, we find that the perfect consciousness we possess with shut eyes of the different peripheral parts of our body, is a perception of relative position: one part is felt in reference to *all the rest.* The blind must have this knowledge. These impressions of consciousness, which are transmitted from the skin, without the contact of external objects, and which carry alongst with them the sense of position, may be termed *tangi-motor* perceptions.

These are more or less distinct, according to the sensibility of the part (8). And it may be remarked, in passing, that it seems to be a general law, that those parts of the body which most frequently touch external objects are endowed with the greatest sensibility. [See an Account of Weber's Experiments in Müller's Physiology, by Dr Baly.]

13. The tangi-motor perceptions must evidently be associated by the contiguity of their position. Adjacent parts of the surface are liable to be excited simultaneously by objects that touch them, and will consequently be associated in the mind by this relation. (See *Association.*) Thus, on placing the hand on the table, contiguous sensitive elements are excited at the same instant of time; and by the indefinite number of such contacts that are continually taking place, it must happen that the nervous fibres distributed over the skin become endowed with the power of suggesting each other, in proportion to their proximity, and therefore in their *order of position.* But as this is the utmost that can be expected from the associating principle, so it is evident that it does not afford us a knowledge of relative position, nor can we derive it from sensations only, in whatever way they may be combined or connected together. We may acquire the perception of

contiguity of surface, but the surface might be folded up into the smallest compass, or expanded into a colossal form. It is in vain, therefore, to look to the course and termination of the nerves, as some physiologists have done, for a solution of the enigma.

14. Will the motor phenomena of the system afford an explanation? Sir C. Bell, in a casual remark (On the Nerves, p. 197), has attributed this function to the muscular sense, transmitted through the sensitive nerves that are distributed to the muscles; and physiologists generally appear to coincide in the opinion that they contribute to our perceptions of space, distance, and position. Yet when we come to consider this closely, there is great difficulty in conceiving how the impress of locality can accompany even the most exact knowledge of the condition of the muscle. The sensation of touch that is excited by drawing the finger over a surface, is the same as when the surface is drawn under the finger; but there is a manifest difference in the perceptions, from the consciousness of the motion of the hand being present in the one case and absent in the other. Now, the same change of place may follow various and different movements of the person; for the hand alone, or the arm alone, may be engaged, or the whole body may contribute to this motion of the finger (21). Are we not therefore justified in concluding, that *the apparatus that conveys the sense of muscular condition is not* sufficient to convey the sense of relative position.

15. It is not obvious that we can weigh a body in our hand by the muscular sense alone; for while the hand rests flat upon the table, we can estimate with great exactness the weight of a body placed upon it, by the pressure affecting the sense of touch only. If we lift it, and make the hand rise and fall with it, we have a clearer perception of its weight; but is this from the consciousness of muscular effort, or from the varying pressure that accompanies this kind of motion? When a weight is held out at arm's length for a time, it seems to grow heavier to the muscular sense, from the feeling of exhaustion; but the pressure on the sense of touch does not vary, and therefore may be expected to afford better means of judging.

The special function of the sense of touch, indeed, appears to depend on its capability of measuring pressure, although both motion and position are inseparably connected with all its impressions.

16. But the precise function attributed to these sensitive nerves of the muscles, is the communicating of a *sense of the condition of the muscle* (Bell on the Muscular Sense). The condition of a muscle varies in two particulars:—viz. the extent or degree of contraction, and the force of contraction. These are obviously independent of each other, and therefore deserve to be carefully distinguished. A muscle may pass through the whole range of its contractibility with little or no force of contraction, and, *vice versa*, the force may be intense, and the degree of contraction indefinitely small. Now, *do the sensitive nerves render a due account of both conditions—of the extent and of the force of the contraction?* The answer to this does not affect the present discussion. But it may be remarked, that so far as our abstract analysis may warrant a physiological inference, we would incline to limit the function of these nerves to conveying impressions of the *degree* of muscular contraction only. Every attitude, gesture, and personal manifestation, would thus have a representative influence in the brain. If, from wound or disease, a muscle has become longer or shorter, and especially if this has happened to several that are associated together in one motion, there will be *bizarre* feelings of position connected with the motion, *if* the preceding conjecture is true.

17. From what has been remarked in § 15, it seems highly probable that the intensity of contraction is sufficiently represented by impressions from the sense of touch alone. The phenomena attending paralysis do not afford any insight into the nature of the muscular sense; but they show distinctly that a tactile sensation is necessary to assist volition in regulating the intensity of the motor excitement. Deprived of this guide, there is nothing left but vague feelings of exhaustion. All voluntary motions have immediate reference to peripheral sensations, derived from the skin or eye, or both, and of sensations of position.

18. [Muscles are never in a totally relaxed condition; and

the yielding in one class is a state where contractile force does not cease to be excited, and is nicely adjusted to that in the other, so as to give *the* proper velocity and intensity of effect to the motion.

It is necessary to keep this in view, when making deductions from the facts referred to in § 10, and accounting for the extraordinary circumstance, that unless the surface of the body is in a position determined by the motions which are natural to it, the harmony of action, that enables us to direct one sensitive point upon another, will be disturbed.]

19. The exactness with which volition regulates the movements, shows a perfect adaptation between the nervous motor fibres and the cerebral power that communicates to them their specific degree of excitement. The mind judges of the degree of that influence, and deals it out with an exact regard to the sensation of the member moved; and is cognizant of the amount by the change in *position* of that sensation, not by any change in its degree or quality. The mind must be directed to the change in position previous to its taking place. The conception of relative position is thus a power of the mind to which all sensations seem to be referred, judged of, and measured. In what does it consist? There must be a picture within corresponding to external objects. There must be a motion within corresponding to that without. Magnitudes and distances being compared, involve the idea of change of place. The mind can leap from the consideration of one position to that of another. The point or locality of consciousness must change, and be cognizant of the amount of change; the *degree* of change must be represented in the brain, and a standard measure of it must exist there as well as amongst external objects.

20. When the finger approaches the knee (§ 8), with the eyes shut, the locality of the perception of each of the parts nearly coincides. The sensitive nerves, expanded on the finger and knee, nearly coincide; but this cannot be the case with the cerebral extremities of the same fibres: only the *consciousness* of them nearly coincides.

*There must be an influence, joined to the sensation of the part, that determines the consciousness of position,—something*

*derived from a source different from the tactile impression, but having an intimate connexion with it.*

21. Let F *m* represent the tangi-motor impression (§ 12) of the finger, and K *n* that of the knee, F and K signifying the simple sensations of the parts, *m* and *n* the perception of relative position, which is influenced and *altered* by the motions of the body, but inseparably attached to F and K respectively. When the finger touches the knee, F *m* and K *n* coincide. F and K are similar in kind, but, although coinciding, they maintain their separate identity, by means of their contiguous association, with the adjacent part of the knee and hand respectively, but *m* and *n* are *now identical.* They are dependent on the motor system. *m* is associated with a certain state of the motors of the body; *n* is associated with another state of the same. Hence two different affections of the motor system communicate to the mind the same result.

It is not sufficient explanation to attribute the consciousness of the finger touching the knee to the impression of contact; because we are aware of the proximity of the two before contact has taken place, and with the eyes shut. It is thus clear, that *m* and *n*, when they have thus become identical, although in appearance depending on, or caused by, the motor states of the system, are certainly not identical with them. We have therefore to explain, how *two distinct motor phenomena may produce one and the same impress of position.*

22. In the preceding chapter we have endeavoured to prove that all the phenomena of association may be explained on the hypothesis, that simultaneous excitement establishes mutual excitability. Similarity—the most obvious principle of association—being resolved into re-excitement of the *same* impression in whole or in part, does not develop any principle of action that is not included in the above. Our attention is also drawn to the fact, that *the elements of neurine are susceptible of re-entering* into an indefinite number of previously excited states, which have no disposition to disturb each other, if they have not been simultaneous.

23. To the most superficial observer there must appear an association that governs our movements as well as our thoughts. Is it necessary for this, that the motor elements of neurine also

should have a disposition to re-enter into previously excited states? or will that of the sensitive elements, which transmit the impress of the motion, be sufficient?

In § 1 it was remarked, that voluntary action consists in the moving of one impression to coincide with another. If these are associated, will there be a tendency to the action? To each action, besides the active impulse, there is required two perceptions, and the motor impulse depends upon them for its degree or quality. It therefore appears that the re-excitable susceptibility need not be attributed to the motor elements.

24. But our attention is here drawn to a law of great importance, which has been overlooked when considering association only in reference to sensitive impressions. Mutual excitability must follow the order of sequence: there is no disposition to proceed in a train backwards; the impulse is always forwards in the order of time. This is so simple and natural, that it scarcely seems to involve a fundamental principle. But it is otherwise when we come to consider, that simultaneousness, as an isolated quality, does not determine the order of re-excitability; it does not determine whether the antecedent or the consequent impression is to be affected. The elementary principle of association may thus perhaps be more accurately defined, as *the inherent tendency of nervous excitability to reproduce that which has previously followed it in succession.*

25. We have previously had occasion to remark the difference between one motion and another, in respect to the degree of effort required to be exerted. Many actions, voluntary and sufficiently important in themselves, are performed almost unconsciously when they have been thoroughly established in a train of association. By such means, greater precision as well as power is acquired, until the results are much beyond ordinary feats of dexterity. This is exemplified in dancing, musical performance, balancing, and all the handicraft trades. *Precision* and *rapidity* of movement characterize the finished *artiste* in these departments. The first attempts are awkward enough, as the natural concatenation of the movements proceeds but a short way in these artificial acquirements; but when they become familiar, or, in other words,

require less effort, the attention is liberated as it were, and is enabled to make a new advance, which practice also makes familiar. Thus the full proficiency that the organization of the individual will admit of is acquired. There is a channel, worn as it were by continued abrasion in the same direction amongst the sensi-motor elements, and through this the vital influence traverses with less and less opposition, until even an effort may be required to restrain its progress, instead of urging it and directing it as at first. The same kind of organic change, which in such cases has been established by repetition of voluntary effort, exists as a part of the organization, directing the instinctive motions, and connecting together the locomotive functions (Chap. III., §§ 75, 76).

26. The feeling of effort varies with the more or less exhausted state of the nervous system. It also varies in proportion to the mental stimulus. Thus, under the influence of a panic, fatigue vanishes. Analogous to this is the power which intense muscular exertion has of averting pain, either of body or mind. Indeed, the proper function of pain seems to be to incite to action; and this remark may be applied, in a more extensive sense, to all the transactions of life. We are urged to act, oftener perhaps to escape painful feelings than to enjoy pleasurable. The tendency or direction of the movements in regard to the object is different: we retreat from it if disagreeable, and advance towards it if agreeable. Thus pain and repulsion go together; pleasure and attraction. These tendencies must be represented in the brain. Pleasure is the consequence of an organism being excited in accordance with its innate constitution or mode of action (Chap VIII., R.): pain, perhaps from an excitement that runs counter to the organization; its effects depressing,—driving back the current of vital influence from the channels where it naturally expands itself. In extreme cases, it induces morbid changes, destructive of the mind and body.

On finding a treasure, the hoarding propensity is affected in one way; on losing it, it is affected in a contrary manner. The corresponding cerebral states are probably opposed to each other in like contrast, which presents some analogy with the attractive and repulsive tendencies of matter.

27. Knowledge of position—in what does it consist? what does it imply? The *direction* and *distance* of the object from the eye. Motion changes this impression: if there were no perception of position, one part could not be distinguished from another. That such should be so, it is necessary that we suppose an arrangement of position should exist in the cerebral termination of the tactile fibres, as in their peripheral expansion. It exists independent of the consciousness of any preceding change of posture.

28. It has been remarked, when treating of sympathy, that the visual impression of gesture, expression, and relative position, is innately associated with inward perceptions of tangi-motor consciousness. May not a like sympathy, or mutual excitability, exist through the eye, for all parts as well as the face? It is a sympathy that is established in the brain, and therefore may exist in blind people. What is the essential feature of this sympathy? The sentient points of the retina are affected in the same way as the sentient points of the skin, in so far as the impressions on each are associated by contiguity. The perception of peculiar expression is obtained by observing all the parts of the countenance in connexion as one impression; —this, both by the eye and by that peculiar consciousness which every one has of the expression of his own face, and which must be derived from the tangi-motor system.

29. Is there not some connexion between the tangi-motor system of the body generally and the sense of vision, so that with every motion the inward eye (so to speak) is directed to it? Perhaps this is a consequence of the sense of touch being also a sense of position. The ideal direction given to the eye is consequent to the feeling of the specific position, not prior to it. The great difficulty is to know—to conceive the difference in the tangi-motor sense that gives to it an impress of position.

30. If the power of judging or measuring motion is determined by sensation, the latter must be intimately associated with the former. The picture on the retina, the visual impression of any gesture or posture of the body, induces a mental condition similar to what is felt when the body assumes the particular attitude. Let A be the picture on the retina of any attitude, and B the tangi-motor perception of the same, during

the act of personating it or thinking of it; let *b* signify the tactile nerves of any part of the surface of the body to which the attention is directed: it must necessarily be associated with *a*, the part of the retina impressed with rays from the same part of the body. But this association must be due to the relative connexion which *a* has with the other parts of the retina, excited by the image A. There is a sentient and motor impression combined in vision A, and in common sensation B; but there is this distinction, that the different parts of the retina maintain their relative position during the motion of the eyeball, whereas the surface of the body does not maintain its relative position. The perception of the particular gesture is caused by particular parts of the retina being excited; and any change in the position is accompanied by a change in the parts excited, whereas common sensation is not altered by change of posture.

Is the power of putting the finger on the knee blindfold (§ 8) derived from an optic impression being associated with every tangi-motor one? The power involves a knowledge of the distance and direction of the sentient part when the eyes are shut.

31. The mind perceives the relative situation of the points of the image and internal consciousness of the sensitive points of the body to be the same; their similarity is the means of associating them. But the point of association is relative arrangement, or an association of simultaneous impressions, that have a specific arrangement. It thus appears that there must be an aptness in the mind to conceive relative arrangement of the sensitive elements, in every possible way in which the muscular system can respond to the visual image.

Now, may not the internal sensitive elements be connected with a visual impression (or idea) in the above manner by organic adaptation. No motion can take place without the impression of the part being excited. When one part of the body is moved to touch another part, the consciousness of the sensitive fibres distributed to those parts must be more prominent than that of any other. The aptness or power to put one sensitive point upon another, is the fundamental characteristic property of the combined optic and tangi-motor systems, and the type of their organic connexion.

32. The position of an object is determined by its direction and distance from the eye. Is the eye adapted to give perception of these two qualities of position? The sensitive points of the retina will be associated together by contiguity, as the nerves of common sensation (13), and they have besides the advantage, that their *relative* position is not subject to change. The distance of two excited points of the retina will always bear the same proportion to the angular distance of the two objects which these points represent. Thus we may know that two points are not contiguous, but separated by so many intermediate points; but how are we informed of direction and distance? how do we acquire the perception of space? Is the motor apparatus of the eye sufficient for this purpose? and if so, in what way can it convey such perceptions?

It is necessary to keep in view the constitution of the retina. The delicacy of sight that enables us to distinguish written characters, or indeed to have a *distinct* perception of anything in the field of vision, is situated only in that singular spot—the optic axis. It is here where the principal part of the optic nerve must be expanded, if delicacy of perception depends on the number of nervous fibres. In viewing an object the eye moves, so that the optic axis traverses every part of the image. The excentral part of the retina serves to intimate the presence of an object; and the central point is instinctively turned upon it, and traverses it, like examining a large object at night with a lantern.

33. When the eyeball is pressed by the finger, the object appears to move in the direction opposite to that which is given to the optic axis. The axis being displaced without the consent of the recti-muscles, the impulse given to the sensitive and motor parts of the impression is the same as if the object were displaced in the opposite direction. If, instead of the finger, it had been the recti-muscles that had moved the axis in the same degree, the object would have appeared perfectly at rest. This well-known fact *demonstrates*, that the direction of an object is known by the exact consciousness we possess of the motion voluntarily given to the optic axis.

34. If the eyes are directed to a lighted lamp for some time, and then closed, the image of the flame will continue present,

and *does not now* appear to move when the eyeball is pressed; but its apparent position changes when the eyeball moves *voluntarily*. It deserves also to be remarked, that the turning of the head, and the motion which is given to the optic axis by any change of posture, generally has the same relation to visual impressions as the voluntary rotation of the eyeball. They do not disturb the correctness of our impressions of relative position or motion.*

35. We might conceive the distance of an object to be known by the adjustment which is required to be made in the focus of the eye for distinct vision, and which must necessarily be due to motor excitement of some kind, although the *modus operandi* is obscure. But a fatal objection to this is the peculiar structure of the eyes of insects, in which no focal adjustment is necessary, yet their motions are as much in harmony with the position of external objects as those of any other creature.

36. We have still to answer the question, How are we

* Sir C. Bell, in his work on the Nerves, p. 162, etc., illustrates the necessity of the eyeball being moved by the voluntary muscles, to give us the true perception of the motion and position of external objects: but is not the explanation which he has given of certain curious nervous affections of the muscles of the eye (App. p. 374) inconsistent with these subjective experiments? The cases are Nos. 104, 105, 107, 108, given in the Appendix. In these cases the eyes were observed to move continually, yet to the person having that defect the objects viewed appeared at perfect rest. If the objects *are* at rest, and also *appear* at rest, the disease must be a convulsive affection of the *voluntary* motors of the eye. Such being the case, there is nothing anomalous in objects appearing at rest. It is an *involuntary* affection of *voluntary* muscles, in every respect similar to the choreal symptoms of diseases affecting the *voluntary* motors of the body. In St Vitus' dance, the patient is conscious of the change in the position of his limbs, although volition is partially suspended. In the explanatory note to Case 108, it is remarked:—"These patients, therefore, having the motion of the eye from a different cause than volition, although the impression of the object moved upon the field of the retina, had simply the sensation on the retina, without the idea of motion, *because there was no muscular effort.*" If there had been no muscular effort, surely the object would appear to move, as when the eye is pressed out of its natural position by the finger. The objects appeared *in* their natural position, because there *was* a muscular effort, although an involuntary one.

cognizant of the *distance* and *direction* of objects? To do so, it is necessary we should first attend to the circumstances upon which a greater or less degree of exactness in our knowledge of distance and direction depends. The adaptation of our movements to the distance and direction of the object seems to be the only circumstance that tests the accuracy of our knowledge. Here, then, we have motor influence, joined with sensation, regulating motor influence. Is this done in a manner similar to the association of motions, of which there are so many familiar examples?

37. The movements of the body referred to are the subject of a double consciousness,—viz. that of the tangi-motor system and of the optic-motor system. The first takes account of or gives the perception of the degree of motor influence exerted and the part moved; the second, of the distance and direction of the motion. These are in all cases associated together, the latter being the antecedent.

38. Is the association of the visual idea with the tangi-motor impression sufficient to account for this adaptation and harmony of muscular effort? A change in posture is accompanied with a change in the visual perception or idea—a change in the visual impress of the direction and distance of the several points of the surface of the body. If the head is turned to one side, the visual idea of the foot changes its relative direction,—it changes with the position of the eye. Can association account for this?

There are visual ideas of the position of objects that are situated so that the eye cannot be directed to them.

When the hand is made to move in the field of vision, we have consciousness of the motion both from the tangi-motor and optic systems. Will the associations of such impressions sufficiently account for the idea of distance and direction?

39. All visual impressions appear to be associated with the position of the optic axis; although the image of the object may not strike upon this point of the retina, yet the idea of the motion of the eye, required to make it do so, is involuntarily suggested, and indeed forms part of the visual impression. And it is not surprising that this should be the case; for each point of the retina being at a fixed distance

from the central point or optic axis, the same motion of the eyeball will be required in every case to bring the image at this point to coincide with the centre. Thus, each point in the retina may be said to have associated with it a specific motor influence peculiar to it alone, and proportional to its distance from the centre. And, further, it appears that two objects out of the centre have associated with them the motion necessary to make the axis traverse from one to the other. There also appears to be a means of registering such motions, as perceptions in the mind, to a much greater extent than can be performed by the eyeball alone, unassisted by a motion of the head. But this subject belongs to the analysis of the perceptive faculties.

40. When the hand is made to move in the field of vision, perpendicular to the optic axis, we observe the motion by the change of direction given to the eye in following it. When it is made to move in the line of the eye, the motion, or change of distance, is indicated by change in the *apparent size* of the image. Of this we must be capable of judging very accurately (although almost unconsciously) from the motor influence associated with each point of the retina. In approaching an object, its apparent magnitude increases very rapidly as we draw near. The proportion is the inverse of the distance; thus, if at the distance of 100 feet, we diminish our distance one foot, the apparent size (linear) of the object increases one-hundredth; but if at a distance of 10 feet, we approach one foot towards it, the increment of size is ten times the former amount, and of surface 100 times. All motions in the field of vision are apparent by change of direction and change of the size of the image.

41. Suppose the hand is moved in every possible way within the field of vision: there will be a train of tangi-motor perceptions and a train of optic-motor perceptions of the same object, which, being simultaneous, are therefore associated together. By this means, tangi-motor impressions are translated as it were into optic-motor, and they are registered in conjunction with each other in consecutive trains. Now, if we might suppose these trains registered in any way that could be presented to the eye for the whole period of life, we should

find that some recurred more frequently than others, and therefore would become more strongly associated together, so as to be repeated with greater precision. This may explain the phenomena remarked in § 8.

42. Every motion of the limbs or body has another responding to it in the eye or optic-motor system, and this must be the case with ideas also. Distance, as perceived by diminution of the apparent size of the object, when it is moved in the line of the eye, is associated with distance perceived by change of direction, when the motion is perpendicular to the optic axis.

Thus, by means of association, each point of the surface of the body has two sensi-motor perceptions associated with it, which, from the very manner in which they have come to be connected in the mind, represent the same absolute position; and this is necessarily contemplated in conjunction with the rest of the person.

When I direct the finger upon the knee with closed eyes, the idea of the finger at the knee already exists in the organization, first as an optic perception, which suggests the corresponding tangi-motor one. What volition effects here, is similar to placing the finger upon some external object with the eyes open; for as we have an optic-motor idea corresponding to every position of the finger, so we can perceive and distinguish if one is going to coincide with the other, in a manner the same or similar as if the eyes were open.

The prodigious extent and influence of association here are very prominently brought forward; as well as its importance in the mental organization, where it acts a part that can only be compared with gravitation in its universality and regulating effects. The actual phenomena are not, however, so complicated as they appear at first view, for the same parts occur frequently in different trains; the new impression, therefore, in most cases only refreshes the old.

43. Having laid down my pen on the table, at a little distance, what phenomena of the optic and tangi-motor systems are involved in the simple act of taking it up? I glance for an instant at it, and having withdrawn my eye, my hand, although it has not been in the field of vision, is instantly moved to its exact position. The previous glance is sufficient;

there is no groping necessary. In trying this, the less effort that is made the more satisfactory the result. Now, let the eyeball be pressed up by the finger so as to elevate the optic axis about 10°, and let the pen be again aimed at, it will be found that the hand stumbles wide of the mark. It is like trying to hit an object under water. The power of directing the hand is therefore derived, not from the actual position of the optic axis, but from the position which is proper to the state of the recti-muscles at the time.

Thus, if we might suppose a small part of the retina to be removed out of its proper situation A, to another B, the image, whose natural place was B, would appear in the position corresponding to A. This is quite similar to what takes place with regard to the skin in like circumstances.

44. This harmony between the optic-motor and tangi-motor systems appears necessary for balancing our bodies and walking steadily; for suppose the whole retina to be tilted out of its natural position, by turning it a little round the optic axis, all upright objects would then appear to incline over,—a tower would seem to be falling, and the level ground would seem sloping. But the most alarming circumstance would be that the individual would see himself inclining over to one side; he would instantly make an effort to recover the perpendicular, and in that effort would fall. Is not this very similar to what takes place in giddiness?—a disposition to balance the limbs in unison with a position of external objects that does not exist, but of which the optic system gives the impression.

45. It is well known, that if a person turns round quickly upon his own axis for a short time, and then stands still, the surrounding objects appear to revolve in an opposite direction; and this is the case even if the eyes are closed whilst revolving. What affection of the optic-motors would occasion like results? If the eyeballs were turned to the right by any influence which did not involve the voluntary muscles of the eye and body generally, objects would appear to move to the left. While revolving from left to right, the eyeball is turned in this direction, and if slowly, objects appear naturally at rest, for they are accompanied with a due perception of the motion

of the body. But if this motion is so rapid that this due perception is wanting, then the result is the same as if the eyeball were turned by involuntary influence, and the objects appear to move in the field of vision. But why does this appearance continue after rotation has ceased?

46. In vertigo, the harmony between the optic-motor and tangi-motor systems appears to be disturbed. Even when lying down in this state, the body feels as if revolving. The associated sensi-motor impressions that determine the perception of position, are affected with a disposition or tendency to a progressive change. In the vertiginous sensations arising from narcotics, the disturbance seems general or of a disconnecting type; but in those caused by rotation they assume specific directions, according to the position of the optic axis, and the direction of its motion in reference to the axis of rotation.

Some curious observations by Purkinje, referred to by Müller (p. 847), illustrate this:—If, while turning round, the head is held with face downwards, so that the optic axis is parallel to the axis of rotation, on standing still, and raising the head, objects appear to revolve in a vertical wheel. If the body, lying upon a disc sideways, is made to revolve with the disc, an apparent motion of objects in tangents is perceived. These appearances occur even when the eyes are closed. On standing still, after turning round in the first manner, there is an irresistible tendency to fall over to the right-hand side, if the rotation has been from left to right, and *vice versa*. The ordinary sensations and exertion of standing are no longer addressed to the line of true equilibrium, but to a false line, which seems to be in the act of revolving to the right. These singular affections do not seem to belong to the eye itself, but to the cerebral organism that combines together the motor and sensitive elements,—probably the optic lobes and cerebellum.

47. Here it is well to remark, (1) That the specific direction of the vertigo is determined by the position and motion of the body; (2) That it takes place with closed eyes. If the perception of position were absent, will the vertigo assume the specific direction? It is difficult to subject this to trial.

It requires that an individual should fall asleep upon a disc, and then be subjected to rotation, and immediately afterwards awakened, to discover his sensations of giddiness.

48. On landing after a voyage, sensations are felt for several days as if the ground had a motion like the deck of a ship. This shows the usual tendency of perceptions to recur; but it also reveals a peculiarity of motor sensation deserving of notice. If for a long period we have been familiar with any object—if daily and hourly our perception of it is liable to be refreshed by reiterated impression—then suddenly one part of it is taken away, the mind can supply the *idea*, but not the *impression* of the part that is wanting. It is different with respect to the above phenomena. The *actual* impressions are those derived from a motionless base, but the *apparent* impressions are those of a moveable base. Does not this show, that the *idea* of preceding motor sensations enters as a constituent part of consequent motor sensations? The phenomenon is quite similar in character to the vertigo of revolution; but the *bizarre* perceptions, in the one case, seem to originate in the tangi-motor system, and in the other, in the optic-motor.

49. Are not all these phenomena quite in harmony with the preceding explanation that has been attempted of the origin of our sense of position,—that it is founded on an established or innate association, existing between the optic and tangi-motor elements, and requires, as in the preceding case, that the *idea* of preceding motor sensations should enter as a constituent part of the motor *impressions* that follow?

### NOTE.—*On the Cerebellum, Spinal Cord, and Medulla Oblongata.*

1. The eye combines motor and sensitive influence; but its motions are directed in a different way from those of any other part of the body. The image of the point that it is desired to fix the eye upon is precipitated immediately upon the optic axis, or the axis is precipitated upon the image. The quickness and rapidity with which this is done, is owing to the sensibility of the retina increasing regularly towards the centre. The attention is directed towards the idea of the object, or to the

indistinct image of it on the outer part of the retina: this seemingly begets an attraction between it and the centre, which is obeyed by the recti-muscles.

This is the most simple effort of volition: the sensi-motor phenomena concerned in it are of a single type; the sensitive apparatus that directs the motion requires no assistance from any other part of the system. But the perception of *actual position* that accompanies the image is extremely complicated; the rotation of the eyeball forms but a small part of it. It is affected by a very extensive system of association with the tangi-motor system of impressions; it is also affected by the workings of the perceptive faculties, in which size of image is made a perfect standard of distance. The precipitation of the optic axis upon any point is accompanied with additional organized perception; and all our voluntary motions must be adjusted to such organized perceptions. Hence, when the movements of the body are regulated by the eye, each emanation to the motors is founded upon a sensitive impression, made up of not only the compound ocular perception, but also upon a very complex tangi-motor impression. The train of causation may thus be spoken of. A remote cerebral cause suggests the idea of a loco-motor act: the eye immediately responds, and subscribes a sensi-motor perception; the body also responds, and yields a tangi-motor perception. These, combining, may be viewed as the proximate cause, or at least as the means of directing or fashioning the emanation of volition.

Thus, amongst the sensi-motor elements that are engaged in the loco-motor phenomena, there must be a threefold union. Now, there are many reasons for believing that this takes place in the anterior lobes of the cerebrum,—in short, that it is effected by the perceptive faculties. The combination of these elements with the motors of the body may take place in the cerebellum.

2. The remarkable experiments of M. Flourens have demonstrated, that the cerebellum is an organism interposed between volition and the corresponding motions of the body; and it is generally believed to form the connexions necessary for executing combined movements. Do the preceding views

throw any light on its functions? or does the necessity of such an organism tend to confirm the preceding hypotheses, and extend our acquaintance with the economy of the nervous system? Does it organize that association between the tangi-motor and optic-motor systems which puts us at all times in harmony with the position of external objects, and which has ever been so great a mystery in abstract physiology?

The simplest effort of volition involves the active state of two impressions (§ 1) of which we are conscious; but it manifestly involves the active state of a vast number of associated motor elements of which we are *not* conscious. Is the cerebellum the mechanism that translates the simple cerebral or conscious phenomena of volition into the complex phenomena of muscular action, and that eliminates, from the state of the sensitive nerves of the muscles and of the skin, the optic perception of position which is associated with the peripheral consciousness?

3. This action and reaction requires that the optic perception should always accompany the motor state of the body and change with it without effort; and from the remark made in § 14, the change in the former must be the effect of the *combined* influence of the whole tangi-motor system.

There is nothing in this but what the principle of association can accomplish in its ordinary course of action; but there is a new and remarkable feature in the connexion, by means of which the cerebral action, that corresponds to the apparent motion of the tangi-motor impression in the mind, operates upon the motor system, so as to produce the actual motion. The transition that takes place in the idea of position, which constitutes the idea of the motion, is accompanied with the sensitive impress of the part: this is sufficient to beget the excitement of the proper motors—an excitement that at all times embraces the whole system of voluntary muscles. No part of this is the mind conscious of; no part of this has any representative effect in the cerebrum: it is therefore an act wholly exterior to it, taking place in a different organism. But if the *complex* effect is not represented in the cerebrum, the *simple* effect must be represented in the cerebellum, and the *complex* trains of association established there, between the

conductors of the *simple* effect and the conductors of the *complex*, which consist of three sets, viz. the *tactile-sensitive*, the *musculo-sensitive*, and the *musculo-motor*. By the tactile-sensitive, the degree of *pressure* is represented; by the musculo-sensitive, the quality of *position* is *represented*, but is only *perceived* when joined with the optic impress. It has the quality, however, of changing this consciousness of position through the representative of the *simple effect*, which, by being associated with the musculo-sensitive system, participates in its changes.

4. The musculo-sensitive system has probably no representative in the cerebrum: its effects are only known by the contemporaneous changes in the optic perceptions. It is otherwise with the tactile sensitive, or the system of common sensitive nerves, which must communicate largely both with the cerebrum and cerebellum. It is by the perception of pressure that we feel the state of equilibrium in all positions of the body. The least departure from this condition diminishes the pressure on the base of support, and disposes the body to involuntary motion, which we are sensible of from the musculo-sensitive system.

We are acutely sensible of the gravity of every part of the body, although in a state of health no exertion is required to sustain it. Thus, on shipboard, the slightest rise of the ship may be observed; the physical effect of which we are conscious being the same as if the weight of every part of the body were temporarily augmented, and in descending, as if it were similarly diminished. The exactness of our estimate of the force of gravity is more perceptible in the adjustment of motor force in preserving equilibrium than in any other respect. On this account, perhaps, it is to be reckoned as an impress that enters as a part of the cerebellar arrangement and assists in distributing motor power.

5. What are the specific functions of the common sensitive system in the cerebellum? They cannot be the subject of consciousness. Are they necessary to the adjustment of muscular motion? In making a leap, how is the intensity of the muscular contractions regulated? It is evident that pressure is the effect of a force acting on the surface pressed, and

that, in so far as that force is derived from the muscles, it will be associated with their condition of tension, which, as was previously remarked (§ 16), is to be carefully distinguished from that of flexure.

Pressure is not, however, the constant representative of muscular intensity, as position is of contraction. The former are independent variables; the latter are always dependent. Pressure, we must therefore conclude, is *not* the measure of the force of the movements, nor can it be employed to regulate their intensity. That there is a very exact measure of contractile force in the mind, by which it is dealt out, and which is registered as part of the idea of the motion, will be evident to those who have had occasion to pour quicksilver from one vessel to another. In all operations of the same kind, the hand is accustomed to fluids with a specific gravity thirteen times less; consequently, to prevent spilling it, the attention is as much required as if it was to execute some difficult feat of balancing.

6. Thus the force used is remembered: it is a *cerebral* impression; it is *not* in the *cerebellum*. This shows that the impress of volition takes place in the cerebrum; and that, besides the two sensi-motor perceptions (§ 1), there is a third, which is associated with the idea of the motion, and which is vivified alongst with it. The difference between the idea of an action and the action itself is a very material distinction, which has an important bearing in reference to the *modus operandi* of the associating organism. The exactness with which the motor emanation is distributed over the proper fibres deserves remark. The antecedent sensitive phenomena may exist with the idea of the motion, and thus every thing be present but the voluntary effort. This effort is merely embodying the idea that already exists,—opening the sluice and allowing the motor energy to enter the mould in the respective degrees that are already impressed in idea.

7. These degrees must also be represented in the cerebellum: the difficulty is to conjecture what represents them to consciousness; for it is with this—the *intensity* of contraction—as with the *extent* of contraction,—a most complicated distribution of motor power is executed, in all the minute details of

intensity required by the arrangement of the muscles without consciousness of them, without any cerebral impression of such details, but with a perfect consciousness of and indeed foreknowledge of their effects.

The change of position is represented in the cerebrum by the optic perception being acted upon, being made to change there by the musculo-sensitive system. It must be in the act of opening the sluice of motor power that there is an impress left of the intensity of the exertion.

8. The analysis of the consciousness of any act resolves it into three parts, viz. the change of position, the pressure, and the motor intensity: these all form part of the cerebral idea of the motion. Is the motor effect which produces intensity and extent of contraction similar in kind? The answer to this must be in the affirmative, as the muscular fibre cannot be supposed to have two kinds of contraction. The motor effect must be one of intensity, which may, according to the resistance, cause a greater or less degree of contraction. What is wanting in due extent of contraction may be made up in degree of pressure; and this perhaps will reconcile the preceding difficulty, that led us to reject the quality of pressure as the representative of intensity. Pressure may thus be supplementary to the musculo-sensitive impress of position, and as such assist in regulating the distribution of power in the cerebellum. But it must be represented also in the cerebrum, as it is the subject of exact consciousness.

9. These views of the cerebellum will be made more distinct by using a diagram.

A represents the cerebral organism, where S P, the common sensitive, and E, optic nerves, mingle; and where, by means of the cerebellar influence passing through U C, Q, the impress of peripheral position, is formed, and the business of volition transacted. The mandates of the will, leaving A, are propelled through Q to C, where the unconscious distribution of the motor power takes place,—where the language of external form is translated into that of muscular arrangement. It is probable that a branch of the common sensitive system, T, enters into C, and, being associated with the extremities of Q and U, assists in the molecular change that supplies power to the motor

exciters O. The duplicates, P, of the fibres in T must ascend from the sensitive system, S, to the chamber of the will, A, there to assist in regulating the emanations that pass through Q. O and U are to be viewed as two systems forming one circle, like the arteries and veins, each fibre in one having a representative in the other, to which it is joined or connected, both in the muscle and in the cerebellum.

10. This hypothetical view of the cerebellum exhibits it as an excitory apparatus, put in a great measure beyond the power of volition, although employed in or necessary to every voluntary act. It is a system of itself, in some degree extraneous to consciousness, being continually active, without in the least occupying the attention, and optic-motor ideas being present, and changing of themselves with each change of bodily position, although the eyes are closed.

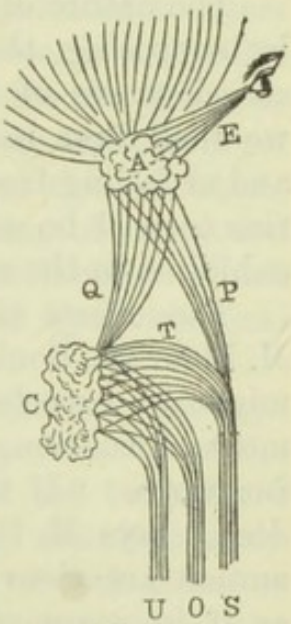

Fig. 7.

11. The analogy of structure cannot be satisfactorily traced. There is no part of the cerebrum that can be recognised as a special organism like A; neither can the hypothesis give any satisfactory account of the motor column, that extends from the spinal cord through the crus cerebri to the striated body. On the other hand, Q may represent part of the inter-cerebral commissure, and T the descending fibres discovered by Mr Solly, that join or are derived from the postero-lateral strand of sensation. A section of Q would cause total paralysis or convulsions; but this does not agree with the experiments of M. Flourens on the extirpation of the quadri-geminal bodies; consequently, these either do not form this connexion, or it includes the deep-seated part of the inter-cerebral commissure.

12. If part of C were destroyed, convulsions and paralysis of the part would probably ensue, alongst with *bizarre* perception of position and equilibrium. The latter would act on the organism of volition A, giving rise to efforts adapted to an unreal standard. Some notion of these may be had from

vertigo, which seems to be a general derangement or incoherence of the cerebellar functions. From the definition that has been given of these, it is evident, that the sensations which naturally attend every kind of voluntary or accidental motion of the body, as revolving, falling, ascending, etc., might be imitated by irritation of the proper localities; for although there is no consciousness in the cerebellum, there are peculiar optic-motor and perhaps sensi-motor perceptions, reflected through Q to the centre of volition A: there is a responsive action in the cerebrum.

The nature of these *bizarre* perceptions may be understood by supposing, that while standing firmly on the ground, the consciousness of its resistance and of our own weight is lost: we shall thus have all the feelings of being without support, and of falling freely in the air. The alarm that this must give rise to will be expressed by violent motions, and fear will be exhibited in the most vivid form.

The effects of lesions of the cerebellum, as described by M. Flourens, Bouillaud, and Magendie, are quite similar to what might be expected, and may be briefly described as derangement of volition, without the least derangement of the mental functions. "If the lesion of the cerebellum is extensive and deep," says M. Bouillaud, "the most curious disorders in locomotion are observed. The animal, trembling and staggering, as if in a complete state of drunkenness, exhausts itself in fruitless efforts to stand; it falls in all sorts of ways, moves to the left when wishing to move to the right, recedes when wishing to advance, rolls upon itself, whirls round upon its belly, and seems to be under the influence of an infinite number of contrary powers, each of which solicits it to action, without, however, securing the preponderance or establishing equilibrium; so that the creature is tormented by *its own wishes*, and is made to suffer apparently extreme torture." This torture must be the agony of alarm.

According to Bouillaud, the faculty of executing partial movements is retained after the cerebellum is entirely removed; and the automatic motions induced by irritation of the spinal cord, show a degree of combination quite distinct from convulsion. The reflex motions of the spinal cord also

show considerable harmony of combination, and perplex us with the appearance of interfering with the functions of the cerebellum.

13. The great problem is to separate the effects, so as to exhibit the motor influence of the cerebrum, cerebellum, and spinal cord, each in an isolated form. The spinal cord and medulla oblongata have properties of irritability similar to those belonging to the spinal nerves themselves. This irritability it seems impossible to view as otherwise than a function of the spinal cord. The species of excitability which the motor nerves convey out of the spinal cord is communicated to them in the spinal organism; and common sensation is a quality excited in the same; for the centripetal fibres in the crus cerebri, that conduct the impressions of pain to the cerebrum when irritated, do not convey any impression of this irritation, showing the necessity of a special organism of sensation external to the cerebrum, and that the excitability which the cerebral fibres convey cannot be supplied by mechanical stimulus.

14. The reflex phenomena of the spinal cord may perhaps admit of explanation, by supposing it to be permeable to the associating influence; for in every motion of the body there are centripetal and centrifugal emanations simultaneously traversing fibres that are either continuous to each other or but little separated; hence, being repeatedly excited in the same order, and altogether in the same manner, an excitable connexion may be established between them. The hands and feet are much more frequently stimulated during the motion of the body than any other part; hence, perhaps, touching the palm of the hand or sole of the foot excites reflex motions in paralytic patients more readily than touching any other part. "The slightest touch applied to the surface of the skin in animals in a state of narcotization is frequently sufficient to give rise to strong spasms: while the reflex actions, excited by irritations of the nerves themselves are much slighter." Müller (Phys. p. 721) takes a somewhat similar view of the reflex function, but without noticing association.

The connexion of the spinal cord with the brain prevents the reflex phenomena from appearing: they require the consent

of the brain when the parts are in a normal condition. The continued oft-repeated simultaneous excitement of the sensitive and motor nerves of the same part gives them associating power, which is only developed when the connexion with the brain is weakened or destroyed. Whatever may be the true account of these appearances, it is singular that the facts *admit* of such an explanation being applied,—an explanation which involves the necessity of nervous excitability having properties analogous to electricity, in passing through the best conductors; and when this is taken away, of passing through the next best. This disposition to admit of, or permeability of the spinal cord to, the associating influence, might perhaps be applied to explain many other of the spinal-motor phenomena (Chap. III., §§ 73, 76).

15. The observation of Dr Reid, that some reflex motions have been added for an ulterior object, deserves the utmost attention; for if the spinal reflex phenomena are not the accidental effects of association, they make their appearance only when the influence of the brain is taken away; while the other phenomena that have been classed in the same system — those that regulate ingestion and egestion, and those that protect the air-passages—are special sensi-motor adaptations, which differ from the animal instincts and tendencies that affect all voluntary motor phenomena, only in being almost excluded from the sphere of the cerebral influence.

16. The hypothesis of a system of excito-motory nerves is more likely to obscure than to elucidate the subject. It is a return to the old system of explaining organic phenomena by mechanical arrangements—the miserable shifts of ignorance—and only to be excused in an infant state of the science. It adds to the complexity of the nervous system, and tends to withdraw the attention from the relations of nervous excitability, of which so little is yet known.

17. In Chaps. V. and VI. attention is drawn to the intimate union that exists between the voice and sense of hearing. The relation being similar to that which subsists between the locomotive and sensitive system, leads us to conclude, that if the cerebellum is the combining and associating organism in one

case, some similar structure is probably required in the other. The intuitive power of imitation, which is the characteristic of the auditory vocal system, if analyzed, appears simply to be the translation of a sensitive impression or idea into a motor impulse, of such a nature that it causes an exact repetition of the same impression. An associated connexion of this kind exists as an innate part of the organization, and is strengthened, confirmed, and extended by voluntary efforts. The same definition may be given of volition in the optic tangi-motor system. The idea, acting through the will, causes a motion, which causes an impression similar to the idea. The difficulty of distinguishing between what is innate and what is acquired is increased by the undeveloped condition of the innate organism of association, as well as of the nervous system generally, in young animals; and thus the effects of its gradual advance to maturity are apt to be viewed as the fruit of experience and effort.

18. The auditory vocal combining structure is not only confined to a much smaller system of nerves, but is independent of the quality of position, which requires a very complicated arrangement in the optic tangi-motor system. Hence we may have reason to look for it in the *posterior pyramids* and *olivary bodies* of the medulla oblongata, where the auditory nerve terminates and the respiratory nerves arise. Mr Solly, in his work on the Brain, has shown (p. 146, plate 6) that the arrangement of the cineritious neurine in the olivary bodies is of exactly the same kind as that employed in the brain, "namely, a contrivance by which an extensive surface of neurine is packed into the smallest space."

The small proportion in size these bear to the cerebellum is not less than might be anticipated, if we consider that the system of common sensitive nerves is represented in it by the auditory nerves only, and that the muscles of the voice and respiration bear a very small proportion to those of locomotion.

19. We must not forget that the same parts of the medulla oblongata are probably subservient to several other functions. *Respiration, deglutition, expression,* are all involved. It is

here that there is so much difficulty in giving an harmonious account of their relations and distinctions (Chap. VI., § 18). Voice is voluntary; respiration is rhythmical and semi-voluntary, having an automatic type, but susceptible of voluntary modification. Thus, the *semi-voluntary* character is imprinted by one interfering with the other. The automatic organism, although complete in itself, is penetrated as it were by conductors of volition. Deglutition, or the rapid action of the pharynx, is subordinate to respiration: the peculiar mechanism for hurrying the food past the glottis is required, *because* respiration is in the way.

The function of expression is peculiar: it is involuntary, but excited by means of conscious states of cerebral excitement. The power of the will may hinder the mental state, but cannot prevent expression, *if* the mental state does exist. Its system is superimposed upon that of respiration, both being exposed to excitement affecting *any* (?) other part of the nervous system, but is not, like it, permeable to volition, neither does it participate in the automatic rhythm.

20. Manducation and coition are probably represented in the cerebellum. The motor portion of the fifth nerve, which is distributed wholly to the muscles of the jaws, arises directly from it: in this respect it differs apparently from all the other nerves of volition; and the reason of this perhaps is, that the muscles it supplies are only used in manducation.

It is otherwise with the voluntary motors affected in coition: most of them are employed in locomotion, but the train is peculiar, and obeys peculiar laws of excitation. In some animals it is excited by the smell, without the presence of the female. The type of this train is probably organized in the cerebellum, and is subject to stimuli from the cerebrum and from the spinal cord, the latter passing through the cerebrum.

Similar trains are probably organized and subservient to the other propensities. Thus, all animals have particular modes of fighting amongst themselves and of hunting their prey. But this is trespassing on a subject that forms an important part of the analysis of the propensities.

## CHAPTER V

### ON HEARING AND THE VOICE

*Including the Sensi-Motor Phenomena that constitute the Auditory Vocal-Motor System*

1. Imitation seems to be the grand element in speech and intonation. We try to imitate certain sounds, and, after a few imitative efforts, we succeed in establishing such an association between the auditory impression and vocal motors, that a very slight voluntary effort only is required for exact repetition. In this we are guided altogether by the sense of hearing: we know nothing of the muscles employed, or even—as with other parts of the body influenced by volition—of the parts moved. In other voluntary efforts, although we have no perception of the muscles put into action, the parts moved and their change of position are distinct objects of contemplation. Not so with the organs of voice; the voluntary effort is accompanied with certain impressions on the acoustic nerve, and this is all that we are conscious of. The muscles of the voice—that elaborate mechanism, so admirably described and enlarged upon by Sir C. Bell—are regulated with the most exquisite precision by means of the auditory impression only.

2. The power exists in a greater or less degree of modulating the voice, so as to produce at the first effort a sound similar to that uttered by another individual. This shows that a state of intimate association must exist between the vocal motor and auditory sensitive nerves, that enables a certain excited state of the latter to act at once upon the former, with that precision requisite to produce an impression on the auditory sense similar to the original one. Does it not appear as if *a vocal motor perception accompanied every auditory impression*—the radical or germ as it were of that motor effect which might produce the sound?

The voice is certainly not capable of imitating every sound exactly or even remotely, although capable of approaching to such an imitation with a greater or less degree of precision; but neither are we capable of imitating the personal expression

of attitude, configuration, or action, that characterizes every object we behold, although by sympathy we are certainly aware of their expression. If such is the case, they will harmonize with the law that governs optic tangi-motor perceptions,—viz. that each impression is accompanied with motor perception, which gives to it force and expression. (See Chapter VI.)

3. This motor influence, however, is only attached to auditory perceptions after they have been organized. Special perceptive faculties are as much required to effect this as in the case of optic tangi-motor impressions. How can this conclusion be avoided? Phrenologists have included all in one organism —Tune, namely, or melody, which has a very subordinate place in their system. But is this consistent with their classification of the perceptive faculties of the optic tangi-motor system? A specific organism is considered necessary for the perception of the colour, of the size, of the weight, of the form, and of the individuality of an object respectively: is not a like separation of function as much required in perceiving the tone, loudness, timbre, and individuality of a sound? The mechanism of the internal ear may, like that of the eye, confer that distinctness on the incident action which prepares it for and gives it excitory power upon the perceptive organs; but if this is the limit in one case, it must necessarily be so in both. A cerebral power is necessary for the perception of each quality of the impression that is presented by the sense.

4. We have endeavoured to show that the sense of vision and of touch are combined with the muscular sense in forming a class of perceptions called optic tangi-motor. We now propose to offer a few thoughts on the sense of hearing, in connexion with the voice, and view them as forming another class of perceptions, which may be termed the auditory vocal-motor.

The analysis of the formation of auditory impressions is a much more difficult study than the physiology of vision; and the difficulty seems to arise from the necessity of having optic-motor perceptions of auditory phenomena to make their action intelligible. Notwithstanding the elaborate inquiries that have been made in this subject, it is still involved in much obscurity, and the mechanism of the internal ear is

an enigma that has hitherto defied all attempts at solution. Are the several parts of the labyrinth and vestibule intended merely to magnify the intensity of the vibrations, and subject the auditory nerve to impressions both from without and within? or is there an arrangement involved which determines and is the means of representing the element time? or that is required for the perception of different simultaneous sounds?

5. It is believed that *one* impulse may cause the sensation of sound, if of sufficient duration and intensity. Of this we have instances in the noise of an explosion, in the crack of a whip, in the beat of a drum (?). May we conclude, that a single vibration of a musical sound, or continued noise of any kind, is audible,—that it is of itself sufficiently intense to excite the auditory nerve? The eye is capable of distinguishing a collection of individuals, each of which viewed by itself is imperceptible, yet we cannot deny that an impression on the sense is made by each individual. This may be the case in hearing: each impression must be sufficiently intense to produce an impression; but it does not follow that this is sufficiently extensive to produce a sensation. Indeed, it appears illogical to refuse excitable power to an individual impulse, when a succession of them has manifestly this property. If they acted all at the same instant, we might be warranted in making such conclusion; but their *successive* impact makes it imperative on us to allow that each has excitable power. The limits of auditory excitability may depend upon both the duration and intensity of the impulse,—a deficiency in one may be compensated by an augmentation in the other. The results of M. Savart's experiments confirm this view. The usual limits of audible sounds are confined between 16 and 2000 impulses in a second; but M. Savart has proved, that, if the *intensity* is sufficiently great, sounds of 24,000 impulses in a second may be heard; and if the individual pulses are sufficiently *prolonged,* sounds of 8 impulses in a second may be perceived.

In this there is some analogy to the mode in which the limit of vision is determined. With ordinary illumination we lose sight of an object, if it subtends a less angle than 30″; but we can perceive fixed stars of the fifth or sixth magnitude;

and it has been proved, that Sirius, the brightest star of the first magnitude, subtends an angle less than $\frac{1}{10}$ of a second.* When the illumination is very deficient, small objects disappear; and when the darkness is considerable, only large objects can be distinguished.

6. In employing an apparatus similar to Savar's wheel, we may perceive how the pitch of the sound is affected by the rate at which the impulses succeed each other. If this is at first slow, and increased by degrees, "we hear at first a rattling noise, then a low murmur, and then a humming, which by degrees acquires the character of a musical note, rising higher and higher in acuteness, until its pitch becomes too high for the ear to follow." Now, although the sensation changes with the rapidity of succession, we are still sensible of the original character which distinguishes the individual impulse. This character, or peculiarity of a musical note thus artificially produced, seems to be as much entitled to the denomination *timbre*, as that difference between the same note sounded on different musical instruments, to which the term is usually applied. We thus conclude, that the sense of hearing distinguishes a difference between two vibrations that has nothing to do with their pitch, intensity, or duration. What does it arise from? Dr Young (Lectures on Nat. Phil.) has attributed it to the compressory force in a vibration varying by different degrees. Indeed, this is the only feature left to account for it.

It may be represented linearly with as much facility as the other two distinguishing qualities of sound—loudness and pitch. These depend on the length and breadth of the wave; timbre upon its contour, or the form of its type, which we must conclude is not affected by any change of the former. The forms that answer to certain characteristics of tone, and the mechanical conditions of the vibrating body required to

* There seems to be an ambiguity in the reasoning of some physiologists as to the size of the ultimate divisions of the retina capable of *independent* sensation. It can be determined only by ascertaining when *two* points cease to be distinguished as separate. This limit probably does not alter, however intense the light proceeding from the two points; but the perception of *one* object appears to be limited by the intensity of illumination only.

produce the different forms, constitute a branch of acoustics of great interest, but one which is so beset with practical difficulties that little progress in it is to be expected. All that has been effected seems to be confined to Dr Young's observations on the forms of the curves described by a bright spot on a violin-string by different modes of drawing the bow. Each of these forms of orbital motion is probably accompanied with as distinct forms of undulations. We instantly perceive that they admit of as much diversity as the character of the various sounds that meet the ear (see Fig. 8), and to which we are unconsciously indebted for most of the knowledge of what is going on in our immediate vicinity, both amongst

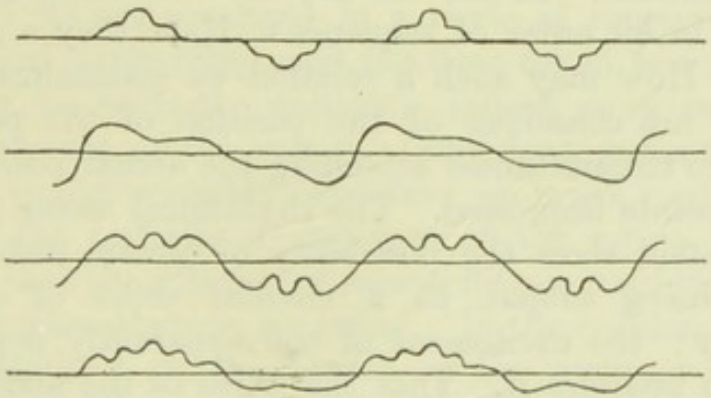

Fig. 8.

inanimate and animate objects. It is chiefly to the latter that the power of perceiving the timbre of sounds seems to be adapted. Articulate speech appears in the abstract to be a train of sonorous phenomena continually changing in timbre. It will be granted, that in reading aloud with a monotonous voice, without intonation, each word is intelligible; now in so doing, neither the pitch nor loudness of the voice alters, it must therefore be the timbre that is continually changing. Inasmuch, then, as language is of importance to man, so is the power of distinguishing this quality of sound. In so far as vocal harmony and expression are concerned, it is subordinate to musical quality or pitch; but its psychical relations are certainly of equal importance, and we are sufficiently warranted in giving to it great prominence as a characteristic or distinguishing quality of auditory perception.

7. Taking for granted the general opinion that is entertained of the cause of timbre, let us direct our attention to the peculiarity of auditory sensation which enables us to perceive it. The intensity of compressory force we may without difficulty conceive to be impressed on the auditory nerve; but the different degrees in which it varies require that the time or duration should be accurately represented. Is this provided for by the mechanism of the sense? or is it due to cerebral reaction? Time to the ear is what space is to the eye: the latter, we have seen, is due to the sense of muscular motion; does the former also involve muscular perception? In the retina different sensitive points are associated with different relative positions. In the ear are different sensitive elements associated in an order of sequence? Have they a consecutive relation? How may such a relation be maintained? In the retina we are conscious of the position of the parts of an image from circumstances attending the actual position of the sensitive points impressed. The rhythmical motor phenomena of the system show the possibility of certain motor nervous elements being subject to a circular order of consecutive excitability; the excitement of one apparently depending on that which precedes it. That the fibres of the auditory nerve should be connected in such a manner, it is necessary that the activity of each fibre should require both the vibratory impulse and the excitement of the preceding fibre to awaken it. As there is nothing inconsistent with organic phenomena in such an hypothesis, let us examine how far it is calculated to assist auditory perception.

8. In doing so, we must not forget, that as auditory sensation lasts longer than the impression, the awakened action of each fibre probably endures so long; and from the experiments of M. Savart, and the phenomena of beats, this embraces the time occupied by many vibrations.* Another question occurs:— Does the excitability endure as long as the sensation? Does it gradually cease? or does it cease in one fibre before it begins in the next? This admits of only one answer: all the advantage

* If it were possible to regulate the intensity and timbre of two sounds from pipes that differed but little in pitch, perhaps the *actual* duration of this excitement could be measured.

that is proposed to be gained by this arrangement would be annulled, if more than one fibre were simultaneously excitable. They must interfere with each other, in regard to the time of their external susceptibility, as little as the different parts of the retina do in regard to position. On the other hand, the sensation in each fibre will admit of the simultaneous consciousness of as many vibrations as are contained in the time that it lasts, although its intensity may probably diminish before disappearing. The endurance of the sensation and that of the excitability of the fibres are thus to be viewed as distinct phenomena. The former probably depends on the reaction of the brain, the latter on the nerve of sense only. According to this hypothesis, the primary fibres of the auditory nerve (or part of it) are arranged in a circle of consecutive action or rhythmical excitability, and the time taken to complete the circle must be perfectly uniform,—indeed more so than any other phenomena of the system. Any derangement or alteration of it would probably introduce as much confusion as a change in the position of any part of the retina. We must also allow that the rotation of excitability is continually taking place even when there is no sound. Is the cochlea the seat of this excitability? If the branch of the auditory nerve that supplies it is taken as $\frac{1}{20}$ of an inch in diameter, and each primary filament to be $\frac{1}{10000}$ inch thick (Müller, p. 605), we shall have 250,000 fibres consecutively active, which are more than sufficient to transmit an exact impress of a considerable train of vibrations.

9. In drawing a toothed wheel along a table, first slowly and then with increasing rapidity, we distinguish at first only a succession of sonorous impulses; but when the number of these in a second exceeds about 16, we become sensible of a new perception. The character of the noise still remains, but a *murmur* accompanies it; and by moving the wheel still quicker, a humming is perceived, which increases in acuteness. In these cases, it appears as if a new kind of excitability was evolved, of a specific character,—an excitability that requires the impact of more than one sonorous impulse, and that varies in degree according to the proportion of impulses in the same time. The relation between degrees of this excitability is

determined by the coincidence of impulses; and this may be viewed as a natural consequence of it being essentially dependent on their rapidity of succession. Thus, if an impulse of C lasts as long as two impulses of C′, the sensation of the latter is similar, but raised a step in pitch. If an impulse of C′ is as long as two impulses of C″, the sensation of the latter is also similar, but raised another step in pitch. The similarity of C C′ C″ is evidently derived from the coincidence of their impulses. Putting $v$, $2\,v$, $4\,v$, to represent the number of impulses in the same tune of the notes C C′ C″ respectively, we may inquire why there is greater similarity between C and C″♯ than between C and G′, which corresponds to $3\,v$, $3\,v$ having a more simple proportion to the fundamental note than $4\,v$. This is evidently because the degrees of excitability ascend in correspondence with the geometrical progression of the rapidity of vibration: $3\,v$ or G′ lies therefore between two degrees of the fundamental note. The harmonics of a fundamental note are thus all dissimilar to it, except when they happen to fall into the geometrical progression. By dividing two vibrations of C into three, we obtain G, the octave below the second harmonic of C, and thus having as an intermediate tone the next place to C″ in regard to harmonic relation with the fundamental C. Two octaves below the fourth harmonic is E, which, being sounded alongst with G and C, forms the most harmonious chord or triad. These three notes do in fact embrace the fundamental note and its first five harmonics. The time of three vibrations of C divided into four, or three of C″ into two, gives F. The time of three vibrations of C divided into five, gives A, which has also the same relation to F that E bears to C,—that is, five vibrations of A make four of F. B is determined from three of its vibrations making two of E, thus being the third of the fifth of C. The last is D, three of whose vibrations make four of G.

10. The natural scale is thus formed by interposing the simplest fractions between the value of 1 and 2: this is shown by the following series:—

| | $\frac{1}{1}$ | $\frac{2}{1}$ | $\frac{3}{2}$ | $\frac{4}{3}$ | $\frac{5}{3}$ | $\frac{5}{4}$ | $\frac{6}{4}$ | $\frac{7}{4}$ | $\frac{6}{5}$ | $\frac{7}{5}$ | $\frac{8}{5}$ | $\frac{9}{5}$ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (I.) | C | C′ | G | F | A | E | G | A♯ | E♭ | F♯ | A♭ | B♭ |
| | | 3 | 5 | 7 | 8 | 9 | 5 | 11 | 11 | 12 | 13 | 14 |

The numbers in the third line represent the sum of the numerator and denominator of the fractions, and indicate the proportion of harmony in the chord, with some exceptions, made by striking it with the fundamental C. If G is now taken as the fundamental note, we have the series

| | | | | | | | | $\frac{9}{5}$ | $\frac{21}{10}$ | $\frac{6}{5}$ | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| (II.) | G | D′ | C′ | E′ | B | D′ | F′ | B♭ | C′♯♯ | E♭ | F′ |
| | | 5 | 7 | 8 | 9 | 5 | 11 | 11 | 12 | 13 | 14 |

in which B, D, and C♯♯, appear thus completing the scale of C. If F, A, and E are taken respectively as the fundamental, we have three additional series (III.), (IV.), and (V.), in which many of the preceding notes are repeated without any change of value.

In the first series we find a difference of $\frac{1}{35}$ between A♯♯ and B♭, and half-way between we find B♭ of (III.). In (III.) we find a difference of $\frac{1}{35}$ between D♯♯ and E♭. In IV., II., and III., we have C♯♯ respectively equal to $\frac{25}{24}$, $\frac{21}{20}$, and $\frac{16}{15}$, the middle value being $\frac{1}{120}$ greater than the first, and $\frac{1}{80}$ less than the last. There is also a small difference in the value of F♯♯. It is evident that we may proceed to divide the octave in this way *ad infinitum*, but it will be found that the sub-divisions soon lose their harmonious relation to the simpler divisions and arrange themselves into sets. To determine the twelve best divisions of the scale to adopt, it is necessary to test the harmonious relation of each, in reference not only to the fundamental note but to all the others. This is easily done (if harmony is indicated by the simplicity of the fraction) by forming a table, as follows (p. 88), of the relative lengths of their vibrations.

The numerator represents the number of vibrations in the note in the left-hand column, the denominator the corresponding number in the note at the top. The perception of harmony does not appear in a chord of two notes, when the sum of the numerator and denominator of their simplest fraction exceeds 11 (?) (see § 11). On this hypothesis, the numbers in column Y will represent their relative musical value.

In column Z the exact difference of pitch between the adjacent notes is given in thousandths of the octave. It will

be observed that the same values are repeated, and that there is a remarkable coincidence in the difference between E and F, B and C, D and E♭, C and C♯, and G and A♭. The difference between C and D, F and G, A and B, are also equal; this is likewise the case between D and E, and G and A. These circumstances are best shown by linear representation in connexion with the logarithmic curve, as in the following figure (p. 89), in which the vertical lines, limited by the curve C C′

| Z | Y | | C | C♯ | D | E♭ | E | F | F♯ | G | A♭ | A | B♭ | B |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 6 | C | 1 | $\frac{15}{16}$ | $\frac{8}{9}$ | $\frac{5}{6}$ | $\frac{4}{5}$ | $\frac{3}{4}$ | $\frac{5}{7}$ | $\frac{2}{3}$ | $\frac{5}{8}$ | $\frac{3}{5}$ | $\frac{4}{7}$ | $\frac{8}{15}$ |
| 93 | | | | | | | | | | | | | | |
| | 5* | C♯ | $\frac{16}{15}$ | 1 | $\frac{16}{17}$ | $\frac{8}{9}$ | $\frac{6}{7}$ | $\frac{4}{5}$ | $\frac{3}{4}$* | $\frac{5}{7}$ | $\frac{2}{3}$ | $\frac{5}{8}$* | $\frac{3}{5}$* | $\frac{4}{7}$* |
| 77 | | | | | | | | | | | | | | |
| | 4* | D | $\frac{9}{8}$ | $\frac{17}{16}$ | 1 | $\frac{15}{16}$ | $\frac{9}{10}$ | $\frac{5}{6}$* | $\frac{9}{11}$* | $\frac{3}{4}$ | $\frac{5}{7}$* | $\frac{2}{3}$* | $\frac{5}{8}$* | $\frac{3}{5}$ |
| 93 | | | | | | | | | | | | | | |
| | 4 | E♭ | $\frac{6}{5}$ | $\frac{9}{8}$ | $\frac{16}{15}$ | 1 | $\frac{24}{25}$ | $\frac{9}{10}$ | $\frac{6}{7}$ | $\frac{4}{5}$ | $\frac{3}{4}$ | $\frac{5}{7}$ | $\frac{2}{3}$* | $\frac{5}{8}$* |
| 59 | | | | | | | | | | | | | | |
| | 5* | E | $\frac{5}{4}$ | $\frac{7}{6}$ | $\frac{10}{9}$ | $\frac{25}{24}$ | 1 | $\frac{15}{16}$ | $\frac{8}{9}$* | $\frac{5}{6}$ | $\frac{4}{5}$* | $\frac{3}{4}$ | $\frac{5}{7}$ | $\frac{2}{3}$ |
| 93 | | | | | | | | | | | | | | |
| | 6* | F | $\frac{4}{3}$ | $\frac{5}{4}$ | $\frac{6}{5}$* | $\frac{10}{9}$ | $\frac{16}{15}$ | 1 | $\frac{20}{21}$ | $\frac{8}{9}$ | $\frac{5}{6}$* | $\frac{4}{5}$ | $\frac{3}{4}$* | $\frac{5}{7}$* |
| 70 | | | | | | | | | | | | | | |
| | 5* | F♯ | $\frac{7}{5}$ | $\frac{4}{3}$* | $\frac{5}{4}$* | $\frac{7}{6}$ | $\frac{9}{8}$* | $\frac{21}{20}$ | 1 | $\frac{14}{15}$ | $\frac{7}{8}$* | $\frac{5}{6}$* | $\frac{4}{5}$ | $\frac{3}{4}$* |
| 100 | | | | | | | | | | | | | | |
| | 5 | G | $\frac{3}{2}$ | $\frac{7}{5}$ | $\frac{4}{3}$ | $\frac{5}{4}$ | $\frac{6}{5}$ | $\frac{9}{8}$ | $\frac{15}{14}$ | 1 | $\frac{15}{16}$ | $\frac{9}{10}$ | $\frac{6}{7}$ | $\frac{4}{5}$ |
| 93 | | | | | | | | | | | | | | |
| | 3* | A♭ | $\frac{8}{5}$ | $\frac{3}{2}$ | $\frac{7}{5}$* | $\frac{4}{3}$ | $\frac{5}{4}$* | $\frac{6}{5}$ | $\frac{8}{7}$* | $\frac{16}{15}$ | 1 | $\frac{24}{25}$ | $\frac{9}{10}$* | $\frac{6}{7}$ |
| 59 | | | | | | | | | | | | | | |
| | 5* | A | $\frac{5}{3}$ | $\frac{8}{5}$* | $\frac{3}{2}$* | $\frac{7}{5}$ | $\frac{4}{3}$ | $\frac{5}{4}$ | $\frac{6}{5}$* | $\frac{10}{9}$ | $\frac{25}{24}$ | 1 | $\frac{20}{21}$ | $\frac{8}{9}$ |
| 70 | | | | | | | | | | | | | | |
| | 5* | B♭ | $\frac{7}{4}$ | $\frac{5}{3}$* | $\frac{8}{5}$* | $\frac{3}{2}$* | $\frac{7}{5}$ | $\frac{4}{3}$* | $\frac{5}{4}$ | $\frac{7}{6}$ | $\frac{10}{9}$* | $\frac{21}{20}$ | 1 | $\frac{14}{15}$ |
| 100 | | | | | | | | | | | | | | |
| | 5* | B | $\frac{15}{8}$ | $\frac{7}{4}$* | $\frac{5}{3}$ | $\frac{8}{5}$* | $\frac{3}{2}$ | $\frac{7}{5}$* | $\frac{4}{3}$* | $\frac{5}{4}$ | $\frac{7}{6}$* | $\frac{2}{8}$ | $\frac{15}{14}$ | 1 |
| 93 | | | | | | | | | | | | | | |

and its axis *c c′*, show the number of vibrations in a second, and the distance between them, measured on the axis, their respective difference of pitch and relative position in the octave *c c′*. The octave is divided by the faint dotted lines into twelfths, being the common scale of temperament; the perfect major diatonic scale is shown by the lines C D E, etc.; the semitones of the preceding table by the thick dotted lines C♯ E♭, etc. The scale may be traced in ascending seven octaves by fifths; and it is remarkable that this gives a very close

approximation to the equal division into twelfths, and that the divergence is regularly progressive, amounting in seven octaves to only $\frac{1}{50}$ of the octave.

If the scale in the preceding table were adopted in any instrument, it is evident that the key-note of a train of melody could not be altered without introducing discord. This may

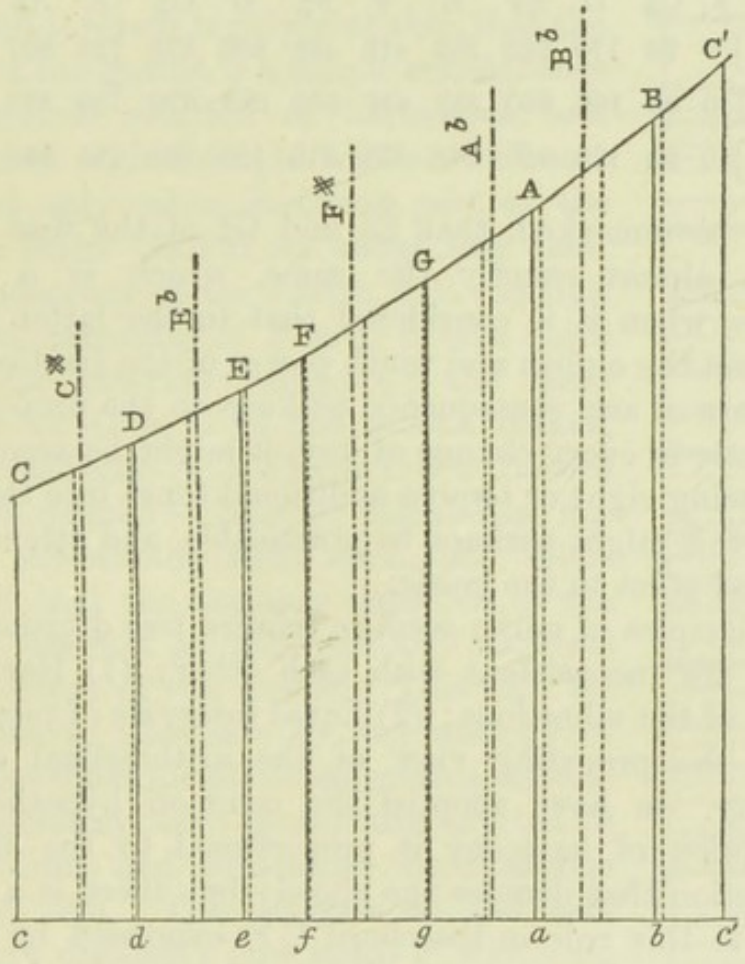


Fig. 9.

be apparent to the eye, by marking off the intervals of the scale from the diagram on a slip of paper, and bringing *c* opposite *d*: it will be found that E and A do not answer for D and G, and that all the semitones are disarranged. This is not so much the case in the equally divided scale; but its gain in this respect is accompanied with a loss of harmony: the fractions that represent the chords are much less simple than those in the preceding table. This deficiency of harmony also attends

the division by fifths, notwithstanding the apparent symmetry of the process. It is fortunate that the ear cannot discriminate intervals with such nicety as to make the deviations of the common scale of much consequence.

The following is a table of the exact pitch of each note in the three scales, calculated in thousandths of the octave :—

| | C | C♯ | D | E♭ | E | F | F♯ | G | G♯ | A | A♯ | B | C |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Scale of Harmony | 0 | 93 | 170 | 263 | 322 | 415 | 485 | 585 | 678 | 736 | 807 | 907 | 1000 |
| Scale of Temperament | 0 | 83 | 166 | 250 | 333 | 416 | 500 | 583 | 670 | 750 | 833 | 917 | 1000 |
| Scale of Fifths | 0 | 95 | 170 | 265 | 340 | 435 | 510 | 585 | 680 | 755 | 850 | 925 | 1020 |

It will be remarked, that E♭ and G♯ of the first and last scales are almost exactly the same, which is a singular coincidence, when it is considered that in the latter they are derived from the eighth and ninth power of the fraction $\frac{3}{2}$.*

If it were of any consequence to keep to the proportions in the first scale in every change of key, it might be accomplished by introducing eight or twelve additional tones into the octave. In organs this might perhaps be practicable, and attended with an improved effect to the music.

The principles of music seem to require two divisions of the scale that are inconsistent with each other: (1) Harmony in the length of the vibrations; (2) Equal intervals of pitch.

11. In the preceding view of the arithmetical synthesis of harmony, we have adopted the common hypothesis that the perception of harmony is proportional to the simplicity of the fraction that denotes the chord; but there is a singular exception to this rule in the chord C F, expressed by $\frac{4}{3}$, which makes it imperative on us to inquire further into the cause of this discrepancy. It has been suggested, that the power of notes to form chords—by which their harmony is tested—

* Rule to find the musical interval of a tone of any given number of vibrations per second from any other tone of a less number of vibrations. —Divide the former by the latter, then take the logarithm of the quotient and divide it by the logarithm of 2 ; the quotient is the interval in terms of the octave unity.

To find the tone corresponding to a given interval from another tone.— Multiply the interval by the logarithm of 2 : the product is the logarithm of the first tone, expressed in terms of the latter.

depends upon their harmonics. The triad, which is the most perfect concord, represents the first six harmonics of C, G, E, and F; the chord of the seventh includes the first seven of C, and the chord of the fifth and second includes six harmonics of A.* This is the most satisfactory theory that has been proposed; and it seems highly probable, that the division of the gamut is a simple consequence of the mutual relation of harmonics, and that a chord with a simple fraction indicates a concord only when it forms part of the harmonic scale. But if we adopt this view, the inconsistency of the fourth still remains. The third harmonic of F is C of the third octave, the interval being $\frac{1}{10}$; the third harmonic of C is G of the second octave, the interval being $\frac{1}{12}$. Why is C G a concord and C F a discord? Neither calculation nor diagram throws any light upon this, unless we admit that the auditory sensation of the latter is affected by and partially suggests the perception of the former. This view we are unable to illustrate by any analogy of vision; but the principle of suggestive association is certainly most powerfully exerted in distinguishing and separating simultaneous sounds (§ 20). The diagram at the side shows very distinctly how much F is isolated from the rhythmical recurrence which combines together C, G, E, and B♭. The lines C C, G G, etc., are divided, according to the respective lengths of the vibrations, into 12, 18, 15, 21, 16 equal parts, and lines are drawn across where these divisions coincide. Now, the material point that the ear judges of so exactly,

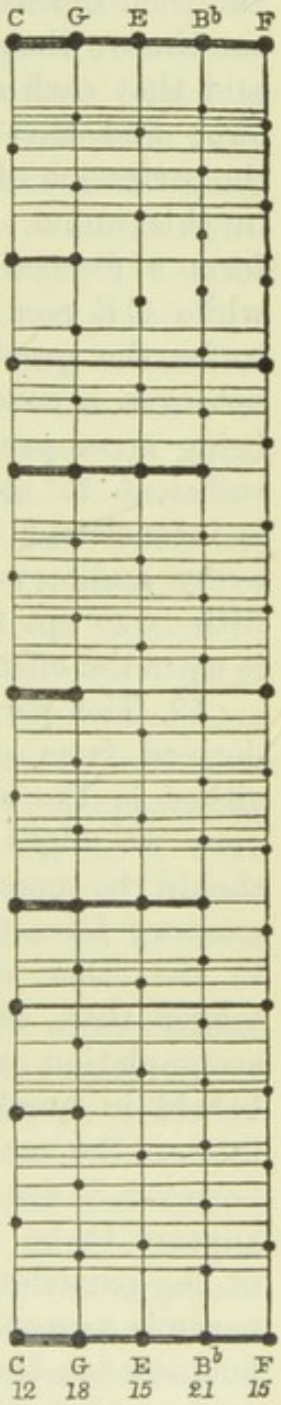


Fig. 10.

* To find if one tone (A) occurs as the harmonic of another (B).—Divide B by A; then, if the product of the quotient by any whole number is equal to a power of 2, A is a harmonic of B.

is that which is here represented by the transverse dark lines and their mutual distance. The vibrations of C and G coincide six times; those of G E and C E three times: thus it may be said that each assists in bringing out the concord of the other two; and this affords the most satisfactory explanation of the perfection of the triad. It is evident that B♭ is also included in this union, but C F is quite isolated, and evidently must form a discord with G E or B♭; but as it recurs four times while C E recurs only thrice, we might expect its harmony to be the greater of the two. That it is not so, that it is not even a concord, seems only to be explained by each note being accompanied by its fifth, which, however faint, may be sufficient to counteract the effects of their unison. If this is inconsistent with acoustics, it may be that, as we perceive every auditory sensation through an associated medium, the fifth is so apt to be suggested when any chord is struck, that it mars the effect of all that are not in unison with it.

12. The perception of the pitch of a sound seems to be derived from an excitability of a specific kind, that exists either in the auditory nerve or in the brain itself. At first view we might be led to expect that a difference of loudness should be perceived between a note and its octave, as the excitory force that impinges is double in one case what it is in the other, and the excitement of each vibration endures longer than the impression. Thus, there ought to be an accumulation of force, just as when a luminous object is beheld in quick revolution, the circle of light becomes more intense the more rapidly it moves. The resulting intensity, however, is not perceptible as loudness but as *pitch*. Loudness appears to be in proportion to the length of the excursions of the particles of air, and the effect of this on the auditory nerve is a greater or less degree of compression. Thus, viewing loudness as the mechanical effect of the compression, it is evident that however often repeated, even before the effect has subsided, it can never exceed the degree that represents the amount of the compressing force.

13. Is the combined perception of loudness and pitch a single or duplex excitability? At first view it seems as much so as colour and intensity of light; yet there is a difference

that deserves notice. No visual perception can take place without colour or a combination of colours accompanying it; but many sounds have no pitch,—thus, the report of a pistol in the open air, at a distance from resonant bodies. Even in visual perception, colour and intensity of illumination may be due to different kinds of excitability, although it is extremely difficult, or indeed impossible, to conceive colour without light, or tone without loudness. But the impossibility of one of these qualities existing without the other does not prove that only one kind of excitability is necessary. We may be sensible of the rapidity and intensity of vibrations by the sense of touch only, and distinguish these two qualities with some precision; but with the idea of time only one kind of sensation is involved—viz. that of degree of pressure; and when the sense of touch is in its normal condition, the perception of one quality necessarily involves the perception of the other. This is not the case with light and sound. There have been many well-attested instances of deficiency in the perception of colours * and of musical pitch, without any other imperfection of sight or hearing. We may conceive the cerebral apparatus, upon which the perception of tone depends, to be as it were a coloured medium, through which the intensity of sound is perceived. This, as well as timbre, it may be the function of the cochlea to assist.

Loudness varies according to the degree of compression; pitch according to the rapidity of succession of the compressory impulses. The lowest limit of an audible note is probably determined by the time a sensation takes to fade away. If the sensation of one impulse subsides before another commences, the continuous humming that characterizes a musical sound will be wanting. Again, in the highest limits, the impulses may follow each other so quick, that time is not allowed for the sensation of one to fade sufficiently to allow the impact of the next to be recognised. Thus, the highest audible notes verge into the perception of a single impulse, and the lower the pitch the easier is the same musical interval distinguished. This also agrees with the experiments of Savart, alluded to in § 5.

* Darwin (?). Dalton.

14. In the hypothetical circle of consecutive action, suggested in § 7, supposing that the excitement of each fibre took a certain fixed time to subside, it is evident that the higher the pitch of the sound the greater would be the number of alternations of action, simultaneously active and equally separate from each other. As in this circle we can distinguish position in time with as much exactness as position in space on the retina, it is evident that the coincidence of impulses will be fully appreciated, and therefore the essentials of harmony fully represented. If we are right in the preceding conjecture, that to produce a musical sound it is necessary that one impulse should succeed the other before the sensation of it has had time to subside, we may infer the cause of the pitch of the sound to be the superposition of one auditory sensation upon another: this awakens the peculiar excitability which gives the *musical colouring* as it were to the intensity, and expression to every auditory perception.

15. It is singular that the cochlea is wanting in amphibia and fishes. Is it an attendant upon voice, and does it assist animals in recognising the call of their mates or their natural enemies? This view might lead us to inquire what peculiarity of auditory perception attends vocal phenomena that is not common to other sounds, and what kind require to be distinguished by those animals that live a life of silence, as the fish tribe. The principal sounds that they are exposed to seem to be that of the waves, also that which may be caused by the motion of other fish in their vicinity, and the noise of running water in streams. What we know of such sounds should lead us to expect them to have a uniform monotony, undistinguished by change either of pitch or timbre. The perception of loudness only would thus be required. To study the comparative anatomy of the ear, in reference to the habits of the animal, might throw some light upon its physiology. There seems to be sufficient proof of a relation between the development of the cochlea and voice. Do they keep pace in the degree of their organization?

16. However little the conjectures we have offered may be consistent with facts, it will perhaps be granted that *intensity*, *timbre*, *pitch*, and *time* are the four primary qualities

which it must be the function of the internal ear to separate and make distinct. One sound is distinguished from another, not only by the variety in the combination of these ingredients, but also in the consecutive changes they may pass through. Many sounds consist of a train of vibratory phenomena, continually changing in the degree of these distinguishing qualities: they are viewed as trains, and distinguished from each other as such. To present these as objects of distinct perception must be the office of the ear, as it is of the eye to arrange the images of surrounding objects. If we required to know only the intensity of the light in the field of view, there would be little need of the complicated mechanism of the eyeball; but the great problem solved by it is the arranging of the luminous points in their proper relative positions. By superficial analogy, we might attribute the chief object of the structure of the ear to be the placing of impressions in their proper position, in reference to time; yet this is done with regard to tactile impressions apparently without any special apparatus being required. If we analyze this, however, it will be found that the perception of their successive occurrence has no reference to any exact estimate of time, but to their order in an associated train. *The whole character of sounds depends on the most exact register being maintained of time.* The general aspect of auditory phenomena seems further to require that different parts of the impression should be incident upon different parts of the sensitive apparatus, because the sensation endures longer than the impression; one part of the latter would inevitably be mixed up with another if all the recipient nerves were continuously susceptible of being excited, as in the other senses; and this circle of consecutive excitability will at once afford the means of registering time.

17. An exact perception of time is required in uttering sounds by imitative efforts. It must be one of the principal means of regulating the voice,—of as much importance to it as the sense of position is to voluntary motion. This may be stated as a proposition:—*The sense of time is to the auditory-vocal system what the sense of position is to the optic tangi-motor system.*

As it has nothing to do with muscular *condition*, it must

be derived from an essentially different cause. The sense of hearing supplies all the information we require of the state of the vocal muscles. The auditory nerve is to the vocal motors (which embrace the whole respiratory system of nerves) what the nerves of common and muscular sensation (the posterior spinal) are to the regular motor or anterior spinal nerves. When the sensation of a part is lost, there is no longer the power of directing its motion with any precision, as knowledge of *position* and *pressure* is wanting. So, for a similar reason, *total* deafness is always (?) accompanied with loss of speech, even if it is but a temporary deprivation.* That it is so in the first instance might also warrant the conclusion that it is so in the latter; for there can be no doubt that the motor nerves are themselves not subject to the associating principle, but are only brought under its influence by means of the sensitive elements. All the cases of paralysis of sensitive nerves illustrate this. That the eye can supply their place in some instances is further corroborative of this induction. There is no such substitute in the auditory vocal-motor system. There is not in it a double sensitive apparatus. The loss of hearing is the deprivation of *all* means of regulating the voice.

18. With every auditory perception there is thus associated an impress of time which regulates the *duration* of each vocal inflection, as the other component parts of the perception regulate the combination of muscles set into action. The mode in which this association may be effected will be understood better by viewing the circle of nervous fibres that are consecutively active in the ear as an *auditory retina*, and that an internal organism registers these circles, and preserves an impress of each in connexion with the impression incident at the time when its excitability was awake. Each part of the auditory idea which the voice is about to imitate may thus be connected with a specific part of this retina, which is further connected with the special vocal motors that are determined by the loudness, pitch, etc., of the auditory idea. The phenomena of sympathy show that there is an innate adaptation between

* May this interpretation be put on Sir C. Bell's Case, 123 (Bell on the Nerves), or Dr Abercrombie's Cases, *Edin. Med. and Phys. Journal*, 1818?

certain auditory sensations and certain vocal motors, similar in kind to that between the visual impression of the attitudes and gestures of the body, the aspect of the countenance, and the motors that produce the same gestures and expression. This properly belongs to the subject of sympathy (Chap. VI). But it is here necessary to remark the aptness of combination that exists among the vocal motors, for reproducing every sound of possible imitation, and for responding even to those which from physical deficiency cannot be imitated, and to express our conviction that it is derived from this innate adaptation, and indeed forms part of it. Many of the observations made on the optic tangi-motor phenomena apply with equal force to the auditory-vocal. In *locomotor* volition, the directing influence is the perception of *position* and *pressure*; in *vocal* volition, it is the perception of *time* and *sound*. Keeping this in view, it will be easy to apply the remarks in Chap. IV., §§ 25, 28, 29, 30, 31, 42, and makes it unnecessary to enlarge more at present on this part of the subject.

19. The loudness of a sound diminishes as we recede from it, yet we distinguish it as the same. The difference of intensity forces itself on our attention only as change of distance of the sounding body. Our estimate of distance in this way is much more vague than that from the size of the visual image. This proceeds from two causes:—(1) In the case of sound there is no exact standard of comparison. A low sound in the vicinity may have the same character and intensity as another at a distance; thus, the Æolian harp and the deceptions of ventriloquists. (2) There is no connecting train of impressions to assist the judgment, as vision is assisted by the perspective of intermediate objects. If these are wanting, as when at night we try to estimate distance by the varying intensity of a light that is approaching, our idea of it is as undefined as that from the sense of hearing. The size of a visual image and the intensity of a sound diminish as the square of the distance increases; but the linear dimensions diminish only as the distance inversely. If, as seems probable, the size of the image is recognised by a linear standard, the difference in the index of distance for a constant difference of distance will in vision vary inversely as the square of the distance,—in hearing, as

the cube. Thus, near sounds ought to indicate distance with greater precision than near objects, with nothing intermediate, —as viewing a bird in the air.

Difference of intensity is the means also of recognising the direction of the sounding body, by comparing the impressions on the two ears and moving the head. This motion is necessary; for if fixed, the same difference of intensity would answer for all those directions that made equal angles with the line that passes through the two ears, or with what may be called the auditory axis; but any angular motion of this line evolves the true direction. This might be stated as a geometrical proposition.

It is by their quality of intensity that sounds are associated with the optic tangi-motor impress of position: the qualities of timbre and pitch connect them with the image of the sounding object and of the action that causes the sound. It is for this reason that unmusical sounds are so seldom thought of, except as indications of certain motions in surrounding objects, and that so few words in any language are descriptive of sounds, in comparison with those that describe visual and tactile impressions. It is thus too, perhaps, that we are apt to overlook the psychical importance of the auditory sense.

To the eye there is an essential difference in the shape of a wave linearly represented, if the ordinates are reduced in a constant proportion, while the abscissæ remain the same. The difference is similar to that between an ellipse and a circle; and the ear must recognise the identity of a sound diminishing in intensity with as much ease as the eye recognises the true form of a circular plate, which, lying oblique to the eye, forms an elliptical image.

Thus, it may be said that sounds are identified by their pitch, timbre, and train only—meaning by train the consecutive changes of the two first qualities—and that their distance and direction, or the position of the sounding body, is identified by intensity.

We thus perceive the necessity of cerebral organisms to separate these qualities in the systems of organic associations that constitute the perceptive faculties; and it will be further evident, that the auditory perceptive faculties must in number and range approach to those of the optic tangi-motor system.

20. The distinct perception of the individuality of simultaneous sounds is one of the most extraordinary circumstances connected with the sense of hearing. To the eye, the linear development of different vibrations referred to one base presents inextricable confusion, as will be apparent at a glance of the figure, in which C and B represent two trains, and A the form or condition in which they impinge upon the ear. The question is, how does the auditory sense separate them? Its power of doing so is quite unlike any thing that characterizes the other senses: in them there must be a difference of position or of time of incidence; but in hearing, two trains are recognised distinctly as separate, although they come at the same time from the same point, and although the condition in which they

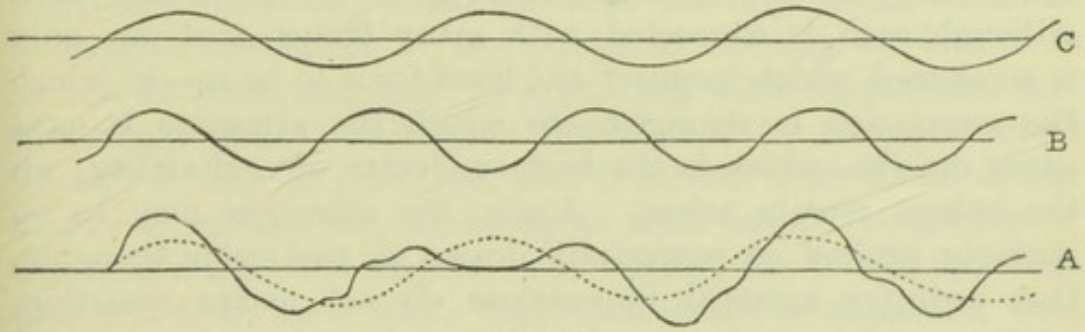


Fig. 11.

present themselves to the ear is wholly different from that in which they are perceived.

It is to be feared that no satisfactory explanation can be given of this, without attributing to previous association *positive* effects on the incident impression. We might proceed a certain length perhaps without this; as, for example, if C and B differed in direction, turning the head would increase the intensity of one and diminish that of the other: the difference between the impressions would thus probably evolve them as two independently variable phenomena. But this is but a short way towards explanation: the sounds can be distinguished as clearly as different objects in the field of vision, without moving the head. The distinctness is the same as if to the eye the contour of C, the sound to which the attention is not directed, had subsided in the compound A into a straight line, and thus allowed the form of the other to be developed.

Has the direction of the attention such a power over the impressions of sense? To this the affirmative answer may be found in the familiar instances of *absence of mind.* When deeply immersed in study, or accidentally excited by stirring events, we may be said to be in a measure both deaf and blind to all unexciting impressions. Indeed, the influence of the attention on the senses cannot be questioned. What is the condition necessary for this species of paralysis of sense? To answer this we require to define attention. It seems to render one part of the organization awake, and set the others to sleep. Ideas that are not present to the mind may be said to be asleep, and when indulging in a reverie—that is, allowing the ideas to flow in their natural order of suggestion, without exerting any effort—they follow the direction of greatest excitability; and although each is connected with many trains, *that* one only is awakened which requires the least force to be so—in which the association is strongest—to which the attention is most easily drawn—which is the best conductor of excitability; all the others remain asleep. Again, the attention may be so strongly excited, so powerfully drawn to particular thoughts, that ordinary external impressions do not excite sensation, or it may be more accurate to say, do not excite cerebral perception. The attention may thus be defined as an *excitability* which traverses the brain, but does not seem capable of diffusing itself in two places at the same instant, although it may fluctuate in position with extreme rapidity. What are the laws of its motion? It is evidently subject to powers both from without and within. Thus, a favourite study attracts the attention to the ideas connected with it, and according to the intensity of the force that has drawn it in this direction, so may we conceive it to resist the attractive force that is engendered by the incidence of external impressions. In like manner, the force of one impression may pull the attention away from others incident at the same time. We examine one object and neglect others; nay, to one quality we may confine sensation as to the form, while we are unconscious of its colour.

While gazing into a fire, or upon the frost on the window, the fancy conjures up shapes of great similitude to many objects, and can even arrange the same elements into different

forms—can view them in different arrangements, according to the idea accidently brought uppermost in the mind. The shaping or predisposing influence which so manifestly affects the sensation, is thus an idea excited from its latent state. In the separate perception of sounds, there is a limit which depends on their similarity. In the rolling of two carriages, or the rattling of iron rods, we cannot individualize the sounds, if their intensity is the same. Thus, to be separated, they must be dissimilar in character, or in different positions; and it may be added, that they, or similar impressions, must have previously been familiarized in some degree to the mind in a separate form: an idea of each must exist ready to be awakened. But is not the new and strange form of the composite vibration, which contains the impression, sufficient to deprive the latter of the power of exciting its idea? Is he not so much disguised by appearing with companions, that his friend and twin-brother inside fails to recognise him? To this it may be answered, that if all the individuals of the cluster simultaneously make up to their respective friends or representatives, the disguise will vanish; for it is only when arm and arm, as it were, that their shape is concealed, that their countenance is hid, that they are withheld from exhibiting their mutual attraction. When thus circumstanced, they may be compared to bodies in a state of unstable equilibrium; the least deflection effectually alters their condition — the least exposure of one sound by itself gives scope to the attractive force, which instantly associates it with its representative idea. Few sounds are all equally continuous for any sensible time: there must necessarily be an intermittance, where several are simultaneous, that allows them all to respond to the attracting and isolating influence of their pre-established ideas. The composite impression will thus be harmoniously disposed of into the respective trains of association, which prevents them being other than separate objects of attention, however rapidly it may travel from one to another. This rapidity is so great frequently, that it can scarcely be distinguished from absolute simultaneousness. Thus, an expert performer on the piano reads off two lines of music at once, without previous preparation and without pause.

This attempt to explain the separate perception of simultaneous sounds rests on two postulates:—(1) That there is an attraction between the idea and impression of a sound, which is sufficient to convey part of the incident effect into a different perceptive locality; (2) That when such an attraction exists, the attention cannot perceive more than one at a time, however rapidly it may traverse from one to the other.

21. That we have succeeded in the preceding pages in determining the organic means of estimating time, is certainly not very probable; but let us not forget, that as time in physics is measured by uniform motion, so time in the mental organism must be represented by a uniform standard of some kind, perhaps of a like nature. There is no kind of organic action that has this degree of uniformity, unless ciliary motion is to be allowed an exception. The excitement of the organism of time must be the subject of exact consciousness, although in other respects most isolated from all the irregularities of the system.

It may be viewed in another aspect, as the means of associating auditory perceptions, through the vocal-motor, with the locomotive functions; and is probably the means of adjusting the latter to the motion of external objects. It certainly enters into the perception of velocity, and tends perhaps to communicate a rhythmical impulse to the acts of volition.

It has also an obscure relation to the enumerating faculty: the motor efforts which are clustered (Chap. VII.) have no other distinguishing quality, no means of separate identification, except from their position in time—in succession.

So far as time enters into the remembrance of past events, or into the conception of the future, a distinct organism seems necessary, which is probably founded on this element of auditory perception, and corresponds to Locality in the phrenological division of the faculties of the optic tangi-motor system.

To determine how far this as a primary element enters into the intellectual phenomena, must be one of the most difficult points of mental analysis. We are accustomed to view time as an abstract idea, not as an impulse of sensation. The view

that has above been taken of its origin, makes it a more distinct subject of contemplation, giving us as it were something tangible to operate upon. As an example of this, we may refer to a method in which volition might be regulated as to time (and almost every act requires this adjustment), by the establishing merely of an association between the motors and certain elements of the auditory sense, the successive awakening of which may call the former into action. It is probably owing to a similar connexion with these elements that the forward impulse is given to the principle of simultaneousness, which joins perceptions together into associated trains (Chap. IV., § 24).

## CHAPTER VI

### ON SYMPATHY AND EXPRESSION

1. Sympathy is the quality of being affected by the affection of another. We propose to inquire into the means that nature has adopted in establishing this quality.

There is perhaps no passion of the mind that displays the effects of sympathy in a more isolated manner than what has been termed Love of Approbation by phrenologists, and none in which it is easier to trace distinctly its workings. Let us therefore attempt its analysis, according to the principles suggested in Chap. II.

2. Approbation or disapprobation must be conveyed by external symbols. In common intercourse, the sentiment is for the most part perhaps conveyed by written or spoken language; but the primary natural excitants of the organism must exist prior to language, and be connected with it only by the great principle of association. How then can this feeling be expressed or understood without language? The feeling of the approbation, or friendship, or affection of another, is pleasant; and a person that experiences a pleasant emotion, if he does not exert an effort to conceal it, displays it in his countenance, voice, and manner; or, to speak physiologically, the muscles of the face and voice are affected by the state of

the mind through the respiratory nerves. That system of nerves has been shown by Sir C. Bell to be only partially under the power of volition, and liable to involuntary excitement by mental emotion of any kind. In some parts it seems to be required for adjusting voluntary motions with the function of respiration; but in others it is impossible to ascribe any purpose for such connexion, unless it be to exhibit in the countenance and voice the involuntary demonstration of mental emotions, *with the view of influencing the minds of others*, and making them aware that such emotions actually influence the party who thus involuntarily expresses them.

3. Now, the question occurs, Why should alterations in the features or tone of the voice convey such information? Plainly, there must be an organic adaptation in the mind prior to all experience, that responds to such expressions in all their astonishing variety. It may be said, in the usual vague way in which such subjects are spoken of, that the expressions are associated by custom with the mental states; that experience and *ordinary penetration* discover such a correspondence; and that in this way the smile of affection, the laugh of derision, the sneer of contempt, and the hoarse ferocity of anger, are easily recognised. But this is a loose way of dealing with the subject. What are the *means* of such associations being established? It evidently requires no learning, education, or exercise of the understanding. We have not to say to a child, Behold this man's face, or listen to his voice—he is angry; as we should say, This is a tree, or that is a book; the language of expression speaks to its feelings direct, and requires no interpreter. There is an inward organism that responds to the external signs of emotion: the attention is immediately arrested if they are directed to self, and pleasure is felt if they denote approval or affection; pain, if the contrary.

So evident does it appear that all can *intuitively* read the language of expression, that further argument seems unnecessary, when it can be so easily put to the test of experience. We are influenced by it every hour of our lives; but we acquire the knowledge with so little effort, that our attention is not awakened to the *cause*. The perception is that of mental state, *not* of the change of features, which is

only the means of exciting the former. We remark that our friend is sad before conversing with him, and an inappreciable instant is sufficient to convince us of this, from the expression of his face, or perhaps tone of his voice; but if we were asked to describe the state of the respective features that led us to this belief, we should be quite at a loss. It is seldom indeed that the expression lasts so long as to afford this opportunity, except in extreme cases. It may be that the power of expression is not fully developed unless the features are in a state of motion,—so that no pictorial representation can exhibit the living effect, unless we allow that the art of the painter can bring out the motion of features as well as of the figure. Nothing is more rare or difficult in painting than the representation of the true simultaneous condition of all the features, when excited by passion when *working*. If it were possible to fix on canvas the countenance of a man of high intellect and energy, in the act of declaiming, with excited feelings and eyes directed to the spectator, the effect would be electrical. Brilliancy of finish is not of such consequence as accuracy in the details and absence of caricature. Hogarth and Wilkie are splendid examples of excellence in this field. In acting, the effects of *true* expression are peculiarly striking, and distinguish a good actor from a bad one. How often may we remark in an audience the powerful stirring of sympathy when the natural expression is given to a passage, although perfectly quiet and unobtrusive; while the violent gesticulation, shouting, and grimace of an inferior artist fall lifeless on the senses. A good actor first *feels* and then *expresses* the situation of the character. It is in vain to simulate expression, if his passions are not duly awakened.*

A striking illustration of the power of expression is given by Dr Ure, in his account of galvanic experiments performed on the body of a criminal newly executed. The electricity being applied to the face, which was perfectly natural in aspect, "Every muscle in his countenance was simultaneously

* "The finest art can create but an exquisite automaton, if the soul—the spiritual impulse—do not inspire the simulation, and veil the mechanism of playing with the *attributes of natural emotion*."—*Theat. Crit. of "Spectator" newspaper.*

thrown into fearful action; rage, horror, despair, anguish, and ghastly smiles united their hideous expression in the murderer's face, surpassing far the wildest representation of a Fuseli or a Kean. At this period several of the spectators were forced to leave the apartment from terror or sickness, and one gentleman fainted." If in such experiments the vocal motors could be brought into a state of activity at the same time with the face, it is probable that the exhibition might become too horrible even for the nerves of the experimenters. The effect of such manifestations on the living sympathies is not at all weakened by knowing that the action is that of an automaton; they are involuntarily assailed with a force that is irresistible. Mrs Shelley's *Frankenstein* derives its interest from this source: the manifestation of expression in a corpse counterbalances by its horrors the weakness of the fiction.

4. To analyze the sympathetic connexion which expression of countenance, tone, and gesture establish, it is necessary to examine minutely every link of the chain of phenomena. First the mental excitement has a condition of the brain to correspond with it. The brain is excited in a certain specific way. The respiratory facial nerve conveys the specific influence to the muscles of the face, which thus experience certain specific contractions. Now, although we cannot always command our features, or banish from them the influence of the facial nerve, *we are conscious at all times of the expression that they assume.* This is a singular fact, which will stand examination. The sensitive branches of the fifth cerebral nerve, distributed to the skin and muscles of the face, may be sufficient to convey an impression of the state of tension and pressure that affects it. But this is accompanied with a perception of the expression as it is pictured on the retina of an observer; the same impress of expression, in short, is derived from common sensation, as by visual sensation when looking at the reflection of the face in a mirror. This is a remarkable point, and deserves the utmost attention.

The facial nerve transmits a motor influence from the brain to the face, and at the same instant an impression is reflected back from the face to the brain. This impression is felt as similar to that received by an observer through the sense of

sight, although it cannot be said to have either the form or colour, or indeed any quality of a visual impression. To enable one party to recognise the expression in the countenance of another, *his brain must be susceptible* of the same conditions that influence the muscles of the features he contemplates. The impression thus awakens the same condition of mind in the observer as in the party observed. It has thus the same relation to the particular state of mind which it represents, as the sense of condition (or whatever other name it may have), which is derived from the common sensitive nerves of the face.

The cerebral elements of emotion (if we may be allowed to use such a phrase) are thus to be viewed as a centre, which is related in a peculiar way to the elements of optic sensation and to the elements of tactile sensation. Any emanation from this centre is accompanied with a disposition to a change of features. This change is to be viewed as a phenomenon from which emanates two influences: the first of these is a tactile impress; the second an optic impress;—and both have precisely the same excitory influence upon the elements of emotion, disposing them to assume the condition that caused the emanation. The influence is thus reflective: the media of reflection are wholly different, yet the effects on the centre are identical. The sympathy of expression, whether as regards the face or the person generally, is to be viewed as a circular train of centrifugal and centripetal excitory phenomena, of which the following is the character:—*From the brain as a centre, affected with a state of excitement A, proceeds an involuntary motor influence, which produces an external change in the countenance or person; from this proceeds two specific sensitive effects—the one tactile, the other visual,—both of which have the same tendency to re-excite the original state of the brain, A.*

5. This may be illustrated by a diagram, as follows:—Let C represent the centre or organism by which the expression of emotion is perceived and distinguished, and is at the same time the medium through which the passions are expressed. F is the circle of tangi-motor facial expression, consisting of centrifugal and centripetal emanations between the countenance *f* and the cerebral organism C. The luminous impulse from

the face to the eye is represented by *f b e*; the optic impression takes place at *e*, and its direction, *e* C, is centripetal. The *rationale* of the action is, firstly, an excitement of C by the passions or impelling principles: this tends to propel a centrifugal motor emanation to the face, which reflects a direct tangi-motor impression, *f d* C, and an optic-motor impression, *f b e* C. These have both the same specific exciting effect on C; and this effect is precisely that which originally came from the passions, and was the cause of the facial emanation. The circle of personal expression is shown by P O, and its type

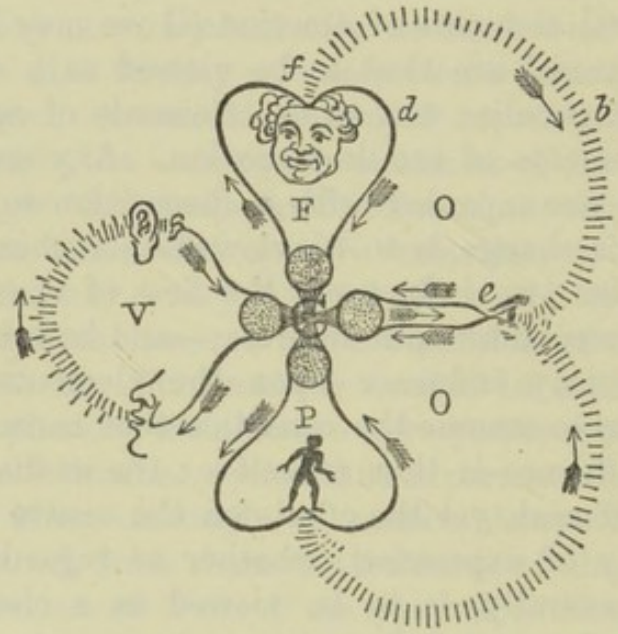


Fig. 12.

is in every respect similar to the above. V is the auditory-vocal circle (Chap. V.), which corresponds exactly in type with the preceding, except that it is not double.

6. When we read a sentence, the attention is confined to the meaning of the words, not to the form of the letters; and the same meaning, the same cerebral impression, is produced if we hear the sentence read: thus, an auditory-vocal impression and optic-motor produce the same perception, and this is effected by means of association. In the same way precisely we may imagine that the sympathetic perceptive faculties recognise the *meaning* of the expression of the same mental emotion, conveyed by the *voice*, by the *countenance*, and by the *person*.

But the association in this case cannot be artificially established; for to effect this, it would be necessary that from infancy we should have been accustomed to see the manifestations of mental excitement in our own countenance—a ridiculous supposition. It is in vain to seek for any other means by which the association could be established by common experience or effort. It must be intuitive—it must form an original part of the organization: there is no avoiding this conclusion. But why should we wish to avoid it? Are not there many other phenomena quite similar? Are not the instincts of animals innate associations? It is satisfactory to be able to see clearly that the explanation of sympathy and expression does not involve any recondite principle; that that connexion of action which simultaneous excitement establishes amongst all sensations is sufficient to explain the whole range of these phenomena.

7. The intuitive nature of expression and sympathy is strongly corroborative of the conclusions set forth in Chap. III., regarding that innate adaptation through which certain external impressions excite specific motor emanations. This principle will be found to be essential to all the mental faculties and impulses; indeed, it is probably the characteristic of cerebral organization.

The common order of perceptive faculties have specific susceptibilities for certain forms of perception,—a disposition to fasten upon certain elements of thought. The sympathetic perceptive faculties have susceptibilities for impressions of expression. The excitement of the passions acts first upon these, and through them upon the motors of expression. The impressions of expression act upon the sympathetic faculties, and there encounter those organisms that connect them with the passions.

8. The muscles of the face have a peculiarity which distinguishes them from all others: their function is wholly or in part to exhibit expression. Some, as the corrugator supercilii—that which knits the brows—participate in no other action whatever. The muscles of all the other parts of the body have either specific locomotive actions to perform, or otherwise assist the animal economy. Those of the face are in a great

measure subservient to the moral economy of the individual,* not casually, but by express adaptation. They are arranged as a telegraph to transmit involuntary signals of what is going on within. Expression is not confined to the countenance; it embraces the action of all the voluntary muscles of the body. We perceive the mental state that accompanies such motions, attitudes, and gestures, both by the eye and by tactile or tangi-motor sensation; but the expressive power of these is small when compared with the countenance, which is certainly the focus of sympathetic emanation.

9. These faculties, from their function of interpreting expression, afford the means by which several of the passions are excited; they are the source from which the impelling principles receive their stimulus. The proofs of this are to be obtained by following out the analysis of Love of Approbation.

10. In § 3 it was remarked, that from this principle of the mind expressions of approbation, of affection, gave pleasure when *directed to self*. It is evident that the latter circumstance is an essential requisite; for such expressions, directed to others, are more likely to be disagreeable than to give pleasure, and to beget that formidable passion, jealousy. How, then, do we know that such expressions are directed to self? What natural sign is instrumental in manifesting this most important circumstance? To answer this, we must inquire of consciousness, which proclaims in a manner admitting of no question that it is the *eyes*. There is no part of facial expression that we are more sensible of than the direction of the eyes. Of this it is impossible to give further proof than is derived from the experience which every one must have of the fact. The large exposure of the white of the eye in man, and the contrast of colour between it and the pupil when compared with the same in the lower animals, are doubtless intended to enable us to judge of its direction with greater precision, and afford the means of developing its extraordinary powers of expression. The direction is not shown wholly by the position of the pupil in the field of the open eye, it depends likewise on the direction of the face, which is determined by the contour of the other

* Sir C. Bell on the Anatomy of Expression, and on the Respiratory Nerves of the Face.

features. This subject has been illustrated by Dr Wollaston (*Phil. Trans.*) The two elements that contribute to the perception of the direction of the optic axis are thus the position of the face and of the pupil in the white field of the eye.

11. The psychical importance of this is manifest, from its being the cause of changing pleasurable into painful sensations, —of converting a state of indifference into that of extreme interest. The smiles and frowns of another, when directed to a third party, may be viewed with some interest; but how different are the sensations that arise when self is their object! The eye arouses the attention and excites the feelings of emotion. The sudden change of mental condition is like the firing of gunpowder.

12. The *external exciting causes* of the Love of Approbation may thus be defined as pleasurable expressions of countenance and voice directed by the eye. Sir C. Bell, throughout his exposition of the Respiratory Nerves, makes many eloquent allusions to facial expression. In reference to the function of the facial nerve, he remarks:—"Laughter and all the changes of the countenance indicative of pleasurable emotion, are neither the effect of relaxation nor of spasm incidentally produced; they arise from a balanced condition of the features, in which certain muscles are in activity, whilst others are thrown out of action. In the painful emotions, still influencing the features through the same nerve, another classification of muscular actions takes place; some muscles in tension, others incontrollably relaxed. Both conditions, and all the intermediate states, *are designed as the outward signs of passion, from which are afforded the highest and most unceasing gratification,—a language which is the charm of life and the bond amongst men.*" This is perhaps the only instance amongst philosophical writers where the psychical importance of facial expression is duly estimated. This is the more remarkable, as we are all so constantly subject to its influence that it is scarcely possible to overlook it in the slightest analysis of the subject. The works of imaginative writers teem with allusions to it; and many words in all languages, descriptive of or defining mental characteristics, are derived from the form of facial expression or tone of the voice.

13. There is a class of pleasurable expressions which may seem to be an exception to this definition. The smile of ridicule expresses pleasurable sensations in the party who manifests it, but is painful to the party who is the cause or object of it. How are the two kinds of pleasurable sensations distinguished? With simple-minded people they are often mistaken for each other; but this may happen from circumstances that do not properly belong to the simple and natural form of the expression. It is singular, if not anomalous, that trains of expression so much alike should have such different effects. The cause appears to be, as in the case of the excitement of jealousy above mentioned, that the pleasurable expressions are *not* directed to self, but to a third party, although *caused* or consequent to some personal manifestation of self. The expressions "to laugh *at*," "to laugh *with*," are thus quite significant of the contrasted mode as well as of the contrasted effects of the difference of direction. But this is diverging from the proper line of argument.

14. It may be objected to the definition of the external exciting cause, that it is the laudation of the mind, not of the features, that stimulates this impelling faculty: that such expressions are naturally acceptable only in so far as they express the real state of the mind. But there is no difficulty in reconciling this, if it is kept in view that the expressions referred to are not the grimaces of an actor, but the involuntary reflection of internal emotion. The natural arrangement of the nerves and muscles concerned in these exhibitions is such that the true expressions of passion cannot be imitated by voluntary effort; the passion itself must be felt; and the simulation of actors, to be effective, must be the simulation of the passion or condition, not of the expression or gesture. By actors is meant not only the *artistes* of the stage, but simulators of every kind, which may be taken to include every person of polite manners who has to exchange courtesies with those he has no esteem for. Such simulations, however, are but the hoisting of the signal of peace. A recent and somewhat philosophical writer thus describes the constituents of highly refined and courtly manners:—"Absolute command of countenance and figure, calm, placid deportment, unbroken

ease, sustained dignity, habitual smiles, indiscriminate respect, nay, the semblance of esteem or even love for every thing that approaches, and the taking a ready interest in whatever concerns every one, but showing none at all in what regards ourselves merely. These imply such an unnatural suppression of feelings, such an habitual restraint upon the emotions of every kind, such a false position of the mind at all times, that," etc. etc. This is perhaps overdoing the description to an extent that is not very flattering to courtly refinement, but it is valuable as the experience of probably an observant witness. To reconcile "*sustained dignity*" with "*habitual smiles,*" an indescribable something probably enters as an element; and this phrase is more expressive perhaps than any other of the personal impress communicated by good society. The simulation of hypocritical attachment is effective, either from the weakness of the mind acted upon, or from the pleasurable expressions being real, but having a cause different from that which is feigned: hence the difficulty of distinguishing interested from disinterested displays of tone and feature. That these are successful and of common occurrence; that a good address should so frequently blind the judgment,—all show that the organism of Love of Approbation may be pleasurably excited in the highest degree by expression of voice and countenance alone, while the corresponding mental emotion is known to be simulated; and this further confirms the truth of the definition.

15. The next point of analysis is the *internal exciting causes.* The preceding discussion has already suggested the existence of sympathetic faculties; as the function of these is to perceive expression, the exciting qualities of the impressions must be here stamped upon them. This exciting quality, as we have remarked above, is pleasurable expressions directed by the eyes. The last-mentioned circumstance is of the highest importance, as the idea of it which is liable to be excited by many kinds of association gives effect to every kind of expression. It is the key or valve, as it were, which opens the susceptibility of the organism. The sympathetic faculties are sufficient to interpret the meaning of the expressions—to organize such impressions; but the direction of the

eye at once opens the passions to their influence, and gives them excitory force. It thus appears that there must be a specific communication between the sympathetic faculties and those passions or organisms that are acted upon by expression, and of which Love of Approbation is taken as the most distinct example. That certain expressions only should have excitory power over this organism, shows that they must be originally imprinted upon it by natural constitution, and reduces the whole of its excitory phenomena to the simple law of association (Chap. III.).

16. But the influence of its excited states on the mechanism of expression demands further attention as an involuntary motor phenomenon. There is a great difficulty of determining, amidst the infinite diversity of expression, what is the peculiar form, or action, or tone, due to its pleasurable and to its painful excitement. This difficulty is increased by the power which is gradually acquired in mature years of overcoming in some measure the involuntary impulses—of concealing emotion, or perhaps, more correctly, of commanding the attention—of withdrawing it so as not to permit the exciting cause to act upon the mind. The contest between voluntary effort and involuntary impulse; the different causes which may lead to similar expressions; the interpretation of innuendoes, and of the general behaviour of others in reference to self,—exercise the sympathetic alongst with the reasoning faculties; and thus the effects of certain phenomena or actions in exciting the passions may and do generally depend on a very complicated exertion of intelligence; but this complicated action is occupied in discovering the *aspect* of the party or parties *towards* self. The exercise of the reasoning powers, by discovering the true relations of events not immediately present to the senses, gives the same excitory power to the idea of certain expressions as the direction of the eye does to the impressions of the same. But the source of excitory power is still *expression of voice and countenance directed by the eye;* it signifies not whether they are present as ideas or as impressions.

17. In children, the expression of emotions is manifested in the least complicated and therefore the most interesting form; and reasoning from the appearances they exhibit, the

probable inference is, that all forms of involuntary expression lie between the two extremes of laughing and crying, and that the infinite modifications to which they are subject, in consequence of the interference of volition, either exaggerate the involuntary tendencies or contend against them. That we are sensible of the effects of volition on expression, and are generally able to separate and discriminate between them, is a proof that the sympathetic faculties include voluntary exhibitions as well as involuntary, and that the consciousness of each which we have in our own persons is perfectly reflected by the personal manifestations of others. This also agrees with the gradual development of expression as it proceeds to extremes. The first muscles of the face which are affected are those which knit the brows and distend the nostrils; the first of the vocal-motor appear to be those which are supplied by the inferior laryngeal branch of the pneumogastric nerve, which modifies the tone of voice in speaking. Voluntary efforts have also the least power over these; they are the first to appear and the last to be subdued. As the emotion increases in intensity, the involuntary impulse extends to the other features supplied by the facial nerve, and to the other respiratory nerves that affect the motions of the chest; next all the voluntary motors of the body participate in the impulse.*

* So many assertions are ventured here in reference to expression, that some apology is due for not supporting them with arguments or instances; but the subject is really impracticable in this respect, or it would require the descriptive powers of a poet, with the knowledge of an anatomist, to do it justice. The proof is therefore left to be suggested by the observation and experience of the reader, which cannot fail to confirm the major part of what is set forth. Some additional evidence may be obtained by reading works of fiction, with the express object of remarking the effect of the many allusions that occur to the manifestations of expression. Among those authors who employ it most successfully may be mentioned Scott, Smollett, Godwin. The following is an example from *St Ronan's Well*: —"Mr Tyrrel has just entered the other end of the parade this moment, and Lord Etherington is walking that way—they must meet each other—I hope they won't fight. * * * In truth, though the meeting afforded little sport to those who expected dire conclusions, it was nevertheless sufficiently interesting to those spectators who are accustomed to read the language of suppressed passion, betraying itself at the moment when the parties are most desirous to conceal it. * * * Tyrrel stepped forward a little before

18. It is in vain to limit expression to the respiratory nerves, nor does it seem proper to include all the muscles supplied by the facial nerve in the respiratory class. The corrugator supercilli has not the most remote connexion with respiration; and the first vocal motors affected are not those which are essential to respiration, but those which are most necessary to give character and expression to the voice. It is in vain to confine the function of any of these nerves to a particular system or circle of organic action. Nature evidently makes every one contribute to as many purposes as possible, thus economizing as it were the material to the utmost degree. The only way to harmonize the complexity of this part of the organization, is to view it as four sensi-motor systems interwoven with each other. These systems are respectively subservient to *manducation, respiration, voice,* and *expression;* the last consisting of two orders—viz. the facial and personal or optic-motor order, and the auditory-vocal order. These all modify and interfere with each other, and act more or less by means of the same parts or tissues. It does not seem possible to trace the lines of separation among the nerves that supply the parts; we must ascend higher; we must look for them in the organization of the brain itself.

his officious companions, his complexion rapidly changing into various shades, like that of one who forces himself to approach and touch some animal or reptile, for which he entertains that deep disgust and abhorrence which were anciently ascribed to constitutional antipathy. This appearance of constraint put upon himself, *with the changes it produced on his face,* was calculated to prejudice him somewhat in the opinion of the spectators, when compared with the steady, stately, yet at the same time easy demeanour of the Earl of Etherington, who was equal to any man in England in the difficult art of putting a good countenance on a bad cause."

The following from the *Antiquary.* Edie Ochiltree has just demolished Mr Oldbuck's pseudo discoveries at the Kaim of Kinprunes:—"This, thought Lovel to himself, is a famous counterpart to the story of *Keip on this syde.* He then ventured to steal a glance at our Antiquary, but quickly withdrew it in sheer compassion. For, gentle reader, if thou hast ever beheld the visage of a damsel of sixteen, whose romance of true love has been blown up by an untimely discovery, or of a child of ten years, whose castle of cards has been blown down by a malicious companion, I can safely aver to you that Jonathan Oldbuck of Monkbarns looked neither more wise nor less disconcerted."

19. The organism of Love of Approbation is capable of being excited to intense pleasurable and painful emotions. This is a peculiarity of all the passions, and is to be reckoned as the first of the train of the internal effects of excitement. The sensations cannot be made the subject of physiological analysis; but there is every reason to believe, that the organism assumes peculiar but contrary states of activity, corresponding to pleasure and pain, and the external effects are as much opposed to each other as the sensations.

Laughing and crying, considered physiologically, are opposite states of the organs of expression: the former is accompanied with convulsive expiration, and the latter with convulsive inspiration; and generally those motors of the voice and countenance which are contracted in the one state are relaxed in the other. The contractile force is as it were withdrawn from one set and propelled upon the other. This contrast of action likewise influences the capillary blood-vessels, upon which change of complexion depends. The two conditions are related to each other like the arms of a balance, the ascent of the one being accompanied with the descent of the other, or like the opposite sides of a Leyden jar charged with electricity, —when one class of motors is charged positively the other is charged negatively; and every emotion of the mind tends to disturb the electric equilibrium.

20. When emotions are in extremes, the whole animal system is affected. Joy excites the organs to a healthy condition, propelling vital force with increased impetus in its natural direction. Sorrow and chagrin have contrary effects, either by retarding the natural development of vital force, or withdrawing it by a different channel. Thus we might conjecture, that when an organism is pleasurably affected, it evolves an extra quantity of vital force from the blood, and distributes it to the other parts of the system; when painfully affected, that it withdraws vital force from the system generally, and thus disturbs the assimilating and secreting processes.

21. Both pleasure and pain, either of mind or body, are accompanied with an excited motor state. In the one case it seems to increase the pleasure, in the other to diminish the pain. In the former, however, the motion seems to be

confined to a particular direction, in reference to the exciting object; and this is probably the case with the latter also, although circumstances prevent the full development of the natural tendencies; or there is not time for the motor excitement to find out its proper channel,—the ideas are too much complicated to point out the direction at once, or the channel is too narrow to pass the accumulated torrent, which thus vents itself in extravagances.

22. Thus, by allowing the natural tendencies to act, we perceive that in one case the pleasure tends to increase, and in the other the pain to diminish, until the object is gained or avoided. The actual motors excited may be the same: hence it would seem as if the same motions may accompany contrary effects upon the animal economy. But we may take a different view; the tendencies in both sensations are in the same direction—viz. towards a diminution of suffering or increase of pleasure. Thus the normal tendency of the system, moral and physical, cerebral and animal, is to cherish, sustain, preserve, or elevate the natural functions, either by acquiring additional stimulative energy, or by throwing off causes of irritation. The analogy between the systems is here complete, and confirms the generally received opinion of the dependence of mind on organization. As the essential nature of the mental phenomena becomes better known, they will probably throw light on the organic processes, and on diseased conditions of the frame; for the molecular phenomena, by which the organism is sustained, are in the nervous system exhibited in an aspect that is so strikingly contrasted with that of all the other tissues, that a higher principle of activity has been associated with it. If it is fully ascertained that the introduction of this principle is unnecessary, we may have reason to reverse the argument, and from the amazing range and diversity of nervous susceptibility, derive the means of enlarging our ideas of the capabilities of matter.

23. When an organism is pleasurably or painfully excited, motor energy is eliminated from the blood, which, if suppressed, tends to derange the whole system, by introducing a source of irritation that disturbs the organic processes. When suffering bodily pain, the agony is much increased by restraining the

*expression* of it. Thus, cries and groans not only exhibit the state of mind of the suffering individual, but tend to alleviate his pain. Long-continued pain of body exhausts the vital powers, just as long-continued pain of mind.

24. When two parts are pained in different degrees, the sensation of the lesser is swallowed up by that of the greater, and it seems to cease. In a surgical operation, the first incision blunts the impression of those that follow; and many similar phenomena show, that the cerebral impression is not exactly a copy of the peripheral irritation, and that it does not accumulate in the same proportion. This may perhaps be explained by admitting that the sensation of pain depends on a specific organism, which is excited to painful action by the state of the nerve. Is the vital force that the excitement exhausts derived from the peripheral part, or is it drawn by the organism from the system generally? If from the peripheral part, it must be eliminated from the blood; but the objection to this is, that the intense pain that accompanies the section of a large nervous trunk is quite disproportionate to the amount of circulating fluid that permeates its substance. Pain would thus seem to depend on the excitement of a distinct organism, which probably exists in the *spinal cord*, as all the sensitive nerves arise from it. This is the case with the fifth pair, which supply the head with common sensation; and their roots are directed downwards, as if they terminated there and did not ascend to the brain. The pleasurable condition of the organism is probably allied to the sexual function, which certainly involves the whole surface of the body, as the passion involves the whole range of the mental functions.

25. We may thus infer, that the painful effect of an irritated fibre on the sensorium may exist from a central cause, without being preceded by the centripetal action of the nerve; but it may beget this action, and probably does so. Every effort of volition seems to be accompanied with a similar excitement (Chap. IV., §§ 1, 4, 7). When impressed with the idea of falling over a precipice, very acute sensations are felt in the soles of the feet, and the muscles of the legs receive a distinct impulse. If this occurs when half asleep, a painful

start is experienced, which seems to agitate all the muscles of the body. The sensitive consciousness which we have of the existence of the different parts of our body must be derived from a constant current of centripetal emanations. When the attention is directed to any part, these emanations are probably rendered more intense; but there does not seem to be any direct proof that such is the case. This leads to remarks on the functions of the spinal cord and cerebellum, which properly belong to the subject of sensation and volition (Chap. IV., § 50).

26. The external effects of the passion we are discussing are exhibited in the conduct of the individual: the internal conscious effects are pleasurable or painful emotions. The constitution of its cerebral organism we can yet have but a vague notion of. It must be a source of power that is personally directive to the individual. It directs his actions according to certain constant and determinate laws. There is an initial inciting tendency or craving, which is characteristic of all the passions. This may be analytically defined as an elimination of motor energy, or disposition to excite motor force, having a specific direction.

27. The pleasurable and painful excitement of a cerebral organism may be compared to the healthy or unhealthy action of a secreting gland, or perhaps more accurately to its condition when the secretion which it has elaborated is prevented from escaping by its efferent ducts; it reacts on the system, and induces a state of irritation and disease. The agency of the blood in conveying impressions of health and disease is probably as important as that of the nerves, and the organization of the globules very complicated. We cannot doubt that there is a special adaptation between the blood and all the secretions, and that its quality may be affected by mental excitement through the organic nerves. Advantage may result to practical as well as abstract physiology, by comparing the active condition of the cerebral organisms with that of the glands. Passion is an active condition of the mind, which requires and demands a certain external stimulus. In this we may distinguish two states: one, when action is eliminated in the craving; another, when this is neutralized or allowed to escape

by its natural vent. It is the efferent duct as it were of the passion, and the secretion is the personal tendencies or actions eliminated. The idea of the stimulus of gratification is excited by the active state of the passion, and the motor tendencies of volition are simultaneous with the image of this gratification. This condition of sustained activity continues until the idea gives place to the impression, to the gratification itself, or consummation of the organism.

Thus, the manifestations of activity arise like waves from the passion as a centre, and they progress in their motion, until like surges they are dissipated by encountering the impressions of consummation.

The analogy of this train of action to secretion may be carried out, by comparing the elaboration of the secretion to the elimination of the specific tendencies; and its escape through the efferent duct by the excitement of the external stimulus, to the dissipation of the eliminated action by consummation. This analogy will perhaps be found not to be superficial, but as having a real foundation in nature.

Are the symptoms of derangement produced by unstimulated or ungratified passion, similar in type to that induced by vitiated or suppressed secretions? To judge of this, to estimate the type, much more enlarged views require to be taken of organic action than are usual.

28. The active state of the passion which sustains the specific personal tendencies may be likened or compared to an excited electric. The personal tendencies of volition are represented by the attractive force of the electric to an insulated body, the latter representing the impressions which consummate the passion. There is no other physical action in the least resembling it. To carry out the analogy would lead to no useful result at present; but, considering that all the mental phenomena undoubtedly depend on organic molecular action, it is by no means unlikely that similar electric conditions are the immediate cause of passion, and that they will at some future period be found to account for the whole range of the inciting forces of the mind, as well as of most of the organic processes.

29. The inciting tendency or craving may be viewed as an

attractive force between the individual and the object of passion. It is not the external object, however, but the idea of it, that issues this attractive force. Thus ideas and impressions are distinguished from each other. In a normal state of the mind, the former cannot act as a substitute for the latter in gratifying the passions.

30. We must not forget that the personal tendencies of this passion are repulsive as well as attractive: disagreeable expressions directed by the eye are as active in repelling as the agreeable are active in attracting. This quality of the passion, in being strongly influenced by ideas and impressions which are the opposite of those that consummate it, is the cause of its prodigious moral force and importance in society. It maintains decency and *external* propriety of conduct. Weak-minded individuals have their whole character shaped by the influence of this passion, and their code of morality bears the impress of the virtues or vices of the society they happen to be brought up amongst.

31. The higher principles of morality seem to be derived from the spirit of independent action, that instigates us to act upon others and to shrink from being acted upon by them. It must be confessed that most of our boasted principles of action are founded on the voice and countenance of others; but each elevates the standard according to his own rank of intelligence, until it at last appears as an abstract rule of conduct, founded upon the immutable principles of justice. It is to this that phrenologists (not Dr Gall?) have given the name of Conscientiousness and the rank of a primary impulse, more for the credit of human nature apparently than from any evidence of it being an original principle of the mind.

32. Ideas make a stronger impression if they have been simultaneous with expressions that excite the passions. In Chap. III. the power of retaining impressions has been attributed to mental adaptation, or a reactive tendency of the nature of passion. The greater the adaptation, the stronger impression it makes. Whatever may be the essential nature of the associating power, it is evidently increased by an emanation from the seat of the passions; and the same idea may be thus strengthened from many different impulses. The power

generated by excited organisms thus communicates strength to the association. As every operation of the mind depends on organic adaptation, the tone and capacity of one faculty may be strengthened, when its natural mode of action is excited by association with another of greater activity.

In connexion with this part of the subject it may be remarked, that as the function of an excited passion is to generate motor power, so ideas associated with it may be considered to possess motor qualities; and as their power of association—their hold upon the memory—is increased by this, it is quite possible that the associating power and motor power of ideas may be identical. The analysis of the perceptive faculties will perhaps throw some light upon this.

33. The knowledge of expression extends to limits that appear boundless; it includes complexion, health, and age. These are all innate perceptions, and belong as much to the principle of sympathy as the knowledge of the expression of active passion. Experience and judgment extend its range, so that it becomes a science which every one involuntarily learns, and is continually extending his acquaintance with. This increased refinement of susceptibility may become transmissible to the offspring, according to the principle discovered by Mr Knight. Indeed, the whole edifice of innate sympathy may thus have been gradually elaborated through successive generations.

34. The energy of the mind also is made apparent through the countenance and voice, and exerts the most dominant influence upon others by its expression alone. To define the difference of the manifestation of the same passion in great and small minds is perhaps impossible; but of the difference of their moral effects there can be no question; every one must be aware of this in his intercourse with society. The excited voice and countenance of a weak-minded person scarcely awakens the senses, but the slightest word or look of a high and energetic character rouses the attention. Thus, orators affect their audience by their *delivery* as well as by their meaning. *Brilliancy of delivery* is in fact synonymous with *power of expression*. The quality of a discourse depends on the personal manifestation of the speaker, as well as on his

meaning and language.* This quality of eloquence is that which principally affects the multitude; it excites and is understood without effort, and will always ensure to the parties endowed with it greater social influence than their talents would otherwise acquire for them.

Power of expression is an index of vehemence of feeling, and may exist without superior intelligence. It is when combined with fluency of language that it communicates the oratorical power—that impressive eloquence by which commonplace subjects are presented with a force that seems almost supernatural, and which entitles it to be considered as "the highest branch of the fine arts."

35. The disposition of the mind to view every thing as having a living character of expression attached to it has already been the subject of remark. It would seem as if every impression was perceived through the medium of the sympathetic faculties (Chap. VIII.). In every object, in every sound, in every motion, a character may be distinguished—may be felt, and we are not sensible of the particulars that give rise to it. Such feelings, when vividly active, may communicate the sense of sublimity and beauty which belongs to the poetic temperament. The focus of beauty may be said to centre in the form and attributes of the female sex; from thence it emanates to the animal and vegetable kingdoms, and ranges over the expanse of nature.

36. But in its departure from the standard of the human form, it is doubtless influenced by many innate susceptibilities that vary indefinitely according to the habits or circumstances that have attended the existence of progenitors. Thus, the Indians of North America are attached to their forests, the Swiss to their mountains, the Arab to his desert, the Dutch to their flats, the Esquimaux to their snows, and each have probably different standards of landscape beauty. If, with

* "We *must* try all means," replied Jeanie, "but writing winna do it —a letter canna look, and pray, and beg, and beseech, as the *human voice* can do to the *human heart*. A letter's like the music that the ladies have for their spinets—naething but black scores, compared to the same tune played or sung. It's word of mouth maun do it, or naething, Reuben."—*Heart of Mid-Lothian*.

mind unoccupied, we wander amidst woodland scenery, a consciousness may be recognised of this innate susceptibility. If we turn back to the recollections of childhood, does it not seem as if the exquisite pleasure of climbing up trees, of lolling on their branches, and peeping through their foliage, had its origin in an impulse like passion, requiring an innate adaptation?

37. The character of motion and form seems mostly derived from the sympathy of personation; and when the object or active phenomenon is greatly superior in size, or power, or activity to the human standard of these qualities, the instinctive tendency to embody them—to embrace them with the powers of sympathy, and clothe them in the garb of expression—give rise to the feeling of sublimity.

38. A character of expression is perceived even where the form is altogether remote from any personal attribute. This shows the extreme activity of the sympathetic faculties. Each letter of the alphabet, each mark of the pen, has an expression peculiar to itself, which is felt, but which it is difficult to describe. To prove this, the following two readings of the Roman capitals were taken at different times, the second being made without any knowledge of the first. The general resemblance between them is sufficient proof that they are not the mere offspring of fancy, although it is extremely difficult to point out any reason for attributing to them such expressions. Sometimes the letter appears as the features of the countenance, sometimes as an attitude of the person, but generally a strange mixture of both is apparent, the same parts serving to represent each. Does not this very power of distinguishing and separating these attributes of expression confirm the preceding views of their isolation from each other in the mental organism?

A (1) Simple, pleased, resigned, respectful; aspect fronting, inclined towards your left.

(2) Mild, simple, contented, unsophisticated; stedfastly regarding with open mouth some one who has been giving him directions at his right-hand side.

B (1) Simple, self-complacent, stupid, full of vulgar gossip; aspect in profile to the left.

(2) Pleased with self, stupid, rustic, harmless; looking in profile to the left, not regardful.

C (1) Narrow-minded, finical, feminine, not vulgar; aspect in profile to the right.

(2) An elderly maiden lady of a particular temper, who has just had occasion to find fault; profile to the right.

D (1) Serious, sensible, knowing, candid; looking upwards, profile to the left.

(2) A steady, intelligent, careful person, mildly receiving directions from his employers; profile to the left.

E (1) Talkative, gossiping, caustic, smiling, hearty, cheerful; in profile to the left.

(2) A simple rustic character, having just been giving a detail of serious matters to a person on the right-hand side, listening to his remarks in answer with open-mouthed attention. (This very fluctuating in expression.)

F (1) Stupid, gaping, well-meaning, oratorical; profile to the right.

(2) Weak expression; countenance long, projecting nose and open mouth; officious, petty, servile.

G (1) Corpulent, hearty, asthmatical; profile to the right.

(2) Grave, serious, religious, benevolent, sympathizing; profile to the right.

H (1) Observant, vacant, staring, simple, feminine.

(2) A person standing fronting you with his hands on his knees, regarding you with open-mouthed serious attention.

I (1) Orderly, upright.

(2) A person standing upright fronting you, looking with attention.

J (1) Vulgar, sagacious, self-sufficient, looking down; profile to the left.

(2) Contemptuous, vulgar, ignorant, insolent, petty.

K (1) Old, narrow-minded, slyly humorous: profile to the right.

(2) Vulgar, sagacious, attentively listening; aspect to the right.

L (1) Servile, contented, stupid; profile to the right.

(2) Pleased, quiet, attentive, subservient; looking in profile to the right.

M (1) Watchful, stanch, steady, bold, intelligent, looking to his right; aspect fronting, head inclined to the left.

(2) Stanch, firm, chiding; aspect front, towards your left.

N (1) Careless, graceful, full dress; looking over his left shoulder.

(2) A person looking upwards towards your right.

O (1) Simple, inexperienced, feminine, attentive, gazing admiringly; fronting,

(2) Mild, reverential, timorous, feminine, good-natured; front aspect.

P (1) Youthful, curious, enterprising, pleased; profile to your left.

(2) Pleased, intelligent, polite; profile to the left.

Q (1) Regardful, sagacious, with lolling tongue; aspect front, inclined to the right.

(2) A child with its tongue lolling out of its mouth; looking up to the right.

R (1) Skating; looking upwards in profile towards the left; pleased, intelligent.

(2) Profile to the left, looking upwards; open, candid, speaking, masculine.

S (1) A lady talking earnestly on a favourite subject; profile to the right.

(2) A lady's face smiling, stupid, fat; profile to the right.

T (1) Experienced, sagacious, attentively listening; aspect front, to the left.

(2) Humble, looking upwards; aspect fronting, inclined to the left.

U (1) Proud, attentive to an application made by an inferior; aspect front, and a little towards the left.

(2) Stupid, fat, conceited; looking upwards to the left with a criticizing air.

V (1) Precise, earnest, intelligent; having just made a pause, is looking complacently towards an individual at his left hand.

(2) Front aspect looking towards the right; precise, singular, pleased, conversing.

W (1) A stout old short-necked man stedfastly regarding you with his hands in his pockets, and an expression of keenness and independence.

(2) Busy, bustling, gossiping; aspect front, towards the right.

X (1) A forward, self-sufficient, good-natured man, who has just taken a pinch of snuff, and while enjoying its zest stares at you with a coarse, gossiping, open-mouthed expression.

(2) Anxious, stupid, receiving directions; aspect fronting.

Y (1) A serious, half-timorous person, peeping with respectful diffidence at some ceremony that is taking place at his left-hand side.

(2) Mean, servile, stupid, poor; aspect front, towards right.

Z (1) Vulgar, knowing, mean, grovelling; profile to the left.

(2) Coarse, tall, stout, pleased, selfish, vulgar; profile to the left.

## CHAPTER VII

### ON THE FACULTY OF ENUMERATION

The power of tracing numerical relations may be taken as an example of the influence exerted by the mind upon the elements of thought, without any motor excitement proceeding directly from it.

1. Objects are abstractedly viewed as so many in number, whatever may be their other qualities; and the passion for numbers that some individuals exhibit shows itself—when most powerful and isolated from other kinds of intelligence—in recognising impressions, and recollecting them in reference to their number. The more complex activity of the faculty shows itself in arithmetical operations, which, on being analyzed, are resolved into addition and subtraction: these again, in reference to the mental power that takes cognizance of them, may be distinguished as the simple recollection of numbers, which is subject to the ordinary laws of association.

2. An individual highly gifted with a power over numbers

has a multiplication table extending to a much higher range than with ordinary calculators. This is a simple fact, which is in some measure a key to the faculty. The product of 9 by 8 is a simple act of memory recalling 72. It is probable that 98 times 72 occurs to extraordinary calculators with the same ease.

3. The distinctness with which the associating principle arranges a diversity of numbers in the mind must be owing to the original aptness of the latter to perceive numerical relation.

4. The associating principle affects all the faculties, but in different degrees, proportionate to the perfect condition of the organ. It is the bond of connexion between ideas; that which determines the succession in which they rise; but that which admits them, and retains them strongly there, is the specific adaptation that subsists between them and the mental organism.

5. The external stimulant of Love of Approbation was shown to consist of certain expressions of countenance and voice, directed by the eyes to the individual thus excited, who courts such expressions by an instinct similar to that which leads all animals to seek pleasurable sensations. With the faculty of number it is different. Its consummation is unconnected with any alteration of external objects, but follows the simple apprehension of their numerical relations; in viewing objects as a group of units, and ascertaining the difference between two or more groups, and the difference of differences, or relation of relations, which expression by symbols enables the mind to trace to an amazing extent.

6. The original type of enjoyment, the simple perception of number referred to the standard of numbers implanted in the mind, is refined upon in the more complicated processes of numerical science; and that which will be understood under the title of the *agreement of results*, affords still higher gratification, yet confined to the enumerating faculty, isolated from others. It is this only that appears to be recognised as the agreeable. The simple perception of objects grouped numerically—what may be called the statistics of objects—has but a passive interest, unless it is connected with considerations that affect other elements of mind. Nevertheless it is this which

seems to present the most simple condition of activity of the faculty, and is therefore to be preferred in attempting to trace the mental adaptation that takes cognizance of it.

7. Let us not forget that motor and sensitive influence attends every mental perception. The simplest perception of objects necessarily affords the fundamental requisites for enumerating.

Let there be three oranges presented to the eye, there is no difficulty in ascertaining their number at a glance; but a greater number requires a proportionally greater effort to do so. This is the fundamental point; mark *the effort* and the desire that accompanies it.

8. What powers does this effort bring into action? The eye informs us at a glance of the number in a cluster of objects, if this does not exceed four or five. This, like the multiplication table, has a very confined range, which may differ with different individuals according to natural talent and practice. To proceed further brings the enumerating faculty into greater activity.

9. The effort is the act of registering the perceptions in a continuous succession. Is each perception accompanied with motor as well as sensitive influence? There must be an inward apparatus that urges the attention upon this specific relation of objects, and takes cognizance of nothing else.

10. The effort and the simple sensation united appear to be the subject of cognizance in this faculty. A youth of extraordinary calculating powers is said to have been annoyed because he could not count the notes of an orchestra, the faculty in him being so active as to retain a distinct trace of the number of rapidly succeeding impressions.

11. If we turn within ourselves, and try to discover the nature of the effort required to keep rapid count, we shall find that the difficulty lies in making a voluntary effort to correspond with every impression: unless such is done, we lose count. Now, there appears to be in different individuals a greater or less power of doing so.

12. But the voluntary effort must be of the numerical kind; so that we have not got out of the original difficulty. The last effort of the mind in keeping count, takes cognizance of all preceding impressions as well as the last.

13. The main effort seems to be, in attaching each succeeding impression to its proper place in that numerical standard that every one is conscious of having imprinted on his mind. This standard of number is not innate, but acquired gradually. Now, what element of mind is this acquired standard based upon or attached to?

14. The standard is such, that we cannot think of any number, such as 782, without having in our mind's eye the position which is held by that number, in reference to the others which precede and follow it. Thus, each number summons up the numerical scale alongst with it; and the distinctness of the perception of the value of a number consists in the distinctness of the scale below that number. Ten is better understood than a hundred, and a hundred than a thousand; because the scale becomes less distinct in ascending; and a good calculator has no doubt a much more extensive scale than a bad one.

15. As to the original formation of this mental scale: it consists of a succession of units, each having a signification proper to and depending upon its place in the scale or order of succession—*a signification exactly equal to its distance from the extremity of the scale or zero.*

16. The numerical power of the mind thus appears to consist in the distinctness and ease with which a multitude of consecutive impressions can be made to present themselves before the mind's eye as one idea.

17. The essential parts of the scale, or traces in the mind that constitute it, seem to be identical with the elements of voluntary exertion, that are so closely associated in our thoughts with numerical symbols that they can scarcely otherwise be recognised. But the enumerating organism takes cognizance only of the perception of motion or action that accompanies impressions—and these are made to arrange themselves in a scale—so that any number is liable to become recognised as a simple idea or perception.

18. Can any mechanical arrangement be suggested, so far analogous in the connexion of its parts as to assist the mind in conceiving more distinctly these abstract relations?

The perceptions of effort are the food adapted to the

organism, and its action upon them is to determine a relative state of excitability. Although of themselves isolated units, they are bound together; so that the perception that has last entered lifts up at once as it were all those that have preceded it; and their re-excitement follows the same law.

19. It is thus that the mental scale (13) may be supposed originally to be formed; and upon it every kind of calculation to be performed by symbols, and the ordinary powers of association, like airs upon a harp.

20. The lifting up of preceding impressions being the characteristic of the registering process of the organism, we may fancy each perception of effort to be represented by a bead. A number of such in succession will be a string of beads close together. Any number being excited in the mind, the corresponding bead is pushed towards the beginning of the row, and it necessarily disturbs all those that precede it. This will serve to fix in the mind the result of the preceding analysis.

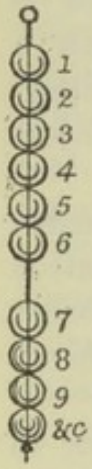

Fig. 13.

21. How does the scale give power over numbers?—5 taken from 12 leaves 7. It is not enough to say that this is known from practice, from experience, from testimony. We are otherwise convinced of its truth, by reversing the operation—5 to 7 makes 12.

22. If we analyze multiplication and division in this way, we shall find that both are but modifications of simple addition, and that addition is but modified enumeration. This modification consists in the use that is made of the suggestions of the associating principle: in short, by availing ourselves of the remembrance of the results of enumeration, we may extend the range of the faculty indefinitely. Thus, 7 to 12 makes 19, 7 from 12 leaves 5; again, 7 times 12 make 84; so that 19, 5, and 84 come to be associated with 7 and 12, and are recognised as their sum, difference, and product, by a simple act of memory. In the same way, we may recollect the logarithms of a few numbers, without any enumerating effort being suspected.

23. When the faculty is strong, the memory for figures probably increases in the same proportion, as it is a memory based on the enumerating process.

24. Extraordinary mental calculators, if their process is similar to what is usually found to be the easiest in the practice of written characters, must recollect figures not only in their order of value, but in the relative position given to them in the process. It is difficult to see how the enumerating faculty can enable them to do this. It is more probable that they never lose sight of the proper value of the digits during calculation, but assist themselves by taking the numbers to pieces, rapidly operating upon them in this form, and then bringing them all together. Thus, the product of 527 by 642 may be found by first taking 600 times 527 = 300 thousand 162 hundred, the latter being perceived from 6 times 25 = 150, and 6 times 2 = 12; *secondly*, 40 times 527 is known to be 80 more than 21,000, because 40 times 25 is 1000, leaving twice 40 to be added. These, added, give 337 thousand 280, and the third term is, from the same property of 25, seen to be 1054; and the final result 338,334. In this, advantage is taken of 25 being the fourth of 100; and we are assisted by taking thousands as units. This product could be found perhaps with less effort by considering $527 \times 642 = 527 \times 6\frac{1}{2} \times 100$ minus $527 \times 8$, recollecting that $27 = \frac{100}{4} + 2$. All numbers may be thus dissected; and such relations occur more readily with some than with others.

25. The distinct perception of numerical value extends to a higher number in some minds than in others. There is nothing in this unlikely; the smaller the number the more distinct our idea of its value; and the same distinctness with which we perceive the relative value of numbers up to 100 may extend in others up to 1000. Upon what circumstances does this difference depend?

26. Referring to the preceding analogy (20) between the mental scale and a string of beads, we may extend it further, and remark, that the difference between the motion of one and two beads is more apparent than between seven and eight, as the difference in the length of a line of one and of two inches is more apparent than between lines of twelve and thirteen inches, although their difference is equal.

27. The enumerating faculty may be conceived to present the successive beads, which are moved by the effort of the attention. It is developed in a greater or less degree according

to the rapidity with which external impressions can be associated numerically, which, according to preceding analysis, means the rapidity with which the mind can accumulate consecutive efforts on the chain of dependent excitability.

28. Although small numbers generally are more distinct than larger, there is an element of their distinctness that has nothing to do with their distance from the beginning of the scale. This is the number and distinctive value of such whole numbers as they are divisible by. Thus, 12 is more distinct than 11 or 13, 16 than 14, 20 than 19. The base of the decimal scale and its products have a factitious prominence in the mind that has nothing to do with their numerical properties.

29. From the example given in § 24, we are led to suspect, that there is something in the precision with which the mind selects the proper associations to assist its enumerating efforts that indicates a greater or less powerful faculty,—a faculty that associates numbers together not from their intrinsic value, but from the exertion of mind that is made preceding their appearance on the field of operations. It is this that prevents the confusion that is apt to take place with figures in the minds of indifferent calculators. The effort that accompanies enumerating or calculating of any kind has a greater or less power of attraction towards the particular impressions acted upon.

30. To proceed further we must have some more distinct idea of what an effort of the mind consists in. What can we suppose to take place in the mental organism when the attention is directed to any particular idea or any train of thought? Other trains evidently slumber during the while. Let us keep in view, that every mental act probably consists in a combination of motor and sensitive influences; that each nervous fibre is capable of being excited in an indefinite number of degrees of intensity, each of which is brought out into an active state by the associated influence of other fibres; that such are the properties of the motor(?) as well as of the sentient elements. (Chap. IV., § 23.)

31. By directing the attention to any idea, the idea is illuminated, and its channels of association are opened by the active condition of all the faculties; and the attention is pulled

along by the preponderating impulse, which exercises a species of attraction on the associated trains that have an excitory adaptation to it. This is perhaps more applicable to the mind in a state of reverie than of active exertion with a distinct object in view. The difference appears to consist in a greater activity in the motor elements, or in that cerebral excitement which immediately precedes muscular contraction, but which is not necessarily followed by it. It is a kind of impulse derived from the passions or active powers. The mental organism appears to elaborate impressions as secreting organs elaborate the blood. After elaboration they may be said to have acquired motor or excitable properties.

32. The enumerating faculty appears to be an organized mode of associating sensitive impressions with motor; and the specific mode appears to confine the associating power to *one direction* only.

33. When the mind is intently occupied, ordinary impressions are not observed; the senses are not awake to them: yet if the attention is set free at the instant any noise, such as the striking of a clock, has ceased, there is not only the consciousness that such a noise has been, but, if the strokes have not exceeded 4 or 5, we may by a little effort identify their number. It is not a consciousness like that which is left by a distinct impression; it is as it were the echo of the sense only. The effort that is successful in identifying their number consists simply in counting one, two, three, etc., attentively. It would seem as if the mind had done the same before automatically, or that the enumerating function had, to the extent of the number of strokes, acquired greater susceptibility, which is *felt* by counting over the numbers attentively.

34. Is each numerical effort, each part of the scale, attached to the same fibres of the brain, or has each a distinct apparatus? What is the trace that is left behind by any mental exertion—any effort of the attention? Does the latter consist of motor excitement, and a concentration of vital force on the particular idea? This concentrating power is more highly developed in proportion to the excitory qualities of the idea. If the same motors are successively excited, how are the different excitements separated from each other in the idea of them that is left

behind? The enumerating organism seems to establish some difference between the successive efforts; because in counting objects the impressions of them may be quite undistinguishable from each other in point of similarity; but the difference in the time of impressions enables the enumerating faculty to separate them, without any assistance from ordinary association.

35. The excitement corresponding to an effort seems to belong to one set of fibres, while the numerical impression belongs to another.

36. The faculty is innately adapted to estimate the value of numbers by their simplest multipliers. Thus, 24 is recognised as four sixes; the unit of value being regarded as 6, or it is recognised as six fours, the unit of value being regarded as 4. With high numbers this manner of reckoning is very obvious. If we pay attention to the mental representation of the numerical scale within ourselves, we will find that rather a distinct impression exists of almost all of the numbers up to 100 *in one line*; that the second hundred forms another line which we can trace, but with less distinctness; that the third is scarcely to be recognised as a distinct line, a few points only being distinguishable; that all the hundreds above this are looked upon as units—they are perceived by the mental eye to be in the unit part of the scale; while the tens and units are seen together in their proper place in the line of the first hundred. Numbers from 1000 to 10,000 are viewed either as units of thousands or tens of hundreds. In making an effort to gain the most precise idea of a high number, such as 5000 or 20,000, we find it indispensable to recur to the value of 1000, this being perceived as ten units of 100 each.

37. The method that has been adopted of expressing any number by a few characters, which have a value increasing in geometrical proportion according to their position, is fully in accordance with the instinctive process by which the mind seeks to assist its enumerating efforts. Thus, 20 is two tens, and 200 is ten times two tens. If the Arabic be compared with the Roman numerals, or with the Greek, we have evidence that the people who originally invented them were the most conversant with and had a greater aptness for arithmetical calculations.

38. The necessity of having symbols adapted to the re-enumerating tendency of the mind, must have been the most strongly felt by the people who had the faculty most highly developed. The Arabians and Hindoos seem to have been particularly distinguished in this respect; and the natives of Hindostan show this at the present day. As logarithms were an invention of necessity among the moderns, to lessen labour when it became so great as to arrest the progress of science —when the mechanical drudgery of calculating checked the aspirations of the mathematician—so does the invention of exhibiting relative value by relative position show the condition of the science of number to be advanced beyond rude times.

39. The natural method of representing numerical value is by the repetition of similar symbols—scoring. Uneducated people will be found to assist their memory in this way when reckoning money. When the number of strokes is more than the eye can easily estimate, it will be found that they either make some new mark or group the lines. The relative denomination of the coins likewise suggests a change in character. The Roman numerals have evidently been originally merely lines grouped.

40. The decimal scale is common to all kinds of numerals, being evidently derived from the habit of counting on the fingers, which is instinctively adopted by many people to lessen the mental effort, especially when summing up impressions that have separate widely-spread associations.

41. The proofs of the decimal scale being derived from the number of fingers, if indeed any proof is required, are, *firstly*, That the scale is general all over the world, however different the system of notation; and, *secondly*, Ten is not more adapted as the base of a numerical scale than any other low number. There is therefore no property belonging to the number ten itself that should give it the universal preference; indeed, if twelve had been originally selected, there is no doubt that numerical operations would be greatly facilitated, inasmuch as twelve is divisible by four whole numbers without a remainder, while ten is only divisible by two. This property affects all the powers of twelve. It would therefore be the means of exhibiting to the eye a multitude of relations that are at

present concealed, or at least not apparent to the eye. The divisors 2, 3, 4, and 6, would be as conspicuous as 2 and 5 are with the present base 10. For such considerations we may acquire a notion of the advantage that might have been expected with the base 12, although the decimal scale is so fixed in the mind, from every act of enumeration being associated with it, that it is extremely difficult to appreciate the advantage or disadvantage of any other.

It is a striking circumstance, that the octave in music naturally divides itself into twelve semitones. This is a direct consequence of the numerical properties of 12, and as such is to be looked upon as an instance unique in the whole range of nature (Chap. V.). The musical quality or tone of a sound bears the same relation to its relative number of vibrations in a given time, as the mental distinctness of the value of a number bears to the properties of the particular number in relation to others.

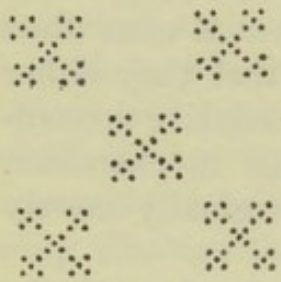

Fig. 14.

42. The assistance that the mind derives from counting on the fingers deserves attention, as typical of its primary mode of action. Each enumerating effort is registered by the position in succession of the finger touched; and the whole number by the number of impressions at once recognised by the eye, and by the number of times all the fingers have been gone over, each time being registered in the mind as one effort. Thus the mind instinctively seeks relief, and finds it in the *hoarding of efforts* (18) in equal clusters, each cluster being retained in the mind as a single effort.

43. The eye finds the same relief in arranging similar objects in symmetrical groups (Fig. 14). The eye views a group as a single impression, and a cluster of such groups as a single impression. Thus, a simple mental effort only is required to perceive that the number of dots in the figure amounts to 5 times 5 times 5. If not so arranged, a much more laborious effort would evidently be required.

44. [The great difficulty in such inquiries is to separate from the mental process the influence due to the ordinary associating principle, and discover the type of specific action which lurks concealed in the midst.]

45. The faculty appears to excite simultaneously a group of perceptions that were originally impressed in succession. The distinctive type of the psychical action of the faculty must thus consist in modifying the ordinary course of association. Impressions subjected to the influence of this organism do not recur consecutively as at their original impression, but strike the attention at once as one perception. They maintain their separate identity, distinct without confusion; but, recurring as they do in a cluster, their separate properties do not recur in a simultaneous manner. We might thus be led to conjecture, that some impulse connected with each impression, and not the impression itself, recurs with others in the grouped form of number, and that it is this impulse which is acted upon by the enumerating faculty. Is this derived from the auditory vocal system?

46. Enumeration cannot be accomplished unless time be allowed for the registering process to take place (11). Now, if we direct our attention to any other process of intellect, we shall find, I think, that a very perceptible lapse of time is required to establish any new association amongst ideas. For the respective fibres to become mutually excitable, they must be for a perceptible time in a state of contemporaneous excitement. The time required for this probably varies considerably according to the faculty employed, according to its power and quality of fibres, and according to the nature of the ideas that are sought to be connected by it. It is therefore a very probable conclusion, that the time that is so obviously required for distinct enumeration, is taken up in establishing an associated connexion of some kind that did not previously exist, not in pursuing an old train.

47. To change a train of association, so that the elements which have originally been excited consecutively become excitable contemporaneously, what is the essential mode of action? In ranging over an object with the eye, a succession of impressions are received that are afterwards recollected as a single and simultaneous perception; the consecutive impressions of touch also lead to the simultaneous perceptions of shape. Hence, *the operation of transforming consecutive excitations to contemporaneous ones must be of very common*

*occurrence.* Indeed, so common, that it will probably be found a universal concomitant of mental action.

48. In the optic system this must be done by means of a superadded organism, which, being awake or excited for a certain time, ties together as it were all the consecutive impressions which have an excitable relation to it. Let us try to obtain a clearer conception of this by reference to the diagram.

Let the fibres *a b*, *m n*, *o p*, etc., be excited one after another, during which time *cd*, which is in connexion with them all, remains in a continuous state of excitement. It is evident, that by the common law of association, an excitement of either of the fibres will excite all the others simultaneously. How may this re-excitability be limited to the preceding impressions only? The excitability of *c d*, the instrument of connexion, may be supposed to take place *in one direction only* (32), as we find in the motor and sensitive fibres of the nerves. Hence, if any fibre, *m n*, is excited, all those to the right of it may be supposed to participate in the action, while all to the left will be as it were in an insulated condition.

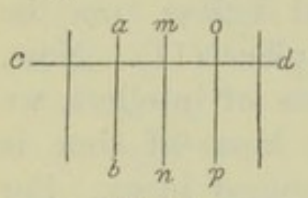

Fig. 15.

49. Some persons can perceive 3 objects without counting, some 4, some 5, very few 6. How is this? Does it not indicate the orginal degree of mental adaptation for numbers which distinguishes some individuals? The organism seems to emanate 3, 4, or 5 simultaneous impulses, which fasten upon the respective impressions. The emanation of 5 such impulses is attended with greater effort than 4 or 3. Any arithmetical effort beyond this is a compound operation of the same faculty that emanates the simultaneous impulses, that is the cause of the distinct perception of a small number of objects without counting. The Roman numerals show this, and IIII. seems the apparent limit of simple numerical perception, being generally represented not as four, but as IV., or one less than V.

50. From whence do we derive the pleasurable feelings connected with making calculations? The greatest satisfaction is generally expressed when the result of one enumerating

process agrees with another; and perhaps the more complicated the process the more the satisfaction. Young expert calculators sometimes perform a computation in two different ways, merely for the pleasure of seeing the results agree, not to check its accuracy, which was otherwise known. Add a column of figures, beginning at the bottom; add them again, beginning at the top: the agreement of the sum in both seems the type of the pleasurable feelings derived from calculating. Two different numerical trains of ideas are manufactured, which, from the results being the same, bring home to the mind in lively colours the harmony of numbers. Those who have a little taste for calculation will probably recollect similar occurrences in their school exercises — perhaps the delight they experienced in exploring the properties of numbers.

51. We may arrive at or acquire a knowledge of every number by different processes of enumeration, and each process may have relations with other numbers. Hence, the mind, in traversing the numerical trains of each, connects both by means of the common result; and more extensive relations are thus recognised. Now, it is by such relations that the mind acquires distinct perceptions of number, and brings large numbers within the grasp of the simplest exertion of the faculty—concentrates a multitude of harmonious relations into a focus; thereby exciting the organism in a proportionate intense degree. The organism thus appears to be excitable in various degrees by the enumerating action, as the retina is by light of various intensity.

52. An algebraic expression necessarily represents the numerical relations of the elements, but does not necessarily express the numerical amount, unless numbers are mixed up in the expression. Nevertheless, the mental activity at work in the operations of algebra is as much of the enumerating class as in those strictly numerical. Individuals gifted with extraordinary arithmetical abilities ought to have equally great algebraic powers. It is said this is not the case. All the results of analysis decidedly point to such a conclusion. Habit may have a certain influence in preventing an equal power being acquired over both kinds of numerical symbols at the same time; but assuredly, if there is any instance of a clever

algebraist being a dull arithmetician in his youth, it will militate against the preceding analysis. A clever arithmetician may not be a clever algebraist, but he must be capable of becoming one. If his superior genius is limited to the enumerating faculty, his efforts will be conspicuous in improving algebraic processes, not in solving physical problems. Euler may be cited as a genius of this description. The discovery of the method of fluxions by Newton, and the differential calculus by Leibnitz, are characteristic of this distinction of mathematical talent. The former was based on ideas of motion or physical change—the latter on strictly numerical considerations.

53. Although quantities may have a distinct relative value, admitting of an indefinite number of particular values, a small change in one of the mutually dependent quantities produces a change in the others, which is not uniform for all the particular values, but is different for each, if the expression involves either the product or powers of the dependent quantities. Hence, the relative proportion of these small changes of value determines the particular values of the mutually dependent quantities. This may be taken as a simple type of the differential calculus. In the integral calculus, knowing the rate at which mutually dependent quantities change their value, their dependent relations become known. The calculus may thus be said to be the science of related quantities in the *act* of changing their numerical values. In this act, or state of transition, they expose new relations, which increase wonderfully the range of the enumerating faculty, but do not appear to involve necessarily the action of any other organism.

54. We proposed the analysis of an organism under three heads (Chap. II.); how far have we advanced in each? The first was the causes external to the organism, that excited it into a state of activity. These appear to consist of perceptions (either impressions or ideas) of several objects that, in so far as they interest us, have a common property. They may be suggested by other organisms.

The second was the structure and function of the organism, keeping in view the physiology of the nervous system. This is the most difficult and important part of the inquiry. I have attempted to discover the type of its simplest action,

and derived it from a specific arrangement of associated excitability. As the complete analysis of any single faculty involves a knowledge of particulars in regard to its organic structure, which is equally applicable to all, and to cerebral action generally, we cannot expect to discover them by studying one only. As we advance, each step may be expected not only to assist further progress, but must throw additional light on what has gone before.

The third head of analysis is the external manifestations. In the case of the enumerating faculty, these do not consist of muscular exhibitions. It manifests its action only while amalgamating with other mental principles; and in combination with them it acts upon external objects.

55. Is there any faculty in preference to others that it connects itself with? If the actions, thoughts, and motives connected with its exercise in the mass of mankind were carefully sifted and appropriated to their true primitive *suggesting* principles, will it not be found that it is in the character of a *hoarding animal* that man has most use for this faculty? It seems to be an essential accessory to the hoarding propensity, although, like the members of the body, it may be at the bidding of all.

## CHAPTER VIII

### ON THE ORGANOLOGY OF DR GALL

Phrenology embraces two distinct branches of study: the first is the configuration of the brain in relation to the innate character of the individual; the second is the analysis of the elements of character to determine the primary faculties and ascertain their modes of action. The first is, in some measure, connected with Lavater's system of physiognomy, in so far as the eyes and forehead form part of the countenance. A very clever person may thus be distinguished from a very dull one. When the countenance is excited with real passion, we are conscious of the state of mind by intuition, and even children and some animals are affected by this sympathy. But to read

off the character from the unexcited countenance must require judgment and experience, if indeed it is possible, except in extreme cases, to do so. This seems to be the weak point of Lavater's doctrines (Chap. VI.).

That we should by intuitive sympathy associate intelligence with a large forehead, is an argument corroborative of the forepart of the brain being the seat of the faculties by means

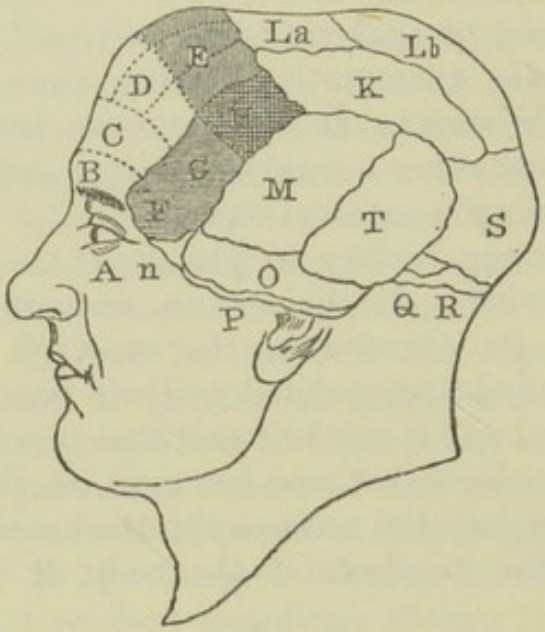


FIG. 16.

A B C D E, Region of the Optic Tangi-motor Faculties.
A n, F G H, Region of the Auditory Vocal-motor Faculties.
E H, Region of Sympathetic Perceptive Faculties and Imitative Impulses.
La, Lb, Region of Gregarious Impulses.
M, Impulse to Hoard.
T, Impulse to Hide.
O, Impulse to Quell.
P, Ingestory Impulses.
K, Region of Sympathetic, Attractive, and Repulsive Impulses.
Q, Impulse to Subdue.
R, The Sexual Impulse.
S, Region of Progenitive Impulses.

of which intelligence is manifested. It is not a direct proof, for the like inference cannot be applied to the rest of the countenance. The direct evidence depends on the workings of a sagacity that few possess. The rare qualification of Dr Gall for establishing his doctrines was, that he could perceive the distinct principles of mental manifestation; by an intuitive analysis he was directed to the primary faculty—

he knew what to look for: without this the comparison of different heads could lead to nothing. Even the proper application of his doctrines requires no small share of a similar endowment; although the general features are so easy of comprehension, and the order of the faculties so simple and harmonious, that we are apt to overlook both the creative effort of genius that has revealed them and the difficulties attending their application.

Phrenology, notwithstanding the damage it has sustained in reputation by the determination of its votaries to support its popular phases, seems to be gradually extinguishing the metaphysics of the old school. It is singular too, and distinguishes it in a remarkable manner from other branches of knowledge, that it owes nothing to the labours of "those great lights of the world by whom the torch of science has been successively seized and transmitted." Indeed, those elegant scholastic writers appear to have been more disposed to controversy and to debate upon abstract nullities, which had gradually acquired a classical interest, than to dispassionate inquiry.

The great and valuable feature of phrenology is the prominence which it has given to the dependence of mental excellence and individual happiness on organization and its laws. We are taught to assimilate our views of the welfare of society with that of the animal tribes. Amongst them we see with what manifold care the degeneracy of the breed has been guarded against by nature; what powerful engines of sudden though not cruel destruction have been sent forth to keep their numbers within the supply of food; and what beneficent instincts to put an end to life when encumbered with disease; all conducing to the great object of maintaining the maximum of enjoyment with the minimum of suffering. How different in civilized society! upon what a mass of misery and disease does it rest: famine, pestilence, and last, but not least, degenerate organization—a cruel subject to contemplate. Let us hope that science will in time solve the great problem of alleviation.

While acknowledging fully the merits of Gall and Spurzheim's doctrines, it may be questioned whether the enumeration and

classification of the primary faculties can be fully determined until their primary functions have been analyzed; and this, it is needless to say, will be a work of time and labour. A great field is thus still open for research, without contravening the general precepts, either as to the nature of the primary faculties, or as to the locality in the brain which has been assigned to them. Under this impression, the following modified classification is proposed. It is needless to enter into the considerations that have led to it. The observations that follow will be sufficiently explanatory: there are many obvious deficiencies; but it is suggested only as an outline, founded upon what precedes, and calculated in some measure to direct the analysis that follows; and which, on its part, may be expected gradually to improve and organize what is here set forth.

In the following classification, the perceptive faculties or specific *associating* functions, arrange themselves into two great divisions: the first organizes impressions of vision, touch, and muscular condition—those senses that make us acquainted with the external world — with the intrinsic qualities of external objects (Chap. IV.); the second organizes auditory and vocal impressions (Chap. V.), following nearly the same type, ascending from the simple perception of the qualities of sound to the qualities of the succession of sounds. These two classes of faculties are as distinct from each other as the senses which supply them with impressions and excite their activity: they are amalgamated together by the great principle of sympathy, which is the bond that connects the intellect with the passions.

Language—the interpreter of feeling and intelligence—is immediately connected with the perceptive faculties, and these by sympathy with the secondary impulses. The secondary impulses are connected with the primary, or the appetites; and these with the different organs of the body to which they minister, and by which they are affected.

Language (A) is symbolical (*a*) (?) and articulate (*b*): the first is related to the faculties of the optic tangi-motor system; the latter to the faculties of the auditory-vocal system.

It may be analyzed as the faculty of associating, and disposition to associate, distinct voluntary efforts of these

systems with all the eliminated products of intellect and sensation. All animals have the means of communicating with each other, which may be defined as a language of natural signs. Amongst insects even it is not wanting. But with man this takes a very artificial form: the workings of passion exhibit themselves in the expression of tone and countenance; but, in addition to this, their action is reflected on the perceptive faculties and those of sympathy combined; ideas are suggested, and these excite the corresponding motor phenomena of language. Thus, considerable power of rhetoric may be exhibited in discussing one class of subjects, and great poverty of language in others; and unusual eloquence is displayed when the passions are excited. In rhyme, poetry, and the modulated succession of words, language is directed by the perception of the harmony of sounds; in the higher specimens of poetry even the ideas appear to have a succession characterized by melody. On this account, perhaps, the organ of Ideality belongs to the auditory-vocal class, as the analogy of its position would lead us to expect.

(B) The perceptive faculties of the optic tangi-motor system include (*Form, Size, Weight, Colour, etc.*). (*a*) The faculty of distinguishing form and relative magnitude, or of associating together the *simultaneous impressions* that constitute the perceptions of distance and relative position. (*b*) Those that constitute the tactile perceptions, and those of weight, *pressure*, or resistance. (*c*) The faculty that notes or registers luminous impressions, in reference to their colour and intensity. These are so simple in their nature as to appear almost a part of the apparatus of sense: (*b*) and (*c*) ought perhaps to come first.

(C) The next set proceed from a combination of the preceding (B), and are mental acts of abstraction of the lowest degree. (*a*) The faculty of associating all the impressions derived from the same object as a single perception. Passive phenomena or appearances are thus registered in the mind. An object, at whatever distance it may be viewed, or in whatever light, is recognised as the same individual, although the optic image may be quite different (*Individuality*). (*b*) The faculty of associating a train of optic tangi-motor perceptions (B) in reference to their locality. Thus, a multitude of impressions

*not* simultaneous are maintained in the relation of position to each other (*Locality*). (*c*) The faculty of associating the impressions of an *active* phenomenon as a single perception (*Eventuality*). (*d*) The faculty of associating together active phenomena with the time of their occurrence (*Time*). The element of time in this is probably derived from the auditory-vocal system.

(D) The next set are mental acts of abstraction of the highest degree. (*a*) The faculty of associating perceptions of active phenomena that are congruous, similar, or analogous in their *mode* of action or *condition* of existence (*Comparison*). (*b*) The faculty of associating perceptions of active phenomena that are *consecutive* in their mode of action (*Causality*). (*c*) The faculty of associating perceptions of active phenomena that are incongruous or contrasted in their mode of action or condition of existence (*Wit*).

These are the highest faculties possessed by man, and distinguish him in an eminent degree from the lower animals. When highly developed, they give an exquisite power of penetrating the motives and character of individuals, and operate upon abstract perceptions of a highly refined nature, of which there are some remarkable examples in the writings of Hume. When they predominate, they give a speculative cast to the mind, which is of various degrees and of various phases, according to the development of the other faculties. They are a necessary ingredient of the talent for mechanical invention, and are of much consequence in the pursuit of experimental philosophy, when combined with mathematical attainments. Newton and Galileo are illustrious examples of this most rare development.

(E) The next class unites (D) with the active instinctive impulses of the second order by means of sympathy, and may be called the sympathetic faculties. (*a*) (*b*) (*c*) The faculties of associating the perceptions of the three modes of action (D) with the expressions of countenance and gesture that are naturally associated in our own persons and in those of others with an active state of the passions, and thus feeling the condition of another as influenced by them. They connect the personal manifestations of others with the consciousness of the

same in ourselves. We thus instinctively personify the conditions and actions of others in our own minds. We sympathize with our fellow-men in every variety of circumstances in which they may be placed.

These, alongst with the faculties included in (H)—the like branch of the auditory-vocal system,—form the great principle of sympathy that connects the perceptive faculties—the instruments of reflection, with the passions—the instruments of action. They give penetration to the character, and are conspicuously manifested in the works of imaginative writers. Shakspeare and Scott are illustrious examples of their excessive development in conjunction with verbal expression. In the works of the former, the higher auditory sympathies have a greater degree of prominence than in those of the latter. Thus, Shakspeare's prose tends almost involuntarily, as it would seem, to rise into poetry, and Scott's poetry sometimes to descend into prose. Shakspeare teems with wit of every phase, from verbal quirks to the most elegant and happy allusions. Scott excels in his power of sympathy of condition. The slightest sketch of a situation or incident is, without an effort, filled up with all the appropriate details of enchanting reality; and in intellectual personation he is probably unrivalled.

(F G H) The faculties of the auditory-vocal system are connected on one side with articulate language, and on the other with the instinctive impulses of the second class.

(F) The faculties that distinguish the several qualities of sound—*intensity, tone, timbre.*

(G) The class corresponding to (C) and (D) of the optic-motor faculties. (*a*) The faculty of associating together, as an individual perception, all the auditory impressions that belong to one sound. (*b*) The faculty of associating a train of auditory impressions in their relation to the time of their occurrence. (*c*) The faculty of associating a train of auditory impressions in their relation to the recurrence, transition, and contrast of tone. This gives the perception of *melody;* and when very active, *suggests* melodious combinations of sounds, or musical composition. This faculty appears to be threefold, and to correspond with (D) of the optic-motor faculties; but this is but a conjecture of analogy.

(H) The faculty of associating the perceptions of (G) class with the expressions of the voice that are naturally associated with an active state of the passions. This is quite analogous with (E) class of the optic-motor faculties; and a considerable development of both are rarely wanting, even in the most commonplace order of minds. The effect of music in exciting the passions acts by means of this sympathetic principle; also, the consciousness we have of the state of the mind from the *tone* of the voice is derived from it. This, alongst with class (E), is common to all animals, even the lowest.

(n) The faculty of associating successive impulses of volition in clusters (*Number*). This is singularly unique in its functions, as much so apparently as *Language*, and has a similarly balanced relation to the auditory and optic systems, but perhaps inclines most to the auditory (Chap. VII., § 45). The least complicated exhibition of its functions is in mental arithmetic. Its higher efforts, both in arithmetic, algebra, and the higher calculus, require the assistance of the class (B) and perhaps (C).

(M) The propensity to *hoard*, to accumulate, to *grasp in the hand*, to *hold* in possession objects of value—objects that pleasurably excite any of the other faculties (*Acquisitiveness*). In this most obvious primary impulse pleasure consists in possession, pain in deprivation, of the object. To analyse the function, we must apply ourselves to examine the essential nature of the relation of the object to the individual when in possession. Does not this consist in the object being within the grasp of the hand? Does not the complete gratification of this impulse take place when the object being within the grasp is seized by the hand, or placed in circumstances that enable us to seize it at any time? If this is the case, it shows that very considerable excitement may attend the appropriation of articles of inconsiderable value. A pickpocket or gambler may thus have as much pleasure in stealing or winning, even to a small amount, as a merchant has in clearing a large sum in the course of his honourable traffic. Hence the excitement of gaming—seductive but degrading. The excitement depends less on the intrinsic value of the sums, than in the *act* of gaining or losing. From the general tendency exhibited by individuals of the lowest grade to gaming, and the prodigious

influence which this hoarding propensity has on the useful activity of the members of society, there is reason to class it almost along with the appetites in regard to its physical importance. It is the cause of the production of capital, which now manifests itself on the large scale as political power of the most dominating influence. We are accustomed to look upon it as a most unamiable feature of individual character; but if we extend our views to its ultimate effects on society, we must alter our opinion, and look upon the miser as a patriot, although an unwitting one.

This appears to be the only one of the passions that is not excited by personal manifestations. There appears to be a close mutual relation between it and the enumerating faculty. Are the voluntary efforts associated in clusters by the latter efforts of grasping by the hand ?

(La) The propensity to follow a leader alongst with the herd —to establish the relation of subserviency between self and an individual who acts upon a multitude (*Veneration*). The excitory influence is derived from the expression of voice, countenance, and gesture—from the zone of sympathy (E H). We are attracted towards the individual of superior power, and seek to hold personal communication with him. Gracious or attractive manifestations on his part excite an ecstasy of devotion in ours, which tends to exalt the character to a pitch of great fortitude and endurance. Haughty or repulsive manifestations excite awe or reverential fear, not retaliation. From the same principle, any extraordinary circumstance or personal manifestation, either of insanity or energy, that excites the attention of a multitude, becomes at once the object of this propensity ; and when intelligence is deficient, the most extraordinary and absurd customs and superstitions prevail: when intelligence abounds, communication is sought with One above all, and the walk through life is made in harmony with and with deference to the institutions of society.

(Lb) The propensity to lead a herd, to direct, to act upon, to command a multitude (*Self-esteem, Firmness*)—the spirit of domination. This prompts to dictation, as the preceding disposes to obedience. Both have analogous relations to society, but they act in different directions. When this predominates

in the character, the individual becomes unfitted for holding a subordinate situation: he is ever disposed to lead, and cannot follow without extreme repugnance. He despises precedent, and is ingenious in finding motives for obedience, independent of the commands of a constituted authority. He is utterly unable to ingratiate himself with his superiors in rank, and is therefore more likely to fall than to rise in society. The deference paid to rank without talent, to sanctity of demeanour without virtue or intelligence, is beheld with excessive disgust; and thus, if of a sensitive disposition, his intercourse with society, where there is difference of rank, is accompanied with a jar to his feelings. If thus constituted, and in dependent circumstances, his situation is an unfortunate one. A servant in whom this prevails is utterly intolerable to his master, whatever may be his valuable qualities in other respects. A nation with this as its characteristic will ever be subject to revolutions under any form of government.

These two elements of character constitute the moral forces that bind the individuals of a society together: thus they enter largely into the events of history. In the writings of Shakspeare and Scott they are brought out with extraordinary power and brilliancy of effect.

There appears to be certain expressions of tone, and countenance, and gesture, that are adapted to these two propensities—that excite and that gratify them; and this they do by their own intrinsic qualities of organic adaptation, and not because they represent any mysterious influence of a higher nature. They are of the earth earthly, form a part of our organization, and their whole scope has reference to a mundane state of existence.

The excitor of the first is derived from the dignity of the object of veneration; and as an individual appears with increased dignity the greater the number of dependants that surround him—the greater the number of his worshippers—the sentiment is excited by the force of a multitude of upturned looks reflected through a single individual. The force is primarily derived from the multitude of individuals, and is in proportion to their number, but is directed not to them, but to the object of their united regard.

The excitor of the second is derived from the presence of individuals, and it is gratified by personal expressions of deference, and by ready obedience, by gestures and attitudes of devotion, which are of the same kind in all nations—bending of the knees, bowing, prostration of the person, amongst uncivilized people; bending, bowing, and prostration of the mind, amongst the more civilized.

They bear some analogy to the opposite polar forces of magnetism. An individual, who by accident or merit is raised to a distinguished place in society, becomes immediately endowed with polarity; a multitude are drawn towards him, like bars of iron susceptible of the magnetic force. The analogy may be extended; for inasmuch as these bars are converted into temporary magnets, so do the individuals patronized by greatness become endowed in a minor degree with the same polarity, and attract around them a minor set of dependants, formed of the same metal.

Some people are less attracted towards power than others; hence they want that polarity which enables an individual to hold a station in society.

These gregarious impulses being most deeply seated in the mental fabric, show the necessity of gradation of rank to the harmony and stability of society. The less abrupt the transition from one rank to another, the more strongly are all knit together, and the less liable to intestine disorder. A despotic and republican government are two extremes equally dangerous. The first is an incubus liable to be thrown off by convulsion, if in weak or unjust hands; the latter is unnatural, and, like an eccentric man, is not to be depended upon by its neighbours where a common interest is at stake.

A due and well-balanced proportion of these propensities is one of the best gifts of nature, if joined with an ordinary degree of intelligence. When joined with extraordinary talents, they ensure the highest success in the profession of the individual; and we have more than one illustrious living example of this. In Napoleon, there seems to have been almost a total want of the first—the spirit of domination overwhelmed every other principle of action; and if he had not by singular accident acquired dominant influence at the very outset of his career,

the Emperor of France might never have risen above a lodging-keeper of Paris. We never find him a courtier. If the Convention had not required his services in their rare predicament with the National Guard, he would probably never have been advanced by any of the ordinary principles that actuate society. Cromwell is another noted example of the same kind of success; his name would never have appeared in history under a prudent monarchy. Thus it is that every revolution of consequence brings forward men of commanding energies, who would never otherwise have been known.

(K) The preceding may be called the two *gregarious* impulses of sympathetic action: the next manifestation of sympathy is in producing an attractive influence between two individuals, leading to friendship and love, which, when perfected by time, establishes a relation of sympathy that renders the joys and sorrows of each mutual.

The manifestation of pleasurable expressions of tone and countenance *directed by the eye* awakens this principle to pleasurable action (*Love of Approbation*), and the contrary expressions to painful action. It thus generates both an attractive and repulsive force between individuals. Its attractive effects are most intensely exhibited between different sexes, and it is thus perhaps to be viewed as subservient to the sexual function. The influence of the eye, in giving excitory force to expressions of tone and countenance which they otherwise do not possess, is a most curious subject of inquiry. The contrast in colour between the pupil and white of the eye enables us to perceive the direction of the look with extraordinary precision; and this is perhaps a reason why a larger proportion of the white is exposed in man than in animals (Chap. VI.).

(O) The propensity to quell animal motion (*Destructiveness*). This is subservient to the appetite of hunger in carnivorous animals. It is excited by the feeling of hunger, also by the manifestations of life and motion in the prey. Flight incites to pursuit. This principle incites to kill, but does not take pleasure in the manifestations of pain; consequently it does not seem liable to be called into action when the spirit of retaliation is roused by injury or insult. This latter spirit is

appeased not by the death, but by the discomfiture of the adversary; it therefore seems more nearly allied to Combativeness.

(P *a*) *Hunger.* This appetite connects the conscious with the unconscious phenomena that are subservient to nutrition. Most of the intellectual faculties of brutes are to enable them to seek their food: their activity must, in a strict point of view, be considered as subservient to nutrition, although they contribute otherwise to the animal's enjoyment.

The study of the widespread relations of this appetite to the mind and to the body connects practical with abstract physiology. But to do justice to it an accurate knowledge of pathology is required.

The feeling of hunger seems to proceed from the stomach and gullet, and is perhaps derived from an excited state of the involuntary motors concerned in the act of swallowing. Nausea is felt in the same parts, but the excitement tends to act in the reverse direction. When excited by a long fast, the manducatory process seems to be exerted involuntarily upon any thing that happens to be in contact with the mouth. In the consummation of this appetite there appears to be a concatenated train of action that involves the arms as well as the manducatory organs. Children manifest this by instinctively putting everything to their mouth as soon as they have the power of grasping with the fingers.

The appetite for food may be considered as threefold, embracing *solid, liquid,* and *aeriform* matter; but the habits of the animal are chiefly formed on the first.

(P *b*) *Thirst* seems to require a cerebral organism distinct from hunger. The object of this appetite is wholly different, and does not vary in different classes of animals. The *act* of drinking too is quite distinct—in some animals being performed by suction, in others by lapping; besides, a special instinct is required to direct the animal to the water. In the process of digestion also it appears to be distinguished from solids. Drunkenness does not appear to proceed from this appetite being in excess. It is similar to the vice of the opium eater. Mental pleasurable excitement is sought, which is the first direct consequence, and is of the same kind as when the

passions of sympathy are acted upon by their natural excitors in a pleasurable manner,—there is a temporary *happiness.*

(P *c*) The *Respiratory Appetite.* Respiration may be called the digestion of aeriform food. The impulse may be viewed as an appetite that recurs much more frequently than hunger, and in the same degree it can be delayed in its gratification a much shorter time. Mental excitement affects the appetites of hunger and thirst; it also affects the respiratory appetite.

The function of respiration, in reference to voice and expression, must be viewed as super-imposed upon its strictly nutritive function. But the tongue and jaws have a like duplex function in connexion with hunger and articulate speech.

(T) The impulse to *Hide.* This includes both *Cautiousness* and *Secretiveness.* Every act of caution seems also to be an act of concealment, or hiding from threatening manifestation. Such is the simplest type of its action; but its effects are modified by the various forms it assumes to adapt itself to different circumstances, and it is assisted in doing so by the perceptive faculties. Extreme panic is accompanied with temporary paralysis; a lesser degree with excessive activity of the locomotive functions. Cunning is a manifestation of this, combined with a selfish motive to gratify.

This is distinguished from all the other principles of the mind: its function is to avoid pain, not to enjoy a specific gratification. Its activity is attended with anxiety and mental distress. It is similar to the skin in being protective: when painfully excited, we exert ourselves to get rid of the pain. Inasmuch as the skin protects all the organs of the body from near and tangible objects, so does this principle protect both mind and body from objects that the former is only conscious of by the exercise of its intelligence. What the skin is to the body, so is this organism to the mind. Nothing shows more forcibly the variety in the mode of action of the cerebral organs, and that we must be prepared to expect as much dissimilarity in the abstract dissection of their functions as, perhaps, in the anatomy of the different organs of digestion.

(Q) The impulse of contention—the disposition to act upon an individual with the natural weapons, so as to overcome,

subdue, triumph—so as to elicit expressions of submission (*Combativeness*).

It appears to be a general law that males contend for the possession of the female: the strongest of the herd drives off the weaker; but when his strength is exhausted, he is driven off in his turn, and thus the health and energy of the offspring are secured. The combat is not deadly, it is a trial of strength: the weaker confesses his weakness and retreats. This principle is excited not only by rivalship for the possession of the female, but for any other gratification whatever. Dogs fight over a bone; they quarrel from jealousy of their masters' attention. So with man there is contention and rivalship in every pursuit; the energies thus become strongly excited to action, which tends to the advantage both of the individual and of society.

The painful affection of any of the other principles of the mind seems to excite this strongly to action. Anger and the spirit of retaliation are manifestations of this kind.

In fighting, (T) and (Q) seem to be the principles immediately active—the latter attacks, the former defends; and the operations of war, to be successful, involve an equal manifestation of both. The success of a general depends as much on caution as courage; and a desperate battle may be hazarded from motives of prudence. Indeed, war as a science is a continual appeal to both principles; it is a system of mining and counter-mining, in which cunning forms the principal ingredient of the manœuvres; but the highest sagacity is needful in the arrangement of details and resources, so as to maintain a moral supremacy.

(R) The *sexual passion* or impulse. This, like *hunger*, connects the body with almost all the faculties of the mind; those especially where mutual sympathy is developed. It communicates the acme of force to all the faculties, and perfection to the organic functions. Unlike any of the other mental principles, it is dormant for a proportion of the first period of existence. But the revolution it makes, in giving a new scope and direction to the active powers, is on this account the more irresistible. The actions and conduct assume a character of daring and independence, and the sympathetic

faculties acquire intense susceptibility. The mind is awakened to the personal graces of the opposite sex, and suggests the most exciting forms. As every living creature knows its kind, the image of the opposite sex must be strongly implanted in the cerebral organization of all living creatures, and excitory influences are conveyed by all the senses (except taste, which is peculiar to hunger). There is a pre-established adaptation to the impressions that proceed from the opposite sex—auditory, odorous, visual, and, lastly, tactile. All are *attractive*, and lead to the act of consummation. Attraction, in this sense, has a singular double meaning—namely, pleasurable sensation, and the direction consequently given to the motion of the individual in reference to the object. Like physical attraction, it becomes stronger as the individuals approach, and continues to increase in intensity; while it directs volition with increasing precision, in circumstances where the intentions of nature admit of no latitude of action. Pleasurable sensations may thus be viewed as the irresistible directing powers of nature, of greater or less intensity according to the importance of the function performed (Chap. IV., § 26). The train of volition is arranged in a duplex form—first securing a mutual state of excitement, and then consummating. The chain of impressions that gratify this passion commences with the interchange of sympathy and intelligence, that raises the highest energies above their ordinary sphere, and terminates in acts that sink to the lowest animal level. Decency, modesty, reserve, in demanding concealment of the act, and influencing the whole social conduct in reference to it, shows how strongly the human character has been framed to support the intention of nature, which, in this case, is simply to ensure conception and secure its fruits.

The dependence of the development of this passion on what are apparently simple glands, is a most remarkable feature, and, considering the importance of the function, shows how much has yet to be discovered of the influence of the nerves on secretion. The minute anatomy of the testes is not different from that of several other secreting organs, and yet what astonishing powers lurk in the minute particle of the secretion which is sufficient for complete impregnation; and the new being that is generated bears the impress of the mental and

physical peculiarities of both parents—even the state of mind and impressions of sense at the instant of consummation are sometimes transmitted. The assimilative powers of every part of the body would seem to be impressed on a single organic particle of the secretion.

A system of organic nerves appears to be as much required by this function as by digestion, and probably exists, connecting it with every part of the organization. Would not the *minute* examination of the nervous systems of mutilated animals lead to results of physiological importance?

(S) The *impulse to nurse* (Philoprogenitiveness). The manifestation of this principle, like almost all of the others, differs in different animals. The exciting stimulus is the infantile aspect of the progeny, and perhaps the smell. The first act after parturition is to bite through the navel cord (?)—the next to give suck. The latter appears to establish a powerful sympathy of protection, by which the mother's impulse of Cautiousness (T) includes her young as its direct object. During the period of lactation, her mind acts under a new impulse, and with a new energy, that modifies the other faculties; the image of the young seems to be continually present to her mind. At other times, it is manifested in a general love for children and young people, and as such is certainly exhibited by males, although it cannot in them be susceptible of the extreme activity that is developed in the female while giving suck. The difference is not to be accounted for by a difference in the size of the organ. It cannot be doubted that the organization of the female brain differs from that of the male in a specific manner throughout, and not merely in the greater or less size of particular organs.

This classification deviates in some points from that which is usually received by phrenologists. Of the perceptive faculties, *Order* is omitted, the function not being considered as primary. Of the others, Language and Number are viewed as peculiar, and belonging to neither of the two great divisions in which the others are classed. These divisions are respectively optic tangi-motor and auditory-vocal, being the two sensi-motor systems, to either of which almost all of the faculties appear

subservient. The arguments in favour of this arrangement may be gathered from the preceding chapters; and the situation of the phrenological organs in some degree gives countenance to it. The distinction is extended to the reflective faculties: those of the auditory-vocal system are supposed to embrace the power of perceiving trains of harmony, and, when strongly developed, the power of suggesting trains of harmony (Chap. V., § 3). The characteristic of genius is an intensity of intellect that is *suggestive;* the lower development is perceptive or imitative. This is the most remarkable in musical composition, but it seems also to characterize the other faculties. In language, the suggestive principle may perhaps be recognised in its gradual improvement and in oratorical effect. In the mathematics it is very conspicuous—the invention of the higher calculus is a brilliant example; and the refined shifts of analytical calculation show a prodigious acquired power of penetrating into the harmonious relations of quantity. In painting, the quality of *effect* seems to be that where the suggestive power is especially required. In relation to facts and events, it is exhibited by the eminent lawyer and man of business in recognising precedent; in the assembling of circumstances that establish a point or question of doubt—that combine to make up an event to render it complete. In relation to the reflective faculties, it suggests relations of dependence, analogy, or contrast between different events—different active phenomena. Hence, the penetration of the philosophical historian, the speculations of the philosopher, the inventions of the mechanist.

But the suggesting characteristic of genius, if analyzed, will probably be resolved into an excessively active condition of perception. It has the remarkable power of advancing society by steps that never retrograde, except by destructive political convulsion. The real discoveries in science and art do not fade like fashions. The productions of the poet and musician are also permanent, if characterized by genius. No false medium or prejudice can prevent this. In them the suggesting extreme of activity must include the sympathies (E H), which stamp the intellectual exertion with expression and personation, and communicate extreme sensibility to the character.

The suggesting excess of the passions seems to be developed in hallucinations of character, peculiarities, eccentricities, which all are more or less subject to, although intellect commonly subdues the exhibition of them. Hence the common practice of building castles in the air — of indulging in visions of wealth, distinction, and social happiness—in false apprehensions, animosities, friendships — in ecstacies of false devotion — in religious enthusiasm.

The appetites, as they are subject to periodic excitement derived from the state of the secretions, cannot be considered in such light, although they are notoriously suggestive of the objects of gratification.

The zone of sympathy (E) (H) includes the Benevolence, Imitation, Marvellousness in part, and Ideality, of Dr Spurzheim's arrangement. In the preceding system, Benevolence is assumed as the sympathy of personal *condition*—Imitation as the sympathy of personal action. Marvellousness, or Wonder, is not considered as a simple faculty, but as a compound exhibition which involves (L a), the impulse to reverence. That part of the zone behind the faculty (D *c*) of perceiving incongruous or contrasted action, is supposed to suggest the contrast of personal condition, to complete the analogy that connects D *a* D *b* with E *a* E *b*. Humorous perceptions are supposed to be derived from this faculty: under the influence of it an individual continually betrays the sense of the ludicrous in his intercourse with society. Images of ludicrous contrast present themselves continually in connexion with the behaviour of others and with his own actions. The faculty exhibits itself under various phases, according to the character of the individual otherwise; but its free and it may be added vulgar scope appears to be in buffoonery and comic action. Falstaff is a bulky personification of it; but the excellence of his wit is not always accordant with the grossness of his character; hence, there is an incongruity that injures the effect of its representation on the stage. Smollett, by describing in detail the expressions of excitement exhibited by his characters when placed in ludicrous situations, contrives to excite the laughter of his readers more perhaps than any other writer.

Ideality (H) is viewed as giving the perception of vocal

expression, being analogous to the preceding (D), which gives the perception of facial expression and that of gesture and attitude. Dr Gall found this part of the brain highly developed in all great poets; but the phrenological definition of it is unusually vague. A great poet must have *all* the faculties highly developed. The harmonious succession of words which characterizes poetry may be due to a faculty depending on the auditory-vocal system; but the inspiration of the poet is a state of general intellectual excitement, in which language and the zone of sympathy predominate in activity. The characteristic of sublimity is addressing or viewing objects and phenomena as beings actuated by human passions. Byron's apostrophe to the ocean may be taken as an example. The mythology of the ancients is a cluster of such poetical fancies; and on this account will ever be an object of classical regard.

It would thus appear that a poet or an orator who can excite the passions by the images he brings forward, ought to have the whole zone of sympathy highly developed. This portion of it (H), to speak in extremes, leads us to give a character of human expression to every sound, as the portion (E) leads us to give a character of expression to every form and motion. But these speculations perhaps carry analogy too far: the natural arrangement and true relations are probably much more complicated.

*Firmness* and *Self-esteem* are viewed as dependent on one principle—*the spirit of domination*—the disposition to act upon others, and the repugnance to being acted upon by them. Pride, obstinacy, and infatuation, are the extreme exhibitions of it; independence, firmness, and moral courage, its proper manifestations. The only means of reversing its tendency to action is by appealing to its antagonist power—Veneration. We recoil from being directed by a person of low degree, but obey cheerfully one who commands our reverence—who has received his polarity, as it were, by standing nearer to the emanating centre of authority.

*Hope* and *Inhabitiveness*, considered as primary principles, are quite unintelligible, and were not originally sanctioned by Dr Gall.

Of the others it is sufficient to observe, that Secretiveness

and Cautiousness are viewed as one principle, and their position is adjacent. Conscientiousness is supposed to be a combination of pride, love of approbation, and cautiousness, which answers both to its assigned position and to its psychical analysis. Adhesiveness is evidently a modification of Love of Approbation, or what may perhaps be more definitely expressed as sympathetic attraction.

Gall and Spurzheim looked upon the medullary part of the brain as the seat of the mental functions; later physiologists are disposed to place them in the cineritious neurine that envelops the cerebral convolutions. If the latter opinion is correct, it will show the necessity of allowing for the uncertainty of development that must arise from the large portion of the cineritious surface that is expanded on the base of the cranium and in the central fissure against the falciform process. Is there any way of determining the relative development of these unexposed parts? It is plain, if this is impossible, that they must occasion a degree of uncertainty in inferring the character from the configuration of the head.

The inferior surface of the cerebrum is probably the seat of the appetites. The experiments of M. Flourens, and the pathological evidence brought forward by Mr Solly in his admirable work on the Brain, must be considered as conclusive against the cerebellum being the seat of the sexual impulse. We must therefore now look for it in the cerebrum—the organ of consciousness; and analogy points to the inferior surface of the posterior lobes.

# II

# GASES AND THE THEORY OF HEAT

# NOTE ON MOLECULARITY

(*From "Thoughts on the Mental Functions,"* 1843.)

Organization is to be viewed, in connexion with the chemical relations of bodies, as a grand exhibition of the capabilities of the elements of matter. While we admire the excessive refinement of structural adaptations, we must not forget that the materials of which they are composed have been fashioned so as to enable them to form part of the wonderful edifice. It is from molecular adaptations that the amazing monuments of creative intelligence have emanated. Assimilation, reproduction, perceptive and motor intelligence, bound together by association—the grand principle of organic attraction—are enclosed in forms which are circles of molecular action, where the divine spirit, the omnipotent principle of adaptation, has been exerted with infinite and perfect display. Arising from the cognizance of the subtle powers of activity that pervade the unfathomable minuteness of atoms, it takes account of the aggregate exhibition of these powers in all their variety and combination; and, ascending to cosmical relations, with one effort of creative intelligence it stamps the smallest organism with the impress of the whole. The members of the animal and vegetable kingdoms have adaptations not only to each other and to the inanimate elements that surround them, but their constitution demonstrates an exact knowledge of the intensity of the force of gravity and of the heat and light of the sun. There are three directions in which we may exert our powers of perception, and from each we are destined to shrink, baffled and confounded at the immensity of the prospect, and humbled in our egotism. These are, firstly, the stupendous magnitudes and distances of the heavenly host—the sublime scale upon which the celestial mechanism has been arranged; next, the inconceivable minuteness, activity, and organized

relations of the elements of matter; and, lastly, the all-penetrating circles of adaptation—the organized foci, which concentrate the relations of all phenomena, so far as these can mutually affect each other. Those very faculties which enable us to have a glimpse of the astonishing scene are but a part of these circles, and endow us with this inestimable privilege, because their proper food—the actions, namely, and motives of our fellow-men—do, as appearances of activity, partake in the same type that distinguishes all other parts of the framework of nature.

We derive from the physiology of the sensi-motor phenomena of the animal system, more perhaps than from any other branch of science, high ideas of the importance and refinement of molecular powers and arrangement. In whatever way we examine the phenomena of nature, molecularity is the grand terminus to which all our investigations lead—to which all efforts and discoveries necessarily tend, and upon which they are now becoming more rapidly concentrated. If we were to contemplate only the extraordinary complication and flexibility of molecular arrangements in organized structures, we might have reason to despair of ever arriving at a knowledge of their essential nature; but, on the other hand, the symmetry of the laws of chemical combination—of the habitudes of elementary substances, with heat, light, and electricity—encourage the hope that all may yet be brought under the dominion of the exact sciences. Although the attempts that have been made in the several departments of molecular philosophy have not as yet been successful in throwing down the barriers that separate them from each other, yet they so far demonstrate that the principle of dynamical sufficiency pervades all, and show the possibility of uniting them in one scheme of mechanism.

Perhaps the most interesting attempt of this kind is that of Mossotti, who derives from the electric theory of Epinus alternations of attractive and repulsive forces which have all the character of molecularity, and leaves a residual attraction that exactly represents gravitation. A medium is supposed to pervade space, consisting of atoms that have an intense mutual repulsion and an intense attraction for the molecules of matter; while these have also a mutual repulsion and a reciprocal

attraction to the atoms. It follows, that each molecule will be surrounded with an atmosphere of the medium, condensed towards its centre; and between adjacent molecules there will be a single attraction by means of the reciprocal atmospheres on the molecules, and a double repulsion from the direct mutual action of the molecules themselves, and indirectly from the mutual action of the atmospheres. At small distances the latter forces greatly exceed the former; but there is a point where they mutually balance, and beyond which the attraction

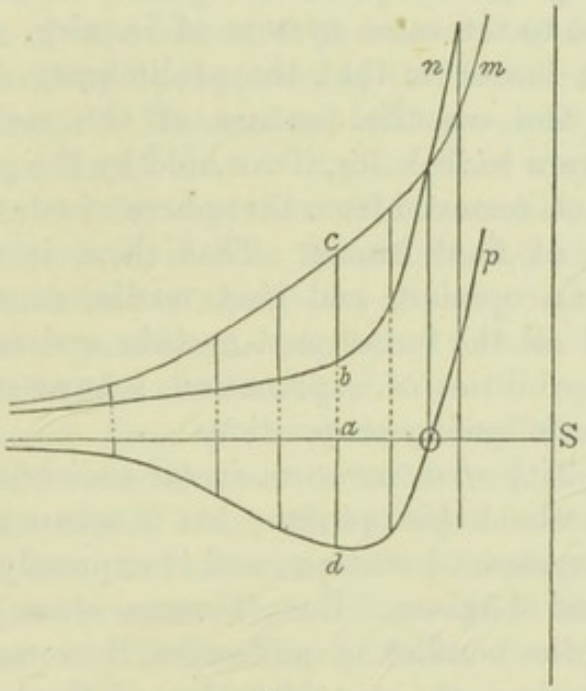


FIG. 1.

preponderates and increases for a short space, and then subsides rapidly. The principle by which the preponderating effects of an attractive and repulsive force issuing from the same centre alternately develop attraction and repulsion, may be conveniently represented by a linear diagram. Let $a\ c$, the ordinate of the curve $c\ m$, represent the attractive force, and $a\ b$ of the curve $b\ n$, the repulsive force simultaneously acting at the distance $a$ S, the effective force is the difference of these, $c\ b$: mark off $a\ d$, equal to it. In this manner, the curve of preponderating force may be constructed which has the molecular type, O being the point of cohesive equilibrium. If we suppose $b\ n$ removed outwards until every part of it is external to $c\ m$,

the curve of preponderance will be wholly repulsive, representing the gaseous state of molecules. Thus all the variety of chemical phenomena might be illustrated. If these curves, and the law of their changes, could be determined from a few simple and invariable principles, then the theory of molecularity would be complete, and there would be no necessity for inquiring into the cause of these powers—we should not require to ascend higher in the chain of causation. The theory of gravitation has accomplished this in astronomy in the most complete manner, and a general expectation is entertained that molecularity will yield to the same system of inquiry. There is good reason to fear, however, that the preliminary data require a knowledge of the essential nature of the molecular forces, which it is in vain to look for, if we hold by the general opinion that it is as much removed from the sphere of our comprehension as the subject of final causes. That there is no ground for holding such an opinion, and that media, considered as the instruments of all the forces that sustain and animate nature, afford great capabilities of explanation, I have endeavoured to maintain in the following propositions:—

1. *The quantity of momentum in the universe is unchangeable.*—Descartes held this opinion; but it seems to have shared the fate of his system of vortices, and is expressly rejected both by Newton and Laplace. But if *momentum* is created or annihilated in the conflict of molecules, it seems evident, that the subject involves the consideration of final causes, which have no place in natural philosophy.

2. *The elements of matter are not immaterial centres of power, but have extension, figure, impenetrability, and inertia.* —It is sufficient proof of this, that matter in the aggregate possesses inertia. It is certainly as absurd to maintain, that a force can exist disconnected with matter, as that motion can exist separate from it. *Matter having velocity*, defines force as well as motion or momentum. There are two other qualities which may be conceived to exist, viz. *unfrangibility of parts that are perfectly solid, and a friction of such a kind that where two surfaces are pressed together by any force, all sliding is prevented.*

3. *Forces that emanate from bodies act by means of an*

*intermedium.*—There are many circumstances in the modern aspect of science that favour this view, and none more so than the undulatory theory of light. The rareness of such media must be excessive, and so must their elasticity. Upon the first quality depends their imperceptible resistance to the planetary motions; on the second depends the rapidity with which they propagate impulses, and the intensity of force with which they may act upon the molecules of bodies. When the exact velocity of light was discovered by the eclipses of Jupiter's satellites, it was considered so prodigious as to be beyond belief; it was easier to think of its emanation as instantaneous—as occupying no time. According to Laplace, if the force of gravity is an emanation, it must be transmitted with a velocity much greater than six million times that of light. This may be considered by some as fatal to the doctrine of intermedia; but, judged by an impartial standard, it does not affect the argument. It is folly to confine nature to the limits of our perceptive powers—one velocity is as possible as any other: there are even proofs of such high velocities close at hand.

4. *The cohesive force of the molecules of many solid bodies is such as would communicate to them a velocity in one second much exceeding* 10,000 *times that of light.*—It has been found that a rod of iron, and of many other kinds of material, will just break with its own weight when about 30,000 feet long. Supposing there are one million of molecules in the length of one inch (the pellicle of gold in water-gilding is not one-millionth of an inch in thickness—Ehrenberg's microscopic researches show that the limit of divisibility must be removed very far beyond this), there will be the weight of 360,000 million of molecules balanced by the cohesive force affecting one; this force must therefore be so much greater than gravity, or it must be capable of communicating to the molecule in one second a velocity of 1920 million miles per second, which is about 10,000 times the velocity of light. Hence, if we take the estimate of the breadth of a molecule one thousand times less, the force of cohesion will be represented by a velocity exceeding six million times that of light. Hence, if this force is derived from media which press molecules together, as the atmosphere presses upon the exhausted receiver of an air-pump,

this velocity will represent that with which the atoms of the medium would rush into a vacuum, if the mass of the molecule were equal to the mass of the medium it encounters, in one second. If less than this, the activity of the medium will be less in the same proportion; but as the molecules are probably much smaller than the millionth of an inch, the mass of a molecule may be much less, and the activity of the medium the same (see 15).

5. *The force of cohesion is upwards of* 360 *million million million million times that of the gravity which emanates from a molecule.*—It will be found, on making the calculation, that if $r$ represents the radius of the earth in feet, $l$ the length of the rod that breaks with its own weight, and $n$ the number of molecules in the length of one foot; then, if unity represents the force of gravitation that emanates from a molecule at the mutual distance of two molecules that cohere, $4n^2lr$ will represent the force of cohesion. This might perhaps be considered as a proof that the gravitating force has its origin of emanation, not at the centre of the molecule, but so far exterior to it as to be nearly at the centre of the one adjacent.

6. *The force of capillary attraction is of the same order of intensity as that of cohesion, if it does not extend in a perceptible degree beyond adjacent molecules.*—The gravity of the raised column is counterbalanced by the attraction of a ring of single molecules at the bottom and top; therefore, when the cohesive force of the fluid remains the same, the quantity of water in the raised column will be proportional to the circumference of the tube, and therefore the height of the column will be proportional to its diameter; and between two parallel plates it will rise as high as in a tube of a diameter equal to twice the distance between the plates.

Taking the rise in a tube of $\frac{1}{100}$ inch bore as four inches, and putting $\frac{1}{M}$ = distance between molecules of water and of glass in parts of an inch, then each molecule of glass supports $\frac{1}{213}$ $M^2$ molecules of water. If $n$ (=180,000) represents in inches the length of a rod of glass that just breaks with its own weight, then $M = 213\ n$ ( = 38 million), or the distance of the molecules is inversely as the length of the rod, if capillary

force is equal to that of cohesion. It would follow from this reasoning, that the cohesive force of fluids is as great as that of solids, the difference of state being occasioned only by a want of *polarity;* and hence some light may be thrown on the subject, by comparing capillary phenomena with moduli of elasticity.

7. *A body may move with a velocity greater than any given velocity, through a medium which may have a density exceeding that of the body in any given ratio, and yet lose in a given time a less than any given proportion of its motion.*—Let D = distance between the centres of two adjacent molecules of which the body is composed; $d$ = diameter of the molecule of the body; then $\frac{d^3}{D^3}$ = density of the body, and $m\ \frac{d^3}{D^3}$ = density of the medium. In the cubic space $D^3$ of the medium there is $m$ times the matter in one molecule. In traversing the distance D, the molecule will encounter the matter contained in a column of the medium, having $d^2$ for its base and D for its altitude, which amounts to $\frac{d^2}{D^2}\ m$ of the matter in the molecule. This, therefore, is that proportion of its velocity which is lost in passing through D: hence the absolute loss of velocity in a given time will be proportional to the square of the velocity. On this hypothesis, the absolute loss of a planet's orbital velocity will be inversely as its distance from the sun, and the effect of the sun in changing the elements of its orbit inversely as the cube of its distance.

Let $v$ represent the velocity of the molecule in miles per second, and $n$ the number of molecules in the length of one inch, then the molecule will lose $72{,}000\ v\,n\,m\,d^2\ \frac{1}{D^2}$ part of its motion every second, and in one hundred years it will lose 227 mill. mill. $\frac{v\,n\,m\,d^2}{D^2}$ part of its motion. If this should amount to one second per year, we shall have 227 mill. mill. $\frac{v\,n\,m\,d^2}{D^2} = \frac{100}{31\text{ million}}$: hence $D = 8500$ million $d\ \sqrt{v\,n\,m}$. Thus, the resistance of a medium, however dense, in proportion to a body that consists of an aggregation of molecules, may

not be perceptible, if the bulk of the molecules is excessively small in comparison to their mutual distance.

8. *The resistance of the medium to a heavy mass, such as a planet will be as apparent as to a light vapour, such as Encke's comet, their velocity being equal;* for $n$ being inversely as D, $\frac{n\,m}{D^2}$ is a constant quantity. This is on the hypothesis that the porosity of matter is so great that the space occupied by the molecules in the body is as nothing to that filled by the medium; hence every molecule will encounter the same resistance, whatever may be its position in the body. If there is a medium that transmits light, according to the undulatory theory, the earth with its orbital velocity, although nearly twenty miles per second, could disturb its equilibrium no more than a globe 500 feet diameter, moving one inch per second, could affect the equilibrium of the atmosphere. And this is supposing the earth to be perfectly solid: if it is extremely porous, the disturbance must be quite imperceptible. This non-disturbance is actually required by the undulatory theory.

Taking the earth's rotatory momentum $= \sqrt{\frac{4}{10}} \times$ velocity at the equator, the *absolute* loss of its orbital velocity is 11,000 times that of its diurnal velocity. An increase of one second in the length of the day corresponds to a diminution of $\frac{1}{86000}$ of its motion, and 11,000 times this is nearly one-eighth, showing that in the time the day takes to lengthen one second, the orbital velocity diminishes one-eighth, supposing the other elements constant; and conversely, during the time that the elements of the earth's orbit take to conform themselves to an absorption of $\frac{1}{31}$ millionth of its force of projection, the length of the day increases only $\frac{1}{34000}$ of a second.

In the *Principia*, sect iv. book ii., Newton has discussed the effects of a resisting medium on planetary motions, and has shown that the orbit becomes a spiral, half the convergence of which is determined by the ratio of the resistance to the centripetal force, and that the diminution of the radius in one revolution to the space traversed in one revolution is in a like proportion: he also shows that the velocity of such spiral motions varies inversely as the square root of the distance. From these theorems, and by comparing together the velocity,

resistance, centripetal force, and period, all expressed in terms of the distance and mass of the central attracting body, we may deduce that the effects of resistance will be much more perceptible in large orbits than in small ones. If the alteration in the period in a given time is expressed in seconds, it will vary in different orbits as the square root of the fifth power of the distance. Thus, while the length of our year diminishes one second, that of Jupiter will become one minute less, that of Saturn five minutes, and that of Uranus half an hour. For equal distances, the *absolute* diminution of the period varies inversely as the square root of the mass of the central body; but the proportional diminution is independent of the mass, and varies simply as the distance. Both the absolute and relative amounts are directly as the density of the medium.

The proportional decrement of distance from this cause varies directly as the velocity of the planet's orbital motion—the *absolute* decrement inversely as the velocity. It might seem at first sight that all the effects should be most perceptible in those planets that move quickest; but it will be remarked, that since the velocity diminishes as the square root of the distance, and the resistance as the square of the velocity, the latter will diminish simply as the distance increases. But the centripetal force diminishes as the *square* of the distance increases; hence the convergence of the spiral is greater the greater the distance, the tangent of spiral convergence being proportional to the distance; and this determines the above-mentioned proportions. Another consequence of the resistance of a medium may be remarked: The elliptic motion of the planets causes an inequality of the velocity during each revolution; hence the resistance will be greater in perihelion; and this leaves a balance of retarding effect in favour of an increase of eccentricity.

In like manner, it may be shown, that the decrements of the period consequent to the absorption of a small proportion of the *vis insita* of a planet increases as the square of the distance, and the absolute decrement of period for the same absolute effect of resistance is as the cube of the distance.

9. *The same quantity of momentum may exist in a body in three modes:*—(1) *Giving it a rectilineal motion;* (2)

*Giving it, if elastic, a motion of vibration between two resilient surfaces; and,* (3) *Giving it a motion of rotation round its centre of inertia.*—Hypothesis. The molecular forces of bodies are derived from media, consisting of atoms perfectly elastic and endowed with momentum, which exists in them in either or all of the preceding modes. That the evolution or absorption of molecular momentum, or change of molecular condition and relations, is caused by a transference of momentum from one mode of existence to another.

10. *A medium of less than any given density may have a greater than any given elasticity; and if the atoms are perfectly elastic, their united vis viva will not diminish by continued impact.*

11. *By the continued impact of the spherical atoms of such a medium, part of their rectilineal momentum will be absorbed and converted into momentum of rotation.*—Billiard-balls are a familiar example. There must be friction (2) between the surfaces of impact, or such inequalities as to admit the momentum of impact to act in a tangential direction. The proportion of the whole rectilineal to the whole rotatory momentum of the medium is probably constant, and might be found perhaps by calculation: it cannot be less than $(\sqrt{2\tfrac{1}{2}}-1)$ to 1.

12. *A medium constituted of elastic spherical atoms that are continually impinging against each other with the same velocity, will exert against a vacuum an elastic force that is proportional to the square of this velocity and to its density. Also, to maintain the same elasticity with atoms of different size, their velocity must be inversely as the mass of the atom.*

13. *Let* $d$ = *mutual distance of atoms* = 1, $v\,d$ = *their velocity per second, and* $s\,d$ = *one inch; then the momentum of elasticity exerted against a square inch of surface in one second is* $v\,s^2$ *times the momentum of an atom.*

14. *The diameter of an atom of the medium is to the diameter of a molecule (considered as a solid sphere) as the mutual distance of the atoms is to a constant proportion of the distance traversed at their velocity, in the time that they would take to communicate such a velocity to the molecule.* —Let $m$ = mass of an atom of the medium, M = mass of a

molecule considered as a solid sphere, and $s^2 d^2$ = area of a great circle of the molecule; then $\frac{s^2 m}{\mathrm{M}}$ = velocity generated in the time an atom takes to traverse the distance $d$, the velocity of an atom being unity. Hence, $\frac{\mathrm{M}}{s^2 m}$ = number of distances $d$ that would be traversed by an atom when the velocity generated is equal to that of an atom. Now, M may be considered as equal to $s^3 m$; therefore $s$ = number of distances $d$ that would be traversed by an atom in the time that the velocity communicated to the molecule is equal to that of an atom. If this time were one second, and the distance corresponded to that (D) which would be communicated by the force of cohesion (4); then, $s = \frac{\mathrm{D}}{d}$, or the diameter of an atom is to the diameter of a molecule as the mutual distance of the atoms is to the distance traversed by them in one second. As this ratio probably far exceeds the truth, to reduce it, it is necessary to suppose that *the cohesive force of molecules is probably derived from only an infinitesimal proportion of the whole elastic force of the medium.*

15. *If any of the forces that emanate from a molecule is directly proportional to its mass, the primary elements of the molecule must be similar, and present a surface to the medium proportional to their mass.*

16. Let $\delta$ = diameter of spherical atom of medium, $n\delta$ = mutual distance of atoms; *then* $\frac{1}{2}n^3\delta$ = *mean distance at which impact takes place.* This may be called the breadth of a film.

17. Let $\delta$ = diameter of atoms, $d$ = their mutual distance; $D$ = *density of medium is proportional to* $\frac{\delta^3}{d^3}$.

18. *If an elastic surface is introduced into the medium* (11), *the atoms by impinging upon it, will lose rotatory momentum and gain rectilineal; consequently their angle of reflexion will be less than that of incidence; hence there will be a deficiency of centripetal elasticity in a direction perpendicular to the surface, and an excess of centrifugal elasticity in directions oblique to the surface.*

19. An indefinitely small change of elasticity of the medium

may in an indefinitely small time produce a motion in a molecule of any given intensity, which may continue for an indefinitely long time before it is absorbed by the resistance of the medium. [Thus, the action of molecular forces may generate an intense vibratory impulse, which would continue for an indefinite time if it were not communicated to surrounding molecules. Hence both heat and light probably consist in vibrations of this kind (4) (7). Chemical combination, considered as a sudden change of molecular equilibrium, must evolve vibratory phenomena.]

20. If a slender, elastic, rigid cylinder is introduced into the medium, and if it is indefinitely longer than the mutual distance of atoms, similar changes of elasticity will be established as in the case of the elastic surface (18).

21. Two such lines will be subject to the influence of mutual attractive and repulsive forces, which tend to bring them into one position of equilibrium with regard to each other *a b*, but with a rectilineal impulse in the direction *a b*. This would be balanced by another line *c*, and *a b c* are in position of equilibrium: *a and c mutually repel each other, but are both impelled towards b.*

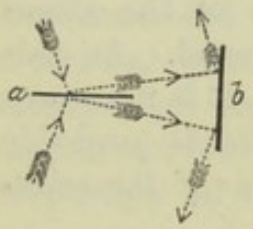

FIG. 2.

22. *A medium constituted of such rigid lines would exhibit a polar arrangement, and the parts would be held together by a force similar to that of cohesion, which does not extend beyond the adjacent lines.*

23. *If a rigid line, indefinitely longer than those of the second medium, were introduced, it would act like b in attracting these lines, and also tend to give them a determinate arrangement, which would render the medium more dense and less elastic in all directions parallel to the line than in those which are perpendicular to it. At each extremity there would also be an expansive impulse given to the medium in the direction of the line centrifugally, and a centripetal deficiency of elasticity perpendicular to it.*

24. *Two such lines, supposed to be moveable round fixed centres like two magnetic needles, would arrange themselves*

*in a direction perpendicular to each other, and thus develop the type of polarity through the instrumentality of the second medium.*

It is of little use pursuing this further, unassisted by mathematics; but there is perhaps enough to show, that *media* present great capabilities of explaining *molecularity*, and in the hands of a mathematician might lead to interesting results. If we viewed the molecules of bodies as consisting of clusters of rigid lines (23), the forces which they eliminate from the media may be sufficient to bind them together in various modes of arrangement, each mode having a corresponding peculiarity in its habitudes with the media, from which may arise its specific molecular powers and chemical qualities.

It is when bodies assume the gaseous state that the most remarkable assimilation of properties becomes manifest. These may be considered under three heads, viz. :—

(1) *Mechanical.* They are perfectly elastic, and at the same temperature their expansive force is inversely proportional to their density. It is singular that this corresponds to the law of the density of media that maintain their elasticity by the continued impact of their molecules on each other (12, 13). Is it by the *vis viva* of its molecules that the atmosphere maintains its elasticity? Newton's theory makes the law of repulsion to follow the inverse of the distance of the molecules, supposing it not to extend beyond the one adjacent. This is exactly the description of a medium constituted as the preceding; for the expansive force is maintained by the number of collisions in a given time, which varies inversely as the cube of the mutual distance of the molecules, or as the density of the gas.

(2) *Their habitudes with heat.* This includes the law of expansion by heat, *a rise of* 1° *Fahrenheit corresponding to an expansion of* $\frac{1}{490}$ *of the volume which it had at* 32°. Thus, an *arithmetical* addition to the temperature causes an *arithmetical* addition to the volume. This fact agrees with the preceding view of the source of gaseous elasticity, if each molecule is supposed to receive an increase in its *vis viva* of $\frac{1}{490}$, that which it had at 32° (13). But this does not account for the evolution of heat by condensation (= 1° *for every reduction of*

*volume equal to* $\frac{8}{3} \times \frac{1}{495}$ *of the volume at* 32°). There must be more heat, more *vis viva* added than $\frac{1}{495}$ part, for a quantity equal to $\frac{3}{8}$ of this part is absorbed in a latent form. The additions to the latent heat, therefore, follow an arithmetical progression also. The next law that seems verified by experiment is, that *the same volume of all simple gases, under the same pressure, require the same absolute quantity of heat to raise them* 1° *in temperature.* Compound gases obey the same rule, or a simple modification of it. This remarkable law follows directly from the hypothesis, that equilibrium of temperature depends on molecules, however different in size, having the same *vis viva* joined to the chemical theory of volumes; which favours the idea that there are the same number of gaseous atoms in the same volume of all simple gases, and probably of all gases whatever.

(3) *Their chemical relations.* These may be embraced by two laws, viz.: (1) *The combining proportions of the simple elements express also their specific gravities in a gaseous state.* In the common table of equivalents there are apparent exceptions to this, which have arisen from water being considered as a combination of one atom oxygen with *one* hydrogen, instead of one oxygen with *two* hydrogen, which gives 16 as the combining proportion of oxygen in the hydrogen scale. This is the proportion adopted by Davy and Berzelius. Surely it ought to be so generally. It does not in the least oppose facts, and is demanded to maintain the symmetry of the chemical relations. (2) *The chemical combination of the simple elements either causes no change in the specific gravity of the compound, or a change that is expressed by a simple fraction.*

In taking a summary view of these relations, the law of volume is a very striking characteristic of them all, and assimilates them in a remarkable manner with the principle of the equilibrium of *vis viva* among elastic molecules of different sizes. There is a difficulty when we come to consider compounds. The harmony of the relations of volume would lead us to assimilate compound molecules in the unity of their composition to simple molecules; and hence to conclude, that there is the same number in the same volume, also the same quantity of heat in equal volumes, and the same specific heat.

It has been remarked, as a principle borne out by experiments made on the vapours of alcohol, ether, and water, that *at the same temperature and pressure the quantities of latent heat in equal volumes are equal.* Now this is precisely what holds with regard to simple gases, and therefore corroborates the latter of these deductions. In order that there should be the same number of molecules in equal volumes of compound gases, it is necessary to suppose that the molecules of some of what are esteemed simple bodies are divisible into two or three parts. If this were admissible (and there is perhaps no fact in chemistry that negatives it), it would certainly introduce great simplicity into the theory of the mechanical and chemical properties of gases.

Any theory of molecularity must represent some mode of action similar to the electric forces, as the modern researches in electro-chemistry show them to be identical with chemical affinity. The organization of molecules must therefore be such as to admit them to exhibit two different forces that have the property of neutralizing each other. The characteristics of these forces are to be learned by observing what conditions attend their evolution or separation—what sustains them in this state—what determines their magnitude, and what attends their return to the quiescent or compound form. Thus, the phenomena of *insulation—conduction—induction and its laws —the conversion of quantity into intensity in the voltaic battery, and quantity being equivalent to the number of molecules—the intensity evolved by chemical action indefinitely less than that which binds the molecules together, and that which is evolved by the electric machine; its quantity equal to the number of molecules disunited, and retaining apparently this molecular relation in its motion through conductors— the exhibition of electricity dependent on the insulating property of the atmosphere—the alienation of affinity by electric power, and the evolution of electricity by preponderating affinities—the calorific effects when the equilibrium is restored.* The only analogy to the laws of elastic media is the principle of the conversion of quantity into intensity. The law of induction is the same as that of all emanating influences, including gravitation, the amount of emanated force in spherical

shells of the medium at different distances being equal. It is singular that this emanation of electric influence does not seem to occasion any loss of electric power: in this respect it is similar to gravitation and cohesion, and dissimilar to heat. Is not the type of such continuous emanation exemplified in the preceding speculation on media (21, 22, 23)? If these hypothetical views of the elastic constitution of the elements of matter are consistent with truth, there is a corollary that follows directly from them which merits notice—viz., that HEAT IS EVOLVED BY GRAVITATION as well as by chemical action. The greater the load a molecule has to bear, the greater will be its temperature; hence the equilibrium of heat in a planetary body requires that the temperature should increase towards the centre, and in the atmosphere that it should diminish as we ascend. A theory of heat might perhaps be built upon this, which would give expressions not only for the temperature at different heights in the atmosphere, but also at different depths under the surface of the ground. It would show that the equilibrium of conduction depends on different laws from that of radiation, and in this way might account for the heat of the sun, and perhaps for its light.

# PHYSICAL CONSTITUTION OF GASEOUS FLUIDS, AND A THEORY OF HEAT

(*From "Thoughts on the Mental Functions,"* 1843.)

Since the preceding remarks on molecularity were sent to be printed, I find that the theory suggested at the end affords simple mathematical expressions for the latent heat, as well as for almost all the other properties of gases that involve heat and pressure. It also appears to be successful when applied to the constitution of the atmosphere, as the formula which is derived from it, for the purpose of measuring heights by the barometer, gives results which agree in a very satisfactory manner with those of the best in common use. I have attempted to give a brief outline of some of its applications in the following theorems, in the hope that it may be found deserving the attention of mathematicians. At the same time I must request their indulgence. Although the mathematics employed belongs to the simpler branches, I am sensible that the results require confirmation.

## SECTION 1.—*On the Circumstances that determine the Equilibrium of a homogeneous Medium, considered* PER SE.

*Definition.* — A medium is supposed to consist of an indefinite number of equal and similar molecules that are perfectly elastic, and are endowed with an independent motion in every possible direction. When confined to a certain volume, they will continually impinge against each other by reason of their *vis insita* alone, and will suffer no loss of collective momentum. The surface of impact is supposed to be spherical, and may either be the actual surface of the solid molecule, or a surface of powerful molecular repulsion surrounding it.

I. The distance traversed by a molecule, after impinging on one and before encountering another, is inversely as the density of the medium.

If all the molecules were arranged equidistant, the number at any distance from a central point will be as the square of that distance, but the intercepting effects of each of that number is as the inverse of the square of the distance; hence the intercepting effects are uniform, and accumulate as the distance increases. Let $d$ = diameter of molecular surface of impact; then *as* the distance $(x\,d)$, beyond which a molecule cannot go without impinging on another, *is to* the mean distance of the molecules $(n\,d)$, *so is* the number of circles $(n^2)$ (whose common diameter is equal to that of the molecular surface of impact) that can be inscribed in an equilateral triangle, the side of which is equal to the mean distance of the molecules *to* unity. Hence, $x\,d \div n^3\,d$, and $x \div n^3 \div$ to the inverse of the number of molecules contained in a given volume. [Upon the accuracy of this reasoning depends almost all that follows.]

II. The density of the medium being constant, the impinging distance will vary reciprocally as the square of the diameter of the molecular surface of impact.

For $n\,d$ being constant, $n \div \frac{1}{d}$, and $x\,d \div n^3\,d \div \frac{1}{d^2}$.

III. The impinging distance being constant, the density will vary directly as the diameter of the molecular surface of impact.

For $x\,d$ being constant, $x = \frac{1}{d} = n^3$, hence $d = \frac{1}{n^3}$ = density.

IV. Let there be two elastic parallel immovable planes, and between them, in a common perpendicular, let two molecules that have unequal velocities continually impinge, then shall the sum of the squares of their velocities be always equal, and the velocities will continually approach the ratio of equality, the common velocity being then equal to the square root of half the sum of the squares of the original velocities before impact.

This follows from the well-known laws of elastic collision.

V. In a given time the number of impacts against a constant surface is proportional to the number that takes place in a constant volume.

For the mean velocity being constant, the number that takes place in a constant volume is as the number of molecules in that volume, or as the impinging distance (I.), or *as the density*. Now, conceive the molecules equidistant, opposite a given surface in one case where the mean distance is 1, and in the other 2. The number opposite the surface in the latter case is evidently one-fourth that of the former; but the same proportional number of these will impinge in times proportional to their mean distance from the surface, hence in double the time one-fourth of the number will impinge; the total effect in a constant time is therefore one-eighth, or *as the density*.

VI. The mean velocity of the molecules being constant, the elasticity of the medium is proportional to its density.

This is a corollary of the preceding, the elastic force being evidently proportional to the number of impacts against a constant surface in a constant time.

VII. The density being constant, the elasticity is proportional to the square of the mean velocity of the molecules.

Because the number of impacts in a constant time increases with the velocity, and also the force of each impact.

VIII. If the elasticity is constant, the density will be inversely as the square of the velocity.

This is a corollary of the preceding, the elasticity being composed of the product of the density and square of the velocity.

[In the preceding (VI. and VIII.), the medium is shown to have the two great distinguishing properties of elastic fluids, if we take the *square of the velocity* of the molecules to represent *temperature*. The elasticity increases as the density, the temperature being constant; and the volume increases as the temperature, the elasticity being constant. The formula derived from observation is $e = t\,d$; that from *a priori* reasoning, applied to a medium consisting of perfectly elastic molecules, is $e = v^2 d$; hence the absolute value of $t$ or $v^2$ would be

determined by the rate at which air expands by an increase of temperature. Adopting the results of Professor Rudberg's experiments, the absolute zero would be $-461°$ on Fahrenheit's scale. It may be remarked from (IV.) that the transference of *vis viva*, and establishing an equilibrium, are quite consistent with this theory of heat.]

SECTION 2.—*On the Circumstances that determine the Equilibrium of two Media that are distinguished from each other by the Weight of their respective Molecules.*

IX. If the centre of gravity of molecules that differ from each other in weight is stationary while they approach and impinge, they will after impact retire without change of velocity.

X. Let there be two elastic parallel immovable planes, and between them, in a common perpendicular, let two molecules impinge with velocities inversely as their weights, then will they continue to vibrate between the adjacent plane and molecule without change of velocity.

XI. If the velocity of such molecules is not inversely as their weights, they will by continually impinging tend to become so.

XII. Two media, whose molecules differ in size and have different velocities, will, if mixed together, interchange *vis viva* until the mean velocity of each is inversely as its weight.

These four propositions are derived from the laws of elastic collision.

XIII. If two media are in equilibrio of *vis viva* and have elasticities that are equal, it is proposed to find the ratio between the weight of the molecules and their surfaces of impact, when the same number of each occupy the same volume.

Let $e$ = elasticity of a medium, $w$ = weight of its molecules, $v$ = their velocity, $n$ = number of them in a constant volume, $i$ = their impinging distance, $d$ = diameter of molecular surface of impact.

If the impinging distance were constant, the elastic force would be (as shown in Sect. 1) $e = w\ v^2\ n = \frac{n}{w}$ (XII.), but if it varies, the elasticity will vary as its reciprocal (VI.) or $e = \frac{n}{i\,w}$; but $i = (x\,d) = \frac{1}{d^2}$ (II.), therefore $e = \frac{n\,d^2}{w}$; and if both $n$ and $e$ are constant, then $w = d^2$; therefore if the impinging surfaces of molecules are proportional to their weight, and the media are in equilibrio of pressure and *vis viva*, then will equal volumes of each contain the same number of molecules.

XIV. If the impinging surfaces of molecules are, as their weights and media are, in equilibrio of pressure and *vis viva*, then shall the specific gravity of such media be proportional to the weight of their respective molecules, and inversely as their velocity.

This is a corollary of the preceding.

XV. If the impinging surfaces of molecules are *equal*, and media are in equilibrio of pressure and *vis viva*, then shall the specific gravity of each be proportional to the square root of the cube of its molecular weight.

For according to the preceding (XIII.) $e = \frac{n}{i\,w}$; but as the impinging surfaces are supposed equal $n = \frac{1}{i}$, hence $e = \frac{n^2}{w}$, and $e$ being constant, $n^2 \div w$ and $n\,w \div w^{\frac{3}{2}} \div$ specific gravity.

XVI. If two media are in equilibrio of pressure and *vis viva*, and have the impinging surfaces proportional to the weight of the molecules, then, if placed in contact, they will become equally diffused through their common volume without any change of elasticity.

For the smaller molecules will impinge more frequently than they did before, and the larger less frequently, the impinging distance being augmented in one case and diminished in the other. Now, as the impinging distance in each medium *per se* is proportional to the velocity (XIII.), and the same number of impacts in the same volume of both media take place in the same time, the increment of effect associated with

the smaller molecules must evidently be equal to the decrement associated with the larger, whatever may be the proportions of the mixture.

[This permanence in the elasticity of mixed gases could not subsist if the atomic weights and impinging surfaces bore any other than the stated proportion to each other. Suppose a case where equal volumes of two gases are mixed, their atomic weight being as $n$ to 1, and their impinging surfaces equal, then shall the elasticity of the mixture be to the elasticity of the separate gases as $\frac{2+\sqrt{n}+\sqrt{\frac{1}{n}}}{4}$ to 1.]

XVII. If two media are in equilibrio of pressure and *vis viva*, and have their impinging surfaces proportional to the weight of their molecules, then in the same volume of each there exists the same *vis insita*.

XVIII. The absolute increment of the *vis insita* of a medium being constant, the corresponding increment of mechanical effect is proportional to the *vis insita;* and conversely, the increment of mechanical effect being constant, the corresponding increment of *vis insita* is inversely as the *vis insita*.

XIX. If the intensity of the force of molecular repulsion increases as the weight of the molecule, and varies inversely as the square of the distance, and distances are taken from the centres of such molecules proportional to the square root of their weight, then shall the repulsive forces at these distances be equal.

It will be remarked, that XII., XIII., XIV., XVII. represent those elegant properties of gases which the brilliant researches of MM. Gay-Lussac, Dulong, and Petit have revealed. According to XII., all gaseous molecules when in equilibrium of temperature have the same quantity of heat or *vis insita*, whatever may be their inequality of weight; and the same increment of *vis insita* or heat causes the same increment of temperature at the same part of the scale in all molecules, and the velocity of each is inversely as its weight; but the same increment to the velocity in the same molecule causes a thermal or mechanical effect which is proportional to

that velocity (XVIII.). Thus it is that the theory represents the velocity corresponding to the highest point of Wedgewood's scale, or 33,000°, as only about seven times that at the temperature of boiling water.

In these inquiries it is necessary to distinguish carefully between the *vis insita* of the molecule, which represents the absolute *quantity of caloric* associated with it, and which increases simply as the velocity, and the thermal or mechanical effect of this *vis insita*, which increases as the square of the velocity (VII.). Thus, specific heat is the relative velocity of the molecules when in equilibrio of temperature, absolute caloric is the absolute *vis insita* of the molecule, and temperature is the mechanical expansive effect of the *vis insita*, and varies in its duplicate ratio.

There are some experiments of Mr Dalton, on the relative value of degrees of temperature in water at different parts of the scale between 32° and 212°, which countenance this theory in a remarkable manner (XVIII.). Taking the absolute zero as before (§ 1) at −461°, the absolute temperature of boiling water is 673°, and at melting ice 493°; and the velocities at these temperatures are respectively as 258 to 222. One degree, therefore, at 493 degrees ought to be equal to $\frac{258}{222}$ degrees at 673°. This gives results that are about one-fifth or one-fourth less than those of Mr Dalton's experiments; and the discrepancy is nearly what is to be expected from the change of volume of the water while giving out its heat to the ice, according to the theory of latent heat given in the next section.

The effects of change of volume always disguise the absolute value of the heat of temperature, and fully account for the discordant results of experiments made to determine the specific heat of bodies.

From the combining ratios of gases in respect to weight and volume, it has been remarked, that the specific gravity in many instances denotes also the atomic weights, or that the same number of molecules in many gases occupy the same volume when in equilibrio of temperature and pressure. In XIII. and XIV. it is shown, that if this is a general law, the theory requires that the spherical surfaces where the reflexion of impact takes place should increase as the weight of the

molecule. This seems to bear on the subject of molecular forces (XIX.).

It might be expected that the theory should exhibit some harmonious relations between the temperature and pressure of vapours. On trying this by means of curves, I find that if the velocity or square root of the temperature is marked off on a line of abscissæ, and the mutual distance of the molecules of steam (= *sixth* root of the density, and the density equal to the quotient of the pressure by the absolute temperature) are taken as ordinates, the line that joins their extremities is almost straight above 140°, the trifling divergence being sometimes on one side and sometimes on the other. This line meets the axis at a point that corresponds to a temperature of about −68°. It affords a steam formula of the following kind: $\sqrt{t} - \sqrt{a^2} = b\sqrt{\delta} = c\left(\frac{p}{t}\right)^{\frac{1}{6}}$, where $a^2$ is the absolute temperature at −68°, the zero of pressure. Hence we have $\left(\frac{\sqrt{t+461} - 19{\cdot}7}{10{\cdot}4827}\right)^6 . (t + 461)$ = pressure in inches of mercury, $t$ being the temperature of the steam in contact with water on Fahrenheit's scale. This will be found to agree very closely with the results of Dr Ure's experiments from 140° to 312.° But to make it correspond with those of MM. Dulong and Arago, the constant 19·8 must be altered to 19·36. If this were the true theory of steam, we might apply XXV., and conclude that the velocity of the molecules of vapour is less than that of the molecules of water by a constant amount.

*Note.*—This rule is applicable also to the vapours of *alcohol*, *ether*, *oil of turpentine*, and *petroleum*. If absolute temperatures are taken whose square roots are in arithmetical progression, the sixth root of the quotient of the elastic force, divided by the absolute temperature, shall also be in arithmetical progression. If this function is laid down in a linear way from Dr Ure's tables of elasticity, no divergence from the straight line is perceptible. The formula for ether is $\left(\frac{\sqrt{t+461} - 16{\cdot}62}{11{\cdot}70}\right)^6 . (t + 461)$ = pressure in inches of mercury, and for alcohol it is $\left(\frac{\sqrt{t+461} - 19{\cdot}16}{10{\cdot}01}\right)^6 . (t + 461)$ = pressure

in inches of mercury. Are we entitled to conclude that the function represents the true theory of vapours? If so, it is evident that experiments made at two different temperatures would afford the means of expressing its force and density at all other temperatures. Those gases that have been liquefied by strong pressure might perhaps be subjected to the same mode of analysis. A chart of such lines would be useful perhaps in bringing into view relations that connect together the vapours of different bodies. More may be expected from treating the vapours of simple bodies in this way than those of compounds. Any line drawn parallel to the axis cuts the lines of density of the several vapours, and points out the temperature at which a constant volume contains the same number of molecules. Other properties of this kind become apparent on drawing the chart.

The dynamics of the function would be satisfied if the molecules were supposed, as they escape from their fluid cohesion, to lose that portion of their velocity represented by the second term of the dividend, and form an atmosphere over the liquid, the density of which increases with the velocity, according to Proposition XXV.; but the principles that determine the equilibrium of such a singular condition do not appear. It is remarkable, that if the latent heat of the above-mentioned vapours is calculated on this hypothesis, the results, although they do not numerically coincide with the experimental results of Dr Ure, do yet maintain a like proportion among themselves.

### Section 3.—*On the Effects of a Change of Volume on the Condition of a Medium.*

XX. If a molecule vibrates with a given velocity between two parallel elastic planes that slowly *recede* from each other, for a small proportion of their distance, it will *lose* a part of its velocity which is proportional to half the increment of the distance of the planes; and *vice versa*, if they *approach*, the molecule will *gain* a similar increment of velocity.

Let $v$ = velocity of the molecule, $d$ = distance between the planes, $\gamma$ = velocity with which the planes recede, $\delta$ = increment

of distance. At each impact the molecule will evidently communicate to the plane $\frac{\gamma}{v}$ part of its momentum. In the time that the planes pass over the distance $\delta$, the molecules will pass over $\frac{v}{\gamma}\delta$; and the quotient of this by twice the distance $d$, or $\frac{v}{\gamma}\cdot\frac{\delta}{2d}$, equals the number of vibrations; and the product of this by $\frac{\gamma}{v}$ gives the proportion of the velocity which is lost $=\frac{\delta}{2d}$.

XXI. The increment or decrement of a constant velocity, corresponding to the same absolute decrement or increment of distance between the planes, is inversely as the distance.

This is a corollary of the preceding.

XXII. The small increment or decrement of the distance being supposed a constant absolute quantity, the absolute value of the increment or decrement of velocity is directly as the velocity, and inversely as the distance.

This is also a corollary of the preceding.

XXIII. If a medium expands under a constant pressure, and experiences a small increment of volume, its molecules require four increments of the square of the velocity to effect this expansion: one is given out to external objects in the act of expansion—the remaining three maintain the increased volume with the original elasticity; and *vice versa* in the case of a small condensation.

Suppose the medium to be contained in a cubical receptacle: let the linear dimension of the cube be increased from $d$ to $d+\delta$ and cubing, the corresponding increment of volume will be $3d^2\delta$ (the two last terms of the cube being omitted because of the relative minuteness of $\delta$); or, expressed in terms of $d^3$, it is $3\frac{\delta}{d}$. This also expresses the increment of the square of the mean velocity (VIII.), or $3\frac{\delta}{d}=2\frac{u}{v}$, the increment of the velocity being denoted by $u$.

Now, let the collisive force of all the impacts that are going on amongst the molecules be separated into the three rectangular directions perpendicular to the sides of the cube, it is evident that the extension in each direction is $\frac{\delta}{d}$, and this affects only the force in that direction; hence the whole collisive force will be affected by the linear extension $\frac{\delta}{d}$ only; and applying (XX.), the loss or gain of velocity will be $\frac{\delta}{2d} v = u^1$; and comparing this with the value of $u$ in the preceding equation, we have $u = 3\,u^1$.

XXIV. If the volume of a medium suffers an infinitely small decrement by compression (not by diminishing the *vis insita* of the molecules), the square of the velocity of the molecules will acquire an increment equal to one-third the apparent velocity that would have to be abstracted from them, to diminish the volume to the same extent under a constant pressure.

This is a corollary to the preceding.

XXV. If the volume of a medium is suddenly changed, the square of the mean velocity of the molecules before the change is to the square of the mean velocity after, in the inverse proportion of the mean distance of the molecules, or as the cube root of the density.

In XXIII. it may be observed that the differential of the velocity is to the velocity as the differential of the distance is to twice the distance; hence by integration (keeping in mind that the differential of the distance is negative) we have $v^2 = \frac{1}{d}$.

XXVI. If a medium changes its volume without communicating to or receiving from surrounding bodies any *vis viva*, its elasticity will vary inversely as the fourth power of the mean distance of its molecules.

This is a corollary of the preceding and VI., VII.

XXVII. If the distance of the molecules of a medium is

increased or diminished, and the *vis viva* that they gain or lose is continually withdrawn or supplied, so as to maintain a constant mean velocity during the change, then shall twice the sum of the increments or decrements of velocity be to the original velocity as the hyperbolic logarithm of the proportionate change of distance is to unity.

This is a corollary of XXIII.

The principle on which the theory accounts for the latent heat of gases is given in its simplest form in XX. The gain or loss of *vis viva* during a change of volume is a necessary consequence of the perfect elasticity of the elements of matter, and probably affects solids and liquids as much as gases. The theory represents that which is usually spoken of as an absorption of heat, as a giving away of *vis viva;* and *vice versa*, that as an absorption of *vis viva* which appears as an evolution or expression of caloric. In the first case it is the appearance of the force of compression which passes *into* the molecules; in the latter it is the disappearance of the force of dilatation which passes *out* of the molecules.

In XXIII. the amount of the mechanical effect of this increment of velocity is shown to be equal to half the increment of the distance of the molecules, and to be one-third of the mechanical effect of the decrement of velocity required to produce the same decrement of volume under a constant pressure. According to the experiments of M. Gay Lussac, the heat extricated from air when it undergoes a small condensation is equal to $\frac{3}{8}$ of the diminution of temperature required to produce the same condensation, the pressure being constant. This countenances the theory; for although $\frac{3}{8}$ is $\frac{1}{24}$ greater than $\frac{1}{3}$, yet as $\frac{1}{3}$ is the nascent ratio, which is true only for an infinitely small decrement of volume, experiment must give results a little greater in amount, as the third term of the cube of the binomial $(d+\delta)$ will have a perceptible value.

But XXV. determines the increment of temperature (or square of the velocity) for any amount of compression; and employing this formula, it appears that if the compression in the experiment amounted to about one-fourteenth, the above ratio would be $\frac{3}{8}$. This affords, perhaps, the means of testing

the theory, for we can find by means of it the rise of temperature corresponding to any amount of compression. Taking, as before, 493° as the absolute temperature of melting ice, if air is suddenly compressed to $\frac{1}{8}$ of its volume, it ought to rise to 525° from 32°, or to 581° from 60°; if to one-half its volume, it ought to rise to 196° from 60°; if to eight times its volume, it ought to fall to − 200° from 60°.

The law of elasticity that would be obeyed, if the equilibrium of temperature were not restored, is given in XXVI. When restored, it follows Mariotte's law.

Prop. XXVII. is given in the expectation of its being applicable in determining the amount of the latent heat of vapours, but the results are not satisfactory. According to the theory, it is important in determining latent heats to specify the part of the scale to which the degrees refer. Thus, Dr Ure has determined the latent heat of steam to be 1000 degrees of such as are found at 45° Fahrenheit. This, according to theory, is 1153 degrees, such as we find at 212°.

In weighing the results of such experiments, with the view of discovering their law, we must consider also the probability of a portion of the heat given to the water by the steam becoming latent—that is, being expended during the liquid's increase of volume. If XXIII. is applicable to liquids, the heat or force that disappears in this way must be exhausted in antagonizing the molecular forces, not in acting upon surrounding objects.

If chemical action consists in the cohesion and separation of molecules, it is easy to account for the evolution of heat and its absorption. Thus, if two free molecules combine to form one, their united *vis insita* is double that which is necessary to maintain the equilibrium of *vis viva* or temperature; they will therefore communicate a portion to surrounding objects—they will become hot, and we may calculate their temperature (XI.). Suppose two molecules at 60° Fahrenheit or 521° absolute, combine; their *vis insita* will now be double what is necessary for equilibrium — their temperature will therefore rise to four times 521°, or 2084° absolute, or 1563° Fahrenheit. If three molecules unite, the temperature will rise to 4168°; if seven molecules, it will rise to 25,000°. In

rapid combustion, it is probable that the surplus from molecules that have combined is received by those that are about to combine, and increases their effects. The apparent rise of temperature may therefore be greatly increased, but the absolute effect in melting ice or boiling water must be constant. The reduction of volume in chemical combination will also be the means of affording heat.

SECTION 4.—*On the Equilibrium of an Atmosphere consisting of free homogeneous Molecules affected by Gravity.*

XXVIII. If the molecules in the lowest stratum of an atmosphere are maintained at a constant velocity, the density of the stratum will be such, that if the atmosphere had this density throughout, a body in falling through three times its height will acquire a velocity equal to that of the molecules.

Suppose a column of the atmosphere to be divided into an infinite number of strata, the thickness of each being inversely as the density $= \frac{1}{d}$. Now, the number of molecules in each stratum being constant, and the density representing the impinging distance (I.), the same number of impacts will take place in the time a molecule takes to pass through a stratum at all heights, and the absolute number of units of repulsion will be equal to half the number of impacts; for two impacts must be taken to represent a unit of elasticity, when comparing such a force with the continuous action of gravity; hence $t = \frac{2}{v\,d}$, and the number that takes place in a constant time $= n = \frac{v\,d}{2}$; and the force of each impact being as $v$, we have the elastic force $= \frac{1}{2}\,v^2\,d$.

Now it is evident that the part of this force (one-third) that acts in a vertical direction is that only which counterbalances the force of gravity on the superincumbent molecules; therefore, gravity acting during the constant time on the superincumbent molecules, will generate in that time a force equal to one-third the elastic force. Let $z$ = number of molecules in

the superincumbent column, and $\gamma$ = force communicated by gravity to each molecule in the constant time, then $z\gamma = \frac{1}{6} v^2 d$, or $v = \sqrt{6\gamma\frac{z}{d}}$.

XXIX.—In an elementary stratum of an atmosphere, the increment of the collective *vis insita* of the molecules in the stratum, due to gravity, is equal to one-half that which they would receive if the gravitating force acted in the line of their motion.

Let the vertical line $ck$ represent the thickness of the stratum, also the *vis insita* of a molecule; $km$ the increment of velocity due to the *direct* action of gravity; mark off $ca = km$; from the centre $c$, with the radii $ck$, $cm$, describe the concentric quadrants $khd$, $mge$; and from the centre $a$, with the distance $am$, describe the quadrant $mnb$. Draw any radius $chg$, and from $h$ draw $hn$, parallel to $ck$ and equal to $km$, join $nc$. Now it is evident that the locus of $n$ is the quadrant $bnm$, and that the increment of $ch$, due to gravity, is not $gh$ but $np$. Now the locus of $np$ is the area $bopkmn$, which, when nascent, is equal to $cdba$ or to Rad. × increment of velocity $km$, and the locus of $gh$ is the quadrant × $km$. Hence the nascent ratio of area described by $np$, to area described by $gh$, is as the quadrant to its radius. Let the quadrant revolve upon $ck$ as an axis, it will evidently describe a hemisphere, while the radius describes a great circle; and the ratio of the surfaces thus described is as two to one. Now it is clear, that as the motion of the molecules takes place equally in all directions, the hemispherical surface represents the collective increment of velocity, if gravity acted in the line of motion in every instance; and the great circle represents the collective increment when acting vertically.

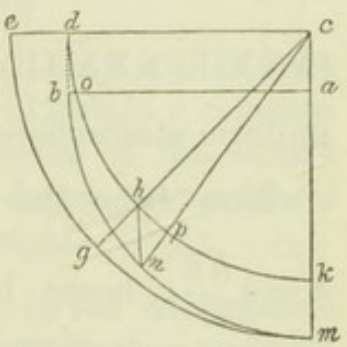

Fig. 1.

XXX. The total height of the atmosphere above any stratum is equal to six times its height, if it had the uniform density of the stratum.

The time that a molecule takes to pass through a stratum being $\frac{\dot{z}}{v\,d}$ (XXVIII.), the collective increment of *vis insita* in the stratum would be $\frac{\dot{z}}{v\,d}\gamma$, if all the molecules were influenced by gravity in the same degree as if they moved in a vertical direction; but it was shown (XXIX.) that, because their motions took place in all directions alike, the true increment will only be one-half of the above, or $\dot{v} = \frac{1}{2}\frac{\dot{z}}{v\,d}\gamma$ and $\frac{\dot{z}}{d} = 2\,v\,\dot{v}\,\frac{1}{\gamma}$. Supposing the atmosphere to have a finite height, and reckoning from the summit downwards, it is evident that the increment of depth is equal to the quotient of the increment of the pressure by the density; hence $\dot{h} = \frac{\dot{z}}{d} = 2\,v\,\dot{v}\,\frac{1}{\gamma}$ and $h\,\gamma = v^2$ (XXXII., XXXIII.) $= 6\,\gamma\,\frac{z}{d}$ (XXVIII.); therefore $\dot{h} = 6\,\frac{\dot{z}}{d} = 6$ uniform atmospheres.

Now from the last equation $d = 6\,\frac{z}{h}$, and because $\dot{h} = \frac{\dot{z}}{d}$, $\dot{h} = \frac{1}{6}\frac{h\,\dot{z}}{z}$; hence by integration, $h = (6\,z)^{\frac{1}{6}}$ (XXXI.) and $d = (6\,z)^{\frac{5}{6}} = h^5$ (XXXIV.).

XXXI. The depth of a stratum below the summit of the atmosphere is proportional to the sixth root of the pressure.

XXXII. The square of the velocity of the molecules in any stratum is proportional to the depth of that stratum below the top of the atmosphere.

XXXIII. The height of the atmosphere above any stratum is proportional to the square of the velocity of the molecules in that stratum; it is determined by the velocity alone, and is not affected by a change of density or pressure.

XXXIV. The density is proportional to the fifth power of the sixth root of the pressure, and to the fifth power of the depth below the summit.

XXXV. If the velocity of the molecules were maintained uniform throughout the atmosphere, and the effects of gravity

not transmitted by collision, the height of any stratum shall be proportional to the logarithm of the inverse of the pressure.

It may be remarked from the demonstration of XXVIII., that if $v^2$ is constant, $\frac{z}{d}$ is so also, and $\frac{\dot{z}}{d} = \frac{\dot{z}}{z} = -h$ (XXX.), taking $h$ to represent the height from the ground; hence by integration, $h = \log. \frac{1}{z}$.

This is the formula with which it is usual to represent the constitution of the atmosphere, but a state of uniform velocity is incompatible with this theory.

XXXVI. Two atmospheres that differ from each other in the weight of their molecules, and whose lower strata are in equilibrium of *vis viva*, will, if separate, have heights inversely as the square of the weight of their molecules.

XXXVII. If such atmospheres are mixed, they will become equally diffused over the same space, and will obtain a common elevation, which is proportional to the inverse of the square of the specific gravity of the mixture, *if the impinging surfaces of the molecules are proportional to their weights* (XVI.).

XXXVIII. The equilibrium of such a mixed atmosphere is in every respect the same as that of a homogeneous atmosphere whose specific gravity is equal to that of the mixture, if the impinging surface of each molecule is proportional to its weight.

XXXIX. The molecules of two atmospheres that differ from each other in the weight of their molecules, but which are of equal altitude, will, if separate, have at the same altitude *vis insita* proportional to their respective weights, and elasticities as the square of those weights.

XL. If the earth were a homogeneous sphere, and the atmosphere supposed to extend downwards to its centre, then, if the distance from the centre is taken as the cosine of an arch, the sine of that arch shall represent the velocity of the molecules at the given distance, and the velocity at the centre shall be equal to the square root of one-half of the product of the radius of the earth by the force of gravity at the surface.

To determine how far the preceding theory represents the condition of the terrestrial atmosphere, two data are required—viz. the height of a uniform atmosphere at a fixed temperature, and the absolute value of this temperature. The latter is determined by the rate at which air expands under a constant pressure. The former is derived from MM. Biot and Arago's determination of the relative specific gravity of air and mercury at the temperature of melting ice, and the height of the mercurial column. The proportion is 1 to 10,467, the height of the barometer being 336·9 Paris lines. Hence the total height of the atmosphere at this temperature is 156,576 feet, or about 30 miles (XXX.), and for every degree of Fahrenheit's thermometer it changes 317·6 feet (XXXIII.), and the temperature diminishes uniformly in ascending, at the rate of one degree in 317·6 feet (XXXII.); and this proportion is constant at all heights, and is independent of any change of pressure. M. Gay Lussac, in his celebrated balloon ascent from Paris to an elevation of 7634 yards, observed the depression of temperature to be $72\frac{1}{2}^{\circ}$, which is at the rate of $1^{\circ}$ for every 316 feet. Such an observation is the best test which the theory could have, and the coincidence is nearer than might be expected; indeed too near, if the air absorbs any sensible proportion of the sun's rays of heat. At the surface of the ground, the rate of the diminution of temperature varies from about 250 to 500 feet each degree, and seems to be much under local influence.

The physical constitution of the atmosphere is adapted to a certain condition of equilibrium, which it endeavours to assume so far as external circumstances will permit. The theory represents this condition as one that involves a diminution of temperature as well as pressure, and assimilates them to one physical mode. A comparison of the barometer and thermometer at two stations, if simultaneous, shows at once the existence and extent of the deviation from the theoretical equilibrium; and in the expression for the difference of height, in terms of the temperature and pressure derived from XXX., XXXI., etc., it affords the means of compensation, by presenting two results, one of which is as much deficient as the other is in excess. Let T and $t$ represent the temperature at the lower

and upper stations in degrees of Fahrenheit's scale, H and $h$ the corresponding heights of the barometer, then

(1) $(146415 + \mathrm{T}\,317{\cdot}6)\left\{1 - \left(\frac{h}{\mathrm{H}}\right)^{\frac{1}{8}}\right\}$ = difference of level in feet.

(2) $(146415 + t\,317{\cdot}6)\left\{\left(\frac{\mathrm{H}}{h}\right)^{\frac{1}{8}} - 1\right\}$ = difference of level in feet.

These will give the same result when the temperature diminishes at the rate of one degree in 317·6 feet. When the rate exceeds this, the first formula will be rather greater than the second. The mean ought to give the height accurately, if the variation of density and temperature from the natural equilibrium is uniform. Comparing this with Laplace's formula, it will be found to give results which are $\frac{1}{1\,6\,0}$ part less. It may be remarked, however, that this coincidence is not to be reckoned as confirming the theory; for if the exponent $\frac{1}{6}$ has any other value less than $\frac{1}{4}$, and the co-efficients are altered to correspond, the results will continue to agree with Laplace's formula. The reason of this will be obvious, on examining the two formulæ. The proper test to the theory is the rate of the diminution of temperature.

As the atmosphere dilates and contracts uniformly with the temperature, we may construct a formula from XXXV. and VIII. combined, which is less troublesome to compute than the above (1) (2). Suppose the atmosphere of uniform temperature T, the height will be proportional to the logarithm of the ratio of the pressures; but the temperature diminishing uniformly from T to $t$, the height determined thus by logarithms must be reduced in the proportion of $(\mathrm{T} - t)\ \frac{1}{2}$ to T; thence the formula will be (log. H − log. $h$) $\mathrm{C}\left(1 - \frac{\mathrm{T} - t}{2\,\mathrm{T}}\right)$ or reducing to Fahrenheit's scale, and according to the preceding value of the absolute temperature of melting ice, we obtain the following formula for measuring heights by the barometer; (Log. H − log. $h$) $20047\left(1 + \frac{\mathrm{T} + t - 64}{986}\right)$. This, as might be expected, is of the same form as that of Laplace, for in both there is supposed to be uniformity in the diminution of temperature. It is not probable, however, that this uniformity

exists close to the surface of the ground, and this may account for the difference in the constants. If we assume the rate of diminution of temperature to be 1° in 319·6 feet, and the exponent in the first formulæ still $\frac{1}{6}$, they would coincide exactly with Laplace's formula; and to reconcile this perfectly with the *a priori* reasoning of the theory, it is only necessary to assume the absolute temperature of melting ice as 488°, instead of 491°, and the expansion of air to be $\frac{1}{180}$ less than what has been given by Professor Rudberg as the results of his observations.

It will be remarked, that Props. XXVIII. and XIV. determine the absolute velocity, corresponding to a given temperature, of the molecules of all aeriform fluids that conform to the commonly received laws of pressure, temperature, and volume. At the temperature of melting ice, the molecules of gases that have the same specific gravity as air move with a velocity of 2244·2 feet per second, and at 1447° Fahrenheit, or 1972 absolute, their velocity is 4488 feet. The molecules of hydrogen have a velocity of 32316 feet per second at 32°.

The theory coincides with the views that are generally held respecting a great central heat. According to the researches of M. Cordier, the temperature increases at the rate of about 1° in every 50 or 60 feet of descent. If the atmosphere were $2\frac{1}{2}$ times its present specific gravity or atomic weight, its temperature would increase at the same rate.

If the atmosphere were supposed to extend through a shaft to the centre of the earth, the *vis insita* of the molecules there would be nine times that at the surface, and the temperature about 40,000°.

If the moon had an atmosphere, its height would be about 180 miles at 32°, if the centripetal force suffered no decrement. If the reasoning in XXX. is modified to correspond with a diminution of gravity in ascending, we obtain the following formula, from which we may estimate the difference in the height of an atmosphere by a change in the surface of temperature in the gravity of the planetary body, and in the atomic constitution of its atmosphere:—

Let $v$ = velocity of the molecules corresponding to the temperature of the surface of the planet,

$r$ = semi-diameter of the planet,

$g$ = the force of gravity at its surface,

$h$ = height of the atmosphere;

$$\text{then, } h = \frac{v^2 r}{g r - v^2}.$$

Hence the moon's atmosphere might extend to a height of 211·7 miles, if the temperature is 32°, and about $\frac{1}{2}$ mile for each additional degree. On the illuminated side, where the temperature is supposed to be very high, it may extend to 300 miles; on the dark side, probably less than 100. This difference would cause violent currents, similar to our trade-winds, which are impelled by a difference of level that cannot much exceed seven miles between the equator and the poles. If the molecules of the lunar atmosphere had such an atomic weight that their velocity, due to the temperature of the moon's surface, was 5438 feet per second, they would be dissipated into space, or drawn to the surface of the earth; and in this way, it may be conjectured, has it been originally lost. The limiting velocity of the earth's atmosphere is much greater—viz. 5·297 miles per second. If the atomic weight of air, therefore, had been as small as that of hydrogen, the earth's attraction could not retain it—it would evaporate into space. This kind of evaporation probably takes place in comets. The principles of the theory of latent heat, examined in § 3, may lead us to anticipate that such a process will be slow, and that it will abstract much heat from the central parts.

If our atmosphere were supposed to be transferred to the surface of the sun, the theoretical equilibrium of temperature requires that there should be an increase of 1° for every 15 feet of descent.

It may be shown, as a mathematical deduction from the theory, that the sun's heat on the surface of the earth is sufficient in the course of ages to establish an increase of temperature towards the centre, similar in character to that which is generally supposed to exist. Suppose the globe was originally wholly divested of heat, the molecules perfectly at

rest, with their position and mutual distance determined by cohesion and pressure, and if it were exposed to the sun's rays, a process of absorption, expansion, and augmentation of *vis viva* would go on in descending, until the true vertical equilibrium of temperature were established. The central parts have the benefit, as it were, of the peripheral expansion: a molecule receives from the one above its supply of *vis insita;* but, in communicating it to the one below, it receives an accession from gravity, acting in the interval of its descending excursion. Thus, expansion, the first effect of heat, necessarily calls in gravitation to determine the vertical equilibrium of temperature. The extent of the vibrations probably bear but a small proportion to the mutual distance of the molecules; and in the same degree, the augmentation of *vis insita* will be less than that which would be the measure of the vertical descent.

If the molecules of solids and liquids are endowed with the same *vis insita,* in proportion to their temperature, as when they are reduced to a gaseous state, we may hope to become better acquainted with the intense forces that keep them together in these two conditions of rigid and mobile equilibrium. Heat and molecularity are quite independent of each other in their mode of action; but they continually interfere with and antagonize each other in almost all the operations that sustain the activity of nature. Knowing one of these powers therefore, we may be enabled to apply mathematics to the other with greater precision.

In conclusion, I may remark, that this theory applied to questions of aerodynamics, seems to be successful in one or two cases that I have attempted, although the reasoning is not similar to that which has usually been employed to solve the same problems. Thus, the velocity of the propagation of pulses, when the impulse is infinitely less than the elasticity of the medium, is equal to half the mean velocity of the molecules. The molecules are here supposed to have their motions from every point of a plane in every direction alike; hence the velocity with which an impulse is communicated, in a direction perpendicular to the plane, will be to the mean velocity of the molecules as the sum of the cosines of a

hemisphere is to the sum of the radii. The mean velocity of the molecules of air at 60° is, according to the preceding analysis, 2282 feet per second; the velocity of sound at that temperature appears by this theory to be 1141. This agrees remarkably with 1142, the number that has been usually adopted by English physical writers. The next problem is the velocity of the medium when rushing into a vacuum. This is usually shown to be equal to that acquired by a body in falling through the height of a uniform atmosphere; and this is true, if the air suffered no diminution of density. The present theory, however, represents both density and temperature, and therefore velocity of the molecules on entering the vacuum. At the first instant, the front molecules have the mean velocity undiminished, which, as previously shown, is equal to that acquired by a body in falling through three uniform atmospheres; but they quickly acquire an augmentation of *vis insita*, at the expense of the part of the medium at the entrance of the vacuum. Hence a reservoir of air, if exhausted suddenly by a syringe, ought to show a diminution of temperature; but if the same reservoir is totally exhausted, and a small portion of air is admitted, the temperature of the vessel ought to experience a small increment. This principle of augmentation of *vis viva* in gaseous molecules entering a vacuum seems applicable to the theory of vapours.

Applied to the calculation of the resistance of projectiles, this theory affords very satisfactory results at velocities under 400 and 500 feet per second, agreeing most exactly with Dr Hutton's experiments with the whirling machine; but at higher velocities the accordance is not so satisfactory, probably arising from not duly estimating the *modus operandi* of the impulse in front.

I trust that what has been here set forth may be sufficient to induce those who have greater mathematical resources to turn their attention to the subject. It is quite in accordance with the views entertained by philosophers generally, before the development of the theory of latent heat by Dr Black suggested the idea of a distinct element—caloric. The more recent discoveries on specific heat, and on its radiation, seem

to have rather strengthened these opinions, without bestowing upon them greater distinctness. If we should be able to account for all the phenomena without having recourse either to a specific fluid or to emanating repulsive forces, it will be the means of clearing and preparing the ground for taking a nearer view of the molecular constitution of bodies.

# ON THE PHYSICS OF MEDIA THAT ARE COMPOSED OF FREE AND PERFECTLY ELASTIC MOLECULES IN A STATE OF MOTION

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*Introduction by Lord Rayleigh, Sec. R.S.*

The publication of this paper after nearly half a century demands a word of explanation; and the opportunity may be taken to point out in what respects the received theory of gases had been anticipated by Waterston, and to offer some suggestions as to the origin of certain errors and deficiencies in his views.

So far as I am aware, the paper, though always accessible in the Archives of the Royal Society, has remained absolutely unnoticed. Most unfortunately the abstract printed at the time (*Roy. Soc. Proc.*, 1846, vol. 5, p. 604; here reprinted as Appendix I.), gave no adequate idea of the scope of the memoir, and still less of the nature of the results arrived at. The deficiency was in some degree supplied by a short account in the *Report of the British Association* for 1851 (here reprinted as Appendix II.), where is distinctly stated the law, which was afterwards to become so famous, of the equality of the kinetic energies of different molecules at the same temperature.

My own attention was attracted in the first instance to Waterston's work upon the connection between molecular forces and the latent heat of evaporation, and thence to a paper in the *Philosophical Magazine* for 1858 [p. 345 below], "On the Theory of Sound." He there alludes to the theory of gases

under consideration as having been started by Herapath in 1821, and he proceeds :—

"Mr Herapath unfortunately assumed heat or temperature to be represented by the simple ratio of the velocity instead of the square of the velocity—being in this apparently led astray by the definition of motion generally received—and thus was baffled in his attempts to reconcile his theory with observation. If we make this change in Mr Herapath's definition of heat or temperature, viz., that it is proportional to the *vis viva*, or square velocity of the moving particle, not to the momentum or simple ratio of the velocity, we can without much difficulty deduce, not only the primary laws of elastic fluids, but also the other physical properties of gases enumerated above in the third objection to Newton's hypothesis. In the Archives of the Royal Society for 1845-1846, there is a paper 'On the Physics of Media that consists of perfectly Elastic Molecules in a State of Motion,' which contains the synthetical reasoning upon which the demonstration of these matters rests. The velocity of sound is therein deduced to be equal to the velocity acquired in falling through three-fourths of a uniform atmosphere. This theory does not take account of the size of the molecules. It assumes that no time is lost at the impact, and that if the impacts produce rotatory motion, the *vis viva* thus invested bears a constant ratio to the rectilineal *vis viva*, so as not to require separate consideration. It also does not take account of the probable internal motion of composite molecules; yet the results so closely accord with observation in every part of the subject as to leave no doubt that Mr Herapath's idea of the physical constitution of gases approximates closely to the truth. M. Krönig appears to have entered upon the subject in an independent manner, and arrives at the same result; M. Clausius, too, as we learn from his paper 'On the Nature of the Motion we call Heat'" (*Phil. Mag.*, vol. 14, 1857, p. 108).

Impressed with the above passage and with the general ingenuity and soundness of Waterston's views, I took the first opportunity of consulting the Archives, and saw at once that the memoir justified the large claims made for it, and that it marks an immense advance in the direction of the now generally

received theory. The omission to publish it at the time was a misfortune, which probably retarded the development of the subject by ten or fifteen years. It is singular that Waterston appears to have advanced no claim for subsequent publication, whether in the Transactions of the Society, or through some other channel. At any time since 1860 reference would naturally have been made to Maxwell, and it cannot be doubted that he would have at once recommended that everything possible should be done to atone for the original failure of appreciation.

It is difficult to put oneself in imagination into the position of the reader of 1845, and one can understand that the substance of the memoir should have appeared speculative and that its mathematical style should have failed to attract. But it is startling to find a referee expressing the opinion that "the paper is nothing but nonsense, unfit even for reading before the Society." Another remarks "that the whole investigation is confessedly founded on a principle entirely hypothetical, from which it is the object to deduce a mathematical representation of the phenomena of elastic media. It exhibits much skill and many remarkable accordances with the general facts, as well as numerical values furnished by observation. . . . The original principle itself involves an assumption which seems to me very difficult to admit, and by no means a satisfactory basis for a mathematical theory, viz., that the elasticity of a medium is to be measured by supposing its molecules in vertical motion, and making a succession of impacts against an elastic gravitating plane." These remarks are not here quoted with the idea of reflecting upon the judgment of the referee, who was one of the best qualified authorities of the day, and evidently devoted to a most difficult task his careful attention; but rather with the view of throwing light upon the attitude then assumed by men of science in regard to this question, and in order to point a moral. The history of this paper suggests that highly speculative investigations, especially by an unknown author, are best brought before the world through some other channel than a scientific society, which naturally hesitates to admit into its printed records matter of uncertain value, Perhaps one may go further and say that a young author who

believes himself capable of great things would usually do well to secure the favourable recognition of the scientific world by work whose scope is limited, and whose value is easily judged, before embarking upon higher flights.

One circumstance which may have told unfavourably upon the reception of Waterston's paper is that he mentions no predecessors. Had he put forward his investigation as a development of the theory of D. Bernoulli, a referee might have hesitated to call it nonsense. It is probable, however, that Waterston was unacquainted with Bernoulli's work, and doubtful whether at that time he knew that Herapath had to some extent foreshadowed similar views.

At the present time the interest of Waterston's paper can, of course, be little more than historical. What strikes one most is the marvellous courage with which he attacked questions, some of which even now present serious difficulties. To say that he was not always successful is only to deny his claim to rank among the very foremost theorists of all ages. The character of the advance to be dated from this paper will be at once understood when it is realised that Waterston was the first to introduce into the theory the conception that heat and temperature are to be measured by *vis viva*. This enabled him at a stroke to complete Bernoulli's explanation of pressure by showing the accordance of the hypothetical medium with the law of Dalton and Gay-Lussac. In the second section the great feature is the statement (VII.), that "in mixed media the mean square molecular velocity is inversely proportional to the specific weight of the molecules." The proof which Waterston gave is doubtless not satisfactory; but the same may be said of that advanced by Maxwell fifteen years later. The law of Avogadro follows at once, as well as that of Graham relative to diffusion. Since the law of equal energies was actually published in 1851, there can be no hesitation, I think, in attaching Waterston's name to it. The attainment of correct results in the third section, dealing with adiabatic expansion, was only prevented by a slip of calculation.

In a few important respects Waterston stopped short. There is no indication, so far as I can see, that he recognised any other form of motion, or energy, than the translatory

motion, though this is sometimes spoken of as vibratory. In this matter the priority in a wider view rests with Clausius. According to Waterston the ratio of specific heats should be (as for mercury vapour) 1·67 in all cases. Again, although he was well aware that the molecular velocity cannot be constant, there is no anticipation of the law of distribution of velocities established by Maxwell.

A large part of the paper deals with chemistry, and shows that his views upon that subject also were much in advance of those generally held at the time.

The following extract from a letter by Professor M'Leod will put the reader into possession of the main facts of the case:—

"It seems a misfortune that the paper was not printed when it was written, for it shadows forth many of the ideas of modern chemistry which have been adopted since 1845, and it might have been the means of hastening their reception by chemists.

"The author compares the masses of equal volumes of gaseous and volatile elements and compounds, and taking the mass of a unit volume of hydrogen as unity, he regards the masses of the same volume of other volatile bodies as representing their molecular weight, and in the case of the elements he employs their symbols to indicate the molecules.

"In water he considers that the molecule of hydrogen is combined with half a molecule of oxygen, forming one of steam, and he therefore represents the compound as $HO_{\frac{1}{2}}$. He does not make use of the term "atom" (although he speaks of atomic weight on p. 231, but thinks it divisible), and if he had called the smallest proportion of an element which enters into combination an atom, he would probably have been led to believe that the molecules of some of the simple bodies contain two atoms, and he might have adopted two volumes to represent the molecule, as is done at the present time. The author calls one volume or molecule of chlorine Cl, one volume or molecule of hydrogen H, and one volume or molecule of hydrochloric acid $H_{\frac{1}{2}}Cl_{\frac{1}{2}}$. If he had regarded the molecules

as containing two indivisible atoms, these bodies would have been represented, as now, by the formulæ $Cl_2$, $H_2$, and HCl respectively, all occupying two volumes. § 15 shows how near he was to this conception. Gerhardt in the Fourth Part of his *Traité de Chimie Organique*, published in 1856, points out the uniformity introduced into chemical theory by the adoption of this system.

"For carbon he makes C = 12, as now accepted, although I do not find how he arrived at this number. He seems to have anticipated one of Ramsay's recent discoveries, that nitrous anhydride (hyponitrous acid, $ON_{\frac{3}{3}}$, No. 26 in the table) dissociates on evaporation into nitric oxide (binoxide of nitrogen, No. 23) and nitric peroxide (nitrous acid, No. 25).

"The values for the symbols for sulphur, phosphorus, and arsenic taken from the vapour densities (and which are multiples of what are believed to be the true atomic weights), cause some complexity in the formulæ of their compounds.

"There seem to be errors in the formulæ of alcohol and ether on p. 274, for they do not agree with those in the table. They ought probably to be written

$$2\,(HC_{\frac{1}{2}}) + O_{\frac{1}{2}}2H_{\frac{1}{2}} \text{ and } 4\,(HC_{\frac{1}{2}}) + O_{\frac{1}{2}}2H_{\frac{1}{2}}.$$

"Considering how nearly Waterston approached what is now believed to be the true theory, it is disappointing to read his controversy with Odling in 1863 and 1864 (pages 565-599 of this book), where he seems to oppose the new formulæ then being introduced. He is very dogmatic about the constitution of hydrate of potash: he very properly insists that we can only obtain a knowledge of the molecular weight of bodies that can be volatilized, and of which the vapour densities can be determined, but he does not see the analogy between the hydrate and oxide of potassium with alcohol and ether, probably because he regards these latter bodies as combinations of water with different quantities of olefiant gas. He writes water $HO_{\frac{1}{2}} = 9$, alcohol $CH_2HO_{\frac{1}{2}} = 23$, and ether $C_2H_4 . HO_{\frac{1}{2}} = 37$, whilst he considers potassic hydrate $KO_{\frac{1}{2}} . HO_{\frac{1}{2}} = 56$, and oxide of potassium $KO_{\frac{1}{2}} = 47$, the hydrate having a higher molecular weight than the oxide. If we

regard these compounds as derived from water by the replacement of hydrogen by ethyl and potassium respectively, the analogy between the two series is complete (ethyl was discovered in 1849 and is mentioned by Waterston).

| | |
|---|---|
| $H_2O = 18$ | $H_2O = 18.$ |
| $(C_2H_5)\,HO = 46$ | $KHO = 56.$ |
| $(C_2H_5)_2\,O = 74$ | $K_2O = 94.$ |

"From a remark in the *Phil. Mag.* (vol. 26, p. 520), I imagined that Waterston had arrived at the double atomic weights of many of the metals now adopted, for he gives that of iron as 56 and that of aluminium as 27 calculated from their specific heats, but there is an error in his arithmetic, for 3·3 divided by the specific heat of iron ·1138 gives 28·998, and 3·3 divided by the specific heat of aluminium ·2143 gives 15·399."

With the exception of some corrections relating merely to stops and spelling the paper is here reproduced exactly as it stands in the author's manuscript.—Dec. 1891.

[*Author's Introduction.*]

Of the physical theories of heat that have claimed attention since the time of Bacon, that which ascribes its cause to the intense vibrations of the elementary parts of bodies has received a considerable accession of probability from the recent experiments of Forbes and Melloni. It is admitted that these have been the means of demonstrating that the mode of its radiation is identical with that of light in the quantities of refraction and polarization. The evidence that has been accumulated in favour of the undulatory theory of light has thus been made to support with a great portion of its weight a like theory of the phenomena of heat; and we are, perhaps, justified in expecting that the complete development of this theory will have a much more important influence on the progress of science, because of its more obvious connection and intimate blending with almost every appearance of Nature. Heat is not only the subject of direct sensation and the vivifier

of organic life, but it is manifested as the accompaniment of mechanical force. It is related to it both as cause and effect, and submits itself readily to measurement by means of the mechanical changes that are among the most prominent indications of its change of intensity. The undulatory theory at once leads us to the conclusion that, inasmuch as the temperature of a body is a persistent quality due to the motion of its molecules, its internal constitution must admit of it retaining a vast amount of living force. Indeed, it seems to be almost impossible now to escape from the inference that heat is essentially molecular *vis viva*. In solids, the molecular oscillations may be viewed as being restrained by the intense forces of aggregation. In vapours and gases these seem to be overcome; vibrations can no longer be produced by the inherent *vis insita* of the molecules struggling with attractive and repellant forces; the struggle is over and the molecules are free; but they, nevertheless, continue to maintain a certain temperature; they are capable of heating and being heated; they are endowed with the quality heat, which, being of itself motion, compels us to infer that a molecule in motion without any force to restrain or qualify it, is in every respect to be considered as a free projectile. Allow such free projectiles to be endowed with perfect elasticity, and likewise extend the same property to the elementary parts of all bodies that they strike against, and we immediately introduce the principle of the conservation of *vis viva* to regulate the general effects of their fortuitous encounters. Whether gases do consist of such minute elastic projectiles or not, it seems worth while to inquire into the physical attributes of media so constituted, and to see what analogy they bear to the elegant and symmetrical laws of aeriform bodies.

Some years ago I made an attempt to do so, proceeding synthetically from this fundamental hypothesis, and have lately obtained demonstration of one or two points where the proof was then deficient. The results have appeared so encouraging, although derived from very humble applications of mathematics, that I have been led to hope a popular account of the train of reasoning may not prove unacceptable to the Royal Society.—1st September 1845.

## Section I.—Of a Homogeneous Medium and the Laws of its Elasticity.

§ 1. The term medium is, perhaps, not quite appropriate to what is here intended to be signified. We speak of a resisting medium, of the medium of light, and in each expression something is referred to as intervening between bodies, and it is the quality of interposition that entitles it to the name. Here, for want of better, it is employed to denote a certain hypothetical condition of matter which it is the object of this Paper to show has physical properties that resemble those that have been found to belong to aeriform bodies. Inasmuch, therefore, as the word may be applied to a simple unmixed gas so as to speak of it as an oxygen medium or a hydrogen medium, etc., so far we may be allowed the use of it in treating of a hypothetical medium, which we have carefully to refrain from assimilating to any known form of matter until, by synthetical reasoning, circumstantial evidence has been accumulated sufficient to prove or render probable its identity.

To have a proper conception of what the medium is that forms the subject of speculation, we must imagine a vast multitude of small particles of matter, perfectly alike in every respect, perfectly elastic as glass or ivory—but of size, form and texture that requires not to be specified further than that they are not liable to change by mutual action—to be enclosed by elastic walls or surfaces in a space so much greater than their aggregate bulk as to allow them freely to move amongst each other in every direction. As all consideration of attractive forces is left out at present, it is obvious that each particle must proceed on a straight line until it strikes against another, or against the sides of the enclosure; that it must then be reflected and driven into another line of motion, traversing backwards and forwards in every direction, so that the intestine condition of the multitude of these that form the medium may be likened to the familiar appearance of a swarm of gnats in a sunbeam.

The quality of perfect elasticity being common to all the particles, the original amount of *vis viva*, or living, acting

force, of the whole multitude must for ever remain the same. If undisturbed by external action it cannot, of itself, diminish or increase, but must for ever remain as unchanged as the matter that is associated with it and that it endows with activity. Such is the case if we view the whole mass of moving particles as one object, but each individual of the multitude must at every encounter give or receive, according to the ever changing angle and plane of impact, some portion of its force, so that, considered separately, they are for ever continually changing the velocity and direction of their individual motions; striking against and rebounding from each other, they run rapidly in their zig-zag conflict through every possible mode of concurrence, and *at each point of the medium we may thus conceive that particles are moving in every possible direction and encountering each other in every possible manner during so small an elapsed interval of time that it may be viewed as infinitesimal in respect to any sensible period.* The medium must in this way become endowed with a permanent state of elastic energy or disposition to expand, uniformly sustained in every part and communicating to it the physical character of an elastic fluid.

The simplicity of this hypothesis facilitates the application of mathematics in ascertaining the nature and properties of such media, and the study acquires much interest from the analogies that it unfolds. For if the reasoning is correct, the physical laws common to all gases and vapours—those laws, namely, that concern heat and pressure—do actually belong to such media, and may be synthetically deduced from the constitution which has now been assigned to them.

The characteristic which renders a medium susceptible of mathematical treatment is that of its being composed of particles perfectly alike in every respect, but it is chiefly their identity in weight or mass that is the important point of distinction. A particle thus conforms to the definition that the eminent physicist Ampère has given to the term molecule, which we may therefore adopt as a more significant name for the element of a medium.

The first department of the subject must naturally be devoted to the consideration of the circumstances that determine

the equilibrium of such a homogeneous medium considered by itself. Its density, by which is to be understood not its specific gravity but the number of molecules in a constant volume,* may be supposed to vary without disturbing its homogeneity. The mean square velocity of the molecules (which in any infinitesimal portion of the medium may be assumed as uniform) we also have to consider as a variable quantity, and the physical qualities of a medium being dependent on these two elements of its constitution, it is necessary to determine clearly their mathematical relations.

§ 2. It is evident from the definition of the hypothesis, that the medium must exert some expansive force on the surface that encloses it; but the nature of the force is not strictly continuous, it is composed of a multitude of successive strokes. Nevertheless, their succession is certainly continuous, and it is not difficult to conceive how they may be sufficient to counterbalance and support a superincumbent weight. To obtain an exact idea of this, let us suppose that a small elastic plane whose weight is $n$ times that of a molecule, is supported by a regular succession of such molecules striking its centre of gravity with a velocity $v$. We seek to know the condition of their mutual action when an equilibrium is maintained.

The following are the well-known equations that express the law of elastic collision. They are necessarily the foundation of all reasoning on the effects of the mutual action of elastic bodies by impact.

### 1. *The Meeting Impact.*

Two molecules, B and D, meet directly in an intermediate point and strike each other with the respective velocities $\beta$ and $\delta$. The velocities after impact are respectively :—

$$\beta_0 = -\beta + \frac{2(\delta+\beta)\,\mathrm{D}}{(\mathrm{B}+\mathrm{D})}; \text{ and } \delta_0 = \delta - \frac{2(\delta+\beta)\,\mathrm{B}}{(\mathrm{B}+\mathrm{D})};$$

the direction of D's motion being reckoned positive.

* [Attention should be directed to this use of the word "density."—R.]

2. *The Overtaking Impact.*

The two molecules, B and D, with the same velocities, $\beta$ and $\delta$, move in the same direction and D overtakes B; the velocities after impact are respectively:—

$$\beta_1 = \beta + \frac{2(\delta - \beta)\,\mathrm{D}}{(\mathrm{B} + \mathrm{D})}; \text{ and } \delta_1 = \delta - \frac{2(\delta - \beta)\,\mathrm{B}}{(\mathrm{B} + \mathrm{D})};$$

the direction of D's motion being reckoned positive.

In the first of these let $\beta_0 = \beta$, $\delta = v$, $\mathrm{B} = n\,\mathrm{D}$; then shall

$$\beta = -\beta + \frac{2(v + \beta)}{n + 1}; \text{ or } \beta = \frac{v}{n},*$$

which evidently expresses the upward velocity given to the plane by the impulse of one molecule when the velocity of incidence and reflexion is the same. The plane ascends and descends the height due to this velocity, and then encounters the next in the succession of molecular impacts without any transference of force taking place between them; and $n$ being taken an indefinitely great number, $\beta$ is infinitesimal in respect to $v$, and the height through which the plane traverses is also infinitesimal, so that it is supported as if by a continuous force of upward pressure. The time between each impact is, according to the law of falling bodies, equal to the time taken by the force of gravity to destroy and reproduce the infinitesimal velocity $v/n$. This is $2v/gn$: the velocity which a free body gains or loses in a unit of time by the force of gravity, being represented by $g$. The number of impacts in a unit of time is therefore $gn/2v = \mathrm{A}$. This, then, is the relation between the weight of the plane, in terms of that of the molecule unity, and the rapidity of the succession of impacts necessary to support it in a condition of statical equilibrium. Now, if the plane forms part of the surface that encloses the medium and that counterbalances by its weight the effect of the impacts of the confined molecules, such effect must correspond with the succession represented

* [The case is that where the particle (mass 1) and the plane (mass $n$) both reverse their velocities at impact. The conservation of *vis viva* is thereby secured, and the condition of momentum gives at once $n\beta = v$.—R.]

by A; and we deduce that *the elastic force of a medium, as represented by the weight or pressure required to confine it, is directly proportional to the number of molecular impacts that take place against a unit surface in a unit time with a constant velocity* (or $e \doteqdot A$, if $v$ is constant) . . . I.

§ 3. Such being the nature of the elastic force, it will not be difficult to prove that it increases exactly as the density of the medium. The proposition stands thus: if the number of molecules in a volume of the medium be doubled, the number of impacts that take place on a constant surface in a constant time will also be doubled, the velocity being unchanged.

Suppose the number octupled, the mean distance is reduced to one-half. If they were equidistant and moving in one direction with the constant velocity, it is evident that eight times the previous number would pass the same imaginary plane in the same time, and if the plane were solid that eight times the previous number would impinge against it. Now, although all do not move in one direction, yet in both cases the same proportion of the whole must in each case do so. Whatever may be the density no preference can be assigned to one direction more than to another in the molecular movements; they must in every case be equally distributed in every direction, and if the number is increased eight times in any one direction it must be so in every other.

This may be viewed in another light. Suppose in both cases, the density being 1 and 8 respectively, that the molecules are arrested in their motion. It is evident that opposite a unit of surface in density 8 there will, in the first row, be four times as many molecules as in density 1, and that the average distance between the rows is only one-half. Suppose the molecules to resume their motion, and compare density 8 with density 1, it is obvious that in half the time four times the number will impinge on the unit of surface, and in the same time eight times the number. Now it has been shown (§ 2) that the elastic force is proportional to the number of molecular impacts made with a constant velocity against a unit of surface in a unit of time, hence we deduce that *the elastic force* (*e*) *of a medium with a constant mean molecular*

*velocity* ($v$) *is proportional to its density* ($\Delta^3$) (or $e \doteqdot \Delta^3$, if $v$ or $v^2$ is constant) . . . . . II.

§ 4. Hitherto the molecular velocity has been supposed constant. We have now to inquire how the elasticity of the medium is affected by a change in the velocity from $v$ to $mv$. The intestine action of the medium may be viewed as the traversing of a certain mean distance, L, by the molecules in a given time, $t$; and in this time a certain mean number, A, of impacts take place against a unit of surface. If the velocity is increased $m$ times, the distance L is traversed in $1/m$th the time $t$, or $t/m$, and in this reduced time the same number of impacts must take place as before took place in the time $t$; for there is nothing in the change of velocity simply that can alter the ratio that subsists between the mean distance traversed and the mean number of impacts, unless that ratio were subject to change without any change whatever in the medium, which is absurd; hence, in the original time, $t$, there is $m$ times the original number of impacts, A.

It was shown in § 2 that if the weight of each of the molecules were represented by 1, their mean velocity by $v$, and weight of plane supported by their impinging action $n$, the number of impacts in a second or unit of time required to support the plane is $\frac{gn}{2v} = \mathrm{A}$, or $n = \frac{2}{g}\mathrm{A}v$, and this equation must evidently be maintained in altering the value of the terms. Now, it has been shown that in changing $v$ to $mv$ in a medium that does not alter its density we cause A to become $m\mathrm{A}$, and $\frac{2}{g}\mathrm{A}v$ becomes $\frac{2}{g}\mathrm{A}mvm = \frac{2}{g}\mathrm{A}vm^2 = nm^2$. Hence $n$, the weight of the plane, or measure of tension, must be increased $m^2$ times so that it may continue to equilibrate the impinging action. Thus, we deduce that while the molecular velocity increases from $v$ to $mv$, the elasticity increases from $n$ to $m^2n$ or *the elasticity of a medium having a constant density is proportional to the mean square molecular velocity or* vis viva *of the medium* (or $e \doteqdot v^2$, when $\Delta^3$ is constant).* III.

* [II. and III. were given by D. Bernoulli. See *Pogg. Ann.*, vol. 107, p. 490, 1859.—R.]

§ 5. Combining II. with III. it appears that when both the density and the *vis viva* are subject to change that the elasticity is equal to their product, or $e \div \Delta^3 v^2$, and this is the law that includes all the conditions of equilibrium of an enclosed homogeneous medium. One other condition only remains to be specified. *Under a constant pressure the density is inversely as the* vis viva *or mean square molecular velocity*

($\Delta^3 \div \frac{1}{v^2}$, if $e$ is constant) . . . . . IV.

§ 6. In concluding this part of the subject, we cannot fail of being sensible of the analogies that subsist between these synthetical deductions and the chief properties that distinguish aeriform fluids.

The first point that was inductively established is Mariotte's law, viz.: *at the same temperature the density of air is as its compression.* This is analogous to the second deduction:— The square of the velocity being constant, the elastic force of a medium is proportional to its density. The accordance appears as complete as could be desired, and there is a residual evidence in favour of $v^2$ being identical with temperature, or being a quality that varies simultaneously with it.

The second point is Dalton and Gay-Lussac's law of expansion. By experimenting upon the same weight of air at different temperatures under a constant pressure, these philosophers found that an increment of one degree caused always the same augmentation of bulk, and that this amounted to $\frac{1}{480}$ part of the space that it occupied at 32°. Thus, if the same law hold good at all temperatures, 480 cubic inches of air at this temperature should diminish one inch in bulk for every degree it was lowered in temperature, and would become zero in bulk at 480° below the freezing point of water, or −448° on Fahrenheit scale.

Now in IV. we had $\Delta^3 \div \frac{1}{v^2}$, or $v^2 \div \frac{1}{\Delta^3}$, when $e$ is constant; but $\frac{1}{\Delta^3}$ is the volume occupied by a constant number of molecules; hence with the same constant number of molecules the volume is as the mean square molecular velocity, and a constant increment of *vis viva* is followed by the same

increment of volume under a constant pressure, and as the constant increment of volume (1 cubic inch) is to the constant increment of *vis viva* (1°) so is the volume (480 cubic inches) corresponding to a certain *vis viva* (32° Fahr.) to that *vis viva* (480°).

The analogy therefore still holds good, and the evidence continues in favour of the absolute temperature being represented by $v^2$.

When air is not allowed to expand and heat is applied, the elastic force increases with the temperature, and a rise of 1° causes an absolute increase in the elasticity, which is the same at all temperatures, and corresponds with the increase of bulk it would assume if allowed freely to expand. This is analogous to III., where the density being constant or bulk unchanged, the elasticity is shown to be proportional to the mean square molecular velocity.

Thus, the laws of Mariotte and of Dalton and Gay-Lussac are represented by the formula $(448 + t)\,\Delta^3 = e$; in which $t$ = temperature, Fahrenheit scale; $\Delta^3$ = density, and $e$ = elasticity.

The law of elasticity in the hypothetical medium is represented by the formula $v^2\Delta^3 = e$; in which $v^2$ is the mean square molecular velocity; $\Delta^3$ = density, and $e$ = elasticity.

The *first* expresses physical laws that have been *found* to belong to a certain *existent* form of matter.

The *second* expresses physical laws that have been *proved* to belong to a certain *possible* form of matter.

The cause of the effect represented by $(448 + t)$ in the first is unknown, but has, at various times, by eminent authorities, been referred to *molecular motion*.

The corresponding term, $v^2$, of the second represents *molecular motion*.

## Section II.—On the Physical Relations of Media that differ from each other in the Specific Weight of their Molecules.

§ 7. The synthetical deductions of last section apply to a homogeneous medium without respect to the absolute weight of its molecules, if the weight of each molecule is the same.

This weight, common to all, may be viewed as the specific molecular weight of the medium, and distinguishes it from any other medium with a different specific molecular weight. We have now to inquire into the relations that subsist between the density and molecular velocity of two such media that have the same elasticity, or that are in equilibrium of pressure and also of *vis viva*.

We deduced from the law of impinging elastic bodies that if $v$ represents the mean molecular velocity in feet per second, A the number of molecular impacts in a second upon a small elastic plane which is equal in weight to $n$ molecules, then $n = \frac{2}{g}\mathrm{A}v$. Let $\omega$ represent the specific weight of the molecules, we have $\omega n = \frac{2\omega}{g}\mathrm{A}v = e =$ the elastic force exerted by the medium on a unit of surface *; and as this must in the present inquiry be assumed constant, we may easily remark how a change in $\omega$ affects $v$ and A.

It is evident that since $\frac{2\omega}{g}\mathrm{A}v$ is a constant quantity and $\omega$, A, and $v$ variable, we have $\mathrm{A}v \doteqdot \frac{1}{\omega}$; but $e = \omega\Delta^3 v^2$ (§ 5) $= \frac{2\omega}{g}\mathrm{A}v$, and, therefore, $\Delta^3 v = \frac{2}{g}\mathrm{A}$, or $\mathrm{A} = \frac{g}{2}\Delta^3 v$, and $\mathrm{A}v \doteqdot \frac{1}{\omega} \doteqdot \Delta^3 v^2$. Hence it is obvious that if $\Delta^3$, the density or number of molecules in a constant volume, as well as $e$, the tension, are constant, while the molecular velocity and specific weight are variable, these variables are bound by the relation expressed by $v^2 \doteqdot 1/\omega$, which signifies that if two media are in equilibrium of pressure and have in the same volume of each the same number of molecules, the squares of their molecular velocities must be inversely as their specific molecular weights. Hence, we deduce that *if any number of separate media have equal density and tension, the molecular velocity of each must be proportional to the inverse square root of their specific*

* $e$ is the absolute weight of the small elastic plane that is supported by the succession of A number of molecular impacts per second, the weight of each of which is $\omega$, and their common impinging velocity $v$ feet per second.

*molecular weight, or to the inverse square root of the specific gravity of the media respectively* * . . . . . V.

§ 8. But media may be in equilibrium of pressure without being of equal density, for a deficiency of density may be compensated by an excess in the molecular velocity. It is plain that if $e$ and $v^2$ are constant in any two media they may still be in equilibrium of pressure if $\omega$ is proportional to $1/\Delta^3$, or if the molecular weight of each medium is inversely as its density. If the specific molecular weights are in a constant ratio to each other and the tension and velocity also constant, the media must be kept in equilibrium of pressure if the density of each is reciprocally proportional to the specific molecular weight of the other.

We have supposed hitherto that the media are separate while their respective elasticities are compared. Let us now inquire into the effects of allowing them to have access to each other. The united media immediately obtain a heterogeneous character, for it requires no demonstration to convince us that the molecules of each will permeate through the volume occupied by the other, the vacuities in the space occupied by each presenting no more obstacle to the motion of one set of molecules than it does to the other; and as collision must take place amongst them in every possible manner and direction, the common space of the united media are free alike to each individual molecule of both to range through in its zig-zag course. Consequently, *media in contact with each other become gradually equally diffused through their common volume* VI.

The internal condition of the mixture must after a time become settled so that in any infinitesimal portion the same mean velocity will be found proper to the molecules of each medium respectively.

But as each of the two sets of molecules, although completely mixed together, preserve their specific weights, so must they have corresponding specific velocities that remain intact,

* [The deduction of V. appears to be correct, though much embarrassed by the irrelevant $g$. In his first memoir on the "Theory of Gases" (*Pogg. Ann.*, vol. 100, 1857), Clausius arrives at the same conclusion. His assumption that the density (in Waterston's sense) of various gases is the same, appears to have been made upon chemical grounds.—R.]

notwithstanding that they as often impinge on molecules of the other set as on the molecules of their own kind. It is of consequence to settle what the ratio of these specific velocities is, for upon this point depends the nature of the *vis viva* equilibrium of different media, and we have to determine the relative condition of two media when they are in equilibrium both of pressure and of *vis viva*.

§ 9. We must now refer back to the equations of impact (§ 2). It is apparent that the sum of the impinging *vis viva* of both molecules does not alter in either the meeting or overtaking impact; what is gained by one is lost by the other, or $\beta^2B + \delta^2D = \beta_0^2B + \delta_0^2D = \beta_1^2B + \delta_1^2D$. But in every case except one a transference of *vis viva* must take place from the one to the other.

The exception is found in the meeting impact when $\beta \doteqdot 1/B$ and $\delta \doteqdot 1/D$; then shall $\beta_0 = \beta$ and $\delta_0 = \delta$, but at the same time $\beta_1$ is not equal to $\beta$, or $\delta_1$ to $\delta$; in every other case $\beta_0$ is not equal to $\beta$ nor to $\beta_1$, nor is $\delta_0$ equal to $\delta$ or to $\delta_1$.

It can seldom happen that the molecules strike each other directly. In taking account of the collective result of their fortuitous concourse we must view the position of the plane of concurrence and the respective inclinations of the line of motion of each molecule to it as three independent variables. The incident velocity of each is the absolute velocity resolved perpendicular to the plane, and the equations apply to this portion only of the *vis viva* of the molecules.

Although the variety in the mode of impact is infinite, it is certain that one direction of motion is as likely as any other, and hence, that the opposite of any direction is equally probable to the direction itself.

Let us confine our attention to any single case of impact and suppose that the directions of the motions of the two impinging molecules lie on one side of the plane of concurrence, then it appears that the nature of the impact must be *overtaking*. Again, let us suppose that they lie similarly disposed on the other side of the plane; the nature of the impact is again *overtaking*. Now, instead of having the opposite of both the original lines of motion, suppose the opposite of one only is taken; it is clear that the nature of the impact is in this

case of the *meeting* kind; and the opposite of the other line of motion being taken while the first is in its original position, the impact is again of the meeting kind.

Each of these four cases are equally probable, and the resolved velocities, or the values of $\beta$ and $\delta$, are the same in all, but two are meeting impacts and two are overtaking, each couple having perfectly distinct numerical equations to define the relation between the incident and reflected *vis viva*.

We are thus obliged to infer that the intestine action of the medium must be viewed in this manner as divided into two kinds of impacts specifically distinct in the numerical relation that subsists between the velocity before and after concurrence, and when employing the equations for summing up the results of the whole indefinitely great multitude that take place in mixed media, the effect of any one *meeting* impact must be considered along with its counterpart overtaking impact with the same velocities.

§ 10. We have remarked that it is only the resolved portion of the whole *vis viva* of a molecule that is dealt with by the equations—that forms the force of impact—and it may be questioned whether the mean of these forces in each kind of molecules bears the same proportion to each other as the whole *vis viva* of each. That the ratio is the same is best seen by the *reductio ad absurdum* method of reasoning. If the ratio is different, the motions of the heavier molecules must be resolved in a different way from those of the lighter, or the plane of concurrence must incline to one set of molecules in a different manner to that of the other set. Now any such effect is quite inconsistent with the fundamental hypothesis, and would require us to admit the influence of a modifying power whose nature and mode of bringing about the effect in question is unknown.

The ratio $\rho$ of the resolved to the absolute *vis viva* is actually one-third, and will become obvious in the next section, but it seems needless to require the demonstration in this place as all that we have to be assured of is the constancy of the ratio, whatever its actual value may be.

In seeking to demonstrate the nature of the *vis viva* equilibrium, the solitary condition that we have to reason

from is that the mean value of $(\beta_0^2 + \beta_1^2)$ is equal to $2\beta^2$ (§ 2) and the mean value of $(\delta_0^2 + \delta_1^2) = 2\delta^2$. That this is a necessary condition is obvious, because if either were less there would be a continual transfer from the molecules B to the molecules D, or from D to B and *vice versa*.

By squaring the equations in § 2 and adding, we have the following:—

$$\beta_0^2 + \beta_1^2 = 2\{\beta^2 - \frac{4D}{B+D}\cdot\beta^2 + \frac{4D^2}{(B+D)^2}\cdot(\delta^2+\beta^2)\}$$

$$\delta_0^2 + \delta_1^3 = 2\{\delta^2 - \frac{4D}{B+D}\cdot\delta^2 + \frac{4B^2}{(B+D)^2}\cdot(\delta^2+\beta^2)\}.$$

If in any case it happens that $\beta_0^2 + \beta_1^2 = \beta^2$, we shall have

$$\beta^2\left(\frac{4D}{B+D}\right) = \frac{4D^2}{(B+D)^2}\cdot(\delta^2+\beta^2),$$

or $\beta^2 B = \delta^2 D$, and $\delta_0^2 + \delta_1^2 = 2\delta^2$. Hence, if the squares of the impinging velocities happen to be in the inverse ratio of the molecular weights, then in either molecule the sum of the *vis viva* of the twofold encounter (one *meeting*, the other *overtaking* with the same impinging velocities) before impact, or $2\beta^2$, is equal to the sum after impact, or to $\beta_0^2 + \beta_1^2$.

But this is only one case out of an infinite number where the ratio is different. Generally, we may express the equation thus:

$$\beta_0^2 + \beta_1^2 = 2\beta^2 + p, \text{ and } \delta_0^2 + \delta_1^2 = 2\delta^2 + q.$$

Now, suppose that in an indefinitely great multitude of impacts the sum of all the individual values of $\beta_0^2 + \beta_1^2$ and $\delta_0^2 + \delta_1^2$ are taken, we shall have the mean of the values of the first equal to $\beta_m^2\rho$, in accordance with the necessary condition of permanence noticed above (by $\beta_m^2$ we mean to denote the mean molecular *vis viva* or mean square velocity of the B molecules, etc.). The mean value of $\beta^2 + p$ is, therefore, $\beta_m^2\rho$, but the mean value of $\beta^2$ is also evidently equal to $\beta_m^2\rho$, as above; hence, the mean value of $p$ is 0, or the positive values of $p$ balance the negative values. In the same way, it may be shown that the mean of the values of $q$ is also 0. Hence, we deduce that $\beta_m^2 B = \delta_m^2 D$, or that *in mixed media*

*the mean square molecular velocity is inversely proportional to the specific weight of the molecules* * . . . . VII.

This is the law of the equilibrium of *vis viva.*

§ 11. Thus, it appears that the inverse ratio of the specific molecular weight is that which is naturally assumed by the mean square molecular velocity of media in contact, and according to the foregoing reasoning (§ 10), this is also the ratio that ensures an equilibrium of pressure between media of the same density, or which have the same number of molecules contained in the same volume. Thus, by combining V. with VII. we deduce that *media in equilibrium of pressure and* vis viva *are of equal density, or have specific gravities respectively proportional to their specific molecular weights* VIII.

§ 12. We may likewise remark that as the mean value of the product $\beta^2 B$ is equal to the mean $\delta^2 D$, or $B_m^{\,2} = \delta_m^{\,2} D$, *there is the same amount of* vis viva *or mechanical force contained in equal volumes of all media that are in equilibrium of pressure and* vis viva . . . . IX.

§ 13. If different media are placed in contact they must diffuse themselves through their common volume with velocities proportional to their mean molecular velocity; but this velocity being in each inversely as the square root of its specific molecular weight, which is equal to the square root of its specific gravity, we may deduce, by combining VI. with VII., that *media in equilibrium of pressure and* vis viva *diffuse themselves through their common volume with velocities inversely proportional to the square root of their specific gravity* . . X.

§ 14. Such are the principal points by which different media are related to each other. Their analogies to the properties of gases may be stated as follows:

(1) The specific gravities of gases of the same *temperature* and pressure are respectively proportional to their atomic weight. [The combining equivalents or proportions may be

* [This is the first statement of a very important theorem. (See also *Brit. Assoc. Rep.*, 1851.) The demonstration, however, of § 10 can hardly be defended. It bears some resemblance to an argument indicated and exposed by Professor Tait (*Edinburgh Trans.*, vol. 33, p. 79, 1886). There is reason to think that this law is intimately connected with the Maxwellian distribution of velocities, of which Waterston had no knowledge.—R.]

viewed as simple multiples or divisors of the atomic weight or specific gravity.]

This is analogous to the VIII. deduction. Media in equilibrium of *vis viva* and pressure have specific gravities proportional to their molecular weight. It will be remarked that here again we have temperature represented by *vis viva*.

(2) It is considered as almost proved that gases in equilibrium of pressure and temperature have, in equal volumes, the same absolute quantity of heat.

We have deduced, in reference to media, that when they are in equilibrium of pressure and *vis viva* they have, in equal volumes, the same amount of *vis viva*.

(3) It is considered probable that the quantity of heat contained in equal volumes of all gases in equilibrium of pressure and temperature is proportional to their absolute temperature; and, in general, that equal increments of heat cause equal increments of temperature or expansion.

In all media in equilibrium of pressure and *vis viva* the same increment of *vis viva* causes the same expansive effect (see also § 6).

(4) Gases, however different in specific gravity, when placed in connection diffuse themselves equally through each other; and according to the elegant induction of Dr Graham (*Edin. Trans.*, 1831) the velocity of diffusion is inversely as the square root of their specific gravity.

This is very exactly responded to by the hypothesis. Dalton's law of diffusion belongs to media as a necessary and the most obvious consequence of the constitution assigned to them (VI.). Dr Graham's law of diffusive velocity or volume applies also to media, because if placed in similar circumstances such an effect must depend on the molecular velocity, which, singularly enough, bears exactly the like ratio to their specific gravity (X.).

§ 15. In the first point of analogy it was stated the atomic weight of a gas corresponded with its specific gravity, but with the reservation that the combining proportions are simple multiples or quotients of the same. This appears to me to be the fair statement of the remarkable connection that is always found between the combining volume and combining weight. It seems impossible that the fact of a volume of every gas containing the same number of molecules can ever be inductively

established, but all analogy leads us to this conclusion. One volume of oxygen combines with two volumes of hydrogen to form two volumes of aqueous vapour. If we inferred from this that one molecule of oxygen combined with two molecules of hydrogen to form one of steam, we must admit that the molecule of steam occupies double the volume of a molecule of hydrogen or oxygen. If it is admitted, on the other hand, that the oxygen molecule is capable of disintegration, and that half a molecule combines with one of hydrogen to form one of steam, the bulk of the three molecules are equal. This last is the view that is responded to by the hypothetical media. The objection to it is plausible from the natural repugnance to the idea of dividing what has been considered as an ultimate element into parts, and of supposing it possible that an element should have a strong affinity to itself—for this point is also involved. Half molecules of oxygen must have a powerful attraction to each other as they never appear separate. It is the same with all the other simple gases and vapours that combine in half or other fractional volumes. We have an analogous example among the hydrocarburets. Methyl is half a molecule of olefiant gas, and enters into distinct combinations similar in every respect to those of olefiant gas, but it never appears in a separate form. Olefiant gas enters into combination with itself in various proportions, forming etherine, ceten, elaen, etc., all of which are isomeric but have different specific gravities as vapours. These the chemist is unable to form directly; they are organic products and show a great molecular capability that is undoubtedly common to all bodies.

There has certainly as yet been no recognised example of a simple element appearing in the gaseous form with two specific gravities, but it would be rash to affirm that such was either impossible or absurd. Indeed, chlorine, bromine, iodine, and fluorine are so similar in their chemical qualities that it is not very unlikely they may be examples of isomeric combinations of one base in which condensation of atomic volume takes place simultaneously with increment of atomic mass. In the hydrocarburets, on the other hand, there is little, if any, condensation of atomic volume, their specific gravity in the liquid form being all nearly the same.

A strong argument in favour of the physical origin of the law of volumes is to be obtained from the combination of acids with ether. Some of these rise up into vapour in the same state of chemical union as when liquid, while others quite similar are decomposed in the act of rising and occupy double the volume of the others. Even those that are fully combined when they first rise, by an increase of temperature not very considerable, are decomposed and immediately assume a double volume. Oxalic and nitrous ethers are examples of the last kind. Sulphate and nitrate of pyroxilic ether are examples of disunion in the act of rising.

If the hypothetical law of volumes is true in all cases, we should have to distinguish between atomic weight and combining proportion. Thus, if the atomic weight of oxygen is 1 (or specific gravity of gas), its combining proportions are $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, $3\frac{1}{2}$ (?). If the atomic weight of hydrogen is 1, its ascertained combining proportions are $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, 3, 4, 5, 6, 8, 9, 16. If the atomic weight of nitrogen is 1, its combining proportions are $\frac{1}{2}$ and 1. The same for chlorine is $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$; for bromine they are $\frac{1}{2}$, 1, $1\frac{1}{2}$; for iodine, $\frac{1}{2}$, 1, $1\frac{1}{2}$; for arsenic, $\frac{1}{4}$, 1; for sulphur, $\frac{1}{6}$, $\frac{1}{3}$; for phosphorus, $\frac{1}{4}$, etc. These are necessarily derived from the specific gravity of the simple gases and of their compounds.

The labours of Dumas, Mitscherlich, Regnault, and Bineau, have extended the list of gases and vapours, whose specific gravities have been accurately measured, to nearly 150. Such determinations throw a light upon the atomic constitution of compounds which it is impossible to obtain from their mere chemical analysis, and form an important guide to theoretical discussion, when the arithmetic of volumes is properly applied.

## Section III.—On the Phenomena that Attend the Condensing and Dilating of Media, and the Mechanical Value of their Molecular "Vis Viva."

§ 16. In the first section a distinct idea of the elastic force of the medium was obtained by viewing it as a rapid succession of impacts on the lower surface of a gravitating elastic plane; and the equilibrium as being maintained when the upward

velocity given to the plane by the shock of one molecule was equal to the downward velocity given to it by gravity acting through half the infinitesimal portion of time that elapses between two successive impacts. During the first half of this time gravity acts in destroying the upward velocity: during the second half it acts and generates the same velocity downwards, and by applying the equation for the meeting impact (§ 2) we found the relation between the pressure and the number of impacts in a given time. This relation is expressed by $gn/2w = A$, in which $n$ is the number of molecules whose aggregate weight is the weight of the plane supported by A number of molecular impacts in a second of time: the impinging velocity being $w$, and $g$ the accelerating force of gravity. It was also shown that the upward velocity given to the plane by one impact is $w/n$, and this is likewise the descending velocity with which it encounters the molecular shock.

We have now to examine the case where the encounter takes place with the plane at rest. Applying the equation for the meeting impact as in § 2, and putting $\epsilon = \beta = 0$; $n = B$; $D = 1$; $w = \delta$; we have

$$\epsilon_0 = \beta_0 = \frac{2w}{n+1} = \text{velocity upwards of the plane after the shock;}$$

$$w_0 = \delta_0 = w - \frac{2wn}{n+1} = -w + \frac{2wn}{n+1} = w - \frac{w}{n+1}$$

= velocity downwards of the molecules after the shock.*

Thus, $n$ being an indefinitely great number, we have the ascending velocity of the plane $\epsilon_0 = 2w/n$, being double what it was in the former case when the result of the impacts was statical equilibrium; and the decrement of molecular velocity $= w - w_0 = w/n$, which is a new and important feature. In

* [It is easy to see that in the case supposed $n\epsilon_0 = 2w$, when $n$ is great, so that the velocity of the plane is $2w/n$; but in the next step there is an unfortunate error which runs through many of the subsequent deductions.

$$w_0 = \delta_0 = w - \frac{2wn}{n+1} = -\left(-w + \frac{2wn}{n+1}\right) = -\left(w - \frac{2w}{n+1}\right),$$

not $-\left(w - \frac{w}{n+1}\right)$. The *vis viva* expended in raising $n$ to the height $2w^2/gn^2$ is thus $4w^2/n$, not $2w^2/n$.—R.]

the former case there was no decrement of molecular velocity: the molecule and plane continually meeting and retreating with velocity of impact and reflexion the same, and inversely proportional to their respective weight.

With its velocity $\frac{2w}{n}$ the gravitating plane ascends to the height $\left(\frac{2w}{gn}\right)^2 \frac{1}{2}g = \frac{2w^2}{gn^2}$, so that the weight of $n$ molecules is raised through this height by the decrement $w/n$ of the impinging velocity $w$ of one molecule. Employing the differential notation, $dw = w/n$; and $2dw\,w = 2w^2/n = dw^2$, the *vis viva* expended in raising $n$ to the height $2w^2/gn^2$.

§ 17. If we recur to § 4 we may remark the necessity of considering molecular velocity in two points of view when applying the arguments of §§ 2 and 16 to an enclosed volume of a medium. The first point is that upon the molecular velocity of impact depends the intensity of the shock on the plane, the ascending velocity given to it, and therefore also the time between the impacts, if the weight of the plane is considered constant. Thus as any one velocity is to the time of ascent and descent of the plane caused by that velocity, so is the mean of the impinging velocities to the mean of the time intervals, or inversely, as the number of impacts in a unit of time; and the equilibrium does not require that the succession of impacts should be regular; the rapidity of the succession may fluctuate, but the average time and velocity must be constant. The second point is that any augmentation of velocity causes an increase in the frequency of the encounters (§ 4). In the equation $ng/2w = \mathrm{A}$, if A were not a function of $w$ it would remain unchanged, if $n$ and $w$ increased or diminished in the same proportion; but it was shown in § 4 that it was proportional to $w$, when $\Delta^3$, the density, is constant, and to $\Delta^3$ or $n$ or $c$ when $w$ is constant; hence $\mathrm{A} = wc\,\Delta^3$, in which $c$ is a constant factor that has to be determined. We have also to determine the ratio between $w^2$, the mean square impinging velocity, and $v^2$, the mean square absolute molecular velocity, in the equation $ng/2w = \mathrm{A} = wc\,\Delta^3$, or $n = \frac{2}{g}\,wc\,\Delta^3$.

Suppose the unit of volume in which the medium is confined to be a cube, the upper side of which is the plane $n$, and let $v^2$ be the mean square velocity of the molecules, so that if the squares of the respective velocities of all the molecules be added together, the sum will at all times be equal to $\Delta^3 v^2$. Resolve the motion of each molecule at any instant into the six rectangular directions parallel to the side of the cube and add up the squares of the resolved velocities that are perpendicular to one side; it is evident that the sum must be $\frac{1}{6}\Delta^3 v^2$, as the force is equally distributed in every direction, and in the stratum of the medium next the plane $n$ one-sixth of the force of the molecules that happen to be in the stratum at any given instant is directed perpendicularly upon the plane. Suppose the breadth of the stratum is $1/\Delta$, the number of molecules that at all times are to be found moving in it is $\Delta^2$, and half of these are diminishing their distance from the plane, and half increasing their distance with the mean square velocity $\frac{1}{3}\, v^2$.

The molecules moving equally in every direction must necessarily impinge equally in every possible direction on the plane, so that if their lines of motion were brought from every point of the surface of the plane where they impinge and made to issue from one central point, they would radiate equally to every part of the hemisphere; and as soon as those belonging to any one direction have impinged and thus withdrawn from forming part of the constant aggregate force $\frac{1}{6}v^2\,\Delta^2$, their place is immediately supplied by others of the same entering the stratum. The time taken by the set whose velocity is $u$ and inclination to the plane $\theta$ to traverse the breadth of the stratum is evidently as $\dfrac{1}{u \sin \theta}$, and in a unit of time the number of the impacts in the succession of these belonging to the set is proportional to $u \sin \theta$. But this is the value of the resolved velocity of the set. Referring back to the reasoning in §§ 2, 16, the supporting effect of each impact on the heavy plane $n$ was shown to be proportional also to the velocity of impact or molecular velocity resolved perpendicular to the plane. The supporting effect of each set in a unit of time is therefore as the square of the resolved or impinging

velocity of the set. But the mean of all the square impinging velocities is $\frac{1}{3}v^2$, and half the molecules in the stratum are continually approaching the plane; the supporting effect of their continuous action is therefore the same as would be derived from the medium reduced to half density advancing against the plane with the uniform velocity $\sqrt{\frac{1}{3}v^2}$. Now as $\Delta^3$ represents the number of molecules in the cubical unit of volume, the side of the cube being the unit of length, and $\sqrt{\frac{1}{3}v^2}$ the number of such units traversed in a unit of time, the supporting effect of the medium on the heavy plane $n$ in the unit of time, is the same as that derived from $\frac{1}{2}\Delta^3\sqrt{\frac{1}{3}v^2}$ molecules impinging with the velocity $\sqrt{\frac{1}{3}v^2}$. Hence it is obvious that $A = \frac{1}{2}\Delta^3\sqrt{\frac{1}{3}v^2}$, and $w = \sqrt{\frac{1}{3}v^2}$, or $c = \frac{1}{2}$, and $3w^2 = v^2$. Also $n = \frac{2}{g}w^2c\Delta^3 = \frac{v^2\Delta^3}{3g}$; or $v^2\Delta^3 = 3gn$; or $v = \sqrt{3g\frac{n}{\Delta^3}}$.

Thus we obtain an expression for the square root of the mean square molecular velocity in terms of the height of a uniform atmosphere $\frac{n}{\Delta^3}$, or what is the same, in terms of the ratio of the number of molecules in a column of the medium of the height of a uniform atmosphere to the number in the column of a unit height: and since $\sqrt{2gh}$ expresses the velocity acquired by a body in falling through the height $h$, we arrive at the following deduction. *The mean square molecular velocity of a medium is equal to the square of the velocity that a body acquires in falling through one-and-a-half times the height of a uniform atmosphere;* if the pressure of the medium is estimated from the effects of the molecular impacts on a perfectly rigid and elastic surface. If it were estimated by the effect on molecular elastic surfaces, there is reason to believe that the mean square velocity is double the amount specified (see Sec. 4) . . . . . XI.*

§ 18. Suppose that the cubical volume of the medium receives such an increment of *vis viva* that under the constant pressure $n$ the volume from 1 becomes $1 + \frac{2w^2}{gn^2}$; it has been shown in § 5 that the molecular *vis viva* must also from 1 have

* [The author here arrives at the correct conclusion, $v^2 = 2q \cdot \frac{3}{2}h$.—R.]

become $1 + \frac{2w^2}{gn^2}$ that it may sustain the same pressure with the reduced density. But the molecular *vis viva* in the cubical volume is $\Delta^3 v^2$: hence we have the proportion

$$1 : 1 + \frac{2w^2}{gn^2} :: \Delta^3 v^2 : \Delta^3 v^2 \left(1 + \frac{2w^2}{gn^2}\right);$$

the increment of *vis viva* in the contained medium is thus $\Delta^3 v^2 \frac{2w^2}{gn^2}$. But it has been shown in § 17 that $w^2 = \frac{1}{3}v^2$, and $gn = \frac{1}{3}v^2\Delta^3$, hence $\Delta^3 v^2 \frac{2w^2}{gn^2} = \frac{2v^2}{n}$.

Now in § 16 it was shown that to raise the weight of $n$ molecules to the height $2w^2/gn^2$ the *vis viva* expended was $2w^2/n = \frac{2}{3}v^2/n$; comparing this, the *vis viva* expended in the act of increasing the volume, with $2v^2/n$, the increment of *vis viva* required to support the increased volume, it is evident that the ratio is exactly one-third. Thus we deduce that *to effect one increment of expansion in the volume of a medium subsisting under a constant pressure, four-thirds of an increment of* vis viva *are required: one of which thirds is expended in the act and does not appear in the medium: the remaining three-thirds, or one increment, appears in the medium and assists in sustaining its augmented volume* * . . . . . XII.

§ 19. This result is a necessary consequence of the perfect conservation of *vis viva* in the impinging action of perfectly elastic bodies. To enlarge a volume that is pressed upon is to raise a weight; is to expend mechanical force; is to expend molecular *vis viva*, and the last train of reasoning has led us to the relation between the molecular force expended and the work performed by it.

The mechanical value of the whole of the *vis viva* of the medium may be ascertained by the following proportion:

$\frac{2v^2}{3n} : \frac{2v^2}{3n^2} :: \Delta^3 v^2 : \frac{\Delta^3 v^2}{gn}$. But $gn = \frac{1}{3}\Delta^3 v^2$, therefore $\frac{\Delta^3 v^2}{gn} = 3$.

* [The corrected version of XII. will be "To effect one increment of expansion in the volume of a medium subsisting under a constant pressure, *five*-thirds of an increment of *vis viva* are required; *two* of which thirds is expended in the act, and does not appear in the medium; the remaining three-thirds, or one increment, appears in the medium, and assists in sustaining its augmented volume."—R.]

Thus the *vis viva* expended in raising $n$, or the constant pressure, through the increment of the unit of volume, is to that increment as the whole *vis viva* of the medium is to three units of volume. Hence we deduce that if a medium is supposed to expand to four times its original volume with its original tension undiminished, it will in doing so expend as much as the whole of its original molecular force. This may be more concisely expressed by: *The molecular* vis viva *of a medium is equal to its tension acting through three times its volume* * . . . . . XIII.

§ 20. If a medium is not allowed to increase in volume while its *vis viva* is increasing, no force will of course be expended, and each increment of *vis viva* engenders a like increment of tension. Thus *if we compare the amount of* vis viva *required to produce an increment of molecular* vis viva *in the medium, in the two cases of constant pressure and constant volume, it is manifest that the ratio is* 4 *to* 3, *or* 4/3† XIV.

§ 21. If we suppose the heavy plane $n$ instead of being raised by the medium to descend upon it through the same differential height $2w^2/gn^2$, it is obvious that the same differential *vis viva* that was formerly abstracted is now communicated to the molecules of the unit volume. Force is exerted by the descending weight upon the medium and is transferred to its molecules.‡ Thus it is evident that the conversion of mechanical force into molecular *vis viva* is subject to the same law as the conversion of molecular *vis viva* into mechanical force. This law is expressed in XII. and XIV. The following is another form of annunciation which refers to an experimental method of ascertaining it if such media were actual existents. *The ratio of the increment of* vis viva *evolved by a small condensation of a medium to the diminution of molecular* vis viva *required to maintain the same condensation under a constant pressure is* $\frac{1}{3}$ § . . . . . XV.

* [This is the virial equation applicable under the supposed conditions.—R.]

† [The ratio of specific heats, commonly called $\gamma$, should be 5 : 3, not 4 : 3.—R.]

‡ Note A (motion indestructible as matter).

§ [This result also requires correction.—R.]

§ 22. If a medium is compressed or dilated and the molecular *vis viva* evolved in it or given out from it by the act of condensation and dilation be retained, let us inquire into the ratio of the density to the pressure. The preceding reasoning has shown that the increment or decrement of *vis viva* is equal to one-third of the increment or decrement of density, or $\frac{dv^2}{v^2}=\frac{d\Delta^3}{3\Delta^3}$; hence $\frac{2dv}{v}=\frac{d\Delta}{\Delta}$, which being integrated gives $v^2 \doteqdot \Delta$. But $v^2\Delta^3=e$, therefore $\Delta^4 \doteqdot e$, and $v^2 \doteqdot \sqrt[4]{e}$. Thus we deduce that *if a medium is compressed or dilated from a given condition of density and* vis viva, *the mean square molecular velocity varies as the fourth root of the tension or as the cube root of the density* * . . . . . XVI.†

§ 23. The tendency of media to have their *vis viva* augmented when being forced into smaller volume is very similar to the rise of temperature that appears in air when being condensed. Thus tinder may be inflamed by the sudden compression of a small quantity of air, and on charging an air gun the condenser and force pump become so hot as to be painful to touch. Again, mercury may be frozen if exposed to a jet of air escaping from a state of high compression and expanding against the atmospheric pressure. Media also lose their *vis viva* if allowed to expand against pressure.

Thus by XVI., if at 60° Fahr. a vessel containing air of double density is allowed to empty itself into the atmosphere, the decrement of temperature in the air that remains ought to be − 48° by the XVI. deduction. If on the contrary its density is suddenly increased from 1 to 2, its temperature ought to rise to 196°.‡

Thus the analogy between media and gases and between *vis viva* and temperature is still maintained, and the phenomena of latent heat in gases appear also in media as the transference of force during a change of volume: *out* of the medium when it expands, and exerts, or gives out, mechanical force; *into*

* Note B (vapours).

† [The corrected argument is:—Since $dv^2/v^2 = \frac{2}{3}\, d\Delta^3/\Delta^3$, we get $dv/v = d\Delta/\Delta$, or $v \doteqdot \Delta$. Accordingly $v^2 \doteqdot \Delta^2 \doteqdot (\Delta^3)^{\frac{2}{3}}$. But $v^2\Delta^3 = e$; therefore, $\Delta^5 \doteqdot e$, and $v^2 \doteqdot e^{\frac{2}{5}}$. Also $e \doteqdot (\Delta^3)^{\frac{5}{3}}$, or $p \doteqdot \rho^\gamma$, where $\gamma = \frac{5}{3}$.—R.]

‡ Note C (temperature of condensed air).

the medium when it is compressed and acted upon, or receives mechanical force.

The phenomena of latent heat thus appear to be the conversion of mechanical force into molecular *vis viva;* the visible into the invisible, as in condensation: and molecular *vis viva* into mechanical force; the invisible into the visible, as in expansion.

It is a necessary consequence of the conservation of *vis viva* or indestructibility of force among perfectly elastic bodies.

§ 24. Several experiments have been made on the *ratio* of the increment of temperature evolved by a small condensation of a volume of air to the diminution of temperature required to produce the same condensation under a constant pressure, and found it to be $\frac{3}{8}$. Mr Ivory (*Phil. Mag.*, 1827) has proved that this ratio is constant under every change of temperature or density so long as Dalton and Gay-Lussac's law is maintained, or the air thermometer is an exact measure of heat. MM. Gay-Lussac and Welter have also proved this experimentally for a considerable range. Mr Ivory has also expressed his opinion that the nascent value of this ratio will be found to be $\frac{1}{3}$, and that the cause is probably connected with the proportion that subsists between the linear and the solid increments of expansion.

The value of this ratio in all media, whatever may be their condition of density or *vis viva,* is $\frac{1}{3}$ (§ 21); and the synthetical demonstration rests on the same fundamental principle that determines the proportion of linear to solid increments of expansion.

This ratio $\frac{1}{3}$ applies only to infinitesimal changes of volume, and it slowly increases with the amount of condensation. When the medium is compressed from 1 to 1·20 the ratio becomes $\frac{3}{8}$.*

In the experiments of MM. Gay-Lussac and Welter referred to in the *Mécanique Céleste,* the condensation did not exceed $\frac{1}{10}$ part of an atmosphere. The discrepancy may be exactly ascertained by performing the same experiments as it were on the medium by computation. The absolute temperature is

* Note D (to find the compression that corresponds with a given ratio between latent and sensible heat.)

denoted by $v^2$ and the absolute zero corresponds to $-461°$ of Fahr. scale (taking Rudberg's constant of expansion, see § 6). By employing the equation $e = v^2\ \Delta^3$ and $\left(\frac{e_0}{e_1}\right) = \frac{{v_0}^2}{{v_1}^2}$; (XVI.), it is easy, by substituting the barometric height for $e$, to compute the result of any given experiment as if it had been performed on the hypothetical medium. This I have done in the case of MM. Clément and Désormes' experiments. Referring to the account given of them in the *Mécanique Céleste*, the minus interior pressure of the medium would have been 3·42 millims.; the mean result is given as 3·61, the difference being only about $\frac{1}{130}$ part of an inch of mercury. If the experiment gave the nascent ratio, the minus pressure at the end would have been one-fourth of the minus pressure at the beginning, or 3·45 millims. In MM. Gay-Lussac and Welter's experiments, the difference of pressure at the end was 4·44 millims.; the nascent ratio in a medium would in a like experiment be 4·09 millims., the difference being about $\frac{1}{70}$ of an inch of mercury.

The evidence afforded by these experiments may be summed up as follows:—

The initial ratio of the increment of *vis viva* under a constant volume to the increment required to effect the same change of *vis viva* under a constant pressure in the medium is . $\frac{1}{1·33}$.

In air the same ratio of the increments of heat under the same circumstances by MM. Clément and Désormes' experiments, is . . . . . $\frac{1}{1·35}$.

In air the same ratio, by MM. Gay-Lussac and Welter's experiments, is . . . . . $\frac{1}{1·37}$.

A difference in the reading of the height of the mercury in the manometer of $\frac{1}{130}$ and $\frac{1}{70}$ part of an inch would bring the respective experiments to coincide with the theory.*

* [The fair agreement of the erroneously deduced value of $\gamma$, viz., $\frac{4}{3}$, with observation, was doubtless the reason of the author not discovering his mistake of calculation. We know that upon his principles the calculated value should be $\frac{5}{3}$, which accords much less well with the results observed for ordinary gases than does $\frac{4}{3}$. It should be borne in mind that the observed value, $\gamma = 1·405$, has not, even at the present time, been reconciled with theory, although reasons may be given for a departure from $\gamma = \frac{5}{3}$.—R.]

§ 25. In the more recent of Mr Joule's physical researches that gentleman has applied mechanical force to the compressing of air surrounded with water, to collect the heat evolved, and has found that about 800 lbs. descending through the height of one foot increases the temperature of a pound of water one degree. The same result nearly was obtained by forcing water through narrow tubes. Mechanical force was expended, and the same proportionate amount of heat was produced in the water. It is remarkable that the same mechanical value was found for the heat generated by the magneto-electric machine. Such accordance in the results, as Mr Joule remarks, seems strongly to favour the *vis viva* or vibratory theory of heat.

If air is similar in its constitution to a medium we may employ the deductions of this section to determine the mechanical value of any quantity of heat applied to it, and assuming the specific heat of air to be 0·238 that of water,* we may ascertain the mechanical value of 1° applied to 1 lb. of water, which is equal to $\frac{1}{0\cdot238}$ degree applied to 1 lb. of air. Since 820 cubic feet of air at 60° and 30 in. tension weigh as much as one cubic foot of water, we have $\frac{820}{0\cdot238} = 3444$ cubic feet of air which, heated one degree without being allowed to change its volume, requires as much heat as one cubic foot of water to raise it one degree. The absolute temperature at 60° is 520°, and one degree added augments by $\frac{1}{520}$ part the absolute heat or molecular *vis viva* of the air. But the whole *vis viva* in 3444 cubic feet of air at the temperature 60° and pressure 30 in., is equal to the whole pressure of the atmosphere on a square foot, acting through three times 3444 feet in height, or 10,332 feet (XIII.). The pressure of a column of 30 inches of mercury on a base of 1 square foot or 144 square inches is equal to 14·722 lbs. × 144 = 2120 lbs. This weight raised through 10,332 ft., corresponds to 21,904,000 lbs. raised one foot high, and $\frac{1}{520}$ part of this, or 42,043 lbs., raised one foot high represents the absolute mechanical effect of 1° of heat applied to one cubic foot of water. Dividing this by 62½, the number of lbs. in a cubic foot of water, we get 673 lbs. raised one foot

* Note E (specific heat of air).

high equal to the mechanical effect corresponding to 1° of heat applied to 1 lb. of water. This compared with Mr Joule's result is not unsatisfactory considering the difficulties that attend the experiments that afford the data.*†

## Section IV.—On the Resistance of Media to a Moving Surface.‡

§ 26. The simplest case of resistance is that attending the motion of a rigid and perfectly elastic plane moving in the direction of its perpendicular.

Let the velocity of its motion be $z$, which we must at first assume to be indefinitely smaller than $v$, the square root of the mean square molecular velocity. Let a molecule with velocity $u$, impinge on the front surface of the moving plane at an angle $\theta$; the impinging velocity is $u \sin\theta$; and applying the formula for the meeting impact (§ 2) the velocity of reflexion is $u \sin\theta + 2z$, and the square of this is $u^2 \sin^2\theta + u \sin\theta\, 4z + 4z^2$. The increment of molecular *vis viva* received from the plane is therefore $u \sin\theta\, 4z$. To find the increment given to the medium in a unit of time we have first to consider the number of impacts in the unit of time if the plane were at rest, and then the additional number owing to its motion. Referring back to the reasoning in § 17, it has been shown that the impinging velocity $u \sin\theta$ is repeated $u \sin\theta$ times in a unit of time; hence the increment of *vis viva* in the unit is $u^2 \sin^2\theta\, 4z$, and as the mean of all the values of

* Note F (M. Clapeyron's view of the motive power of heat examined).

† [This is an independent calculation of the mechanical equivalent of heat, quite distinct from that of Mayer.—R.]

‡ [The weak point in the argument of this section appears to be the neglect of the effect of the altered velocities of the reflected molecules in disturbing the condition of those about to impinge. The results can only apply when the dimensions of the obstacle are small in comparison with the free path of the molecules.

The non-agreement of his theory with observations upon the resistance experienced by obstacles which do not comply with the above condition, unfortunately led the author to take the step in the wrong direction explained in §§ 27, 28. But it is proper to note that the author speaks with hesitation (§ 29).—R.]

$w^2 \sin^2\theta$ is $w^2 = \frac{1}{3}v^2$, we have—following the reasoning in § 17, and supposing the surface of the plane to be equal to the side of the cube that contains $\Delta^3$ molecules—the increment of *vis viva given* to the medium by the front surface in a unit of time is $\frac{1}{6}\Delta^3 v^2 4z$. By applying the same reasoning to the back surface of the plane the same amount of *vis viva* is found to be taken from the medium on that side.

We have now to consider the additional number of impacts due to the motion of the plane. Let us first suppose that no change of density is caused by the motion. The action of a medium on a surface at rest is the same as that of a uniform current of molecules whose mean distance is $\sqrt[3]{\frac{1}{2}\Delta^3}$ and velocity $w$ (see § 17), and in the same manner the surface meeting this current with the velocity $z$, the effect is the same as if the velocity of the current were increased to $w+z$. The additional number of impacts due to the motion of the plane is therefore $\frac{1}{2}\Delta^3 z$, and the mean increment of *vis viva* to each being $4z$, the whole increment in the unit of time is $2\Delta^3 z^2$. The same reasoning applied to the action on the back surface shows that the diminution in the number of impacts is also $\frac{1}{2}\Delta^3 z$, and the mean decrement of *vis viva* caused by each impact being also $4z$, we have the decrement of *vis viva* in a unit of time also $2\Delta^3 z^3$. The sum of these $4\Delta^3 z^2$ is the force required to move the plane with the velocity $z$. The weight $n$, whose pressure is equal to this force, is found as in the last equation of § 17: there we had $w^2\Delta^3 = ng$, and in the same way here we have $4z^2\Delta^3 = n_0 g$, or $n_0 = \frac{4}{g}z^2\Delta^3$.

This result differs very much from the actual resistance of a body moving in air as observed by Robins and Hutton. In Hutton's Dictionary it is mentioned that the resistance to a surface of one square foot, moving 20 feet per second, was found to be 12 ozs., and that it increased as the square of the velocity. Now if we compute the resistance by the formula $\frac{1}{g}z^2\Delta^3 = n_0$, which is the common theory at low velocities, we shall find $n_0$ to be 15 ozs. nearly, when $z$ is 20 feet per second, the weight of a cubic foot of air being represented by $\frac{1000}{820}$ oz.

Here then is a notable discrepancy; the resistance of the

medium that represents air in specific gravity and tension appears to be four times greater than it ought to be.

§ 27. We have all along assumed, for the sake of simplicity and to avoid any addition to the fundamental hypothesis, that the surfaces upon which the medium acts are perfectly rigid as well as perfectly elastic, although no such surface, so far as we know, exists in nature. If molecular *vis viva* is heat, the molecules of solids must be in motion as well as those of gases; and the nature of the motion must be such as to permit the equilibrium of *vis viva* to be established between them respectively. They must also be perfectly elastic; but a surface composed of them cannot be assumed as perfectly rigid.

The original hypothesis in respect to gases involves the necessity of making certain assumptions respecting the physical condition of the surfaces upon which media are supposed to act.

(1) That they are composed of molecules in a state of vibratory motion which results from the struggle that their *vis insita* makes with the attractive and repulsive forces of aggregation.

(2) The nature of these vibrations and forces may remain undefined, further than the *vis insita* proper to a molecule is alternately destroyed at the extremities, and reproduced in the middle of each vibration when it and the surrounding molecules are in equilibrium of *vis viva*.

(3) The impact of the molecules of a medium on the molecules of the solid surface is that of perfectly elastic bodies, and enables the equilibrium of *vis viva* to be established between them.

This equilibrium must be effected by a continual interchange of *vis viva*, the molecules of the solid giving to the molecules of the medium and *vice versa*.

In the case of a heavy molecular plane supported by the elasticity of the medium, as detailed in § 2 and § 16, the impacts that take place on the lower surface establish both the molecular *vis viva* equilibrium, and also the statical equilibrium of the heavy plane.

Does this new condition of surface upon which the medium acts, make any change in the relation $ng/2v = A$, that was shown to subsist between the impinging velocity of succession,

and weight supported when the surface was assumed as perfectly rigid? Then, the molecules of the medium encountered at each impact the whole *vis insita* of the heavy plane and communicated directly to its centre of gravity a certain infinitesimal velocity. Now, it strikes a vibrating molecule which *afterwards* communicates a certain effect or infinitesimal velocity on the centre of gravity of the molecular plane. Let us endeavour to gain a clear idea of the numerical relation between the effect and the cause that produces it, viz., the impinging force of the free molecule of the medium.

The centre of gravity of the whole molecular plane being at rest while the centre of gravity of each of its molecules is in a state of intense vibration, it is evident that the track or orbit described by the centre of gravity of one of its molecules must be exactly imitated but on an infinitely reduced scale and in a reverse direction by the motion of the centre of gravity of the remaining molecules of the plane. The action and reaction of the molecular forces are equal. At any point of the orbit of one of the lower molecules of the plane let a free molecule of the medium impinge. If they are equal in mass they will exchange impinging velocities; this interchange modifies the vibration and disturbs the harmony between the simultaneous motions of the centres of gravity above referred to; the motion of the centre of gravity of the molecule changes suddenly, not so with the centre of gravity of the remainder of the plane. If the connection between the plane and the molecules were broken at the instant of impact, it is clear that the centre of gravity of the remaining molecules of the plane must continue to move in the direction and with the velocity it had at the instant. Now as the concurrence may take place at any part of the vibration, either going or returning, it is plain that the mean motion of the centre of gravity of the remainder of the plane caused by the transference of *vis viva* from the molecule of the plane to that of the medium is zero. But the centre of gravity of the remainder of the plane reciprocates the active effect of the molecular force on the new velocity until it is destroyed at the end of the first vibration; the molecular force acting as much on the remainder of the

plane as upon the molecule. The destruction of this by the mutual binding force destroys in the opposite direction the same amount of *vis insita* in the plane, or generates it in the same direction, and as we have to attend only to the effect upon the centre of gravity of the plane made by the motion transferred from the medium to the molecule of the plane, the mean effect must be equal to the mean incident *vis insita* of the molecules of the medium; in short, the same as if the striking molecule cohered to the plane after impact. This is the case if the plane is at rest when struck, but a condition of statical equilibrium requires that the infinitesimal descending motion by gravity should be equal to the ascending infinitesimal motion given by the impetus of the striking molecules. The upward velocity therefore given to the plane by this impetus is only one-half what it would be if the plane were at rest when struck (see § 2). Thus the expression $\frac{2v}{n+1}$ (see § 2) becomes $\frac{v}{n+1}$, and $v/n$ becomes $v/2n$, and $gn/2v = \mathrm{A}$ becomes $gn/v = \mathrm{A}$.

These alterations make no difference in the subsequent reasoning until we come to § 17 where the equation for A is employed, and in consequence of its change of value the terminal equation $\Delta^3 v^2 = 3gn$ is changed to $\Delta^3 v^2 = 6gn$.

This alters the value of $v$ from $\sqrt{3g\frac{n}{\Delta^3}}$, the velocity acquired in falling through one-and-a-half uniform atmospheres, to $\sqrt{6g\frac{n}{\Delta^3}}$, the velocity acquired in falling through three uniform atmospheres, and the numerical value of $v$ in the medium that corresponds with air at the temperature of melting ice is 2244 feet per second.

§ 28. As this change in the value of $v$ reconciles the discrepancy in the theory of resistance, and in the subjects of the two concluding sections, it may be proper to illustrate by diagram the general principle that the mean impinging effect of free molecules on a cluster of cohering molecules is the same as if the striking molecules cohered at the instant of impact.

Let P be the molecular plane consisting of $(n - 1)$ molecules, and Q one of its lower molecules at rest between attractive and repellent forces.

(1) Let Q receive an impulse in the direction GQ, so that its initial velocity may be QE $= v$; it will proceed along the line QA until its motion is subdued at some point A by the molecular repulsive force; at the instant when the centre of gravity of the molecule Q, and centre of gravity of P, are at their minimum distance both move together with the common velocity $v/n$, and this is the velocity communicated to their common centre of gravity, which is not disturbed further by their mutual action during the vibratory motion that ensues. In this case there is both molecular *vis viva* communicated

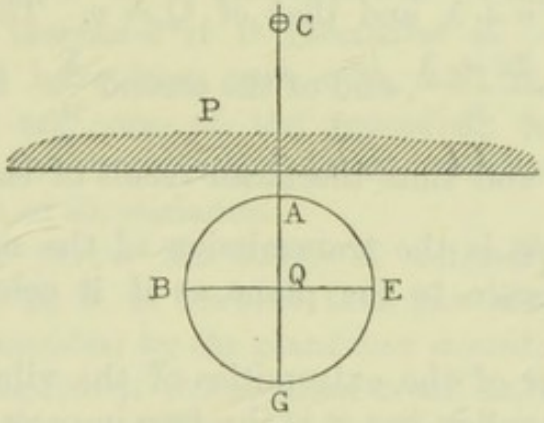


Fig. 1.

to Q, and also the velocity $v/n$ to the common centre of gravity, and a free molecule of the medium has lost the velocity $v$.

(2) Suppose, in the next instance, that the centre of gravity of P + Q or $n$ to be stationary, while Q is continuing its vibrations, and let it be struck when at the centre of its descending vibration by a free molecule having the same velocity QE. They will be reflected from each other without gain or loss of motion, and Q will move back towards A instead of forward to G. Here there is a loss of the down motion and the gain of an up motion. By the first (1) the centre of gravity of P continues to ascend with the velocity $\frac{v}{n-1}$ that it has at the instant of impact; and as the downward

force of Q that subdued this is gone, the centre of gravity of the whole P + Q or $n$ molecules acquires the permanent upward velocity $v/n$. By the second (2) the same velocity $v/n$ is given to the common centre of gravity by the gain of the up motion as in the first case of impact. The result of the second mode of impact is thus to communicate the velocity $2v/n$ to the common centre of gravity.

(3) If the same kind of impact takes place with Q in the middle of the ascending vibration, it is evident that the force of impact is zero at that point, and the upward velocity to the centre of gravity zero. Now $\left(\frac{2v}{n} + 0\right) \div 2$ (impacts) $= \frac{v}{n}$. This result is more obvious if we assume the velocity of the free molecules $v + \lambda$ and that of $Q = v$. The result of the first of these is $\frac{2v + \lambda}{n}$, and of the second $\frac{\lambda}{n}$. Half the sum of these is $\frac{v + \lambda}{n}$, and thus the mean result of these two equally probable impacts is the transmission of the *vis insita* of the impinging molecule to the plane as if it cohered to it after impact.

(4) At either of the extremities of the vibration the same law obviously applies, but it is the two impacts where *vis viva* is taken from and again returned to Q whose effects ought to be viewed together. In the meeting impact let the velocity of Q from $v$ change to $u$ with a different direction; then according to the second case of impact the upward effect on the plane is to give it the ascending velocity $\frac{v + \overline{v - u}}{n}$, and the loss of velocity is $\overline{v - u}$. Let this loss be returned in an overtaking impact so that $\overline{v - u}$ shall become $v$: then according to the first case of impact the upward effect on the plane is $u/n$. In these two impacts Q returns to its original condition of motion, and the mean effect is $v/n$. A continual and equal interchange of *vis viva* being necessary to the persistent molecular condition of the plane and of the medium, the same if effected by means of impacts which take place equally in the ascending and descending vibration. This

equality seems to be a necessary condition because the motions that are taken account of are the velocities of impact resolved in a vertical direction only, and the plane of impact cannot now be assumed always to be horizontal as in the case of the rigid plane; hence the absolute velocity in the vibration and the resolved impinging velocity are independent variables.

§ 29. Such is the view of the phenomena which seems to authorise the change that has been imposed on the value of the mean square molecular velocity. It has no pretension to be considered as a demonstration, and we are therefore not permitted to make use of it as a synthetical deduction from the hypothesis.

Nevertheless, if it is admitted as being probable, the probability is increased if it reconciles at once all the discrepancies that have been met with, and at the same time neither affects any one of the preceding deductions where the analogy to the properties of gases is perfect nor introduces any other point of discordance.

If we now revise the mode of estimating the law of resistance in § 26 it is obvious that the mean increment of velocity communicated by the plane now considered as molecular to the free molecules of the medium is not $2z$, but $z$, and hence the mean increment of *vis viva* in each incident molecule is not $4z$, but $2z$, and the increment in a unit of time not $2\Delta^3 z^2$, but $\Delta^3 z^2$. The sum of the front increment and back decrement is not $4\Delta^3 z^2$, but $2\Delta^3 z^2$; and as $w^2\Delta^3$ is no longer equal to $ng$, but to $2ng$, we have $2\Delta^3 z^2 = 2ng$, or $n = \frac{1}{g}\Delta^3 z^2$, which is the equation derived from the common theory of the resistance of the atmosphere at low velocities.

It will be remarked that the resistance is as much derived from the minus pressure behind as from the resistance in front, whereas the common theory only takes account of the inertia of the front which is assumed at low velocities as constituting the whole of the resistance.*

* Note G (objection to undulatory theory of heat).

## Section V.—On the Vertical Equilibrium of a Medium, Supposing it to form the Atmosphere of a Planet.*

§ 30. Suppose the height of the atmosphere AB to form the axis of a parabola of which the vertex A is at the summit. If a body begins to fall from A it is evident from the law of falling bodies that its acquired velocity at any point is proportional to the ordinate of the parabola at that point.

Divide AB into an infinite number of parts so that the length of each shall be proportional to the ordinate of the parabola at that part. Suppose that in each of these parts one molecule is vibrating upwards and downwards, striking against the upper and lower molecules of the adjacent parts with a velocity proportional to the ordinate of the parabola and equal therefore to what a body would acquire in falling from the vertex. It is evident that each of the parts or infinitesimal divisions will be traversed in the same time $dt$ by its molecule, and that the impinging velocities of each pair are equal, so that there is a perfect equilibrium and constancy of phenomena; but the upper impact of a molecule against the one above it is made with less velocity than the lower impact against the one below it, because the accelerating force of gravity increases the velocity during the interval of descent, and the acceleration is represented by the increment of the parabola's ordinate in that interval. If $g$ be the accelerating force of gravity, or velocity bestowed on a falling body every unit of time, the acceleration in each interval of descent, or infinitesimal division of the height AB, is evidently $gdt$. If

* [This section attempts to deal with one of the most difficult points in the theory. That the loss of velocity suffered by every ascending molecule will lead to a smaller mean velocity above than below seems, at first sight, inevitable. This consideration was urged by Guthrie (*Nature*, vol. 8, p. 67, 1873); and, in his reply (p. 85), Maxwell narrates that a similar argument, which occurred to him in 1866, nearly upset his belief in calculation. Waterston's result really depends upon an assumption that, at a given height, the molecular velocities are all the same; whereas according to the true Maxwellian law, all velocities are to be found at all heights. The force of this consideration will be appreciated when it is remembered that those molecules which at any time move at a low level with low velocities, would not of themselves reach a high level at all.—R.]

this constant increment of velocity should by any cause be reduced in any given proportion, the aggregate effect must evidently be the same as if the force of gravity $g$ were reduced in the same proportion.

In such a vertical column of single molecules it is apparent that the equilibrium acquires * a continually increasing velocity in the molecular motion from the summit to the base; and since the *vis viva* of a molecule is measured by the square of its velocity, it is also obvious that the molecular *vis viva* increases in the simple proportion of the distance from the summit. And knowing $v^2$ the amount of *vis viva* in the molecules at the base, we also know the height of the column $v^2/2g$, which is simply the height due to the molecular velocity.

§ 31. In a medium the nature of the action that sustains the upper molecules must be the same. The mean of the upper molecular impacts of a stratum must have less force than the mean of the lower, the difference being the accelerating effect of gravity in the breadth of the stratum. We have also to remember that the nature of the equilibrium of a medium requires that if the velocities of all the molecules that pass a horizontal or other plane in a given time are resolved perpendicular to that plane, the sum of the squares of these resolved velocities are equal in opposite directions. In a constant infinitesimal time the absolute acceleration of velocity is evidently greater with the vertically moving molecules than with those moving obliquely to the vertical; thus the aggregate effect of the accelerating force of gravity in increasing the molecular velocity must be less than if it acted upon them directly as in the vertical column (§ 30). The question is, how much less? for in such proportion must we consider the force of gravity to be reduced, supposing it to act uniformly on all the molecules of the atmosphere. The retardation of the ascending molecules of a stratum is equal to the acceleration of the descending molecules. Let us consider the latter.

We assume from the original hypothesis that in any infinitesimal area the lines of molecular motion lie equally in every direction, so that if supposed to issue from one point S, they would be directed equally to every point of the surface

* [? requires.—R.]

of a sphere of which S is the centre. Let SB represent one of these velocities and B$n$ the vertical acceleration by gravity in the infinitesimal time $dt$. With the radius SB describe a hemisphere having its base on the horizontal plane PS. It is evident that the locus of the point $n$ is the surface of another hemisphere with its base at the distance of B$n$ below the plane PS, and that the area of the space between the bases is equal to the area between the surfaces of the hemispheres. Now, if gravity acted on each line of molecular motion, instead of acting only in the vertical, the common increment of velocity that would affect all is B$p$ = B$n$ = $gdt$; and this, when B$n$/BS is infinitesimal, is equal to the quotient of the area between the concentric hemispheres $qpf$T, PBAR, by the surface of the inner

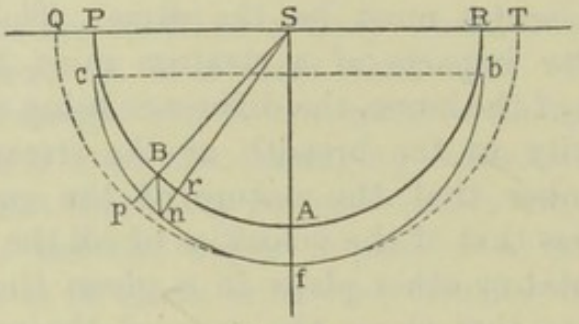


Fig. 2.

hemisphere. But the actual increase of BS is $nr$ = $n$S − BS, and the mean increment is found by adding up all the particular values of $nr$, and dividing the sum by the number of values, or, what amounts to the same thing, taking the quotient of the area between the equal hemispheres PBAR, $cnfb$ by the surface of one of them. Now the first is equal to the area between the bases, which is equal to the product of a great circle by B$n$, and the latter is well known to be equal to two great circles; therefore the quotient is $\frac{1}{2}$B$n$.

Thus, it seems to be clear that the molecules of a medium are collectively only half as much affected by gravity as if they all moved in vertical lines; but it has been shown (§ 30) that if they moved in vertical lines the height of the atmosphere would be the height due to the molecular velocity; but as the increments of their velocity by gravity is only one-half what they would receive if their motion were vertical, that height

must be computed as if the force of gravity were only one-half the actual amount. Thus, if $v^2$ be the mean square velocity at a depth H below the summit of the atmosphere, the height due to this with the full effect of gravity is $v^2/2g$, and with half effect it is $v^2/g = \mathrm{H}$. Thus we arrive at the following deduction: *The molecular* vis viva *increases simply as the depth below the summit of the atmosphere, and the height of the summit above any stratum is equal to the quotient of the mean square molecular velocity at that point by the accelerating force of gravity, or to double the height that a free and unresisted projectile would ascend if projected vertically with an initial velocity equal to the square root of the mean square molecular velocity in the stratum* . . . . . XVII.

§ 32. To ascertain the law of density we have the equation in § 17, modified as in § 27 to $\frac{1}{6}\Delta^3 v^2 = gn$, for the reasons given in last section. By this we have $v^2/g = 6n/\Delta^3 = \mathrm{H}$, which applies to any part of the atmosphere at all heights.

Differentiating the equation

$$\frac{6n}{\Delta^3} = \mathrm{H}, \text{ we have } \frac{6dn}{\Delta^3} - \frac{6n3d\Delta}{\Delta^4} = d\mathrm{H}.$$

But $\frac{dn}{\Delta^3}$ is evidently equal to $d\mathrm{H}$, and

$$\frac{6n3d\Delta}{\Delta^4} = \frac{6n}{\Delta^3} \times \frac{3d\Delta}{\Delta} = \mathrm{H}\,\frac{3d\Delta}{\Delta};$$

therefore

$$\frac{6dn}{\Delta^3} - \frac{6n3d\Delta}{\Delta^4} = d\mathrm{H} = 6d\mathrm{H} - \mathrm{H}\,\frac{3d\Delta}{\Delta}; \text{ or } \frac{3d\Delta}{\Delta} = \frac{5d\mathrm{H}}{\mathrm{H}};$$

and by integration we have $\Delta^3 \doteqdot \mathrm{H}^5$. Thus we deduce that *the density of the medium at any depth below the summit of an atmosphere is proportional to the fifth power of that depth* . . . . . XVIII.

§ 33. As we had $v^2 \doteqdot \mathrm{H}$ we may further deduce that $v^2\Delta^3 = \mathrm{H}^6$, or that *the elastic force of the atmospheric medium at any point is proportional to the sixth power of the depth of that point below the summit and to the sixth power of the mean square molecular velocity* . . . . . XIX.

These deductions are all embraced by the equations $v^2/g = \mathrm{H} \doteqdot \Delta^{\frac{1}{3}}$, and $v^2\Delta^3 \doteqdot \mathrm{H}^6 \doteqdot \overline{v^2}^6$.

§ 34. To compare these results with what is known of the physical condition of our atmosphere, we have first the obvious correspondence between the diminution of molecular *vis viva* and of temperature in ascending. No sufficient explanation of this has, I believe, been yet offered, for it is needless to attempt to do so by supposing the specific heat of air to increase as its density diminishes, as no difference of specific heat disturbs the equilibrium of the temperature of bodies placed in horizontal contact. The very fact of a gaseous atmosphere presenting a constant inequality of temperature at different elevations seems to prove that the law of the vertical equilibrium of temperature is essentially different from the law of horizontal equilibrium.

The actual rate of diminution is very fluctuating and uncertain, varying from 200 to 500 feet for each degree. The formula of our hypothesis applies only to the condition of an atmosphere resting on a horizontal base; such, indeed, as may be found only during a balloon ascent.

M. Gay-Lussac, in his celebrated ascent from the neighbourhood of Paris, found the depression amount to $72\frac{1}{2}°$ Fahr. in 7634 yards. This corresponds to 316 feet of elevation to 1°, if the rate is uniform. We have already determined (§ 27) the value of $v$ in air at the temperature of melting ice to be 2244 feet per second; hence $v^2/g = 156{,}593$ feet = H, the height of the atmosphere at this temperature (being nearly 30 miles). Now, taking Rudberg's expansion of dry air, the value of $v^2$ in degrees of Fahr. is 493° at this temperature, and $\frac{156593}{493} = 317{\cdot}6$, which is the elevation that ought to correspond to 1° by the hypothesis, in which also the rate is uniform. If Dalton and Gay-Lussac's constant of expansion is preferred, the elevation for 1° is 328 feet.

The hypothesis requires that the diminution of temperature should be uniform, and the best authorities agree that it approximates to uniformity at considerable elevations. In M. Gay-Lussac's table of observations taken during his ascent, the indications of the thermometer are somewhat irregular, as might be expected from the manner of making the observations, and the formula (Laplace's) employed to compute the elevations may not, perhaps, answer so well for balloon ascents

as it has been found to do in mountainous elevations. We have also to keep in view that the atmosphere absorbs a large proportion of the Sun's rays in their passage through, besides being supplied with heat from the ground irregularly according to the varying characteristics of its surface. Taking all these circumstances into account, the accordance between theory and M. Gay-Lussac's extreme observations is nearer than might be expected, and probably will not be found so exact at lesser elevations.

But the hypothesis admits of being tested without employing any empirical barometric formula, because, if it is correct, the tension as shown by the column of mercury ought to vary as the sixth power of the absolute temperature (from zero at $-461°$ Fahr.) (XIX.). But the observations must be taken at stationary points during the ascent, so that time may be allowed for the thermometer to acquire the temperature of the stratum of air in which the balloon rests. Let $z°$ be the absolute temperature of Fahr. zero; then for any two observations we ought to have $\left(\frac{z+t_1}{z+t_2}\right)^6 = \frac{e_1}{e_2}$; in which $t_1$, $e_1$, are the temperature and tension at any one altitude and $t_2$, $e_2$, the same at any other. The value of $z$ eliminated from this equation ought to be 448 or 461. I have tried this with M. Gay-Lussac's sixteenth and last observations, which appear to be the most favourable for accuracy, and $z$ comes out equal to 467. The rate of diminution also in the interval of 1800 metres between these two observations agrees well with theory, being 310 feet for each degree Fahr.*

§ 35. We must now endeavour to show that an atmosphere of mixed media follows exactly the same law of equilibrium.

Atmospheres of different homogeneous media supposed separate from each other must evidently have the same height if the mean square molecular velocity at the base of each of them is the same, and, consequently, the molecular *vis viva* in each atmosphere will, at the same height, be proportional to the specific molecular weight of the medium. If, on the contrary, the molecular *vis viva* at the base of each is the same, then will the height which is proportional to the mean square molecular velocity follow the inverse ratio of the specific

* Note H (formula for measuring heights by thermometer).

molecular weight, which is also equal to the direct ratio of the mean square molecular velocity (VII.). Thus, a hydrogen atmosphere ought to be four times the height of an oxygen atmosphere, etc. When mixed, the molecules of each of the media at the same height are necessarily in equilibrium of *vis viva*, and the mean space occupied by each molecule is therefore the same (§ VIII.). To see distinctly that the condition of the mixture is exactly that of a homogeneous medium of equal specific gravity, or, what is the same, whose specific molecular weight is equal to the sum of the products of the specific molecular weight of each by its proportionate volume—and thus having in equal volumes the same amount of *vis viva* as the mixture—we have only to recollect that a constant increment of descent in the atmosphere corresponds in all parts of it to the same constant increment of mean square molecular velocity, whatever the molecular velocity may be, or whatever the weight of the molecules with which it is associated, and the increment of *vis viva* in each medium for the same constant increment of mean square molecular velocity is as the product of its specific weight by its constituent volume. But the increment of mean square molecular velocity in the homogeneous being the same as in each of the constituents of the heterogeneous medium, and the product of its specific weight by its constituent volume being equal to the sum of the products of the specific weight and constituent volume of each, it is obvious that for the same increment of descent through the atmosphere the increment of *vis viva* in the homogeneous is the same as in the mixed medium, and that generally the physical condition of an atmosphere consisting of various media mixed together is exactly the same as if it were composed of one homogeneous medium whose specific gravity is equal to that of the mixture.

If each constituent of an atmosphere were supposed to form an atmosphere by itself, and ranged by each other side to side, and having all the same height, the ratio of their densities or proportional number of molecules in a constant volume would be the same at all heights, but the molecular *vis viva* would be respectively as the specific weight of each. If in this condition they were brought together so that all might occupy the space of one, an immediate change in the molecular *vis*

*viva* of each medium would ensue, the heavy molecules losing and the light molecules gaining *vis viva* until the *vis viva* equilibrium is established; and this, as well as their united density, corresponds with the same qualities of the homogeneous medium at the same height. All this is an obvious corollary of what precedes. *The vertical condition of equilibrium of an atmosphere is the same whether that atmosphere consists of one homogeneous medium or of a mixture of different media having the same specific gravity* . . . . . XX.

§ 36. The relation between the total height of an atmosphere of the medium to the height due to the molecular square velocity (XVII.) enables us easily to estimate the effect of diminished gravity, and assign the limits of temperature at the base, beyond which an atmosphere cannot be retained.

If the force of gravity is constant, it has been shown that the height of the atmosphere is equal to the height a body would ascend with the molecular velocity, and supposed to be acted upon by half the force of gravity; but as it really diminishes as we ascend, according to the ratio $\left(\frac{R}{R+H}\right)^2$ (in which R is the radius of the planet and H the height above its surface), the true height must be computed with the variable half-force $\frac{g}{2}\left(\frac{R}{R+H}\right)^2$, instead of with the constant half-force $\frac{g}{2}$.

Let $v$ be the initial velocity of the vertical projectile at the surface of the planet, $w$ its velocity at the height $h$; then shall $\frac{dh}{w} = dt$, and $dt\,\frac{g}{2}\left(\frac{R}{R+H}\right)^2 = -dw =$ the retardation in the differential time. Substituting the value of $dt$, the differential of the time in this equation, we have $-dw = \frac{dh}{w}\cdot\frac{g}{2}\cdot\left(\frac{R}{R+h}\right)^2$, and multiplying by $2w$ we have $-2w\,dw = -dw^2 = dhg\cdot\left(\frac{R}{R+h}\right)^2$. Integrating this expression gives $v^2 - w^2 = Rg\left(1 - \frac{R}{R+h}\right)$, so that when $w^2 = 0$, we have by eliminating $h$ (which then represents the total height of the atmosphere) $h = \frac{Rv^2}{Rg - v^2}$; being its value in terms of the radius of the planet, of the

mean square molecular velocity at its surface, and of the force of gravity at its surface. In the former expression for H, where the force of gravity was supposed constant, we had $H = v^2/g$, or $v^2 = gH$. Substituting this value of $v^2$ in the equation for $h$, we have $h = \frac{H}{1 - \frac{H}{R}} = H\left(1 + \frac{H}{R}\right)$, if $H^2/R$ is infinitesimal in respect to unity.

Thus the correction to be applied for the diminishing power of gravity in ascending increases as the square of the height, and employing the preceding data, the total height of the earth's equilibrated dry atmosphere, considered as a medium at the temperature of melting ice, is by this theory 157,776 feet, being 1183 feet more than the last determination, with constant force of gravity; and the correction to be added to the height computed with constant force of gravity is in feet $1{\cdot}2 \times H^2$ the square of the height in miles.

§ 37. We may express the last equation in a more general form with the molecular *vis viva* as the constant instead of the mean square molecular velocity. If the specific weight of the medium that corresponds with air is taken as unity, and $1 \times v^2$ is the value of the molecular *vis viva* at the surface of the planet: with any other medium whose specific weight is $s$, its mean square molecular velocity with the same *vis viva* is $v^2/s$, (VII.); and the absolute height of its atmosphere is $h = \frac{Rv^2}{Rgs - v^2}$. This equation gives the absolute height of an atmosphere under every variety of condition, and determines the limit of *vis viva* at the surface of a planet beyond which the medium cannot be retained, for when $h$ is infinite we have $Rgs - v^2 = 0$ or $v^2/s = Rg$. With regard to the medium that represents the atmosphere of our planet, we have already determined the value of $v^2$ to be $(2244)^2$, when the absolute temperature is 493° and $s = 1$; consequently, when $v^2/s = Rg$, the surface temperature must be 65,760° Fahr. for air, and 4556° for a hydrogen atmosphere whose specific weight $s$ is $\frac{1}{14{\cdot}432}$. At these surface temperatures such atmospheres would slowly evaporate into space.*

* Note K (central heat).

At the surface of the Moon the limit of temperature for an atmosphere of air is 3008° absolute or 2505° on Fahr. scale. For a hydrogen atmosphere it is 208° absolute or −253° Fahr. But the proximity of the Earth reduces these limits respectively 100° and 7°, so that if the Moon's surface had even a higher temperature than 2405°, the Earth, according to this theory, would then gradually withdraw the whole of any atmosphere of air that it might then have possessed.

By employing the same equation, it appears that the temperature at the surface of a body like the Sun in magnitude and mass requires to be 13,400° to sustain an atmosphere identical in constitution and height to that of the Earth.*

§ 38. It will not fail to be remarked that the positive evidence in favour of the reasoning of this section turns on one point. Does the law of vertical equilibrium of temperature correspond with the law of vertical equilibrium of *vis viva?* We have seen that the correspondence is more exact than might be expected, although it is extremely difficult to put the question to the test of direct experiment. In a column of air 318 feet high the temperature at the bottom ought to be 1° higher than at the top in any state of the atmosphere. If air is made to circulate quickly in two tubes of this height lined with non-conducting material, the difference of temperature ought to be very distinctly shown by thermometers at the top and bottom. The quick motion of the air downwards and upwards may be expected to compensate for the disturbing effect of the sides of the tube, and even to cause their internal surface to assume the proper atmospherical gradient of temperature.

The accuracy of the formula for measuring heights by the barometer that may be derived from the deductions of this section depends on the integrity of the law of diminution of temperature, but as this varies from local causes, the theoretical rule does not seem to apply so well as those in common use, which are partly empirical.

But a strictly theoretical formula may be deduced if it is permitted to assume that the change of temperature between two stations is uniform, whatever that difference may amount to, and likewise that the change of density is conformable.†

* Note L (nebular hypothesis). † Note M (barometric formula).

## Section VI.—On the Velocity with which Impulses are Transmitted through a Medium.*

§ 39. The reasoning on the subject of this section is founded on the principle that the velocity must correspond with the average velocity resolved through the medium in any one direction.

We have seen in § 17 that the mean square velocity resolved in one direction is equal to one-third of that mean square velocity, and it is easy to prove, if all the velocities of the molecules are equal, that the average resolved velocity in one direction is equal to one-half the common velocity.

As the equal lines representing the molecular velocities on one side of a plane may be assumed to radiate equally in every direction from one point, they will spread to every point of the hemisphere, resting on the plane; let perpendiculars be dropped from these points upon the plane. *The quotient of the sum of these divided by their number is equal to half the common length of the equal lines.* The proof of this is derived from the integration of simple circular functions that give the quotient of the sum of the sines of a hemisphere divided by their number, or by the surface of the hemisphere, equal to half the radius. Thus, if $\theta$ be the inclination of the radiating lines to the plane, $v\,d\theta \sin\theta \cos\theta 2\pi$ represents the aggregate of the perpendiculars upon the base of the hemisphere, and $d\theta \cos\theta 2\pi$ represents their aggregate number. Collecting

* [The idea of the direct connection between the velocity of sound and that of the molecules is of great interest, and leads at once to the conclusion that the velocity of sound is independent of density, but proportional to absolute temperature. The next person to raise the question was Stefan (*Pogg. Ann.*, vol. 118, 1863, p. 494), but his calculation is as defective as that of the author. On Waterston's principles, the ratio of the velocity of sound to the molecular velocity of mean square should be $\sqrt{5/3}$, as was shown by Maxwell (Preston, *Phil. Mag.*, vol. 3, 1877, p. 453). In the *Philosophical Magazine* for 1858 (vol. 16, p. 481) Waterston returned to the subject. It is curious that he regarded the ordinary hydrodynamical investigation, not merely as needlessly indirect but as inconsistent with the molecular theory. A result in harmony with experiment cannot be obtained on the basis of a hypothetical medium constituted of elastic spheres, for such a medium would have a ratio of specific heats different from that observed in gases.—R.]

the quotients of the first by the second for every value of $\theta$ from 0° to 90°, or what is the same, integrating these functions, and dividing the first by the second, we have the quotient equal to $\frac{1}{2}v$, which is the mean velocity resolved perpendicular to the stratum when the molecular velocity $v$ is constant.

§ 40. But the hypothesis does not admit of the molecules having all the same velocities; we have therefore to inquire into the effects of this diversity upon the velocity of transmission.

The molecules in a small sphere of the medium at any given instant may be classed in respect to their velocity into sets, and of each set we are allowed, by the hypothesis which we are following, to assume that there is an equal number moving in every direction, and that since continuous uniformity in the density requires that the number contained in the spherical space should be always the same, the exit of one of a set may be conceived to be immediately followed by the entrance of another of the same moving in the same direction and with the same velocity. One-half the number in a set is increasing their distance from a given plane, and the other half diminishing their distance. Let the motions of one of these halves be resolved in the direction perpendicular to this plane, and let us add together such resolved spaces as are described by all the molecules of the set that happen to be in the sphere during a constant time for so long as they remain in it, and divide by the constant number of molecules of the set in the sphere at all times; the quotient must evidently be the mean velocity in that direction and set, and must be the uniform rate with which an impulse is conveyed in one direction by means of an infinite series of impacts, the space between two impacts measured in the constant direction being the step forward made by the infinitesimal portion of the impulse contained in the traversing motion of the molecules from one of the impacts to the other.

The number moving in any one direction with the velocity $u$ is equal to the number moving in any other direction with the same velocity, and each of these numbers takes the same time to traverse the sphere. If we compare this time with that taken by the molecules of another velocity or set, it is obvious that these times must be inversely as the velocities,

and *the number that continuously pass through the sphere or any other constant space in a constant time must be as the velocity:* for this may be estimated as if there were continuous currents of molecules moving in every possible direction with the respective velocities: the encounters that may be imagined to interrupt this continuity being infinite in number do not alter the general average of velocity or direction or proportionate number, and therefore each velocity and direction may be taken as constant. Now, for any one velocity $u$ viewed thus as constant, the mean resolved velocity in one direction of all the molecules that happen to move with this velocity at any instant is, as above demonstrated, equal to $\frac{1}{2}u$; *but if we add up the resolved spaces traversed by all the molecules that have been in the sphere with this velocity during a constant time, and divide by a constant number that are in the sphere at any instant,* we require to multiply $\frac{1}{2}u$ by a factor that is proportional to $u$, so that $\frac{1}{2}u^2$, the resulting product, is proportional to the mean distance traversed in a constant time by the molecules that have appeared in a constant space to move with this velocity during the constant time.

Now, suppose an impulse to be given to the medium at any point, and an indefinitely long cylinder of the medium to extend from this point; the impulse given to the molecules at its extremity can be conveyed through the medium only by means of the molecular encounters, each of which contributes to this effect with different velocities resolved in the direction of the cylinder. It has been shown that the mean traversed distance in a given space during a given time due to any one molecular velocity is as the square of that velocity multiplied by the proportion of the number of molecules associated with it. Therefore, the united effect of all velocities must be equal to the sum of these products, and this sum is unity, or the whole medium multiplied by the mean square molecular velocity. But it was shown that if the molecular velocity were constant and equal to $v$, the absolute value of the mean resolved velocity or traversed distance, during a constant time in one direction, would be $\frac{1}{2}v$. Now, as the traversed distance or transmitting effect of this velocity in comparison to the other velocities $u$, etc., as they actually exist in the medium, is as $v^2$ to $u^2$, etc., we

arrive at the conclusion that $\frac{1}{2}v$ is actually the uniform velocity with which an impulse is transmitted through a medium.

§ 41. According to the reasoning in §§ 27 and 28, the value of $v$ is the velocity acquired in falling through three uniform atmospheres. This gives $\frac{1}{2}v = 1122$ feet per second at the temperature of melting ice, and 1176 at the temperature of 80°, being an increase of $\frac{9}{8}$ of a foot for each degree. In the article "Acoustics," *Penny Cycl.*, it is stated that 1125 feet per second at 62° accords nearly with the mean of the best experiments, and that the difference for 1° is $\frac{8}{7}$ of a foot. The velocity by the hypothesis is therefore about $\frac{1}{40}$ part greater than is found by observation, which, in a distance of 8 miles, amounts to a difference of one second in the time of travelling that distance. This is probably a greater difference than can be allowed between observation and a correct theory. M. Moll's observations, which seem to be standard authority, were taken with such precautions that an error of 1 in 40 seconds can hardly be admitted. They likewise agree remarkably well with Mr Goldingham's observations at Madras.

In taking astronomical observations of the same kind with the same instrument, it has lately been discovered that two individuals differ sensibly from each other. This has led to the suspicion that in all observations there is a personal error due to some obscure physiological cause that allows a small interval of time between sensation and perception, or volition. If this were the same for the sense of hearing as for vision, it could not affect the results of experiments on sound; but it is quite possible that it may be different, and the very fact that such personal errors do exist, may justify a suspicion that such an effect might interfere and prevent a perfect degree of accuracy from being obtained.

We have made no hypothesis of the nature of the impinging surfaces of the molecules. May the discrepancy arise from something omitted in this?

In other media, according to this theory, the velocity varies as the inverse square root of their specific gravity, and at different temperatures as the square root of the absolute temperatures. It depends wholly on the thermometer, and is quite independent of the indications of the barometer. This accords

with the generally received theory. The molecular velocity of watery vapour being to that of air as $\sqrt{14{\cdot}4}$ to $\sqrt{9}$, the moisture in the atmosphere ought to have the effect of accelerating the velocity about 4 feet per second in temperate latitudes and 10 feet in the tropics at a maximum.

## Notes.

### *Note A.—Motion Indestructible as Matter.*

The force of the descending weight is apparently *expended*, but it is only *transferred* to the medium. If the elements of matter are perfectly elastic, this kind of transference must be of general occurrence wherever force is exerted, because the exertion of force is then but its transference either from its invisible constant condition, as in the medium, to its visible transitory condition, as in the ascent of the weight, or *vice versa*. In the former case force exists in the matter of the medium without change; in the latter it is being transferred to the agent of gravitation, so to speak, and apparently released and disconnected from matter. The force of a medium, when it equilibrates a force of gravity, is similar to the force of a wind or a current of water on a stationary surface. Is is not possible to view all forces as inseparable from some form of matter, and all the phenomena of nature, as not consisting of the creation and annihilation of force, but in its transference from one form of matter to the other?

### *Note B.—Vapours.*

This enables us conveniently to represent the relation between the density and the square root of the mean square molecular velocity of a medium while it is being dilated or compressed.

Take Q as the origin of co-ordinates, and let QP represent $v$ and PN the $\sqrt{\frac{1}{\Delta}}$. Join NQ. If the medium is compressed so that $\sqrt{\frac{1}{\Delta}}$ becomes TS, then shall $v$ become QT, and if it dilates so that $\sqrt{\frac{1}{\Delta}}$ becomes UW, then shall $v$ become QU.

Each point in the positive quarter of the co-ordinate axes represents a medium of a given density and temperature or *vis viva;* the sixth power of the ordinate $x$ represents its density, and the square of its abscissa $y$ its *vis viva.* Thus, $x^6y^2 = e$ is the expression for the tension or elastic force of a medium whose point on this chart (as it may be called) is defined in position by the co-ordinates $x$, $y$.

If $e$ is supposed constant, $x$, $y$ to vary, their *locus* traces out a hyperbolic curve (such as NR) whose equation is $x^6y^2 = e$. It is a curve of constant pressure, of which kind is STPC for one atmosphere drawn on the accompanying large chart of vapours (Chart 1). The sine of the inclination of its tangent

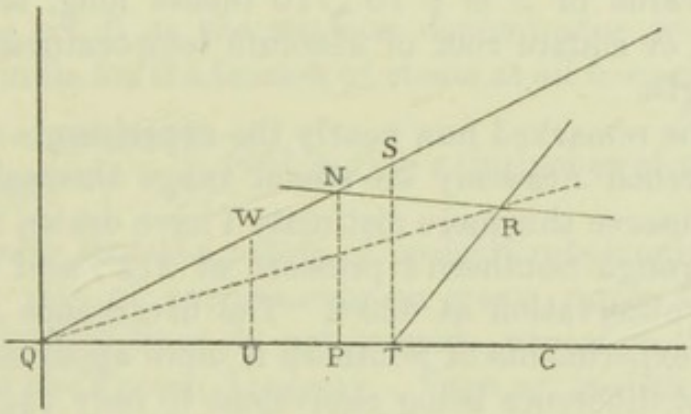


Fig. 3.

to the axis of $y$ is $\frac{1}{3}\, x/y$. Any gas expanding or contracting under a constant pressure traces out a curve of this kind with its varying density and *vis viva.*

It is remarkable that if the points corresponding to the density and *vis viva* of a vapour in contact with its generating liquid are laid down on this chart (fig. 3), they range themselves in a straight line, such as TR, that issues from some point advanced on the axis QC. As this fact applies to all vapours that have been experimented upon, it seems to point to the true physical law of their equilibrium with the liquid. On the accompanying chart of vapours I have projected the points of several sets of experiments. It may be viewed as a portion of the fig. 3 enlarged, the point Q, or origin of co-ordinates, being about 40 inches to the left of the outer margin.

The following details will be sufficient, with the chart, to enable any one to satisfy himself of the truth, and, if he pleases, to construct the formula of any new vapour by means of *two* simple experiments on its tension.

In vapours, as well as gases, the pressure or tension being equal to the product of the absolute temperature or $(t + 461)$ by the density, to find the latter we have only to divide the tabular tension (in inches of mercury) opposite $t°$ Fahr. scale by the former. The sixth root of the quotient is the value of $x$, and the square root of $(t + 461)$ is the corresponding value of $y$. In the accompanying chart I have projected several sets of tables of pressures in this way. The unit value of $x$ is $\frac{1}{3}$ 10 $\sqrt{10}$ inches long, and the unit value of $y$, or square root of absolute temperature, is $\frac{1}{6}$ of an inch in length.

It will be remarked how nearly the experiments of Southern and the French Academy on steam range themselves in one line. To observe this more distinctly I have drawn the straight line SF through Southern's pressure at 212°, and the French Academy's observation at 429·4. The divergence at the four lowermost experiments of Southern is more apparent than real, the greatest difference being equivalent to only $\frac{3}{100}$ of an inch of mercury.

The general equation for a straight line TR (fig. 3) is $x=(y - \mathrm{G}) \tan \mathrm{H}$, in which $\mathrm{G}=\mathrm{QT}$ and $\mathrm{H}=\mathrm{RTC}$. Each vapour being represented by such a line with two constants G and H, to find these constants, which may be done by two experiments on any one vapour, let $e_0$ be the tension at $t_0$ temperature Fahr., and $e_1$ the tension at $t_1$ temperature; then since $y_0^2 = 461 + t_0$

and $y_0^2 x_0^6 = e_0$, we have $x_0 = \left(\frac{e_0}{y_0^2}\right)^{\frac{1}{6}} = \sqrt{y_0 - \mathrm{G}} \tan \mathrm{H}$,

or

$$\tan \mathrm{H} = \sqrt[6]{\frac{e_0}{461 + t_0}} \times \frac{1}{\sqrt{461 + t_0} - \mathrm{G}}$$

and by 2nd experiment

$$\tan \mathrm{H} = \sqrt[6]{\frac{e_1}{461 + t_1}} \times \frac{1}{\sqrt{461 + t_1} - \mathrm{G}}.$$

From these we eliminate *

$$G = \frac{\sqrt{461 + t_0}\sqrt[6]{\frac{461 + t_0}{461 + t_1} \times \frac{e_1}{e_0}} - \sqrt{461 + t_1}}{\sqrt{\frac{461 + t_0}{461 + t_1} \times \frac{e_1}{e_0}} - 1} \cdot \dagger$$

From the two observations corresponding to the points S and F, I have computed the value of G = 19·4923 and tan H = 0·092308, which define the equation for the steam line that best answers to the experiments of Southern and the French Academy combined. This equation is

$$\tan^6 H \, (\sqrt{461 + t} - G)^6 \, (461 + t) = e,$$

or by putting cot H as the common denominator we have the following formula for the tension of steam at all temperatures :—

$$\left\{\frac{\sqrt{461 + t} - 19{\cdot}4923}{10{\cdot}883}\right\}^6 (461 + t) = e \text{ (in inches of mercury).}$$

It is singular that the points of projection belonging to each of the many sets of experiments on steam range themselves in a line, but these lines do not coincide, except in the case of Southern and the French Academy. Each set is thus consistent with the general law, and on this account their want of accordance with each other is difficult to explain. It might be caused by an error of the standard scales, but this is hardly possible; or impurity of the water, which is not very likely where every precaution has been so carefully attended to. Dr Ure's line of observations is more inclined to the axis than SF, the cot H in the formula being 10·3 and G = 19·8. The line of the American Institute's experiments has yet a greater elevation, the cot H being about 9·8.

The other lines of vapour on the chart explain themselves. It will be remarked that the deflections from the straight are all of a zig-zag character, there is no general bending to one side or another, and this seems to show that there is some physical law upon which the equilibrium of vapours with their

* [? determine.—R.]

† If Dalton and Gay-Lussac's constant of expansion is preferred, 448 is to be substituted in the place of 461.

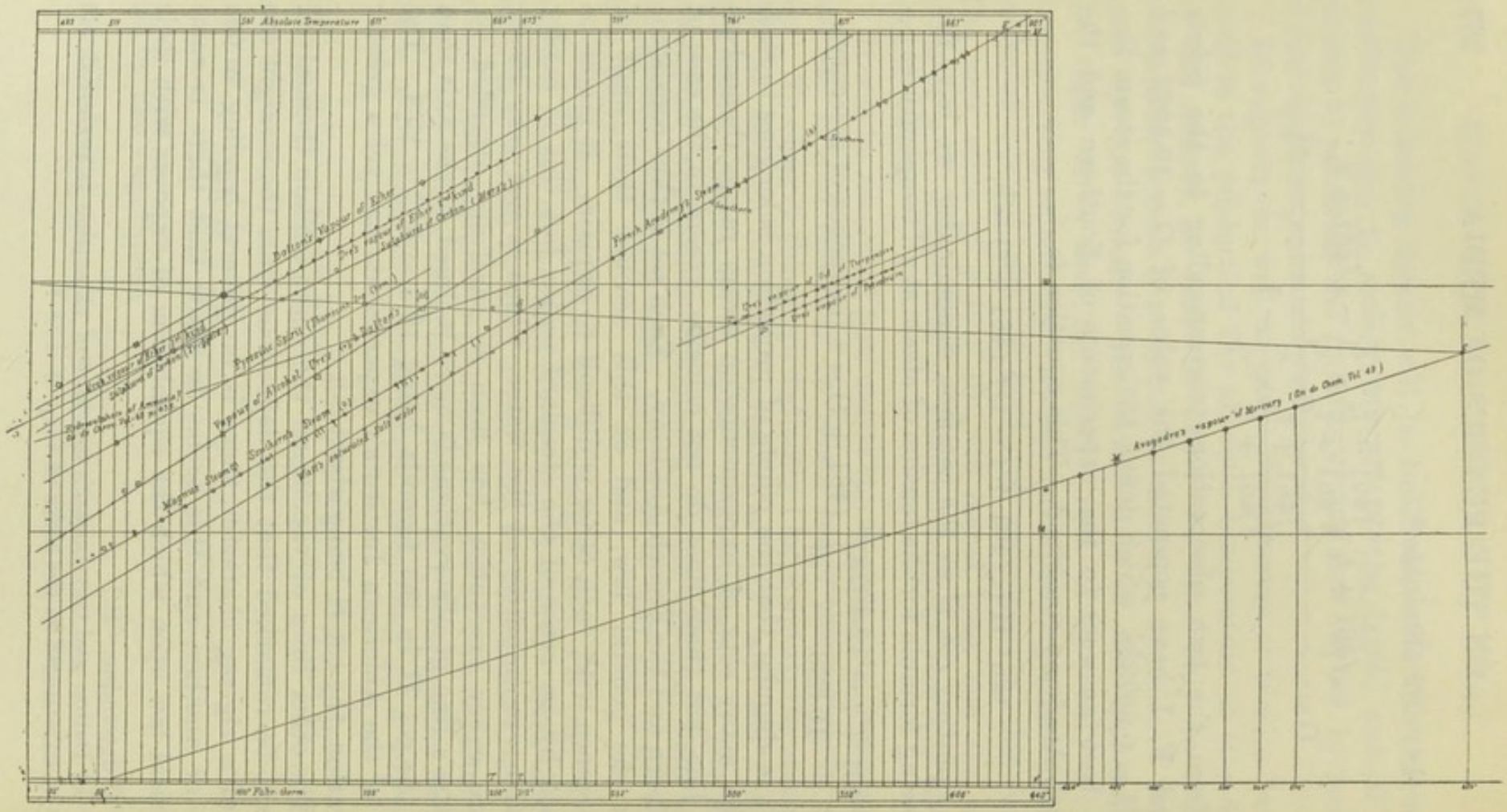
Absolute Temperature
Regnault's Vapour of Ether
Dalton's Vapour of Ether
Dalton's vapour of Ether (Mixed)
Pyrenic Spirit (Thenard's) (Dalton)
Vapour of Alcohol (Dalton)
Magnus Steam
Regnault's Steam
Dalton's Steam
Pyrenic Academy's Steam
Avogadro's vapour of Mercury

Chart 1.

liquids is arranged that is represented by the above function of the temperature. As it seems to apply to all vapours, it is probably not beyond the grasp of physical research, if the *vis viva* theory is admitted, for it evidently does not depend on the chemical qualities of the body any more than does the law of volumes in gases.

It must be confessed that as yet we have but few materials wherewith to found a process of investigation. The fundamental point is the specific heat of steam.

If it is more than about one-third that of water, if for equal volumes it is not the same as that of air and the other gases, then is the theory incomplete and altogether defective.

It is stated to be about 0·84, but this requires confirmation. The specific heat of the vapour of alcohol ought to be only $\frac{1}{5}$ that of the liquid. The vapour of ether only $\frac{1}{7}$ that of the liquid. Vapour of etherine $\frac{1}{4}$. Vapour of sulphuret of carbon $\frac{1}{4}$. Vapour of oil of turpentine $\frac{1}{10}$. Vapour of bromine $\frac{1}{4}$, etc.

These are from MM. de la Rive and Marcet's experiments.

Is there any direct evidence in favour of or against this view? So far as I can discover, little, if any, on either side. It is quite undetermined as yet by experiment, and is so surrounded by practical difficulties that it will probably long remain so.

But analogy favours it in so many ways as to make it highly probable. Liquid etherine has four times the specific heat that its vapour ought to have, judging from its specific gravity. Now, olefiant gas is isomeric with etherine, and its specific gravity is one-half that of etherine vapour. It can hardly be doubted that the specific heat of liquefied olefiant gas for equal weights is the same as that of etherine, which corresponds very nearly with naphtha, turpentine, and the other hydrocarburets. If it is so it will be double what it ought to be, if for equal volumes it is the same as that of air and the other gases. Now, the eminent French chemists who have at different times made experiments on the specific heat of this gas agree that it is nearly 1·5 times that of air, while Mr Haycraft (*Edin. Trans.*), on the other hand, with his simple and apparently most efficient apparatus, found it to be the same as that of air, and accounts for the higher number of

the French chemists by the great difficulty there is in freeing it from ethereal vapours.

It is the same with carbonic acid gas (the only other exception to the law of equal specific heat for equal volumes). The French chemists agree that its specific heat lies between 1·175 and 1·258; Mr Haycraft, by many experiments, that if carefully dried, it offers no exception to the general law. Judging from the analogy of other similar binary compounds, there cannot be a doubt that the specific heat of liquefied carbonic acid is double, if not three times, that of the gas.

It would be a most valuable addition to our knowledge if this great change in the specific heat of a body when it becomes vapour could be thoroughly established, and it is in vain to proceed with the subject of vapours until it is so.

Sulphuric ether is probably better adapted for the experiment than any other body. Its boiling temperature is very low, and there is a vast disproportion between the specific heat of the liquid and what may be expected in the vapour. Suppose a volume of it and of air are maintained in equilibrium of pressure and temperature by means of a bent tube with mercury. If a sudden small and equal dilatation is made in both at the same instant, the difference of pressure that will then become apparent will indicate the value of the latent heat of the vapour in terms of its specific heat, and, as we know the value of the same in terms of the specific heat of water, we shall have the ratio of the specific heat of the vapour to that of water. The air follows the line RQ (fig. 3) in its expansion, while the vapour is obliged to keep to its line RT.

The latent heat of vapours is another subject where there is room for much additional research. Dr Ure, in his *Dictionary of Chemistry*, has given a table of eight vapours, but none of the liquids appear to have been pure. The ether boiled at 112°, and the specific gravity of the alcohol was 0·825. If a correction is made for this want of purity, it is singular that the latent heat of each is almost exactly in the inverse ratio of the specific weight of its vapour. This may indicate that the heat required to vaporize a molecule of each of these bodies is the same, and amounts to 3000°, referred to the constant specific heat of a gaseous molecule, which is the proper theoretical standard.

Is this the measure of the force of liquid cohesion? On the *vis viva* theory of heat this, for 1 lb. of water, amounts to the force required to raise 1 lb. to the height of about 680,000 feet. According to Mitscherlich, the *vis viva* generated by the union of the constituent elements of the same quantity of water amounts to ten times this force.

Considerable attention has lately been given to thermo-chemistry; but it is to be regretted that no notice has been taken of the permanent change of specific gravity that is usually found to ensue in chemical mixtures that evolve heat. It would be interesting to ascertain if there is any harmonious connection between the quantity of heat evolved and the change of atomic volume. Dr Ure has given a curious example of hydrated nitric acid, where the permanent change of volume appears to be the same as would be caused by a permanent change of temperature equal to the heat evolved.

Since only two experiments are required to fix a line of vapour on the chart, it would not be a very arduous undertaking to accomplish this for all bodies that throw off vapours at accessible temperatures. We might then have the means of answering the various questions that cannot fail to suggest themselves on looking at the chart; and, first of all, do the vapours of arsenic, iodine, camphor, salts of ammonia, and the other solids that rise into vapour before becoming liquid, follow the general law? This question has yet to be determined.

How are the lines of vapour of the simple bodies related to each other? We have only one example as yet, viz., the vapour of mercury by M. Avogadro. It will be remarked that the line drawn through MR, the third and the second last observation, agrees very well with the position of the other points. This line produced meets the axis at 50°, which is certainly lower than the temperature assigned by Dr Faraday's delicate experiment as the point of no vapour. In judging of this discrepancy, however, we must recollect that the density is represented not by the ordinate to the line of vapour, but to its sixth power; hence, at the temperature of 75°, the density indicated by the line on the chart is only $\frac{1}{120,000,000}$ of the density of an atmosphere. The formula that corresponds to this line has the constant G = 22·6065 and cot H = 20·0023.

It will be remarked that the line for Dalton's ether (which, from its low boiling point, must have been nearly pure), Thompson's pyroxilic spirit, and the well determined line for steam, are nearly parallel; is this parallelism perfect? It is also remarkable that the projection of two experiments by Dalton on aqueous ammonia is exactly parallel with the steam line, and further, that the same parallelism is maintained by the vapours of liquefied ammoniacal gas and carbonic acid (by Thilorier). It would be extremely desirable if Dr Faraday's experiments on chlorine and the other more condensable gases could be repeated on a large scale so as to determine their position on the chart, and by two or three observations on each to eliminate the constants G and H. It is by such experiments and those of M. Cagniard de la Tour, made at the other extremity of the scale of heat and pressure, and likewise by Mr Perkins, all of which may be classed under the head of chemical physics, that we may expect to extort from nature some of her most hidden secrets, to come in sight of new continents in the world of natural science, not dreamt of in our philosophy, because removed beyond the bounds of suggestive analogy. Such pressures appear to us great, and are certainly dangerous to operate with, but in respect to those which exist in nature, and that everywhere surround us, restrained by internal forces, they can only be considered as infinitesimal.

*Note C.—Temperature of Compressed Air.*

These changes of temperature are certainly much greater than are said to have been observed by Darwin, Dalton, and others. Not having access to the original account of these experiments, I am unable to ascertain how far they accord with the theory; but the specific heat of air is so small in comparison to that of the materials of which thermometers are composed that the actual difference of temperature in a single condensation or dilatation must be much greater than what is indicated by any thermometric apparatus.

A more effectual way of ascertaining this seems to be by continually and quickly repeating the same condensation with different portions of air, so that after some time, by proper

care, the condensing syringe ought to exhibit the temperature of the air at its maximum tension.

If air is a medium, we have in XVI. the means of computing the temperature that ought to be shown by a thermometer placed at the bottom of the syringe.

Thus, $t_0$, $e_0$ being the temperature and tension of the air outside, $e_1$ the tension corresponding to the load on the eduction valve of the syringe; then $\frac{e_1}{e_0} = \left(\frac{t_0 + 461}{t_1 + 461}\right)^4$, and $\left(t_0 + 461\right) \sqrt[4]{\frac{e_1}{e_0}} - 461 = t_1$, the temperature of the air when condensed.

The air engines of the atmospheric railway may perhaps afford the means of testing this equation.

*Note D.—To find the Compression that Corresponds to a Given Ratio of Latent to Sensible Heat.*

This may be computed by means of IV. and XVI.

Suppose the medium is compressed so that the mean molecular distance changes from 1 to $\frac{x}{x+1}$, the *vis viva* increases from 1 to $\frac{x+1}{x}$ (XVI.), the increment being $1/x = \mathrm{L}$.

By withdrawing molecular *vis viva* under a constant pressure, let the mean distance change from 1 to $\frac{x}{x+1}$; the molecular *vis viva* must be diminished from 1 to $\left(\frac{x}{x+1}\right)^3$ (IV.), the decrement being $\frac{(x+1)^3 - x^3}{(x+1)^3} = \mathrm{K}$. The ratio K/L is given to find $\left(\frac{x+1}{x}\right)^3$, which is the amount of compression from unity that makes the ratio between the sensible and latent, or evolved *vis viva*, equal to K/L.

If we put $x/(x+1) = y$, the equation resolves itself into $y^3 + y^2 + y = \mathrm{K/L} = \frac{8}{3}$ in the case given. This equation may easily be solved by inspection of a table of square and cube numbers. $1/y^3$ is the compression from unity to give the ratio K/L of the sensible to the latent heat.

*Note E.—Specific Heat of Air.*

It is probable that the specific heat of mercury and water are better determined than that of any other bodies. Assuming that the specific heat of liquid mercury (0·033) is the same as that of its vapour, and that all gases and vapours have for the same volume the same specific heat when *in equilibrio* of pressure and temperature, the specific heat of air in terms of that of water is 0·238. The mean experimental value is, according to the French chemists, 0·267. Mercury is thus the only liquid, so far as is known, whose specific heat is the same as what it ought to be in the state of vapour, if it conforms to the general law. Water is nearly three times greater than steam ought to be. Alcohol five times that of its vapour. Ether seven times, etc.; see Note B.

This accordance of the specific heat of mercury with its vapour seems to prove that there is little or no part of the heat required to raise the temperature of the liquid absorbed in a latent form. This is an important point in the *vis viva* theory.

It is remarkable that there is no instance, so far as yet known, of mercury combining in fractional parts of a volume. Arsenic, sulphur, phosphorus, and some others combine in fractional volumes, and their specific heat in the form of vapour by the theory of gases is the same fractional part of the specific heat of the solid. This is somewhat confirmatory of the theoretical interpretation of the law of volumes (§ 15). The vaporous molecule of mercury is indivisible. The vaporous molecule of arsenic is divisible into four parts; of sulphur into six parts; of phosphorus into four parts; of hydrogen into two parts; of oxygen into two parts; of water ($O_{\frac{1}{2}}2H_{\frac{1}{2}}$) into three parts; of alcohol ($2(HC_{\frac{1}{2}})+2H_{\frac{1}{2}}O_{\frac{1}{2}}$) into five parts; of sulphuric ether ($4(HC_{\frac{1}{2}})+2H_{\frac{1}{2}}O_{\frac{1}{2}}$) into seven parts; sulphuric acid, anhydrous ($3O_{\frac{1}{2}}+S_{\frac{1}{3}}$) into four parts; etherine $4(HC_{\frac{1}{2}})$ into four parts, etc. We may thus predict the specific heat of pyroxilic spirit to be 0·83, and of pyroxilic ether 0·72.

*Note F.—M. Clapeyron's Fundamental Position.*

The density and tension of a medium expanding according to Mariotte's law, are represented linearly by the co-ordinates to the common hyperbola CMEL referred to its asymptote AG; the abscissa AB, etc., representing the volume, and the ordinates BC, etc., the tension.

We have shown that each incremental expansion is made at the expense of the molecular *vis viva* of the medium, so that to maintain the expansion, according to Mariotte's law, the loss of *vis viva* must be continually made up; and the

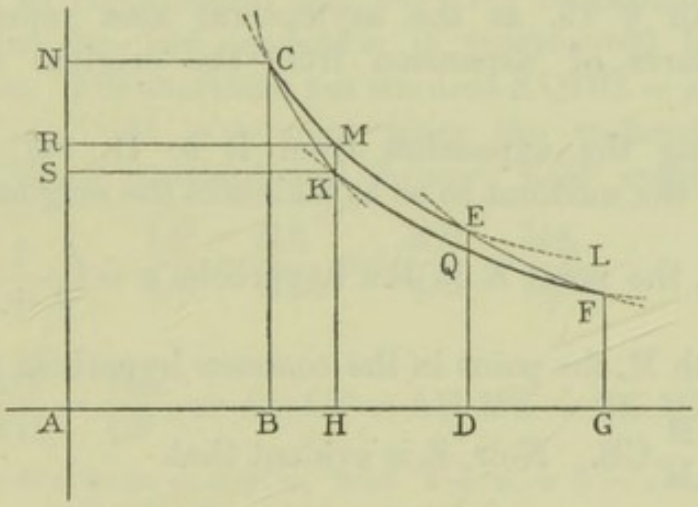


Fig. 4.

amount required to be supplied for any given expansion, as from B to D, is to the constant original amount as the area CEDB is to 3 CB . AB, or to the original tension CB acting through three times the original volume AB (XIII.).

Suppose the medium to expand from B to H (against pressure) without having its loss of *vis viva* supplied; then, according to § 22, its tension from CB, or unity, becomes $KH = \left(\frac{AB}{AH}\right)^{\frac{4}{3}} CB$. Assuming the original volume and tension as unity and $BH = y$, we have $KH = x = \left(\frac{1}{1+y}\right)^{\frac{4}{3}}$ and $x\,dy = dy\left(\frac{1}{1+y}\right)^{\frac{4}{3}}$ = the differential of the asymptotal area CKHB.

To integrate this put $\frac{1}{\Delta^3} = 1 + y$, and differentiating gives $dy = -\frac{3d\Delta}{\Delta^4}$; and since

$$x = \left(\frac{1}{1+y}\right)^{\frac{4}{3}} = \Delta^4,$$

we have

$$\int x\,dy = 3 - 3\Delta = 3 - \frac{3}{(1+y)^{\frac{1}{3}}} = \text{area CKHB}$$

in terms of ABCN unity. In this, $y$ being made infinite, we have the area of the whole asymptotal space = 3ABCN, which accords with § 19, as the asymptotal area represents the collective force of expansion from the original volume to infinity.

If, during the expansion from B to H, *vis viva* were supplied to the medium so as to maintain the original quantity unimpaired, the point K in the hyperbola $x = \left(\frac{1}{1+y}\right)^{\frac{4}{3}}$ would coincide with M, the point in the common hyperbola $x = \frac{1}{1+y}$, or $\text{MH} = \frac{\text{AB}}{\text{AH}}.\text{CB}$. Now, it is evident that

$$\text{MH} : \text{KH} :: \frac{1}{1+y} : \left(\frac{1}{1+y}\right)^{\frac{4}{3}} :: 3 : \frac{3}{(1+y)^{\frac{1}{3}}};$$

but the preceding integral gave $\frac{3}{(1+y)^{\frac{1}{3}}} = 3\text{ABCN} - \text{CKHB}$,

therefore MH : KH : : 3AB.CB : 3AB.CB − CKHB = ratio of original *vis viva* of the medium to the force remaining after expansion from B to H. Thus, MH : MK : : original *vis viva* : decrement of *vis viva* owing to expansion; and three times the area KNAS is equal to the asymptotal area on the other side of KH. These relations evidently hold good in whatever part of the conic hyperbola the point C may be taken.

Suppose the medium is maintained at its original *vis viva* while it expands from C to E, it will exert the mechanical force CEDB = $p$, and absorb the *vis viva* CEDB, the original

quantity in the medium being 3AB.CB. From E let it expand to F without being supplied with *vis viva;* then, as before, $FG = \left(\frac{AD}{AG}\right)^{\frac{4}{3}}.ED$ and $\frac{LF}{FG}$ is the proportion of original *vis viva* expended represented by EFGD = $(m)$, its equivalent mechanical force exerted. Let it now be compressed from F to K, the *vis viva* communicated to the medium being continually withdrawn. The amount withdrawn and the force exerted is represented by the area FGHK = $q$. From K let the medium be compressed without withdrawing the *vis viva* generated until the original tension CB and density AB are regained. The force of compression and *vis viva* communicated to the medium in the last operation is represented by the area CKHB = $n$. For shortness put the area KQDH = $s$, CEFK = $\delta$, and EQF = $\epsilon$. It is evident since the molecular *vis viva* through CEL is constant and through KQF constant, that

$$\frac{LF}{LG} = \frac{MK}{MH} \text{ and } \frac{MH}{LG} = \frac{MK}{LF}.$$

Also,

$$\frac{MH}{LG} = \frac{AG}{AH} = \frac{MK}{LF}, \text{ or } AG.LF = AH.MK = SK.MK = n = m.$$

But $q + e = s + m = s + n$, and $s + n + \delta - \epsilon = p$. In this equation substitute for $s + n$ its equal, $q + e$, and we have $q + \epsilon + \delta - \epsilon = q + \delta = p$. Thus, the curvilinear area $\delta$, or CEFK, is the excess of the force exerted by the medium expanding from C to E at the higher constant temperature, over the force exerted upon the same, compressing it from F to K at the lower constant temperature. It is also the excess of the *vis viva* absorbed in the first part of the process over the *vis viva* given out in the last part.

This is the interpretation of M. Clapeyron's fundamental position * applied to the hypothetical medium. M. Clapeyron assumes that the quantity of heat taken from the body A (p. 349, Taylor's *Memoirs*) during the expansion from C to E is necessarily equal to the quantity given to the body B during compression from F to K, and that from E to F and K to C *no*

* Essay on the motive power of heat, *Journ. Polyt.*, translated in Taylor's *Memoirs*, vol. 1, p. 349.

heat is taken from or given to the volume of gas, a proportion only of the sensible heat being reduced to a latent form and again thrown out. Thus, there appears the anomaly of the mechanical force $\delta$ being exerted or generated without any loss of heat. M. Clapeyron's view is here so much at variance with the *vis viva* theory of heat that it seemed proper to enter into a full explanation of the same condition in the hypothetical medium. The case is very instructive, and throws light on the *vis viva* theory, which is at the same time the means of clearing up the anomaly in causation of mechanical force seeming to be generated without expenditure of heat. M. Clapeyron's conclusions, so far as gases are concerned, are quite independent of any hypothesis, and seem to be strictly deduced from the laws of Mariotte and Dalton and Gay-Lussac combined with the relation that has been found to subsist between simultaneous increments of sensible and of latent heat. It is satisfactory to observe that they agree, so far as they go, with the physical properties of the medium.

### *Note G.—Objection to Theory.*

The only difficulty I can discover in the *vis viva* theory of heat applies in some measure to the undulatory theory of light. The ethereal medium that transmits the undulations is affected by vibrations of the elements of matter, but there is not the least symptom of it affecting by its resistance the planetary motions, and yet theory shows that it must permeate through the very substance and heart of all bodies with such quick and subtle power as not in any sensible manner to be affected in its equilibrium by any part of their motion except what is vibratory. Now, the *vis viva* theory of heat shows that the greatest ordinary velocities of this vibratory motion do not much exceed 2000 feet per second, but the velocity of the earth in its orbit is upwards of 50 times this amount. In the first case, if a hot body were isolated from all surrounding matter, the whole of its motion would be withdrawn from it in a very short time by the ethereal medium. This we can affirm inductively from the laws of the radiation of heat. In the second case not the slightest resistance is made manifest. The

ether only affects and is affected by vibratory motion. Any other kind, however great, it neither affects nor is affected by. What are we to infer from this incongruity? If vibratory motion differed in no other point from the rectilinear and rotatory motion of masses of matter, than in the sudden change of direction, it is plain that if there was no resistance in the one case there could be no resistance in the other, and *vice versa*. We are, therefore, compelled to infer that the disturbance of the ethereal medium is not caused directly by the motion of material particles as with the grosser media of gases and liquids when affected by the motion of bodies passing through them. Something takes place in the vibratory motion of the elements of matter that does not take place in their collective motion.

Molecular vibration consists in the struggle, as it were, between the *vis insita* of molecules and the forces that bind them together. It seems, therefore, as if the disturbance of the medium which answers to radiation of heat and light were derived from the disturbance of the molecular forces, and that when there was no such disturbance there was no resistance.

But the difficulty yet remains of a body moving in a material medium without resistance. It is almost inconceivable in the present state of our knowledge of molecular physics. If it is at all possible (for there is almost an absurdity involved in asserting that it is possible) it can only be by means of a persistent or continuous relation of intense mutual organised activity between the ether and the perfectly elastic elements of matter, of which we can as yet have very little conception.

The disturbance of molecular forces that allows the medium to absorb the *vis insita* of vibrating molecules may be likened to the ascent and descent of planetary bodies to and from the sun while revolving in elliptic orbits. There is a disturbance when the centripetal force on a body increases or diminishes, so that if a body revolved in a circle there is no disturbance, no resistance, no absorption of its *vis insita* by the active medium, and if it revolves in an eccentric ellipse there is disturbance, resistance, and absorption. The number of luminous vibrations in a second is, by the undulatory theory, from 458 million million to 727 million million, and the

vibrations that cause heat are probably not very different in the velocity of their succession. If a set of molecules thus vibrating in a perfectly cold region took one second to dissipate their molecular *vis viva* by radiation, this is sufficient time for so vast a multitude of revolutions in the molecular orbits that the loss of *vis insita* in our vibration is probably as infinitesimal as that of the planets during one revolution, and we might conjecture that this infinitesimal ratio was a function of the ratio of the elasticity of the ether to the molecular velocity of the vibrations, the elasticity of the ether being apparent only as an active centripetal force. Such a retardation would certainly follow if the velocity of the action of gravitation were not infinite, but there still remains to be suggested the physical condition of a medium that offers no resistance. Is such an entity possible?

### *Note H.—Formula for Measuring Heights by the Thermometer.*

The tension of the atmospheric medium varies, as we have seen, in the proportion of the sixth power of the depth below the summit, and the elasticity of steam varies as the sixth power of the ordinate to the line SF on the chart multiplied by the absolute temperature or square of the corresponding abscissa (see Note B). This suggests a method of measuring heights by the thermometer that requires little or no computation.

Let ST on the chart of vapours, which there represents the sixth root of the density of steam at 212°, be also taken to represent the whole height of the atmosphere above a point where the water boils at the temperature of 212°. Then from $\bar{\omega}$, the ordinate to the steam line at 200°, draw $\bar{\omega}q$ parallel to QS, the line of one atmosphere pressure, or rather, it ought to be converging to the point where the cord QS produced meets the axis. It is evident that SQ represents very nearly the height where water boils at 200°. Now, if such lines are drawn at each degree between 200° and 212°, they will divide S$q$ into parts that are sensibly equal.

The value of each of these parts depends on the value given to ST, which, according to our theory, is in feet 318

times the absolute temperature of the air at the station where water boils at the temperature corresponding to the square of the abscissa of the ordinate ST.

The following is the accurate formula by the theory for any vapour of which the constant G (see Note B) is known:—

$$317{\cdot}6\left\{1-\left(\frac{\sqrt{\tau+461}-G}{\sqrt{t+461}-G}\right)\left(\frac{\tau+461}{t+461}\right)^{\frac{1}{8}}\right\}(T+461)=h,$$

in which T is the temperature of the air at the lower station, $t$ = temperature at which the liquid boils at the lower station, $\tau$ = temperature at which the liquid boils at the upper station (all expressed in degrees on Fahr. scale), G the first constant of the vapour (see Note B), and $h$ the difference of height between the stations in feet. By boiling temperature is meant the temperature at which the tension of the vapour is the same as that of the external atmosphere.

Let us apply the formula to the vapour of water, in which G is 19·4923, and let us take an example where T is 60°, $t = 212°$, and $\tau = 211°$; the value of $h$ is 528·6 feet. It will be found, by taking other values for T and $\tau$, that this elevation for 1° difference in the boiling point increases about $\frac{7}{8}$ of a foot for each degree that $\tau$ diminishes, and increases exactly 1 foot for every degree that T increases, and *vice versa*.

Professor Forbes, who has discussed this subject fully in a recent paper, finds that his observations indicate a number between 540 and 550 feet. The accordance is thus satisfactory; by the formula, 550 feet per 1° is the mean value for $T = 75°$, $t = 212°$, $\tau = 202°$, which corresponds with Professor Forbes' mean; and what it wants of uniformity is too small to be discovered in practice.

Suppose that we wished to employ the vapour of sulphuric ether for this purpose. It boils at 96° under a pressure of 30 in. Dalton's observation on this ether gives a line of vapour which has G = 16·86. Compute the above equation with this value of G, and $t = 96°$, $\tau = 95°$, $T = 60°$. The result is $h = 568$, being $\frac{1}{13}$ greater than in the former case of steam.

The less this ratio, the more exact, of course, is this method of estimating heights, and it depends greatly on the value

of the constant H (see Note B), for the less the inclination of the line of vapour is to the axis the smaller is the height that corresponds to a difference of 1° in the boiling point. For mercury this is only 248 feet, but the temperature is inconveniently high.

Pure sulphuric ether appears to be the best adapted, as its boiling temperature is low, and the disadvantage of the great amount of the difference of elevation for 1° might, perhaps, be compensated for, by employing a very delicate alcohol thermometer, with the divisions of the scale large and extending only from 60° to 100°. The ether need not have access to the air; the equilibrium of pressure may be indicated by the rising of a thin metallic capsule air-tight and elastic.

Such an instrument would require little more than the heat of the hand to bring it into action, and perhaps might be made sufficiently delicate to be used as a machine for taking levels and making sections of a country.

### *Note K.—Central Heat.*

It is not difficult to compute the temperature that the atmosphere would have if it were supposed to be continued through a shaft to the centre of the earth. Suppose the force of gravity to diminish with the central radius, which is the case in a homogeneous sphere, the temperature of the air at the centre would be about 29,000°, and the density much greater than that of any known solid.

Heat, if it is motion, cannot be propagated upwards without loss or conducted downwards without gain, any more than can a body when projected upwards retain its original velocity without diminution, or when projected downwards without increase.

The temperature in mines (according to M. Cordier) increases in descending at the rate of 1° to 50 or 60 feet. Is this the natural condition of vertical equilibrium of molecular *vis viva* within the crust of the globe? If such were the case some regular difference might be found between the top and bottom temperatures in pillars or lofty buildings if precautions could be taken to prevent horizontal conduction.

*Note L.—Nebular Hypothesis.*

The *vis viva* theory appears to harmonize well with the Nebular Hypothesis of Laplace. The intense activity of the molecules of the Sun's mass may be viewed as the result of, or to have been originally produced by, its centripetal force while condensing. The motion generated is not lost, as it is in appearance when inelastic bodies meet each other with equal momenta. The clashing together of the descending elastic matter is followed by equal recoil in the opposite direction, and molecular *vis viva* is generated. We see this take place on a minute scale when metals are hammered, or compressed, or rubbed. Friction and every other expenditure of mechanical force gives birth to heat or molecular *vis viva*, which is dissipated by radiation and conduction.

The observations of Sir John Herschel and Professor Forbes with the actinometer (*Phil. Trans.*, 1842) have recently supplied a knowledge of the absolute value of the solar radiation before it passes through the atmosphere. They have found it to amount to 388·4 actines, each actine being one-millionth of a metre in thickness of ice melted per minute. This is equal to 1·835 ft. of ice melted in a mean solar day.

Now, since it is known that ice requires 140° of heat to melt it, and the mechanical value of 1° in water is equal to the weight of the water raised through 673 feet against the force of gravity on the Earth's surface (§ 25), we have the means of computing exactly the absolute mechanical power of the solar radiation—the absolute force thrown out by the Sun in a given time.

There are various ways of reckoning this and obtaining a clear conception of it with reference to different standards. The fundamental principles are contained in Section 3, and an example of their application is given in detail in § 25. The results of four computations are as follows:—

1. During one year the solar force upon a square foot at the Earth's mean distance from the Sun is equal to 20 tons raised 20 miles, or to about one ton raised one mile per day, which is equivalent to $\frac{1}{4}$ of a horse-power, according to engineers' mode of reckoning.

2. At the Sun its amount in one year is equivalent to the descent of a stratum of the Sun's surface (and of its mean density) $3\frac{1}{3}$ miles thick through its own breadth.

3. If the Sun is supposed to contract uniformly throughout its mass so that its radius becomes $3\frac{1}{3}$ miles less in consequence of the general increase of density, the force generated is sufficient to supply the solar radiation for about 9000 years.

4. If a mass equal to the Earth descended to the Sun's surface from its mean distance, it would acquire a velocity of 390 miles per second, and the *vis viva* generated when it strikes the Sun would amount to the force thrown out by the Sun in 45 years.

The density of the Sun being little more than that of water, it is possible that the mere gradual contraction of its bulk, or natural subsidence of the mass, may generate sufficient force to supply the amount of radiation without any diminution of temperature, and it would appear from the third computation that the decrement of the apparent diameter of the Sun owing to such condensation may not amount to more than $\frac{1}{50}$ of a second in 9000 years.

### *Note M.—Barometric Formula.*

This may include the effect of aqueous vapour by the formula in Note B. Let $\tau$ = mean dew point, $t$ = mean temperature of the air at the two stations. Find

$$\mathrm{P} = \left\{\frac{\sqrt{\tau + 461} - 19{\cdot}736}{10{\cdot}422}\right\}^6 (t + 461),$$

then

$$\mathrm{H} = (\log.\ \mathrm{B} - \log.\ b) \cdot \frac{1250801}{1 - 0{\cdot}376\left(\frac{2\mathrm{P}}{\mathrm{B} + b}\right)} \cdot \left(\frac{461 + t}{9958 + t}\right)$$

is the correct theoretical formula, in which

H = height in feet between the two stations.
B = height of barometer at lower station.
$b$ = height of barometer at higher station brought to the same temperature as B.

*Addition to Notes* B *and* H *of the Paper, "On the Physics of Certain Media," recently submitted to the Royal Society.*

Received January 27, 1846.

In Note B it was shown that the formula of a vapour might be obtained from two experiments on its tension, and in Note H, that the function which defines the law of density in vapours is analogous to what defines the law of tension in ascending the atmosphere, thereby enabling us to construct a rule for measuring heights by the thermometer. It may, perhaps, be useful to add what relates to the law of the tension of mixtures of air and vapour.

In some cases it seems impossible to clear vapours entirely of permanently elastic matter, and it will be allowed to be very desirable, in a practical point of view, that we should be able to deduce the necessary constants from experiments made upon them in their usual state of commixture. It will be found, I believe, that this may be accomplished by means of the data afforded by not less than three experiments if the volume occupied by the gas and vapour remains constant, or if the proportionate changes in it are capable of being accurately determined. We do not require to know anything of the quantity of air enclosed with the vapour: this forms one of the three unknown quantities involved in the three equations afforded by the experiments; the other two being the constants G and H that develop the law of density of the pure vapour.

In the accompanying chart (No. 2), which is drawn on the same scale as the general chart of vapours given in Note B, it may be remarked how the straight lines of vapour are transformed into a high order of hyperbolas when any permanently elastic matter is allowed to contribute its effect of tension. The mode of laying off the points is simply as follows. Suppose we wish to know the effect that air of $\frac{6}{100}$ of an inch of tension at 51° has upon the chart line of aqueous vapour; we have $F^6 t = F^6 (51 + 461) = 0{\cdot}06$. From this we obtain the value of $F^6$, which we employ in the general equation for such mixtures, viz., $F^6 t + t\left(\frac{\sqrt{t} - G}{H}\right)^6 = e$, or $\sqrt[6]{F^6 + \left(\frac{\sqrt{t} - G}{H}\right)^6} = \sqrt[6]{\frac{e}{t}}$ = the ordinate on the chart to the abscissa $\sqrt{t}$. The curve traced out

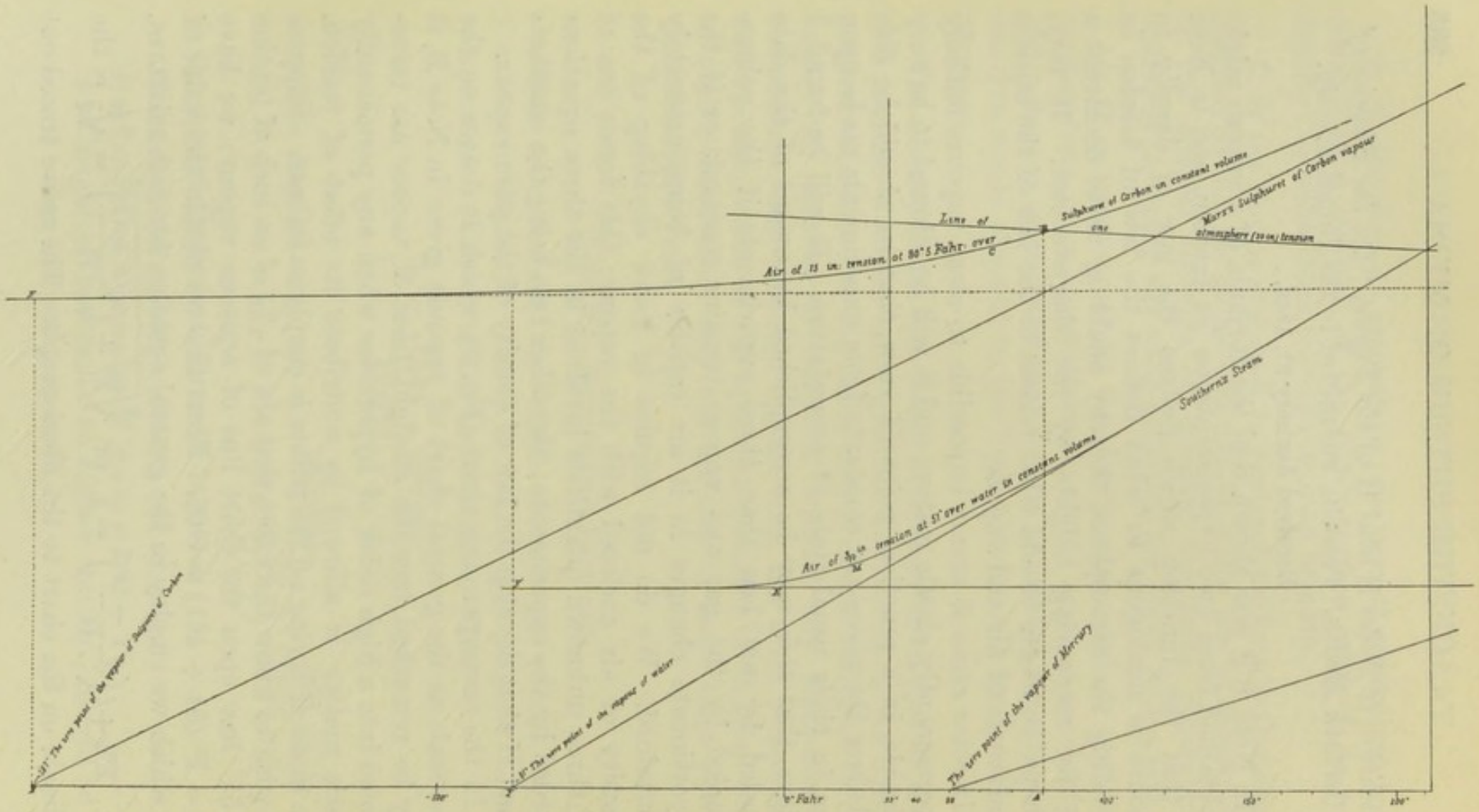
Line of
vapour
Sulphuret of Carbon in constant volume
Mars's Sulphuret of Carbon vapour
atmosphere (30 in.) tension
Air of 15 in. tension, at 80°.5 Fahr. over
C
Air of 30 in tension at 51° over water in constant volume
M
Southern Steam
51°. The zero point of the vapour of Sulphuret of Carbon
32°. The zero point of the vapour of water
The zero point of the vapour of Mercury
0° Fahr
32°
40
50
60
100°
120°

CHART 2.

by these co-ordinates is one leg of a species of hyperbola. The apex of this hyperbola has its ordinate F′F′ (see chart) equal to the element F in the equation, and the corresponding abscissa is G, which is equal to $\sqrt{t}$ at this point which is the zero of the vapour. It makes $\frac{\sqrt{t}-\mathrm{G}}{\mathrm{H}} = 0$, and $\sqrt[6]{\frac{e}{t}} = \mathrm{F}$.

As the tension of a vapour is excessively small for a considerable range of temperature above its zero point, the curve, which begins at F′, does not sensibly leave the tangent at its vertex, F′E,—which is also parallel to the axis at the distance from it, F,—for some distance beyond the point of contact. It then takes a sudden bend, having the greatest curvature at the point where the tension of the vapour is nearly one-half the tension of the air, and ascends along the line of vapour converging towards it as an asymptote. This curve answers very well to the general run of the experiments on aqueous vapour at low temperatures, and those of Professor Magnus that have recently appeared in the 14th number of the *Scientific Memoirs* correspond with it almost exactly.

Are we then to infer from this coincidence that the general divergence from the straight at low temperatures is the effect of a minute portion of air that clings to the water, in spite of all the precautions taken to prevent it, and that it only becomes sensible when the tension of the vapour, *per se*, has descended to the same attenuated proportion; or is the law that is represented by the general equation of Note B, defective to this trifling extent?

Although no attempt has yet been successful to give a physical interpretation of the function of the temperature that represents the density of a vapour, yet it must be considered as a circumstance favourable to the possibility of doing so on the *vis viva* theory, that it corresponds so far with several of the laws of gases or media as like them to involve the sixth power of an element of the temperature. Thus in XVI. (§ 22) it was shown that when a medium was compressed the *vis viva* increased as the mean molecular distance diminished, or, what is the same, that the sixth power of the molecular velocity increased in the same ratio as the density. This actually enables the condition of a gas in respect to density and

temperature, while dilating or being compressed, to be represented on the chart of vapours, and has already been referred to in Note B. The physical demonstration of this peculiarity of function depends ultimately (as shown in Section III.) on the six rectangular directions of space. It seems highly probable, therefore, that the same primary cause shapes the function in the case of vapours, and we may thus be led to hope that in the liquid condition of bodies their molecules are arranged upon a plan more simple and less interwoven with the essential nature of the molecular forces than might otherwise have been anticipated.

In the upper curve, FCS, the ordinates represent the sixth root of the respective densities when the air and vapour are in equal proportion at the ordinary pressure of the atmosphere. The vapour is that of the sulphuret of carbon employed by M. Marx* (*Scient. Mem.*, Part 14), which has 117°·12 as its boiling temperature. With half its volume filled with air it assumes the ordinary pressure of the atmosphere (30 inches) at 80°·55. At this point it crosses the line of the constant pressure of one atmosphere (see Note B) as may be seen on the accompanying chart. It may also deserve to be remarked that the curve crosses this line in a much more sloping direction than the straight line of the pure vapour, and that, consequently, according to what is stated in Note H, such a mixture is so much better adapted to the measurement of heights by the thermometer, inasmuch as 1° represents a much less difference of height in the atmosphere when applied to mixtures of air and a vapour, than with the same vapour in its pure state. We may thus, perhaps, with such mixtures, be enabled to construct an instrument for measuring heights by means of the thermometer which will have all the advantages that are anticipated in Note H from employing the pure vapour of a volatile liquid with a chart line of density having the smallest possible inclination to the axis.

As an example of this let us take the mixture represented by the upper line of density, FCS.

At the temperature 80°·55 and the mercury in the barometer standing at 30 inches, a small quantity of air saturated with

* [? Magnus.—R.]

sulphuret of carbon is enclosed and at the lower temperature, 70°, the tension of the mixture of air and vapour is reduced to 26·76; it is required from these data to determine the height corresponding to a lowering of 1 degree in that temperature which brings the tension to an equilibrium with the external atmosphere.

We have first to compute G by the formula given in Note B as follows:—

$$G = \frac{\sqrt{t_0}\sqrt[6]{\frac{t_0 e_1}{t_1 e_0}} - \sqrt{t_1}}{\sqrt[6]{\frac{t_0 e_1}{t_1 e_0}} - 1};$$

in which $t_0 = 461 + 70$, $t_1 = 461 + 80{\cdot}55$, $e_0 = 26{\cdot}76$, $e_1 = 30{\cdot}00$.

By this we obtain G = 8·742. It is making use of the small arc, CS, as if it were a straight line.

We have next to employ the value of G in the formula of Note H, viz.:—

$$317{\cdot}6\left\{1 - \left(\frac{\sqrt{\tau} - G}{\sqrt{t} - G}\right)\left(\frac{\tau}{t}\right)^{\frac{1}{6}}\right\}. \quad T = h.$$

In this the absolute temperature at the lower of two stations is denoted by T, and $t$ is the temperature at which the tension of the enclosed air and vapour equilibrates the atmospheric pressure at the lower station, the same at the upper station being $\tau$.

Suppose

$$T = (461 + 60),\ t = (461 + 80{\cdot}55), \text{ and } \tau = (461 + 70);$$

the formula computed gives $h = 3127$, which, divided by $t - \tau = 10{\cdot}55$, gives 296½ feet as the value of 1° in such an instrument. This amount varies but little, through a considerable range of temperature and pressure.

This value may be obtained by observing the temperature at the bottom and at the summit of a known height, and dividing the elevation in feet between the two stations by the difference. Neither the law of the vapour nor the amount of air enclosed with it is required.

What if we dismiss the vapour altogether and enclose dry air only? It is evident that the line CS will then become

parallel to the axis and distant from it F, the sixth root of $F^6$, the density, which is constant. The element G becomes infinitely negative, and $\frac{\sqrt{\tau} - G}{\sqrt{t} - G} = 1$, thus simplifying considerably the expression for $h$, which is now converted into $h = 317{\cdot}6\left\{1 - \left(\frac{\tau}{t}\right)^{\frac{1}{6}}\right\} T$.

Let $t = b^6$, and $\tau = (b - \beta)^6 = b^6 - 6b^5\beta$, when $\beta/b$ is a small fraction. By division we have on this hypothesis $\tau/t = 1 - 6\beta/b$, and $(\tau/t)^{\frac{1}{6}} = 1 - \beta/b$, which converts the equation for $h$ into $h = 317{\cdot}6\frac{\beta}{b}T$. To express $\frac{\beta}{b}$ in terms of $t$ and $\tau$, we have $\overline{t - \tau} = 6b^5\beta$, and $\frac{t - \tau}{6t} = \frac{\beta}{b}$. Hence, so long as this fraction is small in comparison to unity, we have the following simple expression for the height in terms of the indications of the thermometer:—

$$h = 317{\cdot}6\left(\frac{T}{t} \cdot \frac{t - \tau}{6}\right).$$

This gives the nearly constant value, 53 feet, for each degree of Fahr. thermometer, at moderate elevations and ordinary temperatures.

This is the lowest possible value of difference of temperature that can be obtained. In ascending through an increment of the height of the atmosphere, we experience one decrement of temperature, and five decrements of density, which, together, make six decrements of tension. These six decrements of tension must be effected in the enclosed air of the instrument before an equilibrium is established, and as the density is a constant quantity they must be produced by means of a lowering of temperature to the amount of six decrements. Thus, six decrements of temperature in the instrument correspond to the same differential height as one decrement of temperature in the atmosphere, or six degrees correspond to 317·6 feet, the difference of height that causes a difference of 1° while in its natural condition of vertical equilibrium.

It appears, therefore, that dry air is in every respect the best in theory for measuring heights with the thermometer by means of such an instrument as is referred to in Note H.

The theory upon which its theory rests has been shown to agree with M. Gay-Lussac's observations during his balloon ascent, § 34, and also with Professor Forbes' original determination of the nearly constant difference of level in the atmosphere that corresponds with the same constant difference in the boiling point of water.

Suppose, then, we take a small glass vessel, in which there is fixed a delicate thermometric apparatus with a large scale. We heat it to about 100°, and the inside being perfectly dry we close it and make it perfectly air-tight by means of a thin, finely polished silver capsule. From 100° down to 60° corresponds to an altitude of only about 2100 feet, so that

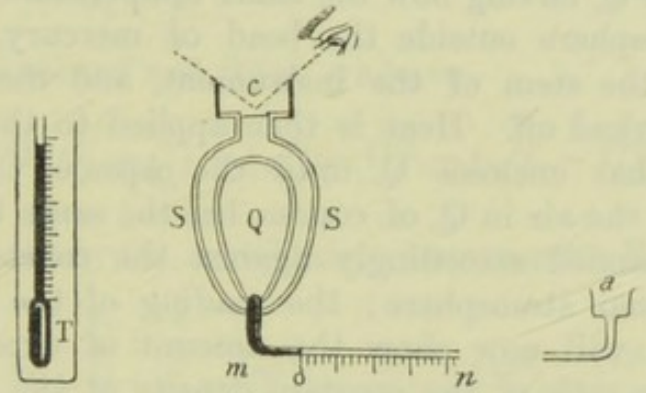


Fig. 5.

for greater elevations or a greater range of the barometer we would require to seal it at a higher temperature, or what amounts to the same thing, partially exhaust the air while fixing the capsule. In afterwards employing this instrument the polished capsule will be a concave mirror so long as the pressure of the atmosphere exceeds the tension of the enclosed air. It will become a plane reflector when they are in equilibrium, and convex when the tension exceeds the atmospheric pressure.

Now, the image of an object is so different in these three kinds of reflectors, that I conceive it will be possible to recognise the point of equilibrium with very considerable accuracy, or, perhaps, better by means of an eye-piece adjusted to a certain angle of reflexion.

The principal difficulty in such an instrument would probably

be in getting the temperature of the air and of the thermometer to be perfectly the same at the instant of equilibrium. M. Brequet's metallic spiral thermometer is, perhaps, the best adapted, and would make the apparatus very portable. But it is the practical artist only who can judge if such an instrument can be made effective.

The annexed sketch is another form of the apparatus, to be used with a delicate mercurial thermometer that may show the temperature of the atmosphere at the station. Q is an air thermometer, with a bead of mercury as index, which, before observing with it, must be blown into the bulb by putting the finger upon the open end, *a*.

The air in Q, having now the same temperature and density as the atmosphere outside the bead of mercury, is allowed to fall into the stem of the instrument, and the reading of the scale marked off. Heat is then applied to the shut glass bottle, SS, that encloses Q, until the capsule, C, shows the equilibrium; the air in Q, of course, has the same temperature, and has expanded accordingly against the constant pressure of the external atmosphere; the reading of the mercury in the scale, *n*, will now show the amount of expansion, and, therefore, the ratio of the constant density of the air enclosed in SS to the density of the atmosphere at the station. Thus, we ascertain the absolute temperature and absolute density of the atmosphere at every station from scales with as large a reading as we please.

To obtain the law of tension of a vapour by three experiments made upon it at different temperatures when mixed with an unknown quantity of air, let $t_0$, $t$, $t_1$ be the three absolute temperatures found by adding 461 to the reading of Fahr. scale, and $e_0$, $e$, $e_1$ the corresponding tensions. Also, let G and H represent the two unknown constants of the vapour, and $F^6$ the constant that represents the quantity of the enclosed air, or number of gaseous molecules, which is the same at all temperatures, while the number of vaporous molecules $\left(\frac{\sqrt{t}-G}{H}\right)^6$ changes with the temperature $t$. These expressions mean the number of molecules in a constant volume, so that the experiments require to be made with

the enclosed volume over the liquid constant. The general expression for the observed tension is

$$e = t\,\mathrm{F}^6 + t\left(\frac{\sqrt{t} - \mathrm{G}}{\mathrm{H}}\right)^6 \quad . \quad . \quad . \quad . \quad . \quad (1).$$

By eliminating $\mathrm{F}^6$ from each of the three experiments, we have

$$\mathrm{F}^6 = \frac{e_0}{t_0} - \left(\frac{\sqrt{t_0} - \mathrm{G}}{\mathrm{H}}\right)^6 = \frac{e}{t} - \left(\frac{\sqrt{t} - \mathrm{G}}{\mathrm{H}}\right)^6 = \frac{e_1}{t_1} - \left(\frac{\sqrt{t_1} - \mathrm{G}}{\mathrm{H}}\right)^6 \quad . \quad (2).$$

From the first and second of these, we have

$$\mathrm{H}^6\left(\frac{e_1}{t_1} - \frac{e}{t}\right) = (\sqrt{t_1} - \mathrm{G})^6 - (\sqrt{t} - \mathrm{G})^6 \quad . \quad (3).$$

From the second and third, we have

$$\mathrm{H}^6\left(\frac{e}{t} - \frac{e_0}{t_0}\right) = (\sqrt{t} - \mathrm{G})^6 - (\sqrt{t_0} - \mathrm{G})^6 \quad . \quad . \quad . \quad (4).$$

Dividing (3) by (4), we obtain a known ratio R,

$$\mathrm{R} = \frac{\dfrac{e_1}{t_1} - \dfrac{e}{t}}{\dfrac{e}{t} - \dfrac{e_0}{t}} = \frac{(\sqrt{t_1} - \mathrm{G})^6 - (\sqrt{t} - \mathrm{G})^6}{(\sqrt{t} - \mathrm{G})^6 - (\sqrt{t_0} - \mathrm{G})^6} \quad . \quad . \quad . \quad (5).$$

From this we may eliminate G by trial and error. The shortest way of making this computation is, perhaps, the following:—

Assume any value A for G, and substitute it in (5), and compute the corresponding value of R, which let us denote by N. If we make the proportion, as the differential of N is to the differential of A, so is the difference between N and R to $\delta$, the difference between A and G, we have

$$\tfrac{1}{6} \times \frac{\{(\sqrt{t_1} - \mathrm{A})^6 - (\sqrt{t} - \mathrm{A})^6\} - \{(\sqrt{t} - \mathrm{A})^6 - (\sqrt{t_0} - \mathrm{A})^6\}\,\mathrm{R}}{\{(\sqrt{t} - \mathrm{A})^5 - (\sqrt{t_0} - \mathrm{A})^5\}\,\mathrm{N} - \{(\sqrt{t_1} - \mathrm{A})^5 - (\sqrt{t} - \mathrm{A})^5\}} = \delta, \text{ and } \mathrm{A} - \delta = \mathrm{A}_1,$$

which approximates nearer to G. Substituting this value in the place of A in the above equation, we obtain the next value of $\delta$, which call $\delta_1$, and $\mathrm{A}_1 - \delta_1 = \mathrm{A}_2$, which approximates still nearer to G.

We arrive more quickly at the exact value of G by making $A - \frac{N}{R}\delta = A_1$, and $A_1 - \frac{N_1}{R}\delta_1 = A_2$. Having thus found $A_1$, $A_2$, N, $N_1$, $N_2$, we may lay off A as the ordinate to N, $A_1$ to $N_1$, and $A_2$ to $N_2$, then, drawing a curve through these points, the ordinate to it opposite R is G, which, in this way, may be obtained very exactly.

As an example, the following three observations are taken from Professor Magnus's experiments on the elastic force of steam, that have recently appeared in the 14th number of the *Scientific Memoirs*.

| | |
|---|---|
| $e_0 = 0{\cdot}178$ | $t_0 = 493 = (461 + 32)$ |
| $e = 3{\cdot}793$ | $t = 585{\cdot}1 = (461 + 124{\cdot}1)$ |
| $e_1 = 29{\cdot}920$ | $t_1 = 673 = (461 + 212)$ |

Computing the preceding formulæ with these data, a few trials give G = 19·625. Then, by (3) or (4), we get H = 10·62, and by (2), from the first experiment at the lowest temperature, we obtain the value of $F^6$, and then $F^6$ (51 + 461) = 0·08, or $\frac{1}{375}$ part of an atmosphere of permanently elastic matter at 51° Fahr.

The line on the chart which answers to the experiments of Southern and the French Academy has G = 19·492, and H = 10·83.

It is obvious that one of the experiments ought to be taken at as low a temperature as possible, and that $F^6$ should be computed from its data.

The general formula for vapour, $t\left(\frac{\sqrt{t} - G}{H}\right)^6 = e$, is easy to compute when the tension for a given temperature is required; but when the temperature that corresponds to a given tension is sought, the equation does not admit of direct solution. The following is, perhaps, the simplest method of overcoming the difficulty. It is founded on the property of the tangent to the curve of constant pressure, alluded to in Note B.

The length of that portion of it intercepted between the point of contact and the axis of the curve, is equal to three times the length of the abscissa.

The tension given being $\epsilon$, and the corresponding absolute temperature $\tau$ being required, assume any absolute temperature as near to $\tau$ as may be guessed roughly; then, by the formula for the given vapour, $t\left(\frac{\sqrt{t}-\mathrm{G}}{\mathrm{H}}\right)^6 = e$, compute $e$. The value of $\tau$ may then be found directly, with all necessary precision, by the following equation:—

$$\tau = \left\{\frac{4\mathrm{H}\sqrt[6]{\epsilon} + 3\mathrm{G}\sqrt[6]{t}}{\mathrm{H}\sqrt{t}\sqrt[6]{\epsilon} + 3\sqrt[6]{t}}\right\}^2.$$

From this value of $\tau$ subtract 461, and we have the temperature required on Fahr. scale that corresponds to the elastic force $\epsilon$.

*December* 15, 1845.

[TABLE

## TABLE OF GASES

### I.—Elementary

| | | Symbol. | Chemical Equivalent. | Boiling Temperature. | Specific Gravity. |
|---|---|---|---|---|---|
| | | | | 1. | 2. |
| | | | | Deg. | |
| 1 | Oxygen . . . . | O | 8 | ... | ... |
| 2 | Hydrogen . . . . | H | 1 | ... | ... |
| 3 | Nitrogen . . . . | N | 14·1 | ... | ... |
| 4 | Chlorine . . . . | Cl | 35·4 | 6 | 1·33 |
| 5 | Bromine (55/31) . . . | Br | 78·4 | 116 | 3·00 |
| 6 | Iodine . . . . . | I | 126·3 | 350 | 4·95 |
| 7 | Arsenic (55/32) . . . | As | 37·7 | 356 | 5·88 |
| 8 | Sulphur (50/170; 55/31) . | S | 16·1 | 600 | 1·99 |
| 9 | Phosphorus (49/211; 50/170) | P | 15·7 | 500 | 1·71 |
| 10 | Mercury (55/32) . . . | Hg | 202 | 650 | 13·57 |
| 11 | Fluorine . . . . | F | 18·7 | ... | (0·73?) |
| 12 | Carbon . . . . | C | 6·1 | ... | 3·52 / 2·50 |
| 13 | Boron . . . . . | B | 10·9 | ... | 1·18 / 1·84 |
| 14 | Silicon . . . . . | Si | 7·5 | ... | ... |
| 15 | Selenium . . . . | Se | 39·6 | 650 | 4·31 |
| 16 | Tin . . . . . | Sn | 58·9 | ... | 7·29 |
| 17 | Antimony . . . . | Sb | 64·6 | ... | 6·70 |
| 18 | Titanium . . . . | Ti | 24·3 | ... | 5·30 |
| 19 | Tellurium . . . . | Te | 32·3 | ... | 6·11 |
| 20 | Chromium . . . . | Cr | 28·0 | ... | 5·90 |

AND VAPOURS.

SUBSTANCES.

| Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. | Bulk of a Molecule. | Combining Proportions in Volumes. | Vapour Constants.* | |
|---|---|---|---|---|---|
| 3. | 4. | 5. | | G. | H. |
| 16 | 16 | ... | $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, ($3\frac{1}{2}$?) | | |
| 1 | 1 | ... | $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, 3, 4, 5, 6, 8, 9, 16 | | |
| 14 | 14 | ... | $\frac{1}{2}$, 1 | | |
| 36 | ... | 27 | $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$ . . . . | 17·0 | 7·24 |
| 80 | 24 | 27 | $\frac{1}{2}$, 1, $1\frac{1}{2}$ | | |
| 125 | 35 / 39 / 60 | 25 | $\frac{1}{2}$, 1, $1\frac{1}{2}$ | | |
| 152 | 40 | 26 | $\frac{1}{4}$, 1 | | |
| 96 | 16 | 48 | $\frac{1}{6}$, $\frac{2}{6}$ | | |
| 64 | 16 / 8 | 38 | $\frac{1}{4}$ | | |
| 101 | 101 | 7·3 | 1 . . . . . | 22·606 | 20·002 |
| [19] | ... | ... | $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$ | | |
| [12] | 26 / 12 / 19 | 3·4 | $\frac{1}{2}$, 1, 2, 3, $3\frac{1}{2}$, 4, $4\frac{1}{2}$, 5, $7\frac{1}{2}$, 8, 10 | | |
| [11] | ... | ... | $\frac{1}{2}$ | | |
| [15] | ... | ... | 1 | | |
| [40] | 40 | 9·2 | 1 | | |
| [58] | 58 | 8 | 1 | | |
| [64] | 64 | 9·5 | 1 | | |
| [24] | ... | 4·5 | 1 | | |
| [64] | 64 | 10·5 | 1 | | |
| [28] | ... | 5·5 | 1 | | |

* See Note B.

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 21 | Water . . . . . | 212 | 1·00 | 9 | 3·2 |
| 22 | Peroxide of hydrogen . . | ... | 1·45 | [8½] | .. |
| 23 | Binoxide of nitrogen . . . | ... | ... | 15 | ... |
| 24 | Nitrous oxide . . . . | ... | ... | 22 | ... |
| 25 | Nitrous acid . . . . | 82 | 1·45 | 46 | ... |
| 26 | Hyponitrous acid (55/32) . . | [0?] | [1·1?] | 25·3 | ... |
| 27 | Nitric acid . . . . | ... | 1·70 | 54 | ... |
| 28 | Quadrihydrate of nitric acid (68/417) | 248 | 1·42 | 18 | ... |
| 29 | Carbonic oxide . . . . | ... | ... | 14 | ... |
| 30 | Carbonic acid . . . . | [−160?] | {0·83 / 0·45} | 22 | 22 |
| 31 | Oxalic acid . . . . | [330] | [2·4?] | [36] | ... |
| 32 | Oxalic hydrate . . . . | ... | ... | [45] | ... |
| 33 | Arsenious acid (55/35) . . | 380 | 3·7 | 200 | 25 |
| 34 | Arsenic acid . . . . | ... | [4·3?] | [232] | ... |
| 35 | Sulphurous acid . . . | 14 | 1·42 | 32 | ... |
| 36 | Sulphuric acid (55/32) . . | 115 | 2·29 | 40 | 9·2 |
| 37 | Sulphuric hydrate . . . | 620 | 1·85 | [49] | ... |
| 38 | Ammonia . . . . . | −46? | 0·76 | 8½ | ... |
| 39 | Arseniuretted hydrogen . . | ... | ... | 39½ | ... |
| 40 | Selenious acid (55/36) . . | ... | ... | 56 | ... |
| 41 | Sulphuretted hydrogen . . | −90? | 0·90 | 17 | ... |
| 42 | Bisulphuretted hydrogen . . | ... | 1·77 | [16½] | ... |
| 43 | Phosphoretted hydrogen . . | ... | ... | 17½ | ... |
| 44 | Seleniuretted hydrogen (68/423) | ... | ... | 41 | .. |
| 45 | Tellurated hydrogen . . . | ... | ... | 65 | ... |
| 48 | Muriatic acid . . . . | −160? | 2·30? | 18½ | ... |

COMPOUNDS.

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| 9·0 | $O_{\frac{1}{4}}H$ | . . . . . | 19·592 | 10·830 |
| [6·0] | $[O_{\frac{1}{4}}H_{\frac{1}{4}}]$ | | | |
| ... | $O_{\frac{1}{4}}N_{\frac{1}{4}}$ | | | |
| ... | $O_{\frac{1}{4}}N$ | | | |
| 32 | $O_2N$ | | | |
| ... | $ON_{\frac{1}{4}}$ | $2(O_{\frac{1}{4}}N_{\frac{1}{4}}) + O_2N = 3$ vols. | | |
| 32 | $O_{2\frac{1}{4}}N$ | | | |
| 63 | $O_{15}N_{\frac{1}{4}}H_{\frac{1}{4}}$ | $O_{2\frac{1}{4}}N + 4(HO_{\frac{1}{4}}) = 5$ vols. | | |
| ... | $O_{\frac{1}{4}}C_{\frac{1}{4}}$ | | | |
| ... | $OC_{\frac{1}{4}}$ | . . . . . | 10·09 | 10·62 |
| ... | $[O_{1\frac{1}{4}}C]$ | | | |
| ... | $[O_2HC]$ | | | |
| 54 | $O_3As$ | | | |
| [54 ?] | $[O_5As]$ | | | |
| ... | $OS_{\frac{1}{4}}$ | . . . . . | 17·38 | 6·97 |
| 17 | $O_{1\frac{1}{4}}S_{\frac{1}{4}}$ | | | |
| [27] | | | | |
| 11·3 | $H_{1\frac{1}{4}}N_{\frac{1}{4}}$ | . . . . . | 13·40 | 10·80 |
| ... | $H_{1\frac{1}{4}}As_{\frac{1}{4}}$ | | | |
| ... | SeO | | | |
| $18\frac{1}{2}$ | $HS_{\frac{1}{4}}$ | | | |
| ... | $[H_{\frac{1}{4}}S_{\frac{1}{4}}]$ | | | |
| ... | $H_{1\frac{1}{4}}P_{\frac{1}{4}}$ | | | |
| ... | HSe | | | |
| ... | HTe | | | |
| [8] | $H_{\frac{1}{4}}Cl_{\frac{1}{4}}$ | | | |

II.—Binary

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 49 | Octohydrate of muriatic acid (68/423) | ... | 1·16 | $10\frac{1}{18}$ | ... |
| 50 | Hydrobromic acid (55/9) . . | ... | 1·76 | $40\frac{1}{2}$ | ... |
| 51 | Hydroiodic acid . . . | ... | ... | 63 | ... |
| 52 | Hydrofluoric acid . . . | 60 | 1·06 | [10] | ... |
| 53 | Cyanogen . . . . . | −8 | 0·90 | 26 | ... |
| 54 | Bisulphuret of mercury (55/36) . | ... | ... | 80 | ... |
| 55 | Sulphuret of carbon . . . | 117·2 | 1·27 | 38 | 10 |
| 56 | Protoxide of chlorine (euchlorine) (m) | ... | ... | 35·2 | ... |
| 57 | Peroxide of chlorine (m) . . | ... | ... | 34·0 | ... |
| 58 | Perchloric acid . . . . | 392 | 1·65 | [92] | ... |
| 59 | Quadrichloride of nitrogen . | 160 | 1·65 | [79] | ... |
| 60 | Sesquichloride of arsenic . . | ... | ... | 92 | ... |
| 61 | Sesquibromide of arsenic . . | ... | ... | 158 | ... |
| 62 | Periodide of arsenic (55/35) . | ... | ... | 225 | ... |
| 63 | Chloride of sulphur . . . | 147 | 1·62 | 52 | ... |
| 64 | Dichloride of sulphur . . | 280 | 1·69 | 68 | ... |
| 65 | Sesquichloride of phosphorus (m) | 205 | 1·45 | 70 | ... |
| 66 | Perchloride of phosphorus (m; 55/5) | 180 | ... | 71 | ... |
| 67 | Protochloride of mercury . . | ... | 7·14 | 118 | ... |
| 68 | Bichloride of mercury . . | ... | 5·42 | 136 | ... |
| 69 | Protobromide of mercury . . | ... | 7·31 | 140 | ... |
| 70 | Bibromide of mercury . . | ... | 5·92 | 180 | ... |
| 71 | Biniodide of mercury . . . | ... | ... | 227 | ... |
| 72 | Dichloride of carbon . . . | 400 | ... | [144] | ... |
| 73 | Protochloride of carbon (71/374) | 252 | 1·62 | 84 | ... |

COMPOUNDS—*continued.*

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| ... | $H_{1\frac{1}{2}}Cl_{\frac{1}{16}}O_{\frac{1}{2}}$ | $Cl_{\frac{1}{2}}H_{\frac{1}{2}} + 8(HO_{\frac{1}{2}}) = 9$ vols. | | |
| 23 | $H_{\frac{1}{2}}Br_{\frac{1}{2}}$ | | | |
| ... | $H_{\frac{1}{2}}I_{\frac{1}{2}}$ | | | |
| [9] | $H_{\frac{1}{2}}F_{\frac{1}{2}}$ | | | |
| 29 | NC | . . . . . | 17·08 | 6·55 |
| ... | $S_{\frac{1}{2}}M_{\frac{1}{2}}$ | $S_{\frac{1}{2}}M + S_{\frac{1}{2}} + M = 3$ vols. ? | | |
| 31 | $S_{\frac{1}{2}}C_{\frac{1}{2}}$ | . . . . . | 16·254 | 12·760 |
| ... | $O_{\frac{1}{2}}Cl_{\frac{1}{2}}$ | $[OCl_{\frac{1}{2}} + Cl] = 2$ vols. ? | | |
| ... | $OCl_{\frac{1}{2}}$ | | | |
| ... | $[O_{3\frac{1}{2}}Cl]$ | | | |
| ... | $[Cl_2N_{\frac{1}{2}}]$ | | | |
| ... | $Cl_{1\frac{1}{2}}As_{\frac{1}{2}}$ | | | |
| ... | $Br_{1\frac{1}{2}}As_{\frac{1}{2}}$ | | | |
| ... | $I_{1\frac{1}{2}}As_{\frac{1}{2}}$ | | | |
| 32 | $ClS_{\frac{1}{2}}$ | | | |
| 40 | $ClS_{\frac{3}{2}}$ | | | |
| 48 | $Cl_{1\frac{1}{2}}P_{\frac{1}{2}}$ | | | |
| ... | $Cl_{\frac{3}{2}}P_{\frac{1}{2}}$ | $Cl_{2\frac{1}{2}}P_{\frac{1}{2}} + Cl + Cl_{1\frac{1}{2}}P_{\frac{1}{2}} = 3$ vols. ? | | |
| 16 | $Cl_{\frac{1}{2}}Hg$ | | | |
| 26 | ClHg | | | |
| 19 | $Br_{\frac{1}{2}}Hg$ | | | |
| 30 | BrHg | | | |
| ... | IHg | | | |
| ... | $[Cl_3C_3]$ | | | |
| 52 | $Cl_2C$ | | | |

II.—Binary

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 74 | Perchloride of carbon . . | 360 | 2·00 | [120] | ... |
| 75 | Perchloride of muriate of methyle (71/385) | 173 | 1·60 | 78 | ... |
| 76 | Terchloride of boron (55/14) . | ... | ... | 59½ | ... |
| 77 | Fluoride of boron . . . | ... | ... | 33 | ... |
| 78 | Chloride of silicium . . . | 124 | ... | 87 | ... |
| 79 | Fluoride of silicium . . . | ... | ... | 62 | ... |
| 80 | Bichloride of tin . . . | ... | ... | 130 | ... |
| 81 | Sesquichloride of antimony (55/36) | ... | ... | 118 | ... |
| 82 | Bichloride of titanium (55/13) . | 220 | ... | 96 | ... |

III.—Ternary,

| | | | | | |
|---|---|---|---|---|---|
| 83 | Hydrocyanic gas . . . | 80 | 0·70 | 13½ | ... |
| 84 | Chloride of cyanogen . . | 12 | 1·32 | 31 | ... |
| 85 | Terchloride of cyanogen (68/424) | 374 | 4·0 ? | 93 | ... |
| 86 | Bromide of cyanogen . . | 100 | 2·2 ? | 53 | ... |
| 87 | Chlorocarbonic oxide gas . . | ... | ... | 50 | ... |
| 88 | Oxichloride of chromium . . | ... | ... | 80 | ... |
| 89 | Chlorosulphuric acid (71/445) . | 170 | 1·66 | 68 | ... |
| 90 | Anhydrous carbonate of ammonia (68/434) | ... | ... | 13 | ... |
| 91 | Anhydrous hydrosulphate of ammonia | 180 | ... | 12¾ | ... |
| 92 | Anhydrous hydrocyanate of ammonia | ... | ... | 11 | ... |
| 93 | Anhydrous hydrotellurate of ammonia | 174 | ... | 36¾ | ... |

COMPOUNDS—*continued.*

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| ... | $[Cl_3C]$ | | | |
| 49 | $Cl_2C_{\frac{1}{2}}$ | | | |
| ... | $Cl_{1\frac{1}{2}}B_{\frac{1}{2}}$ | | | |
| ... | $F_{1\frac{1}{2}}B_{\frac{1}{2}}$ | | | |
| ... | $Cl_2Si$ | | | |
| ... | $Fl_{2\frac{1}{2}}Si$ | | | |
| ... | $Cl_2Sn$ | | | |
| ... | $Cl_{1\frac{1}{2}}Sb$ | | | |
| ... | $Cl_2Ti$ | | | |

ETC., COMPOUNDS.

| | | | | |
|---|---|---|---|---|
| 19 | $H_{\frac{1}{2}}N_{\frac{1}{2}}C_{\frac{1}{2}}$ | $H_{\frac{1}{2}} + \frac{1}{2}(NC)$ | | |
| $23\frac{1}{2}$ | $Cl_{\frac{1}{2}}N_{\frac{1}{2}}C_{\frac{1}{2}}$ | $Cl_{\frac{1}{2}} + \frac{1}{2}(NC)$ . . . . . | 16·43 | 8·42 |
| 23 ? | $Cl_{1\frac{1}{2}}N_{1\frac{1}{2}}C_{1\frac{1}{2}}$ | $3\,(Cl_{\frac{1}{2}} + \frac{1}{2}(NC))$ | | |
| 23 ? | $Br_{\frac{1}{2}}N_{\frac{1}{2}}C_{\frac{1}{2}}$ | $Br_{\frac{1}{2}} + \frac{1}{2}(NC)$ | | |
| ... | $ClO_{\frac{1}{2}}C_{\frac{1}{2}}$ | $Cl + O_{\frac{1}{2}}C_{\frac{1}{2}}$ | | |
| ... | ClOCr | Cl + OCr | | |
| 41 | $ClOS_{\frac{1}{2}}$ | $Cl + OS_{\frac{1}{2}}$ | | |
| ... | $O_{\frac{1}{2}}C_{\frac{1}{2}}N_{\frac{1}{2}}H$ | $OC_{\frac{1}{2}} + 2\,(N_{\frac{1}{2}}H_{1\frac{1}{2}}) = 3$ vols. | | |
| ... | $S_{1\frac{1}{2}}N_{\frac{1}{2}}H_{1\frac{1}{2}}$ | $S_{\frac{1}{2}}H + N_{\frac{1}{2}}H_{1\frac{1}{2}} = 2$ vols. | | |
| ... | $N_{\frac{1}{2}}C_{\frac{1}{2}}H$ | $H_{\frac{1}{2}}N_{\frac{1}{2}}C_{\frac{1}{2}} + N_{\frac{1}{2}}H_{1\frac{1}{2}} = 2$ vols. | | |
| ... | $N_{\frac{1}{2}}Te_{\frac{1}{2}}H_{1\frac{1}{2}}$ | $HTe + N_{\frac{1}{2}}H_{1\frac{1}{2}} = 2$ vols. | | |

III.—Ternary, Etc.

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 94 | Anhydrous muriate of ammonia (sal. am.) | ... | ... | 13½ | ... |
| 95 | Hydroiodate of phosphoretted hydrogen | 174 | ... | ... | ... |
| 96 | Hydrobromate of phosphoretted hydrogen | 85 | ... | ... | ... |
| 97 | Hydrochlorate of phosphoretted hydrogen | ... | ... | 18 | ... |

IV.—Organic

| | | | | | |
|---|---|---|---|---|---|
| 98 | Carburetted hydrogen . . | ... | ... | 8 | ... |
| 99 | Olefiant gas or etherin or hydrocarbon | ... | (0·63) ? | 14 | 14 |
| 100 | Etherine or bietherin . . | 0 | 0·63 | 28 | 6·7 |
| 101 | Elaen . . . . . | 230 | ... | 63 | ... |
| 102 | Ameline . . . . . | 320 | 0·85 | 70 | ... |
| 103 | Ceten . . . . . | 527 | ... | 112 | ... |
| 104 | Benzin or bicarburet of hydrogen (55/45) | 187 | 0·85 | 39 | ... |
| 105 | Mesitylene (70/103) . . . | 330 | ... | 40 | ... |
| 106 | Naphtha . . . . . | 180 | 0·75 | 41 | 6·5 |
| 107 | Naphthalin (50/183) . . . | 412 | .. | 64 | ... |
| 108 | Paranaphthalin . . . . | 572 | ... | 96 | ... |
| 109 | Camphene or oil of turpentine (75/41) | 332 | 0·86 | 68 | 6·6 |
| 110 | Caoutchin or citrene (70/103) . | 440 | 0·67 | 68 | ... |
| 111 | Petrolene or sweet oil of wine (71/411) | 545 | 0·90 | 136 | ... |

COMPOUNDS—*continued*.

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| ... | $Cl_{\frac{1}{4}}HN_{\frac{1}{4}}$ | $Cl_{\frac{1}{4}}H_{\frac{1}{4}} + N_{\frac{1}{4}}H_{1\frac{1}{4}} = 2$ vols. | | |
| ... | $I_{\frac{1}{4}}HP_{\frac{1}{4}}$ | $I_{\frac{1}{4}}H_{\frac{1}{4}} + P_{\frac{1}{4}}H_{1\frac{1}{4}} = 2$ vols. | | |
| ... | $Br_{\frac{1}{4}}HP_{\frac{1}{4}}$ | $Br_{\frac{1}{4}}H_{\frac{1}{4}} + P_{\frac{1}{4}}H_{1\frac{1}{4}} = 2$ vols. | | |
| ... | $Cl_{\frac{1}{4}}HP_{\frac{1}{4}}$ | $Cl_{\frac{1}{4}}H_{\frac{1}{4}} + P_{\frac{1}{4}}H_{1\frac{1}{4}} = 2$ vols. | | |
| COMPOUNDS. | | | | |
| ... | $H_2C_{\frac{1}{4}}$ | | | |
| (22 ?) | $H_2C$ | $\frac{1}{2}$, 1, 2, $4\frac{1}{2}$, 5, 8 (combining ratios) | | |
| (45) | $H_4C_2$ | $2\,(H_2C) = (H_2C)_2$ | | |
| ... | $H_9C_{4\frac{1}{2}}$ | $4\frac{1}{2}\,(H_2C) = (H_2C)_{4\frac{1}{2}}$ | | |
| 82 | $H_{10}C_5$ | $5\,(H_2C) = (H_2C)_5$ | | |
| ... | $H_{16}C_8$ | $8\,(H_2C) = (H_2C)_8$ | | |
| 44 | $H_8C_3$ | | | |
| ... | $H_4C_3$ | | | |
| 56 | $H_5C_3$ | . . . . . | 19·36 | 14·64 |
| ... | $H_4C_5$ | | | |
| ... | $H_6C_5$ | | | |
| 80 | $H_8C_5$ | . . . . . | 18·59 | 15·56 |
| 102 | $H_8C_5$ | | | |
| 151 | $H_{16}C_{10}$ | | | |

IV.—Organic

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 112 | Alcohol . . . . . | 173 | 0·795 | 23 | 5·0 |
| 113 | Sulphuric ether . . . . | 96 | {0·734 / 0·632} | 37 | 5·8 |
| 114 | Oxalic ether (m) . . . | 400 | 1·09 | 73 | ... |
| 115 | Nitrous ether (m) . . . | 80 | 0·89 | 37½ | ... |
| 116 | Succinic ether (m) . . . | 515 | 1·04 | 87 | ... |
| 117 | Formic ether (m) . . . | 132 | 0·92 | 37 | ... |
| 118 | Acetic ether (m) (see 132 and 139) | 205 | 0·89 | 44 | ... |
| 119 | Mercaptan or hydrosulph. of etherine (71/391) | {96 / 143} | 0·83 | 31 | ... |
| 120 | Hydrosulphuric ether or hydrosulph. of etherine (71/389) | 160 | 0·825 | 45 | ... |
| 121 | Benzoic acid or anhydrous carbonate of benzin (55/46) | 528 | 0·66 | 61 | ... |
| 122 | Nitrobenzide or nitrite of benzin (57/87) | 400 | 1·21 | 62 | ... |
| 123 | Oxidized ether or aldehyde (m) (59/295) | 72 | 0·79 | 22 | ... |
| 124 | Chloric ether (m) . . . | 220 | 1·07 | 32 | ... |
| 125 | Chloride of hydrocarbon (58/307) | {178 / 146} | {1·26 / 1·17} | 50 | ... |
| 126 | Bromide of hydrocarbon (59/359) | 322 | 2·16 | 94 | ... |
| 127 | Iodide of hydrocarbon . . | ... | 3·20 | ... | ... |
| 128 | Hydrochloric ether (Thenard) . | 57 | {0·84 / 0·77} | 32½ | ... |
| 129 | Hydrobromic ether . . . | ... | ... | [54½] | ... |
| 130 | Hydroiodic ether (Gay-Lussac and Thenard) | {176 / 148} | 1·92 | 77 | ... |
| 131 | Hydrocyanic ether . . . | 216 | 0·79 | 27½ | ... |

COMPOUNDS—*continued.*

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| 29 | $H_3CO_{\frac{1}{2}}$ | $H_2C + HO_{\frac{1}{2}}$ . . . . . | 19·20 | 10·00 |
| 58 | $H_5C_2O_{\frac{1}{2}}$ | $(H_2C)_2 + HO_{\frac{1}{2}}$ . . . . | 16·86 | 10·99 |
| 66 | $H_5C_3O_2$ | $O_{1\frac{1}{2}}C + (H_2C)_2 + HO_{\frac{1}{2}}$ | | |
| 42 | $H_{2\frac{1}{2}}ON_{\frac{1}{2}}C$ | $\overline{O_{1\frac{1}{2}}N} + \overline{(H_2C)_2 + HO_{\frac{1}{2}}}$ = 2 vols. | | |
| 84 | $H_7C_4O_2$ | $O_{1\frac{1}{2}}H_2C_2 + (H_2C)_2 + HO_{\frac{1}{2}}$ | | |
| 80 | $H_3C_{1\frac{1}{2}}O$ | $\overline{O_{1\frac{1}{2}}HC} + \overline{(H_2C)_2 + HO_{\frac{1}{2}}}$ = 2 vols. | | |
| 100 | $H_4C_2O$ | $\overline{O_{1\frac{1}{2}}H_3C_2} + \overline{(H_2C)_2 + HO_{\frac{1}{2}}}$ = 2 vols. | | |
| 38 | $H_3CS_{\frac{1}{2}}$ | $H_2C + HS_{\frac{1}{2}}$ | | |
| 55 | $H_5C_2S_{\frac{1}{2}}$ | $(H_2C)_2 + HS_{\frac{1}{2}}$ | | |
| 92 | $H_3C_{3\frac{1}{2}}O$ | $H_3C_3 + OC_{\frac{1}{2}}$ | | |
| 51 | $H_3C_3ON_{\frac{1}{2}}$ | $H_3C_3 + ON_{\frac{1}{2}}$ | | |
| 28 | $H_2CO_{\frac{1}{2}}$ | $(H_3C_3)_{\frac{1}{2}} + HO_{\frac{1}{2}}$ or $H_2C + O_{\frac{1}{2}}$ | | |
| 30 | $H_2CCl_{\frac{1}{2}}$ | $H_2C + Cl_{\frac{1}{2}}$ | | |
| 40 | $H_2CCl$ | $H_2C + Cl$ | | |
| 43 | $H_2CBr$ | $H_2C + Br$ | | |
| ... | $[H_2CI]$ | | | |
| 40 | $H_{2\frac{1}{2}}Cl_{\frac{1}{2}}C$ | $H_2C + Cl_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |
| ... | $H_{2\frac{1}{2}}Br_{\frac{1}{2}}C$ | $H_2C + Br_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |
| 40 | $H_{2\frac{1}{2}}I_{\frac{1}{2}}C$ | $H_2C + I_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |
| 35 | $H_{2\frac{1}{2}}N_{\frac{1}{2}}C_{1\frac{1}{2}}$ | $H_2C + N_{\frac{1}{2}}C_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |

IV.—ORGANIC

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 132 | Acetic ether (49/209). Pyro-acetic spirit (see 118 and 139) | 132 | 0·79 | 29 | ... |
| 133 | Methylal (70/394) . . . | 107 | 0·85 | 38 | ... |
| 134 | Benzoic ether (m) . . . | 502 | 1·05 | 75 | ... |
| 135 | Pyroxilic spirit (58/11) . . | 150 | 0·80 | 16 | ... |
| 136 | Pyroxilic ether (58/21) . . | ... | 0·80 | 23 | ... |
| 137 | Nitrate of pyroxilic ether (58/42) | 156 | 1·18 | 38½ | ... |
| 138 | Sulphate of pyroxilic ether (58/34) | 370 | 1·32 | 63 | ... |
| 139 | Acetate of pyroxilic ether (49/209; 58/47) (see 118 and 132) | 136 | 0·92 | 37 | ... |
| 140 | Formicate of pyroxilic ether (m) | ... | 0·80 | 30 | ... |
| 141 | Chloride of methyle (71/380) . | 85 | 1·34 | 43 | ... |
| 142 | Muriate of methyle (58/27) . | ... | [0·82?] | 25½ | ... |
| 143 | Hydroiodate of methyle (58/30) | 110 | 2·24 | 70 | ... |
| 144 | Hydrofluorate of methyle (61/194) | ... | ... | 17 | ... |
| 145 | Benzoate of methyle (58/52) . | 380 | 1·10 | 68 | ... |
| 146 | Formic acid (see 117 and 140 and 147) | ... | 1·16 | [37] | ... |
| 147 | Hydrate of formic acid (68/421) | ... | 1·12 | 23 | ... |
| 148 | Hydrate of ameline (56/317) . | 265 | 0·82 | 44 | ... |
| 149 | Acetate of ameline (75/197) . | 257 | ... | 65 | ... |
| 150 | Hydroiodate of ameline (71/95) | 248 | ... | 98 | ... |
| 151 | Camphor or oxide of camphene (m) | 400 | 1·00 | 76 | ... |
| 152 | Bichlorhydrate of camphene (75/37) | ... | 1·02 | [86] | ... |

COMPOUNDS—*continued.*

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| 37 | $H_3C_{1\frac{1}{2}}O_{\frac{1}{2}}$ | $H_2C + O_{\frac{1}{2}}C_{\frac{1}{2}}H$ | | |
| 44 | $H_4C_{1\frac{1}{2}}O$ | $(H_2C + O_{\frac{1}{2}}C_{\frac{1}{2}}H) + HO_{\frac{1}{2}}$ | | |
| 71 | $H_5C_{4\frac{1}{2}}O$ | $H_2C + (H_3C_3 + C_{\frac{1}{2}}O)$ | | |
| 20 | $H_2C_{\frac{1}{2}}O_{\frac{1}{2}}$ | $(H_2C)_{\frac{1}{2}} + HO_{\frac{1}{2}}$ . . | 18·06 | 11·00 |
| 29 | $H_3CO_{\frac{1}{2}}$ | $H_2C + HO_{\frac{1}{2}}$ | | |
| 65 | $H_{1\frac{1}{2}}C_{\frac{1}{2}}O_{1\frac{1}{2}}N_{\frac{1}{2}}$ | $\overline{O_{2\frac{1}{2}}N} + \overline{H_2C + HO_{\frac{1}{2}}} = 2$ vols. | | |
| 48 | $H_3CO_2S_{\frac{1}{2}}$ | $\overline{O_{1\frac{1}{2}}S_{\frac{1}{2}}} + \overline{H_2C + HO_{\frac{1}{2}}} = 2$ vols. | | |
| 80 | $H_3C_{1\frac{1}{2}}O$ | $\overline{O_{1\frac{1}{2}}C_2H_3} + \overline{H_2C + HO_{\frac{1}{2}}} = 2$ vols. | | |
| 38 | $H_2CO$ | $\overline{O_{1\frac{1}{2}}CH} + \overline{H_2C + HO_{\frac{1}{2}}} = 2$ vols. | | |
| 32 | $HClC_{\frac{1}{2}}$ | $Cl + (H_2C)_{\frac{1}{2}}$ | | |
| [32] | $H_{1\frac{1}{2}}Cl_{\frac{1}{2}}C_{\frac{1}{2}}$ | $Cl_{\frac{1}{2}}H_{\frac{1}{2}} + (H_2C)_{\frac{1}{2}}$ | | |
| 31 | $H_{1\frac{1}{2}}I_{\frac{1}{2}}C_{\frac{1}{2}}$ | $I_{\frac{1}{2}}H_{\frac{1}{2}} + (H_2C)_{\frac{1}{2}}$ | | |
| ... | $H_{1\frac{1}{2}}F_{\frac{1}{2}}C_{\frac{1}{2}}$ | $F_{\frac{1}{2}}H_{\frac{1}{2}} + (H_2C)_{\frac{1}{2}}$ | | |
| 62 | $H_4C_4O$ | $(H_2C)_{\frac{1}{2}} + (H_3C_3 + C_{\frac{1}{2}}O)$ | | |
| [31] | $[HCO_{1\frac{1}{2}}]$ | | | |
| 20 | $HC_{\frac{1}{2}}O$ | $\overline{HCO_{1\frac{1}{2}}} + \overline{HO_{\frac{1}{2}}} = 2$ vols. | | |
| 54 | $H_6C_{2\frac{1}{2}}O_{\frac{1}{2}}$ | $(H_{10}C_5)_{\frac{1}{2}} + HO_{\frac{1}{2}}$ | | |
| ... | $H_7C_{3\frac{1}{2}}O$ | $H_2CO + (H_{10}C_5)_{\frac{1}{2}}$ | | |
| ... | $H_{5\frac{1}{2}}C_{2\frac{1}{2}}I_{\frac{1}{2}}$ | $I_{\frac{1}{2}}H_{\frac{1}{2}} + (H_{10}C_5)_{\frac{1}{2}}$ | | |
| 76 | $H_8C_5O_{\frac{1}{2}}$ | $H_8C_5 + O_{\frac{1}{2}}$ | | |
| ... | $H_{8\frac{1}{2}}C_5Cl_{\frac{1}{2}}$ | $H_8C_5 + Cl_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |

IV.—ORGANIC

| | | Boiling Temperature. | Specific Gravity. | Gaseous Specific Gravity. H=1. | Ditto inferred from Specific Heat. |
|---|---|---|---|---|---|
| | | 1. | 2. | 3. | 4. |
| | | Deg. | | | |
| 153 | Bromohydrate of camphene . | ... | 1·28 | [108½] | ... |
| 154 | Hydroiodate of camphene . . | ... | 1·51 | [131] | ... |
| 155 | Valeric acid (m) . . . | 344 | 0·94 | 81 | ... |
| 156 | Chloride of benzide (m) (see 160) | 400 | 1·46 | 91½ | ... |
| 157 | Chloroetheride or chloroforme (71/382; 56/119) | 141 | 1·48 | 60½ | ... |
| 158 | Bromoetheride or bromoforme . | ... | 3·1 ? | 126½ | ... |
| 159 | Iodetheride or iodiforme . . | 255 | 5·0 ? | [194] | ... |
| 160 | Bichloride of muriatic ether (71/365) (see 156) | 167 | 1·37 | 67½ | ... |
| 161 | Trichloride of muriatic ether . | 216 | 1·53 | 85 | ... |
| 162 | Quadrichloride of muriatic ether | 295 | 1·64 | 102½ | ... |
| 163 | Chloral or carbonite of chloroforme (56/29) | 200 | 1·50 | 74½ | ... |
| 164 | Bromal or carbonite of bromoforme | 260 ? | 3·35 | ... | ... |
| 165 | Hydrate of chloral (56/135) . | ... | ... | 41¾ | ... |
| 166 | Monochloride of methylitic ether (71/399) | 220 | 1·31 | 60 | ... |
| 167 | Bichloride of methylitic ether . | 265 | 1·61 | 93 | ... |
| 168 | Perchloride of methylitic ether . | 212 | 1·59 | 64 | ... |
| 169 | Muriate of benzin (58/319) . | 0 | ... | 31½ | ... |
| 170 | Hydrobromide of benzin (59/364) | 95 | 1·52 | 53½ | ... |
| 171 | Hydroiodate of benzin . . | 135 | | | |
| 172 | Oxichlorocarbonic ether (54/228) | 200 | 1·13 | 54½ | ... |
| 173 | Urethane (54/236) . . . | 226 | 1·11 | 44½ | ... |

COMPOUNDS—*continued.*

| Bulk of a Molecule. | Chemical Analysis. | Chemical Arrangement of Molecules (Hypothesis). | Vapour Constants. | |
|---|---|---|---|---|
| 5. | | | G. | H. |
| ... | $H_{8\frac{1}{2}}C_5Br_{\frac{1}{2}}$ | $H_8C_5 + Br_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |
| ... | $H_{8\frac{1}{2}}C_5I_{\frac{1}{2}}$ | $H_8C_5 + I_{\frac{1}{2}}H_{\frac{1}{2}}$ | | |
| 86 | $H_5C_5O$ | $H_3C_3 + H_2C + 2(O_{\frac{1}{2}}C_{\frac{1}{2}})$ | | |
| 62 | $H_{11}C_3Cl_{11}$ | $(H_{\frac{1}{2}}Cl_{\frac{1}{2}})_3C_3$ | | |
| 41 | $H_{\frac{1}{2}}Cl_{11}C_{\frac{1}{2}}$ | $ClC_{\frac{1}{2}}(Cl_{\frac{1}{2}}H_{\frac{1}{2}})$ | | |
| [41] | $H_{\frac{1}{2}}Br_{11}C_{\frac{1}{2}}$ | $BrC_{\frac{1}{2}}(Cl_{\frac{1}{2}}H_{\frac{1}{2}})$* | | |
| [41] | $[H_{\frac{1}{2}}I_{11}C_{\frac{1}{2}}]$ | $[IC_{\frac{1}{2}}(Cl_{\frac{1}{2}}H_{\frac{1}{2}})]$* | | |
| 50 | $H_{11}Cl_{11}C$ | $(H_{\frac{1}{2}}Cl_{\frac{1}{2}})_3C$ | | |
| 55 | $Cl_2HC$ | $Cl(H_{\frac{1}{2}}Cl_{\frac{1}{2}})_2C$ | | |
| 62 | $Cl_{2\frac{1}{2}}H_{\frac{1}{2}}C$ | $Cl_2(H_{\frac{1}{2}}Cl_{\frac{1}{2}})C$ | | |
| 50 | $Cl_{11}H_{\frac{1}{2}}CO_{\frac{1}{2}}$ | $Cl(H_{\frac{1}{2}}Cl_{\frac{1}{2}})C_{\frac{1}{2}} + C_{\frac{1}{2}}O_{\frac{1}{2}}$ | | |
| 42 | $Br_{11}H_{\frac{1}{2}}CO_{\frac{1}{2}}$ | $Br(H_{\frac{1}{2}}Br_{\frac{1}{2}})C_{\frac{1}{2}} + C_{\frac{1}{2}}O_{\frac{1}{2}}$ | | |
| ... | ... | $\overline{\vert Cl(H_{\frac{1}{2}}Cl_{\frac{1}{2}})C_{\frac{1}{2}} + C_{\frac{1}{2}}O_{\frac{1}{2}}\vert} + \overline{\vert HO_{\frac{1}{2}}\vert} = 2$ vols. | | |
| 46 | $ClH_2CO_{\frac{1}{2}}$ | $ClH_2C_{\frac{1}{2}} + C_{\frac{1}{2}}O_{\frac{1}{2}}$ | | |
| 58 | $Cl_2HCO_{\frac{1}{2}}$ | $Cl_2HC_{\frac{1}{2}} + C_{\frac{1}{2}}O_{\frac{1}{2}}$ | | |
| 40 | $Cl_{11}C_{\frac{1}{2}}O_{\frac{1}{2}}$ | $\overline{\vert Cl_2ClC_{\frac{1}{2}}\vert} + \overline{\vert C_{\frac{1}{2}}O_{\frac{1}{2}}\vert} = 2$ vols. | | |
| ... | $Cl_{\frac{1}{2}}H_{11}C$ | $Cl_{\frac{1}{2}}H_{\frac{1}{2}} + \frac{1}{3}(H_3C_3)$ | | |
| 36 | $Br_{\frac{1}{2}}H_{11}C$ | $Br_{\frac{1}{2}}H_{\frac{1}{2}} + \frac{1}{3}(H_3C_3)$ | | |
| 48 | $Cl_{\frac{1}{2}}H_{2\frac{1}{2}}C_{11}O$ | $Cl_{\frac{1}{2}}H_{\frac{1}{2}} + H_2C + OC_{\frac{1}{2}}$ | | |
| 40 | $N_{\frac{1}{2}}H_{3\frac{1}{2}}OC_{11}$ | $N_{\frac{1}{2}}H_{11} + H_2C + OC_{\frac{1}{2}}$ | | |

*[*Sic* in MS.]

## Explanation of Table of Gases and Vapours.

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Having found the preceding Table useful to refer to while studying the subject of gases and vapours, I have been led to hope that it might be made available, so far as it goes, in shortening the labour of drawing up a complete view of their physical constants. That such a condensed view of their physical character and constitution is a desideratum will probably be generally admitted, and principally with reference to theoretical chemistry does it seem to be of importance to have their molecular characteristics placed before the eye in a clear and concise manner.

The tables of this description that are usually inserted in chemical treatises do not, perhaps, give to the arithmetic of volumes all the clearness that it is susceptible of. This is a consequence of employing the chemical equivalent or lowest combining proportion as the unit, whether or not it happens to correspond with the specific gravity of the gas, and it is generally either half this ratio or, as in the case of sulphur, of phosphorus, and of arsenic, even a smaller fraction of it.

Thus we have H + O the symbol for water. In Dr Turner's *Chemistry* its constitution is thus defined: 1, or one equivalent of hydrogen, + 8, or one equivalent of oxygen, = 9 the equivalent of water; and, by volume, 100 of hydrogen combines with 50 of oxygen to form 100 of steam. If we take 16 as the equivalent of oxygen, which corresponds with its specific gravity, then $HO_{\frac{1}{2}}$ expresses distinctly the constitution of steam both by weight and volume. Another objectionable point may sometimes be remarked though it has now almost disappeared, the combining ratio by volume is inserted before any determination of the fact has been made: thus Dr Turner defines the peroxide of hydrogen, 100 volumes, to consist of 100 oxygen + 100 hydrogen, whereas, since the vapour has never been weighed, it is just as likely to consist of 50 volumes of each; both gases unite with others in several instances in half volumes, so that, until the experiment has been made, all that can be stated is that 17 by weight of the peroxide consists of 1 hydrogen + 16 oxygen. As soon as we have ascertained

the space occupied by the 17 of the peroxide in comparison to the 1 of the hydrogen we can state its composition by volume, but it is surely confusing the subject of volumes altogether to infer this from the combining weights alone.

In this Table the specific gravity of a simple gas in terms of hydrogen unity is taken as the value of its symbol (Col. 3, Part I.), and when this is inserted in parentheses (as from Nos. 10 to 20), it represents the value that has been assigned to the symbolical letter of the element in the compounds that follow, and indicates that it has not as yet been weighed in the simple vapour.

Thus the value of C is 12, of F 19, etc., throughout the Table wherever these letters occur, and at the side these numbers are taken as unity in denoting the several proportions with which they have been found to combine in a single volume of compound gases and vapours. Opposite hydrogen, for example, we see numbers from $\frac{1}{2}$ up to 16, which informs us that from $\frac{1}{2}$ a volume up to 16 volumes of this gas enters into one volume of its compounds, and the same with regard to the others. These indicate in some degree the molecular capabilities of the element. They are ratios that have been taken from vapours that have actually been weighed as well as analysed. A large proportion of them are of recent determination, and the original details of the experiments by Dumas, Mitscherlich, Regnault, Laurent, and Bineau are to be found for the most part in the *Annales de Chimie*; the reference to the volume and page of this invaluable work is given in parentheses after the name of the vapour, and the letter (m) is a reference to Mitscherlich's *Chemistry*, where several specific gravities of vapour are given that are not to be found elsewhere.

In the table of binary compounds a column is occupied with the chemical constitution of a single volume of each in terms of volumes of its elementary components. Thus, nitric acid is represented by $O_{2\frac{1}{2}}N$, which means that one volume of nitric acid vapour is composed of $2\frac{1}{2}$ volumes oxygen united to one volume nitrogen, and where a volume has not been weighed, although its constitution by weight is known, the symbol is within parentheses, thus, [ ].

In the ternary and organic compounds the simple constitution of a volume is given in the first place, as with the binary, and in the next column the most probable arrangement of the constituents, when there is any ground for making a hypothesis.

Thus we have oxalic ether, No. 114, evidently composed of one volume oxalic acid and one volume of sulphuric ether condensed into one volume. This also allows us to infer with great probability the specific gravity of oxalic acid vapour. The next is nitrous ether, No. 115, which is quite a similar compound in the liquid form, but it will be remarked that in the act of rising into vapour it is decomposed, and what is one volume in the case of oxalic ether vapour is two volumes in the case of nitrous ether. Several other examples of this kind will be found. I have already referred to such facts as being favourable to the hypothesis of media which attributes a mechanical origin to the law of volumes, and have likewise referred to the remarkable circumstance that compounds which thus disunite in the act of vaporization nevertheless obey the general law of vapours (see Note B). As it seems, from the nature of the function that expresses this law, that a mechanical origin may be found for it also, the investigation of the subject might perhaps be made easier if the chart lines of vapour were determined for mixtures of pure alcohol and water in all proportions, and also for mixtures of alcohol and ether.

We should then, perhaps, discover the law of variation of the two constants G and H of the chart line, and this might provide us with a new condition or effect of the primary cause pointing to its origin from a new ground.

The two last columns contain the constants G and H, referred to in Note B; where there are three places of decimals the numbers are nearly exact, when two only they are to be considered as approximate.

Column No. 1 contains the temperature on Fahrenheit scale at which the vapour in contact with its generating solid or liquid equilibrates a pressure of 30 inches of mercury.

Column No. 2 contains the specific gravity of the body in its usual liquid or solid form.

Column No. 3 contains, as before mentioned, the specific gravity of the body in its gaseous form in terms of hydrogen

unity. It expresses the weight of a molecule of the hypothetical medium that answers to the gas in its physical relations.

Column No. 4 contains the inverse of the specific heat of the body in its usual liquid, solid, or gaseous form. The numbers are found by dividing the constant 3·2 by the specific heat. This constant is the product of the specific heat of air by its specific gravity in terms of hydrogen unity, and to the same product of all gases that conform to the law of equal volumes having the same specific heat. It is likewise the product of the specific heat of mercury in its liquid form by the specific gravity of its vapour (Note E). In other elementary bodies this product is a simple multiple of the same constant. In compounds the same product is also in most cases a simple multiple of the same constant. On the *vis viva* theory of heat the numbers in this column probably show the mean weight of the component parts of the gaseous molecule that have an independent motion when the body is in the liquid or solid form. Thus, No. 33, arsenious acid: the specific gravity of the vapour is 200 times that of hydrogen; but its specific heat in the solid form is 8 times what it is in the state of vapour, if in this form it obeyed the law of the specific heat of gases. Hence 25 is the number opposite in this column, which, since it goes 8 times in 200, shows that the molecule consists of about 8 parts, each of which has an independent motion. We see further that $O_3As$ expresses the constitution of one volume of this acid, and referring to As, No. 7, we find that its specific heat in the solid form is nearly 4 times greater than it ought to be in the vapour.

The probable general inference is that such molecules consist of several parts, more or less free to move independently of each other, and that when they escape from the bonds of liquid cohesion and become free projectiles, these parts can no longer assume *vis viva* of their own, but are in subjection to the impressed condition of their common centre of gravity.

The great question in this department is, Do such compound bodies which have so great a specific heat in the solid form have it all at once reduced in so vast a proportion when raised to vapour? This interesting point, as remarked at length in Note B, has yet to be determined by an experiment made on

the specific heat of sulphuric ether vapour, as being the most accessible, which, if these views are correct, ought to be only about $\frac{1}{6}$ of the specific heat of the liquid.

Column No. 5 contains the quotient of the gaseous specific gravity by the specific gravity of the liquid or solid, and represents the relative size of the molecules. The subject of atomic volume has recently been the subject of interesting discussion by M. Kopp (*Ann. de Chim.*, vol. 75, 1840, p. 406), and, doubtless, will increase in importance as science advances.

To these physical characteristics of gases it would be well if we could add the temperature of liquefaction, the latent heat of liquefaction, or the measure of the solid polar cohesion of molecules according to the *vis viva* theory, the differential of expansion through a range of temperature, and the latent heat of vapours or measure of liquid molecular cohesion.

*December* 24, 1845.

This paper being the last in connection with the *vis viva* theory of gases that the writer is likely to have an opportunity of submitting to the Society, he begs, in taking leave of the subject, to express a hope that, although the nature of the fundamental hypothesis is likely to be repulsive to mathematicians, they will not reject it without a fair trial. The principle of the conservation of *vis viva* involves the indestructibility of force, and is a necessary consequence of the quality of perfect elasticity or reaction in the ultimate elements of matter: if this last is a universal property the first must also be of universal effect, and, as it does not admit of any diminution of force in nature, we may question whether, in such intense chemical action as the phenomena of combustion and explosion manifest, the sudden evolution of force is not merely an exhibition of its transference from one form of elastic matter to another.

Are not the properties of aeriform fluids and of the medium of light glaring proofs of the widely spread existence of this quality of perfect elasticity, whatever may be its essential mode of reaction; and do not they even demand of us a ready assent to its all-prevailing influence in the phenomena of Nature?

Although the utmost caution in adopting any hypothesis is the proper accompaniment of a sober spirit of inquiry, it does not appear inconsistent with such a spirit to advocate the trial of these principles as a foundation for mathematical research in the several departments of molecular physics.

It is the matured conviction of the writer that upon such foundation we shall have to build if we are destined ever to become acquainted with the secret mechanism of Nature. Would that his feeble voice could call attention to the subject, could direct upon it some portion of the vast mathematical talent that this country can now, more than at any former period, boast of.

## Appendix I.

Extract from the *Proceedings of the Royal Society* (vol. 5, p. 604—March 5, 1846).

*On the Physics of Media that are composed of free and perfectly elastic Molecules in a state of Motion.* By J. J. Waterson (*sic*), Esq. *Communicated by* Captain Beaufort, R.N., F.R.S.

This memoir contains the enunciation of a new theory of heat, capable of explaining the phenomena of its radiation and polarization, and the elasticity of various bodies, founded on the hypothesis of a medium consisting of a vast multitude of particles of matter endowed with perfect elasticity, and enclosed in elastic walls, but moving in all directions within that space, with perfect freedom, and in every possible direction. In the course of these motions, the particles must be supposed to encounter one another in every possible manner, during an interval of time so small as to allow of their being considered infinitesimal in respect to any sensible period; still, however, preserving the molecular *vis viva* constant and undiminished.

The author then enters into extensive analytical investigations; first, of the conditions that determine the equilibrium of such a homogeneous medium as is implied by the hypothesis, and of the laws of its elasticity; secondly, of the physical relations

of media that differ from each other in the specific weight of their molecules; thirdly, of the phenomena that attend the condensing and dilating of media, and of the mechanical value of their molecular *vis viva;* fourthly, of the resistance of media to a moving surface; fifthly, of the vertical equilibrium of a medium surrounding a planet and constituting its atmosphere; and lastly, of the velocity with which impulses are transmitted through a medium so constituted.

In an Appendix, the author enters into a full explanation of a table of gases and vapours, drawn up with reference to the subjects discussed in his paper.

## Appendix II.

Extract from the *Report of the* 21*st Meeting of the British Association,* Ipswich, 1851. (Transactions of the Sections, p. 6.)

*On a General Theory of Gases.* By J. J. Waterston, Bombay.

The author deduces the properties of gases, with respect to heat and elasticity, from a peculiar form of the theory which regards heat as consisting in small but rapid motions of the particles of matter. He conceives that the atoms of a gas, being perfectly elastic, are in continual motion in all directions, being restrained within a limited space by their collisions with each other, and with the particles of surrounding bodies. The *vis viva* of those motions in a given portion of gas constitutes the quantity of heat contained in it.

He shows that the result of this state of motion must be to give the gas an elasticity proportional to the mean square of the velocity of the molecular motions, and to the total mass of the atoms contained in unity of bulk; that is to say, to the density of the medium. This elasticity, in a given gas, is the measure of temperature. Equilibrium of pressure and heat between two gases takes place when the number of atoms in unity of volume is equal, and the *vis viva* of each atom equal. Temperature therefore, in all gases, is proportional to the mass of one atom multiplied by the mean square of the velocity of the molecular motions, being measured

from an *absolute zero* 491° below the zero of Fahrenheit's thermometer.

If a gas be compressed, the mechanical power expended in the compression is transferred to the molecules of the gas, increasing their *vis viva;* and conversely, when the gas expands, the mechanical power given out during the expansion is obtained at the expense of the *vis viva* of the atoms. This principle explains the variations of temperature produced by the expansion and condensation of gases—the laws of their specific heat under different circumstances, and of the velocity of sound in them. The fall of temperature found on ascending in the atmosphere, if not disturbed by radiation and other causes, would correspond with the *vis viva* necessary to raise the atoms through the given height.

The author shows that the velocity with which gases diffuse themselves is proportional to that possessed by their atoms according to his hypothesis.

# AN ACCOUNT OF A MATHEMATICAL THEORY OF GASES

*Being an Outline of the Demonstrations contained in a Paper* "On the Physics of Media that consist of perfectly elastic molecules in a state of motion" *submitted to the Royal Society in Oct.* 1845 *by* J. J. Waterston. [1845.]

The paper aims at deriving the well-known common properties of gases from one assumption as to their molecular constitution, viz.: that they consist of a multitude of indefinitely minute elastic particles, moving freely (*e.g.* as projectiles in a vacuum) and promiscuously striking against each other. By such encounters there is no loss of motion on the whole, because they are subject to the law of conservation of *vis viva*. In consequence of the continued succession of impacts at every part of each volume of such a medium, the molecular *vis viva* becomes equally distributed, and the density or number of molecules in an element of space equalized. The same reactive tension is maintained throughout the space that they occupy, and the condition of equilibrium is defined, when supposing the velocities of all the particles that cross an elemental plane in a given time to be resolved into six rectangular directions, the sum of the squares in each direction is the same.

As a mathematical exercise, some years ago, the author undertook to examine the laws of equilibrium of such a medium, not imagining at the time that it had any relation to a gas; but as he advanced in the inquiry the conviction grew upon him that its physical comportment exactly resembled an elastic fluid, and it was under this impression that two or three years afterwards he drew up this paper for the Royal Society.

The first result of Synthetical reasoning from the hypothesis, was, that if its density or the number of its molecules contained

in a volume, were supposed to be augmented or diminished in any proportion, its tension or pressure on an elemental plane increased or diminished in the same proportion. This is the law of Mariotte, the first characteristic of elastic fluids. A solution of this law was thus afforded, that did not require the assistance of a central force of repulsion, which Newton's analysis (*Principia*, Bk. II. prop. 23) has shown, must be assumed to vary inversely as the mutual distance of the particles; and must not extend beyond those that are adjacent,—a mode of action so artificial, is almost to be considered as an argument against the existence of such a force.

The proof of Mariotte's law by the present hypothesis consists of two steps.

1°. From the well-known equation that expresses the law of elastic collision,* we derive the value of the infinitesimal upward velocity given to a heavy elastic elemental plane by the impact of one particle, and thence, the infinitesimal time taken by the force of gravity to destroy and to renew it to the same amount, in the downward direction. A continuous succession of such impacts maintains the plane in equilibrium, if its weight indefinitely exceeds that of one particle. If the weight of the plane and the rapidity of succession are supposed to vary, the equilibrium exists only when they preserve the same ratio to each other.

2°. It is shown that with the same velocity, the rapidity of succession, or rather the number of impacts on an elemental plane in a constant time, follows the ratio of the density. This is almost self-evident, and completes the proof of the proposition, that the weight supported being as the pressure exerted varies as the density.

But if gases are really so constituted, what represents heat? Is their temperature dependent on the quickness with which the free particles move? To answer this question, it was necessary to suppose the density constant, and the *vis viva* of the medium to be a variable quantity. Now we may almost see without demonstration, that as the particles traverse the same distance as before, while completing a given number of impacts, the number of impacts in a constant time must increase

* That has been experimentally determined by Wren and Wallis.

with the velocity, and also the magnitude of the impulse given by each impact. Hence the sustaining effect must augment according to the square of the velocity: and this is in fact the result of the mathematical analysis of the question: the weight supported being equal to the product of the number of impacts in a given time by the impinging velocity. Now, since the mean square velocity of the particles does not alter by their mutual encounters, let M represent its amount in any case, and suppose while M increases to M − m, the elastic force of the medium increases from 1 to $1 + \frac{1}{480}$, then M : m : : 1 : $\frac{1}{480}$ or M = 480 m. Thus, if the mean *vis viva* of the particles [M] represents their temperature, this relation between the square velocity and pressure expresses Gay-Lussac and Dalton's law of equal expansibility. A volume of gas heated from 32° F. to 33 has its tension augmented from 1 to $1 + \frac{1}{480}$ (taking the old constant of expansion) and if from 32° to 512° = (32 + 480) or from 32 + $a$ to (32 + 480) + 2$a$, it has its tension doubled, and if the same law holds good at all temperatures, its tension at (32° − 480°) or − 448° must be zero. The consideration that the elastic force depends on the temperature, probably induced MM. Clement and Desormes many years ago, to place the absolute zero of temperature at − 448° F. (the absolute zero of tension): and this is what is required by the present hypothesis.

The laws of Mariotte, and of Gay-Lussac and Dalton, are represented by the simple equation (480 + t) D = T, and the laws of elasticity of the hypothetical elastic fluid by the equation $V^2$ D = T, in both of which T is the tension, D the density, t the temperature reckoned from melting ice, and $V^2$ the mean square velocity of the molecules.

The next section of the paper analyses the condition of equilibrium in two or more media (distinguished from each other by the weight of their molecules), when in equilibrium of pressure and *vis viva*.

The attention is called to Dalton's law of diffusion as obviously belonging to gases, if they have the constitution assigned to them by the hypothesis. The molecules of each of two gases must be as capable, and as much disposed, to

permeate through the vacant interstices of the other's volume, as through those of its own. The encounters of the particles of one gas with those of another, no wise differ from those of the particles of the same gas with each other. Each particle makes its way through the common space, by a series of zig-zag encounters with its neighbours, which act merely as mechanical obstacles to its progress. The character of the diffusive quality of gases, the more it is considered, seems to be exactly such as might be predicated from the hypothesis. But it also follows a numerical law, which admits of direct comparison with the results of the *a priori* reasoning of the paper.

Dr Graham, in the *Edin. Trans.* for 1831, details a course of experiments which are admitted as having proved that the rate at which different gases diffuse themselves, follows the inverse ratio of the square root of their specific gravity. Again the law of volumes, discovered by Gay-Lussac, shows that there is a simple relation between the specific gravity of a gas and its combining equivalent, which has led generally to the opinion that a volume of each gas contains the same number of molecules. We have also Dulong and Petit's law of specific heat, which, if extended to gases, leads us to expect, that their specific heat is equal for equal volumes. This has been established for some gases though not for all.

Such are the chief points that physically distinguish one gas from another, without reference to their purely chemical qualities, and in this paper the author attempts to demonstrate that they are all responded to by the theory. It was necessary, in doing so, to inquire into the condition of two media when in equilibrium of tension and *vis viva*. The results of the mathematical treatment of the question are, that when in such equilibrium they have in equal volumes the same number of molecules, and likewise the same amount of *vis viva*; that the mean square velocity of the molecules is inversely as their weight, and consequently inversely as the specific gravity of the gas. These theorems are deduced chiefly from the well-known laws of elastic impact, by a simple course of reasoning of which the following is an outline:—

The equation that expresses the result of an overtaking impact is essentially different from that which defines the

meeting impact. The impinging velocities are the absolute velocities of the particles resolved in the direction perpendicular to the plane of concurrence. The situation of the plane of concurrence in respect to the angle of concurrence, determines the ratio of the absolute, to the resolved velocities (the mean square resolved velocity is one-third of the mean square velocity). One angle of concurrence is as probable as any other: consequently, with any one velocity, the angle ($a$) as frequently occurs as its supplement (180—$a$), but if the resolved velocities of impact in the first are both on one side of the plane of concurrence, the resolved velocities in the second are on different sides, and *vice versa*. Hence the result of an indefinite number of encounters, is defined, as much by the equation for the overtaking impact, as by the equation for the meeting impact: the effect of every meeting impact must therefore be considered along with its counterpart, overtaking impact, with the same velocity. Two sets of particles (B and D) supposed to be mixed together, the particles of one kind being heavier than those of the other, we seek to know the ratio of their mean square velocities ($b^2$ to $d^2$), and how this ratio stands with the ratio of the specific weights (B to D). First, by squaring the equation for the two kinds of impact and adding them together, we find that when the sum of the *vis viva* of the two-fold encounter (one *overtaking* and the other *meeting*) with same velocity, is the same after impact as before, the squares of the velocities are inversely as the weight of the particles (corresponding with Dulong and Petit's law). But this being only one case out of an infinite number, where the ratio is in excess, or falls short, we may represent the variable excess or deficiency by the symbol $\pm p$ for the $B$ particles, and $\pm q$ the same for the $D$ particles. Now since the permanent condition of equilibrium requires that the *vis viva* of each set of molecules should be the same after a series of encounters as before, the mean value of $p$ and $q$ must be zero; that is the negative values at all times balance the positive; and $Bb^2 = Dd^2$ is the condition that fulfils the *vis viva* equilibrium, and it corresponds with Dulong and Petit's law.

If the B and D media are supposed to exist separately with such a ratio between their mean square velocities, what is the

ratio of the number of particles contained in a volume of each, when they are in equilibrium of pressure? The pressure or power of supporting a weight depends, as before remarked, on *the number* of impacts in a given time, and on the mean *force* of each impact. In the volumes of media that contain the same number of molecules, the *number* depends on the velocity alone; the *force* of impulse on the velocity, and on the weight of the particle combined, so that the pressure is measured by the product of the weight by the square of the velocity, and when this product is equal in two media, the pressure of each medium must be the same. Now when two media are in *vis viva* equilibrium, the value of this product is the same in both; consequently, media in equilibrium of *vis viva* and pressure have the same number of particles contained in a volume.

Thus the fundamental hypothesis seems able to explain the laws of specific heat, of volumes, and of diffusive velocity. They are embraced by one theorem, so that the existence of one implies that of the others.

It is true that there are at least apparent exceptions to Dulong and Petit's law, and the theory requires there should be none. Bromine, Iodine, Arsenic, Phosphorus, have four times the specific heat they should have in the state of vapour. Arsenic and Phosphorus combine in quarter volumes; Bromine and Iodine are known to combine in half volumes, perhaps also in quarter volumes; but it has not been experimentally ascertained. Sulphur has six times the specific heat proper to its vapour, and it combines in sixths of a volume. The theory requires that the integrant molecules of the vapour in these cases, should be composed respectively of four and six of the solid or liquid molecules, and there is nothing known at present that makes such a result impossible. It has yet to be determined experimentally whether or not the great change of specific heat required by the theory takes place when these bodies rise into vapour. It is perhaps practicable to do so in the case of *ether*, which has a specific heat about seven times greater than is required for its vapour. It is not difficult to conceive that the parts of a molecule may have an independent motion when in the liquid or solid form, which is wholly

transferred to their common centre of gravity by the shocks of impact, when as free projectiles they promiscuously encounter each other. The specific heat of mercury is exactly the same as what its vapour should have, if it agrees with the theory, and it is remarkable that it has been found to combine only in single volumes. It would have told against the theory, if, like the others, it had combined in fractional volumes and yet had a specific heat that corresponded with its density as a vapour. Oxygen, Hydrogen, and Nitrogen, combine in half volumes, and yet they have no greater specific heat than the law of volumes requires.

The increase of temperature that appears on compressing air, and the cold produced by dilating or exhausting it, are commonly spoken of as phenomena of *latent heat*. They have been made the subject of observation by Darwin, Dalton, and other philosophers, but it was not until the experiments of MM. Gay-Lussac and Welter, and of MM. Clement and Desormes, that we became acquainted with the initial ratio of heat developed, to the amount of forced change of volume or density, that causes the development. The results of these experiments are given in the *Mécanique Céleste*. If the compression is what would be caused by a diminution of one degree of temperature, the consequent rise of temperature according to Gay-Lussac and Welter is 0·37° and according to Clement and Desormes 0·35°. This ratio was found to hold good for a considerable range of both temperature and density, and Mr Ivory (*Phil. Mag.*, 1827) afterwards proved that it must hold good at all temperatures and densities, so long as the air thermometer is an exact measure of heat. Mr Ivory also remarked that the initial ratio was probably 0·33° or exactly $\frac{1}{3}$ and that he felt impressed with the conviction that its cause was fundamentally connected with the proportion that subsists between the linear and the solid increment of expansion.

The *vis viva* theory appears to be successful in explaining the nature of these phenomena and it realises Mr Ivory's conjectures. The theoretical value of the ratio is $\frac{1}{3}$, and it is so, because of the three dimensions of space, which (as is

well known) determines also the ratio of the linear to the solid increment of expansion.

The theory exhibits the increase of temperature in suddenly condensed air as the appearance of the force of compression. It is transferred to the molecules of air during the act of compression, by reason of their elasticity, and the elasticity of the molecules of the solid substance that is the immediate agent in contact with them. This transference is of the same nature as the transference of the motion of one billiard ball to another (if they were perfectly elastic) and is subject to the same laws. Suppose an enclosed column of air at the temperature of melting ice, to be 480 inches high, with a piston at the top: remove the pressure of the atmosphere and substitute its equivalent weight on the piston, the slightest pressure of the finger will now make the piston descend through 3 inches, and the resulting effect is an increase of one degree in the temperature of the 480 inches of air. The force expended in this operation is the descent of the weight through 3 inches, and this is the equivalent of the heat generated. [From this we may compute the mechanical value of heat; taking accouut of the specific heat and specific gravity of air in comparison to the same of water. One degree of heat applied to water thus appears equal to the force required to raise the same quantity of water to the height of 673 feet.]

The mathematical reasoning on this subject given in the paper cannot be abridged to be intelligible. It sets out with employing the equation for elastic impact, in a way similar to that mentioned at the beginning, in reference to Mariotte's law: but the heavy elastic plane there made mention of, is now supposed to be at rest when struck by a molecule; not as before, meeting it with its infinitesimal downward velocity: and the infinitesimal height to which it rises, is compared with the infinitesimal loss of *vis viva* sustained by the particle.

An idea of the *modus operandi* may be obtained by supposing an elastic ball to traverse in a vertical between two horizontal elastic planes, impinging upon and being reflected from each alternately. If both planes were at rest, the velocity of the ball would continue unchanged. If one of them advanced

towards the other with a velocity u, each time the ball impinged upon this moving plane it would have u added to its velocity (say N u), so that after n reflexions, it would become (N+n) u. The reverse of this would take place if the plane increased its distance from the other. Increments of velocity such as these are given to the molecules of a gas (while it is being compressed) in one direction, by the agent of compression: but their intestine encounters instantly distribute it equally in all directions throughout its volume: the reaction upon a plane (or upon the agent of compression) of the consequent increment of *vis viva* in the gas, is therefore less than this primary action upon the gas (*one third part* by the demonstration).

The mathematical synthesis of the theory presents us with two results, and it is remarkable that both may be deduced from M. Gay-Lussac's experiments independent of any hypothesis. The first is, that the mechanical value of the heat in a gas is equal to its tension acting through three * times its volume [see Note A at the end], and thus by our knowledge of the specific heat of air in comparison to that of water, we infer, that 1° F. applied to 1 lb. of water, is equivalent to a force of 1 lb. descending through 673 feet. This is tolerably confirmed by Mr Joule's physical researches (*Phil. Mag.*, 1842-45). By compressing air surrounded with water; by forcing water through narrow tubes; he compared the heat evolved with the force expended, and found the results to be from 500 to 900 lbs. raised 1 foot high, required to raise 1 lb. of water 1°. It is remarkable that about the same mechanical value was obtained for the heat generated by the magneto-electric machine.

The second result of this part of the theory is, that if a gas is suddenly condensed from $D_0$ density to D, its *vis viva* increases from $t_0$ to $t_0 \left(\frac{D_1}{D_0}\right)^{\frac{1}{3}} = t_1$. Gay-Lussac and Welter's experiments give us $\frac{dt}{t} = \frac{\frac{2}{3}dD}{D}$ (reckoning t the temperature from − 448° F. or 480° below melting ice, the zero of tension);

* Gay-Lussac's experiments require this to be $\frac{2}{3}$ of the volume.

whence we deduce $t = D^{\frac{3}{8}}$ and $t_0 \left(\frac{D_1}{D_0}\right)^{\frac{3}{8}} = t^1$. This may be stated as the inductive results of M. Lussac's experiments; the first as the deductive results of the theory.

In all that precedes, which occupies the first three sections of the memoir, the author is engaged in determining the value of certain ratios and he believes the mathematical synthesis to be rigidly accurate. The three concluding sections treat of the resistance of a gas (medium), or force of its current against a solid body:—the vertical equilibrium of the atmosphere; and the velocity of sound. In these, both ratios and absolute magnitudes are sought to be determined; but that the absolute quantitive results should agree with observation, we have to make an assumption as to the *vis viva* equilibrium between free molecules (*i.e.* molecules of gases), and those confined by the forces of liquid or solid aggregation (*i.e.* molecules of liquid or solid bodies). The quality of perfect elasticity must necessarily be extended to all the ultimate parts of bodies if this theory is a true representation of the phenomena of gases; because the reasoning is founded on their being a perfect reaction at every impact, whether between gaseous molecules themselves, or between these and solid or liquid bodies. Such a general property seems to be indispensable, if the motor or dynamic theory of heat be admitted.

This view of the nature of heat has been more or less advocated since Lord Bacon's crude essay. Count Rumford's famous experiment of boring cannon is generally referred to as strong evidence in its favour and it has lately received more precise experimental illustration in the hands of Mr Joule and Mr Grove. The discoveries of Professor Forbes and M. Melloni have further added to the weight of evidence in its favour. That the singularly elegant common properties of gases, in their relation to heat and pressure, should to all appearance admit of so easy an explanation by the same theory, will perhaps be admitted as another favourable circumstance. The author hopes that such views will ere long gain the attention of mathematicians better qualified to do them justice, and to follow out their manifold applications to molecular physics.

*Note A.*

We may also deduce the value of the square root of the mean square molecular velocity to be the velocity acquired in falling through three uniform atmospheres. At the temperature of melting ice this amounts to 2244 feet per second. In the sixth section of the memoir, an attempt is made to prove that the velocity of sound is one-half this amount. A sonorous impulse must be conveyed by the actual traverses and impacts of the particles, and if their velocities, as they appear in an element of space during a given time, are resolved in one direction, the mean result is one-half the square root of the mean square molecular velocity.

There is another inference from the theory which may be mentioned; as the demonstration of it given in Section V. of the paper is believed to be a rigid deduction from the hypothesis. It is, that there must be an equable diminution of temperature in ascending the atmosphere, caused by the force of gravity affecting the *vis insita* of the particles in the same manner as it affects all other projectiles, and that the absolute height of an atmosphere (supposing the force of gravity constant) is directly as the temperature, reckoned from $-448°$ F. (the zero of tension), and inversely as the specific gravity of the gas. The numerical results (which are given with less confidence) are,

1. In our atmosphere the diminution of temperature is 1° F. for each 317 feet of vertical ascent.
2. The absolute height of the atmosphere is six uniform atmospheres.*

*Note B.*

The importance attached to the temperature of the zero of tension, in the theory, led the author to try if vapours in contact with their generating liquid, showed any relation to it, that could be expressed by a linear function. Taking Dr Ure's table of the elastic force of steam, and dividing the tensions by the respective temperatures, reckoning from $-448°$ (or rather

* The unsound basis of these calculations is pointed out in Lord Rayleigh's footnote, page 250 above.—J. S. H.

adopting $-461°$ from the amended constant of gaseous expansion afforded by the late experiments of Rudberg, confirmed recently by Magnus and Regnault) we obtain numbers that represent the densities of the vapour at the respective temperatures. Now since these temperatures in the theory are represented by square numbers, it is natural we should take the square root of each respectively as the abscissa of a curve, of which the corresponding density is made the ordinate. The curve thus described, appeared to be of the parabolic kind, but not familiar to the eye. If we now take the cube root of each of the numbers that represent the densities, we get a new set of numbers that represent the mutual distance of the molecules of the vapour. If these are laid off as the ordinates, the curve will be immediately recognized as the common parabola, and to see exactly how nearly it corresponds, we have only to take the square root of the new set of numbers (corresponding to the sixth root of the densities) which on being led off as ordinates, their extremities will be found to range in a straight line, the discrepancies being of a zig-zag character.

In this way the author has laid down about 20 sets of experiments on the tension of different vapours, and he finds that there is no exception.

In Note B of the memoir he has entered fully into the subject, and given a copy of the chart of these lines. If it represents the true law of vapours, it affords us a general equation of the form

$$\left\{\frac{\sqrt{461 + t} - G}{H}\right\}^6 (461 + t) = \text{Force in inches of mercury,}$$

in which the constants G and H may be determined for any vapour from two experiments on its tension.*

The experiments of Mr Southern and the French Academy on Steam, range themselves in one line, of which the value of G is 19·492, and of H, 10·830. The like numbers for Ether, Mercury, and Sulphuret of Carbon are respectively 16·86, 22·606, 16·254; and 10·99, 20·00, 12·76.

* See the amended data and simplified equation given in the further paper, page 371 below. The same remark applies to the very important Note B of the full paper.—J. S. H.

# ON THE DEVIATION FROM THE PRIMARY LAWS OF ELASTIC FLUIDS

*Indicated by the Experiments of* M. Regnault *and of* Messrs Thomson *and* Joule.

(From the *Philosophical Magazine*, October 1857.)

§ 1. The results of Messrs Thomson and Joule's experiments on the thermal effects of fluids in motion, are stated by the authors to prove that air and carbonic acid, on being compressed, evolve more heat than the amount mechanically equivalent to the work of compression (*Phil. Trans.*, 1854, p. 341). This announcement being calculated to weaken our confidence in the principle of constancy in the mechanical equivalent of heat, I was induced to study closely the connexion between these experiments and those of M. Regnault, so as to obtain a clear idea of the modifying influence of a deviation from the law of Mariotte in experiments that involve the conversion of heat into work, and reconversion of work into heat.

§ 2. It is first necessary to keep in view the exact extent of the information afforded by M. Regnault's researches, and the nature of the deviation indicated by them. He has determined the value of the difference of volumes under constant pressure, and the difference of pressures with constant volume, corresponding to the difference of temperature between the freezing- and boiling-point of water. He has done this for several gases, and for the same gas at different pressures, and found all to differ from each other, which they would not do if the primary laws were exactly maintained. But M. Regnault *has not determined the change of absolute volume between any two different pressures*: thus the absolute amount of deviation from the law of Mariotte between any two

pressures has not been ascertained. We have also to keep in view, that *the influence of temperature* as affecting the amount of deviation has not been ascertained by M. Regnault. All his observations were made on differences of volume and of pressure between 0° and 100° C.; they tell us nothing of similar differences taken between 10° and 110°, or between 50° and 150°, etc.

§ 3. On examining the *rationale* of Messrs Thomson and Joule's experiments with plugs, I find that by a simple process of computation we may deduce from them the two important items of information wanting in M. Regnault's researches.

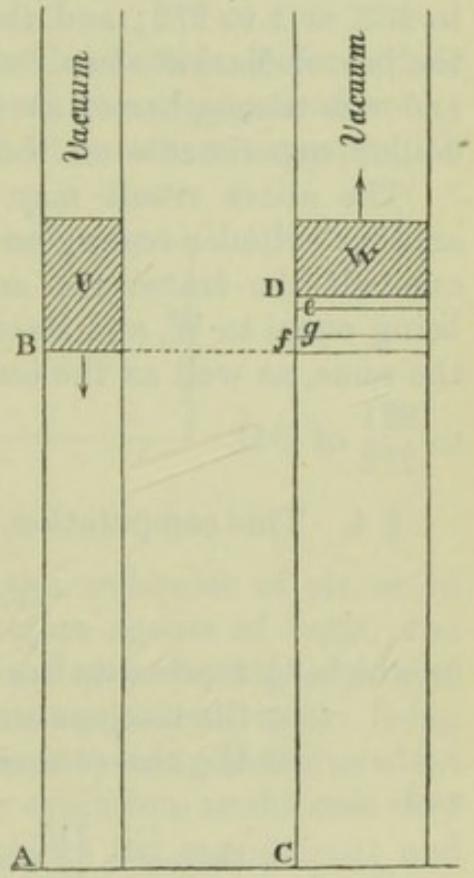


Fig. 1.

The details of the process are as follows:—

The temperature of the experiment being, say 10° C.; the difference of pressure from two to one atmosphere; and the cooling effect in passing through the plug 0·26° C.—let C D, fig. 1, represent a cylinder of air at the pressure of one atmosphere and temperature 10° C. Let the elastic force of the air in this cylinder be wholly maintained by the counterbalancing weight of the piston W, so that we have to suppose a perfect vacuum to exist on the upper side of it. By placing a small additional weight $w$ on W, suppose it to descend to $g$, so that the consequent rise of temperature in the air contained in the cylinder may be 0·26° C. This increment of heat being withdrawn, the piston W descends from $g$ to $f$. We may now suppose $w$ withdrawn from W, which consequently rises from $f$ to $e$ by the elastic force of the air in the cylinder. There is a consequent loss of heat, and the temperature of the contained

air sinks 0·26°. This decrement of heat being restored, the piston rises to D, its original position.

The ratio of $fg$, or its equal $De$, to D C is nearly the same as that of 0·26° to 273° + 10°, or of the increment of temperature to the G temperature, or temperature reckoned from the zero of gaseous tension. The ratio of $De$ to $Df$ is nearly 1 to 4, as determined by the experiments of MM. Clement and Desormes, also of MM. Gay-Lussac and Welter (*Mécanique Céleste*, book 12). The ratio of $Df$ to D C is thus nearly 1·04 to 283, or 1 to 272; and this is the amount of deviation from the law of Mariotte in air at 10° C. between the pressure of two and one atmospheres, as derived from Messrs Thomson and Joule's experiments on thermal effect.

The above result may be exhibited in diagram by A B, another cylinder resting on the same plane as C D, and having one-half the transverse area. The weight of the piston U being equal to W, and the weight of air in each cylinder being the same, as well as the temperature, we have A B = $f$C equal to $\frac{271}{272}$ of C D.

§ 4. This computation, expressed by symbols, is

$$\Delta = \frac{n\theta}{273 + t};$$

in which $\Delta$ represents the deviation,
$t$ the temperature on Centigrade scale,
$\theta$ the observed cooling effect,

and

$n$ the ratio $\frac{Df}{De}$ (which is 3·78, taking the mean of the experiments from the *Mécanique Céleste*).

§ 5. In the demonstration of this process, it is assumed as an axiom that air, in passing from a higher to a lower state of density *without performing work*, does not gain or lose heat upon the whole. (This is called Mayer's hypothesis by Prof. Thomson.) Thus suppose S W, fig. 2, to represent a cylinder impervious to heat, and that it is divided by a partition $kk$. On one side of this (S) let there be air, and on the other (W) a perfect vacuum. If we now imagine $kk$ to

be instantaneously withdrawn, the air in S rushes violently into W. At first, great motion in the direction S W is generated, and a corresponding lowering of temperature: then the motion is reconverted into heat. After the action has subsided, the resulting temperature of the expanded air is assumed to be the same as before, and this whether or not there was a vacuum originally in W. Mr Joule has put this to the test of experiment (see *Phil. Mag.*, May 1845, vol. xxvi., p. 369), and found no sensible deviation from it. (Note A, Appendix.)

If the resulting temperature were lower than before, then heat has disappeared without performing any *apparent* work; but there may be conversion of heat into work not apparent to

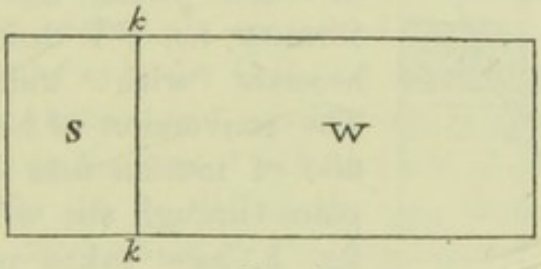



Fig. 2.

the senses, into work concealed in the molecules of air, or in their physical habitudes with the higher agents of force; and *vice versa*, the resulting temperature may be higher; heat appearing, without apparently the equivalent work being converted, the separation of the molecules being the antecedent of the phænomenon of absorption or evolution, as the case may be. To determine this is a fair subject for experiment; and if the deviation from the law of Mariotte was known by direct observation to the degree of accuracy required, we should, by comparing it with the amount assigned to the deviation by the experiments with plugs, have the means of testing the point in question. If the results differed sensibly, we *then* would have direct proof that air, on being compressed or dilated, did not alter its temperature in correspondence with the mechanical equivalent of the apparent work. Assuming, therefore, that there is no recondite evolution or absorption of heat in consequence of the molecules of gases changing their mutual

distance, the following is the demonstration of the above process for computing the deviation by means of the thermal effect of Messrs Thomson and Joule's experiments.

§ 6. Let the two upright cylinders G H and E F, fig. 3, of unequal diameters, being connected at P P by a plug of compressed cotton, as in Messrs Thomson and Joule's experiments. Let the weight of the piston H W be first considered as equal to F V, and above them a perfect vacuum, so that the elastic force of the air in each cylinder may be maintained by the superincumbent weight of the pistons only. We have also to assume that the motion of each piston has attained uniformity, *i.e.* F V descends, and H W ascends with uniform velocity. The conversion of heat into motion, and of motion into heat, that took place through the whole space S W, fig. 2, now takes place insensibly within the pores of P. The space in which the alternate conversion and reconversion take place, instead of being, as in S W, one and of very sensible magnitude, now consists of an infinite number of indefinitely minute receptacles, in each of which the same kind of reciprocal action may be supposed to be going forward. If the law of Mariotte were perfectly maintained, the obvious result would be a rise of W equal to the descent of the same weight V, and the air changed from the higher density to the lower without either the performance of work or change of temperature.

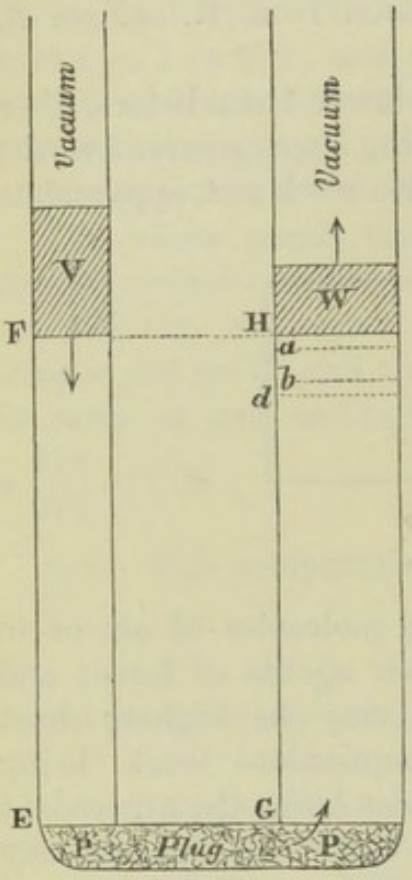


Fig. 3.

If, in condensing air at a constant temperature, its elastic force increases in somewhat a less proportion than its density (which we can infer to be the case from M. Regnault's experiments, although not the absolute extent of the divergence between any two given densities), the weight of the piston V

must be taken as somewhat less than that of W, to maintain the densities in the inverse ratio of the square diameter of the cylinders. In consequence of this diminution (say of $\frac{1}{100}$) of the weight of V, the work performed by it in descending (F E) a unit of height is less than the work required to raise W through (G H) a unit of height. It is thus obvious that the equivalent work would be completely transferred to W in raising it through $(Gd) = \frac{99}{100}$ of the unit. But the density then, instead of being of the normal amount corresponding to the ratio that subsists between the weight W and the transverse area of its cylinder G H, would exceed that by $\frac{1}{99}$: hence to arrive at an equilibrium, the ascent of W would be continued a little further beyond the $\frac{99}{100}$ (viz. to $a$), and the work required to effect this must be derived from the conversion of some of the heat of the air contained in the $\frac{99}{100}$ of the unit of the height of the cylinder G H. A lowering of its temperature is the consequence, and equilibrium is attained before W has reached fully to the top of $d$H, the last $\frac{1}{100}$ of the unit.

The proportion $da$ to $d$H of the $\frac{1}{100}$ through which W is driven has been determined by the experiments above referred to, as detailed in the twelfth book of the *Mécanique Céleste*. It is nearly three-fourths; and it is such, that if the small amount of heat lost were exactly replaced from external sources, W would be carried through $a$H, the remaining fraction of the $\frac{1}{100}$. It will be remarked, that the ratio of $da$ to $d$H is the well-known ratio of the increments of heat required for the same increment of temperature with volume constant and with pressure constant.

In the above demonstration we must keep in view, that, to facilitate our reasoning, it is allowable to assume the plug to extend from the last or lower position of V to the first or lower position of W; also that the unit of height may represent an incremental quantity, and thus the phænomena which, in order to fix our ideas, we consider as taking place in consecutive order, may be simultaneous.

§ 7. Professor Thomson assumes that the cooling effect in his experiments with plugs is proof that the gas, on being compressed, evolves more heat than the amount mechanically equivalent to the work of compression; that, in short, it does

not alter its temperature in correspondence with the mechanical equivalent of the apparent work. It has not occurred to him that the depression of temperature may simply be caused by the conversion of heat into the mechanical force or work required to acquire against the atmospheric pressure that small augmentation of volume which a deviation from Mariotte's law demands. It is surely proper to take this first into consideration, yet the necessity of doing so seems never to have suggested itself. The experiments were originally proposed as a means of testing Mayer's hypothesis only* (see *Phil. Mag.*, vol. iv., pp. 432, 433, §§ 73, 78), and up to the present time this is supposed to have been successfully accomplished (see *Phil. Trans.*, 1854*).

§ 8. The formula, § 4, $\Delta = \frac{n\theta}{273 + t}$, expresses the law of the deviation, viz. that for a constant difference of density it varies directly as the amount of the thermal depression or cooling effect, and inversely as the temperature of the experiment reckoned from the zero of gaseous tension. In the experiments of Messrs Thomson and Joule with carbonic acid, the cooling effect was about four and a half times that of air, giving a deviation of $\frac{1}{800}$ for a difference of density equal to one atmosphere.

If the deviation at 100° were the same in absolute amount as at 0°, the cooling effect ought to be greater in the ratio of 373° to 273°. But if the deviation were less in proportion with the diminished density, the cooling effect ought to be unchanged.

If the cooling effect diminish as the temperature increases, the deviation must diminish for the same constant difference of pressure, not only with the density, but also in some inverse ratio of the temperature. This last seems to be the case, judging from Messrs Thomson and Joule's experiments with carbonic acid.

§ 9. M. Regnault's observations provide us with the coefficients of expansion under constant pressure, and of augmentation of pressure with constant volume, all at the temperature of 50° C. or 273 + 50 = 323° G temperature, or

* See Appendix (1) (2).

temperature reckoned from the zero of gaseous tension. If we denote this G temperature by T, the volume by V, and tension or elastic pressure by P, the primary laws of gases in their entirety are expressed by $T = VP$, the equation of the hyperbola referred to its asymptote, in which we set out with a unit of each of the elements as a basis of comparison with standard measures.

From M. Regnault's experiments alone, we may infer that the actual co-ordinates of volume and pressure do not trace out this curve exactly, because his coefficients increase with the density; but the amount of the deviation cannot be inferred, because only one value of V for the same value of T has been supplied.

The actual co-ordinates of volume and pressure may nevertheless trace out the exact hyperbola, if they are assumed not to have reference to the actual asymptotes, but to a subordinate unknown hyperbolic curve lying near to and belonging to the same asymptotes, and that expresses by its equation the unknown cause of the deviation. Upon this assumption we obtain the means of computing the deviation by the coefficients alone. In this way the deviation comes out $\frac{1}{210}$. This value, compared with $\frac{1}{272}$, the value computed by the first process, does not show such accordance as to establish the hypothesis, but sufficient perhaps to make it worthy of passing remark.

## *Appendix.*

(1) § 7 (*Phil. Mag.*, vol. iv., p. 432, § 77, Prof. Thomson on the Dynamical Theory of Heat): the following extracts prove this statement:—

"From equation III. it follows, that if Mayer's hypothesis be true, there is neither emission nor absorption of heat, on the whole, required to reduce the temperature of the air after passing through the orifice to its primitive value. . . . Hence *the simplest conceivable test of Mayer's hypothesis* would be, to try whether the temperature of the air is exactly the same on the two sides of the orifice."

P. 433, § 78. "Should the differential method of experimenting just described indicate *any difference of temperature whatever* on the two sides of the orifice, Mayer's hypothesis would be shown to be not exactly fulfilled, and, according as the air leaving the orifice is found to be warmer or colder than the entering air, we should infer that the heat absorbed, when air expands at a constant temperature, is less than or greater than the equivalent of the mechanical effect produced by the expansion."

(2) Note A referred to in § 5. In *Phil. Mag.*, vol. iv., p. 429, § 71, Prof. Thomson thus refers to those experiments of Mr Joule:—

"In Joule's actual experiments, the test is simply this:—the total external thermal effect is determined when air is allowed to expand, through a small orifice, from one vessel to another previously exhausted by an air-pump. Here the first mechanical effect produced by the expanding gas is *vis viva* generated in the rushing of the air. By the time equilibrium is established, *all this mechanical effect has been lost in fluid friction*; and no truth in physical science can be more certain, than that by the time thermal as well as mechanical equilibrium is established at the primitive temperature, the contents of the two vessels must have parted with just as much *more* heat than they would have parted with had the air in expanding pushed out the piston against an external resisting force, as is equivalent to the mechanical effect thus produced externally." How are we to reconcile this with the fact, that the contents of the two vessels were found not to have parted with any heat? Perhaps the word *more* is a misprint, and should have been *less*. Prof. Thomson frequently makes use of the word friction. His view appears to be, that the motion of the air rushing *e.g.* from S to W, fig. 2, is lost in "fluid friction"; and then the "heat of friction" restores the temperature to nearly what it was before, but not quite, the difference being what is shown in the experiments with plugs. Thus in the second paper on Thermal Effects (*Phil. Trans.*, 1854, p. 339), he expresses himself as follows:—The thermal effect "shows *precisely* how much the heat of friction in the plug falls short of compensating the cold of expansion.

But *the heat of friction* is the thermal equivalent of all *the work done* actually in the narrow passages by the air expanding as it flows through." Thus the plug does not allow the air to pass from a higher to a lower density without performing work. Again, a little further on, Prof. Thomson thus writes:—"*In any case,* w *denoting the whole work of expansion,*" etc. The value given to $w$ is found at p. 341, viz. $w = \mathrm{PV} \log \frac{\mathrm{P}_1}{\mathrm{P}}$, the hyperbolic area of expansion at constant temperature. Prof. Thomson assumes that the work represented by this area must inevitably be done when air passes from one state of density to another at constant temperature. If not by "*pushing out a piston against external resisting force,*" then by generating "*heat of friction,*" "*stirring its own mass.*" P. 338.

(3) The dynamical theory of heat rests on the idea that heat is the motion of the elementary parts of bodies,—intestine *vis viva,* accompanied with apparent quiescence, because the motion exists in every direction alike, *e.g.* through every part of the air contained in S (fig. 2). By the removal of the partition *k k,* the intestine action in the direction S W has now scope to make its appearance; and when the particles of air have filled the whole of S W, the apparent motion in one direction relapses into intestine motion in all directions alike. This theory of heat involves no necessity for introducing the idea of friction. So far from fixing our ideas or facilitating our reasoning, its manifest tendency is to obscure and confuse. As an illustration, we may take the case of a rigid vessel impervious to heat, filled with air and moving at a high velocity in a straight line. Suppose it to be stopped instantaneously, and without fracture or any change in the vessel. At this instant the motion of translation of the air would necessarily be converted into heat, and there would be a rise of its temperature. Now it is impossible in such a case to introduce the idea of friction, but it is easy to conceive the apparent motion of the particles in the one direction changed into motion in all directions alike by simple impact with the sides of the vessel and with each other. In the arrangements for making the experiments with plugs, all work by the

expansion of the air in passing through the plug seems to have been carefully prevented. If the air acted disruptively on the fibres of the cotton, then work would be performed. Also the extra velocity of the current of air in passing out of the plug would cause a slight effect equivalent at a maximum to about $\frac{1}{100}$ of a degree. Even this might be prevented by having the plug shaped like the frustum of a cone, and thus equalize the velocity of the current on both sides. It may also be remarked, that as the cooling effect is a quantity of the differential kind, the differential thermometer seems to be the proper instrument to measure it by.

(4) There appears to have been considerable difficulty in equalizing the pressure of the air entering the plug; might not this be attained with greater facility by interposing between the condensing pump and plug a cylinder with a heavy piston, with its weight adjusted to the pressure required? The action would be similar in principle to the bellows of an organ. The induction and eduction pipes might be at the bottom of the cylinder, and the motion of the piston would probably be confined to so narrow limits as to enable it to act sufficiently as a regulator.

(5) In the second paper on Thermal Effects, at p. 336 a small Table of results appears, the last column of which is headed "*Theoretical cooling effect for* 100 *lbs. pressure.*" On comparing this with the adjacent column of experimental results, we find such complete accordance that apparently Prof. Thomson's calculus has enabled him to predict in a satisfactory manner the results of observation. This is the test of a successful theory. On inquiring further what this theory is, we come to what Prof. Thomson calls an "*empirical formula*" (that is, a formula derived from experiment), communicated by Mr Rankine, who states that its constant coefficient $a$ "has been determined *solely* from Regnault's experiments on the increase of pressure at constant volume between 0° and 100° C." Mr Rankine further states, that "it gives most satisfactory results for expansion at constant pressure, compression at constant temperature, and also (I think) for cooling by free expansion [*i.e.* the cooling effect in our (Messrs Thomson and Joule's) experiments]."

The formula is

$$\frac{PV}{P_0V_0} = \frac{T+C}{C} - \frac{a}{T+C}\cdot\frac{V_0}{V}.$$

No explanation is given as to the method of determining $a$, nor is there any proof of it giving satisfactory results for compression at constant temperature, or how any results of this kind are possibly derived from M. Regnault's experiments.

The same remark applies to the formula

$$\frac{P'V' - PV}{PV} = f\frac{P - P'}{H},$$

given by Prof. Thomson at p. 340. No explanation is given of how $f$ has been determined "from the results derived by Regnault from his experiments on the *compressibility* of air, of carbonic acid, and of hydrogen." Prof. Thomson must be sensible that upon these the proof of the soundness of his "theoretical deductions" mainly depends.

The value of $f$ for air is ·00082 (p. 340), and the formula expresses that the deviation from the law of Mariotte (or of Boyle) for a difference of pressure equal to one atmosphere is ·00082 or $\frac{1}{1220}$.

How this has been deduced from M. Regnault's observations is not explained; but both formulæ cannot be right, inasmuch as *they contradict each other.* The deviation may be computed from either, and they give different results.

If, in Mr Rankine's formula, we put $T=0$, $V=2V_0$, we have

$$\frac{2P}{P_0} = 1 - \frac{1\cdot9}{274\times2} \text{ and } P = \tfrac{1}{2}P_0\left(1 - \frac{1}{288}\right).$$

Hence the deviation for carbonic acid is $\frac{1}{288}$. By the value of $f$ given by Prof. Thomson at p. 340, it is ·0064 or $\frac{1}{156}$.

If we test the formula of Mr Rankine by taking $T = 0$, we have

$$PV = P_0V_0\left\{1 - \frac{1\cdot9\ V_0}{274\ V}\right\};$$

and when V approaches the value of $V_0$, we have

$$P = P_0\left\{1 - \frac{1{\cdot}9}{274}\right\}.$$

Let PV represent 17145, the value given (p. 336) for the actual product of volume and pressure at 0° C., or when T = 0, then

$$\frac{PV}{P_0V_0} = \frac{17145}{17116} = 1 + \frac{1}{590} = \frac{T+C}{C} - \frac{a}{T+C}\left(\frac{V_0}{V}\right) = 1 - \frac{1{\cdot}9}{274}\left(1 - \frac{1}{590}\right).$$

Thus we have the arithmetical absurdity of

$$1 + \frac{1}{590} = 1 - \frac{1{\cdot}9}{274}\left(1 - \frac{1}{590}\right) = 1 - \frac{1{\cdot}9}{274}.$$

(6) Since M. Regnault's experiments on carbonic acid have afforded Mr Rankine the means of determining the value of the constant $a$, and thus enabled Prof. Thomson to predict by his calculus the thermal effect in passing that gas through the plug (see Table, p. 336) we may ask, why has the same process not afforded the means of computing the results of the experiments on air? Some explanation of this may fairly be expected from Prof. Thomson.

(7) Table XII. p. 335, contains a series of experiments on air and carbonic acid at the temperature of about 90° C. The first part of this table compared with the last, shows such discrepancy, that it is impossible to accept the results, either in the case of air or carbonic acid, as established thereby. In the first part, the thermal effect in air would appear to be augmented by the high temperature; in the last, to be diminished. Prof. Thomson merely remarks that the lesser number is probably the more correct value, and thus that the thermal effect is lessened. The calculus that in Prof. Thomson's hands has so exactly anticipated results in the case of carbonic acid, appears to be of no avail in the case of air, although the experimental data to work upon are more abundant.

22 London Street, Edinburgh,
*August* 25, 1857.

# ON THE THEORY OF SOUND

(From the *Philosophical Magazine*, Supplement to Vol. 16, 1858.)

In a paper on the Integral of Gravitation that appeared in the *Philosophical Magazine* of May [p. 545 below], I endeavoured to draw attention to the principle of physical causation supplied to us by the mechanical theory of heat,—how it leads us to study natural phænomena in their dynamical sequence, and suggests the arranging of problems of molecular statics so that they may present the aspect of motor transition to the mathematical inquirer. It may perhaps be of advantage, as testing the general argument, to discuss the question, *Can the theory of the propagation of sound developed by Newton and Laplace be viewed, consistent with the modern idea of heat, as a correct exposition of the actual mode in which pulses are transmitted through air?* It appears from recent memoirs, that this opinion is still generally entertained by physicists; and the theory of Laplace has in some instances been considered so perfect as to afford the means of deducing the specific heat of air from the velocity of sound, and this by zealous professors of the mechanical theory of heat.

The ideas upon which Laplace's theory was constructed are to be found in the second chapter of the second book of the *Mécanique Céleste*. The following extracts show how very remote they are from those now universally prevalent:—

Page 105, line 8. "Doubtless it is necessary to admit that between the molecules of air there is a repulsive force which is only sensible at imperceptible distances: the difficulty consists in deducing from it the laws of elastic fluids. This may be done by the following considerations."

P. 105, line 4 from bottom. "I suppose then that these molecules retain the heat by their attraction, and that their mutual repulsion is due to the repulsion of the molecules of the

heat which I assume to extend to an insensible sphere of activity."

P. 111, line 9. "But what ought we to understand by the temperature $u$, and what is its measure? It appears natural to take for this measure only the density of caloric produced in a space by the radiation of neighbouring bodies."

P. 111, line 19. "A supposition that it appears very natural to admit is, that the action of the caloric of a molecule of gas on the caloric of another molecule, etc."

P. 113, line 6. "We have supposed, in that which precedes, that the caloric of a molecule was retained on it by the attraction of the molecule, which experienced no sensible action except by the repulsive force exerted on this caloric by the caloric of the surrounding molecules."

P. 117, line 16. "It is then extremely probable that the attractive force of the caloric of one molecule by another molecule is insensible in the state of gas."

Professor Thomson, who has written largely on the dynamical theory of heat (*Philosophical Magazine, Edinburgh Phil. Trans., London Phil. Trans.*), has the following in a memoir "On the Thermal Effects of Fluids in Motion" (*Phil. Trans.*, 1854, p. 361):—

"In the notes (Prof. Thomson's notes) to Mr Joule's paper on the Air-engine (*Phil. Trans.*, 1852, p. 82), it was shown that if Mayer's hypothesis be true, we must have approximately—

| | |
|---|---|
| Specific heat of air with constant pressure | ·2374 |
| Specific heat of air with constant volume | ·1684 |

*because* observations on the velocity of sound, with *Laplace's theory*, demonstrate that $k = 1{\cdot}410$ within $\frac{1}{100}$ of its own value. Now the experiments at present communicated to the Royal Society prove a very remarkable approximation to the truth in that hypothesis*; and we may therefore use these values as very close approximations to the specific heats of air."

* At p. 341, under the heading "Theoretical deductions from these Experiments," Prof. Thomson arrives at the conclusion that air and carbonic acid "evolve more heat than the amount mechanically equivalent to the work of compression," thus representing the experiments as proving

The ratio $k$ was found by MM. Gay-Lussac and Welter to be 1·37, and by MM. Clement and Desormes 1·35. The same experiments repeated by the author of the article "Hygrometry" in the *Encyc. Brit.*, impressed him with the conviction that the *initial* ratio was exactly $\frac{4}{3}$.

Here, therefore, we have an instance of a zealous adherent and expounder of the dynamical theory of heat showing such confidence in Laplace's views, such conviction of their truth to nature, that he considers them entitled to supersede the result of direct observation; and no attempt is made to reconcile the statical theory of caloric with the *vis viva* theory of heat; nor is a necessity for doing so apparently even felt as a preliminary step to the adopting of one of the deductions of the former as an ally to the deductions of the latter.

As Mr Joule's name is conjoined with that of Professor Thomson in this memoir, it is perhaps necessary to assume that he also approves of this manner of deducing the ratio of specific heats.

Another instance of the commingling of statical and dynamical theories is to be found in a memoir by M. Masson, "On the Correlation of the Physical Properties of Bodies," which appeared in a recent number of the *Annales de Chimie*. M. Masson also adopts the mechanical theory of heat (see § III. chap. 2). Yet in the beginning of the same chapter he writes, "Laplace has discovered the true mathematical expression of the velocity of sound in gas." Further, M. Masson states that he has found, by experimenting according to the method employed by MM. Gay-Lussac and Welter, that the value of $k$ is actually what is required to make Laplace's formula agree exactly with the best observations on the velocity of sound. No details are given, although the amount of compression in such experiments is of some importance, as the ratio increases

an *exception* to Mayer's hypothesis. That such cooling effects must take place in consequence of the *deviation* from Mariotte's law discovered by Regnault, if Mayer's hypothesis and the dynamic theory of heat hold good, seems obvious enough (see *Phil. Mag.*, vol. xiv., p. 279) [p. 332 above]. So that they are actually a delicate test of the accuracy of that hypothesis; and taken as supplementary to Mr Joule's previous experimental inquiries, seem completely to establish it as a theory proved by induction.

with this amount; and the formula has only to do with the initial ratio, or when the change of density is infinitely small. M. Masson has also computed the velocity of sound in a considerable number of composite gases and vapours from the pitch of the sound given by an organ pipe while immersed in them; and thence, employing Laplace's theorem, obtains the respective values of $k$, which are thus found to range from 1·26 to 1·42 in the composite gases, and from 1·06 to 1·27 in the vapours.

Thus all authorities, both of the statical and dynamical school, seem to agree that Newton and Laplace's theory of sound is a perfect representation of nature, and that its success is as complete as the theory of gravitation.

Newton, in several parts of the *Principia*, calls upon us to keep in mind that his principles are mathematical, not philosophical. At the beginning of Book 3 he thus expresses himself:—"In the preceding books I have laid down the principles of philosophy; *principles not philosophical, but mathematical, such to wit as we may build our reasonings upon in philosophical inquiries.*" Again, after proving in Prop. 23, Book 2, that "particles flying each other with forces that are reciprocally proportional to the distances of their centres, compose an elastic fluid whose density is as the compression," he adds, in the scholium, "*But whether elastic fluids do really consist of particles so repelling each other, is a physical question. We have here demonstrated mathematically the property of fluids consisting of particles of this kind, that hence philosophers may take occasion to discuss that question.*"

In Prop. 47 of Book 2, Newton shows that "if pulses are propagated through a fluid, the several particles of the fluid going and returning with the shortest reciprocal motion are always accelerated or retarded according to the law of the oscillating pendulum." It is assumed that the elastic force is proportional to the density; and in the direction of the pulse the fluid is supposed to be divided into *physical lineolæ, which are expansible and contractile*, and exhibits a force that resists compression inversely as their breadth. The mathematical reasoning defines the law by which the breadth of these lineolæ must vary while they go and return,—and hence the law of the

difference in the breadth of two adjacent lineolæ, and consequently the law of the accelerative force operating on each corpuscle, which is thus found to be the same as a body moving in a cycloid is subject to under the influence of gravity.

Newton's fundamental hypothesis is, that the particles of air in the direction of the pulse are *successively* agitated with like motions; that both the dynamic condition and the static force of repulsion, which is determined by the length of the line that separates two adjacent particles (called a lineola), is transferred onwards in the direction of the pulse from one particle to the next adjacent in regular succession.

The demonstration takes account of three orders of magnitudes: 1, the breadth of a pulse (L); 2, the breadth of an oscillation of a particle ($2l$); 3, the length of a lineola ($\lambda$), each considered as infinitesimal with respect to the preceding.

If the motion of a particle forward and backward in the line $2l$ corresponds to that of a cycloidal pendulum, *i.e.* if the relation between the accelerative force (acting in the line of motion), the acquired velocity, and the time is the same in the line $2l$ as in the complete cycloid, the force in this line must vary simply as the distance ($y$) from its middle point. The value thus assigned to the force implies that $\delta$, the difference between the lengths of two adjacent lineolæ, should vary also in this proportion. If a semicircle is described on the diameter $2l$, $y$ is the cosine of an arc, of which $x$ being the sine, we have $\delta \div y \div dx$; so that the differential of the lineola ought to be equal to the differential of the sine, and hence the absolute magnitude of the deviation of the length of a lineola from its mean length ought to be proportional to the sine.

Thus if the motion of a particle is oscillatory, like a complete cycloidal pendulum, the required sequence of force demands the above specific sequence of change in the distance of the particles. Again, if the motion of each particle is oscillatory, the required sequence in its velocity (viz. that it should vary as $x$ the sine) demands also a specific sequence of change in the distance of the particles; and this sequence is precisely the same as what is required by the sequence of force.

To obtain a clear idea of the proof of this (which is a

problem of pure mathematics), we may suppose with the same radius $l$ another semicircle to be described, placed also in the line of the pulse, and removed to the distance $\lambda$ from the preceding semicircle. Let a third also be drawn, removed the same distance $\lambda$ from the second. We have further to suppose these semicircles divided into as many parts ($aa_1$, $a_1a_2$, $a_2a_3$, etc.; $bb_1$, $b_1b_2$, $b_2b_3$, etc.; $cc_1$, $c_1c_2$, $c_2c_3$, etc.), beginning at where the line of pulse intersects them as $\lambda$ enters into L (or $\frac{L}{\lambda}$ number of parts). The length of each of these parts or *steps* is thus $\frac{\lambda}{L} 2l\pi = s$ (being infinitesimal with regard to $\lambda$).

Having made this construction, we have next to consider that the motion of each particle to be oscillatory must be such that, at the instant when particle A has traversed the versed sine of $aa_n$, the particle B (next in *advance* of A) being one *step* behind in its motion, has traversed only the versed sine of $bb_{n-1}$, and particle C the versed sine of $cc_{n-2}$. If B had traversed as many steps as A, the distance $\lambda$ that separates them would not alter; but since it is a step behind, AB is at this point less than $\lambda$ by the difference between vers $aa_n$ and vers $bb_{n-1}$, or vers $aa_n$ − vers $aa_{n-1}$, which equals $s$ . sin $aa_{n-\frac{1}{2}}$. In the same way, C being a step behind B, their distance is less than $\lambda$ by $s$ . sin $aa_{n-1\frac{1}{2}}$. Thus we have $BC - BA = s(\sin aa_n - \sin aa_{n-1}) = s \cos aa_n \frac{s}{l}$. [Here $s$, being an absolute magnitude, has to be divided by the absolute radius $l$ to represent the differential of arc.] At the beginning of the vibration $n = 1$, and cos $aa_n$ = radius; hence with B at the initial point $b$, C at $c_{-1}$ (a step back on the returning half of the previous oscillation), and A at $a_1$ (the points on the circle being supposed projected on the diameter), the difference

$$BC - BA = \frac{s^2}{l} = 4l\left(\frac{\pi\lambda}{L}\right)^2.$$

This initial amount determines the accelerative force acting at the beginning of the motion of each particle, which is obtained by comparing it with the reciprocal of $\lambda$, which represents the whole static force of repulsion between two

particles at the distance $\lambda$. This force having to support the weight of $\frac{H}{\lambda}$ particles (H being the height of a uniform atmosphere), $\frac{1}{\lambda}$ represents the force $\frac{H}{\lambda}g$, viz. a force that in one second is capable of communicating a velocity of $\frac{H}{\lambda}g$ feet per second.

To obtain the value of the initial force acting on particle B when it is at $b$, we have the following proportion:

$$\frac{1}{\lambda} : \frac{1}{\lambda} - \frac{1}{\lambda - \frac{s^2}{l}} :: \frac{H}{\lambda}g : \frac{s^2}{l\lambda}\frac{H}{\lambda}g = Hg4l\left(\frac{\pi}{L}\right)^2.$$

The time ($\tau$) taken by a particle to traverse $2l$, with this force diminishing as the distance from the centre of the semicircle, is the same as the time required for one oscillation of a pendulum whose length is $l$, if subject to an influence of gravity equal to this force, and is the same as the time taken by the pulse to travel through $\frac{1}{2}$L. By the law of the pendulum, $\tau$ is equal to $\pi$ multiplied by the square root of quotient of length of pendulum by force of gravity, hence

$$\tau = \pi\sqrt{\frac{l}{\pi^2}\frac{L^2}{Hg4l}} = \tfrac{1}{2}L\sqrt{\frac{1}{Hg}};$$

and the velocity of the pulse per second is $\sqrt{Hg}$.

This supposes Mariotte's law maintained; but the experiments made at the suggestion of Laplace, proved that for rapid compressions the elastic force increases in a higher ratio than the density; and for small increments of density, the correspondent increments of elastic force are very nearly $\frac{4}{3}$ those computed on the hypothesis that Mariotte's law is maintained; that is, as if the repulsive force of a particle supported the weight of $\frac{4}{3}\frac{H}{\lambda}$ particles: hence Newton's theorem for the velocity of sound in elastic fluids is strictly represented by $\sqrt{\frac{4}{3}Hg}$.

The numerical results from this formula, taking Regnault's data, compared with observations made at low temperatures

(so that the influence of aqueous vapour should be avoided), show a difference at most of about $\frac{1}{360}$, or as if $\frac{4}{3}$ should be augmented $\frac{1}{178}$ part. Thus no objection to Newton's theory can well be made on the ground of its not according with observation, as no theory can be expected to embrace all the circumstances that may affect the result; *e.g.* the repulsive force may not emanate from the centre of the particles, so that the size of the particles may influence the result. Also the repulsive action is necessarily assumed to be limited to adjacent particles, not extending through the interstices of these to the particles beyond (for such is the extraordinary and improbable hypothesis required to deduce Mariotte's law from a static repulsive force). This may be supposed subject to modification during vibratory action.

But the hypothesis upon which the mathematical demonstration rests is open to three grounds of objection: 1. It does not take account of the condition of the front of a pulse when the particles from a condition of rest enter into the cycle of motion defined by the theory. 2. The force of repulsion between two adjacent particles required by the theory is extravagantly large. 3. The other physical properties of gases are not deducible from the hypothesis.

To these may be added, that the dynamical theory of heat has suggested another hypothesis which is free from these objections, and which therefore claims a preference according to Newton's first "rule of reasoning in philosophy," viz. "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. To this purpose the philosophers say that Nature does nothing in vain, and *more is in vain when less will serve;* for Nature is pleased with simplicity, and affects not the pomp of superfluous causes."

1. The theory does not take account of the condition of the front of the pulse, or rather of the front of the first of the series of pulses of which a sound consists. This is apparent if we consider that a particle is represented by the theory as at rest at each extremity of its oscillation, and at those points the accelerative force is at its maximum, and is derived from the difference between the lengths of the lineolæ that issue

from the particle in front and in rear. The front lineola cannot differ from the mean length so long as the front particle is at rest unaffected by the advancing pulse. The rear lineola is less than the mean length by a certain small amount $a$. If the front particle were in action in a pulse cycle, the length of the front lineola would be greater than the mean length of a lineola by the same amount $a$, so that the accelerative force at each extremity of the oscillation of a particle is represented by $2a$; and unless it were so, the condition required to sustain the beautiful relation of velocity and propelling force would be wanting. But at the front of the first pulse the lineola does not differ from the mean length, so that the accelerative force is represented by $a$, and this is only one-half the amount required by the theory to begin the oscillation. In truth, the demonstration only applies to a pulse having similar pulses operating on both sides.

2. The force of repulsion between two adjacent particles required by the theory is extravagantly large. The recent advances in the theory of heat have, in a measure, compelled us to realise the dynamic value of natural forces. To compute the absolute value of the repulsive force acting between two adjacent molecules of air, we have to consider that it has to support the gravity of the number of molecules in the height of a uniform atmosphere $\left[\frac{H}{\lambda}\right]$; it must therefore exceed the force of gravity of one molecule in this ratio. Now the force of gravity in one second can communicate a velocity of 32 feet per second, so that the force of repulsion between two adjacent molecules of air must be capable in one second of communicating a velocity of $32\,\frac{H}{\lambda}$ feet. The absolute value of $\lambda$, the distance between two adjacent molecules of air, we can now with great probability deduce from the phænomena of capillarity (*Phil. Mag.*, vol. xv.) [p. 407 below]. At the boiling-point of water the number of molecules of steam in a cubic inch is the same as the number of molecules of air in the same volume. At 86° the number of layers of aqueous molecules in a cubic inch is 215 millions (*Phil. Mag.*, vol. xv.) [p. 419 below]. Hence at ordinary temperatures the distance between two

adjacent molecules of air must be about $\frac{1}{17}$ of a millionth of an inch, and the value of $\left(32\frac{H}{\lambda}\right)$, the velocity communicable in a second, is 160 thousand times the velocity of light. Can we for a moment believe that such a force has any real existence, that it is other than a mathematical fiction?

3. The other physical properties are not deducible from the hypothesis of a static force of repulsion. The deductive power of Newton's theory is confessedly limited to Mariotte's law and the velocity of sound. Laplace, by the invention of calorific atmospheres, is allowed to have added to these Dalton and Gay-Lussac's theory of expansion; but it is a question whether the reciprocal action between heat-atmospheres and molecules, which he expresses by mathematical symbols, can be realised by the mind. In judging of this, we must not forget the chapter of the *Mécanique Céleste,* in which the author speculates upon what the laws of motion would have been if force had been as a function of the velocity, instead of as the simple velocity.* What is to be expected from a superstructure resting upon such a foundation as this reveals? Nevertheless, granting that Mariotte's law, Dalton and Gay-Lussac's law, and the velocity of sound are represented by the statical hypothesis, we have still Dalton and Graham's law of diffusion and diffusive velocity; Gay-Lussac's law of volumes; Dulong and Petit's law of specific heat, extended to the more simple gaseous bodies by Haycraft and the French physicists; the law of latent heat partially discovered by Gay-Lussac and Welter's experiments; also the diminution of temperature in ascending the atmosphere,—all as yet undeduced from any statical theory of elastic fluids. It may be that additions to the mathematical hypotheses of Laplace will be attempted with the view of extending their capacity, as indeed there seems to be no limit to this artificial and barren system of procedure, which is as far removed from the simplicity of nature as the hideous epicycles of Ptolemy.

* The author of the article "Virtual Velocities" in the *Penny Cyclopædia* has the following remark upon this chapter of the *Mécanique Céleste*:—"We have never met with anyone who could give us an intelligible account of the meaning of this investigation."

There is another mode by which pulses may be conceived to be transmitted, which admits of being set forth in a popular way.

Suppose we range a number of ivory balls in a straight line upon a billiard table, and strike the first of the row upon the second, the initial velocity will be carried forward from one ball to the next adjacent, and so will make its appearance in the last—supposing perfect elasticity and no resistance in rolling—undiminished as if the motion of the first ball had continued, and the impulse had been carried by it alone, and not transferred by impact through others. These balls, confined to one line, are supposed to be in motion among themselves, so that those adjacent alternately strike against each other in opposite directions; the end ones being reflected from the cushions, and then back again after striking the next adjacent ball, the *vis viva* in one direction being at all times equal to the *vis viva* in the opposite.

If we now suppose one cushion to move forward with comparatively slow velocity, each time the adjacent ball strikes it, it will be reflected with a velocity greater than that which it impinged. This increment of velocity it transfers to the next ball, and so on; and the velocity with which the impulse is transferred along the line is equal to the common velocity with which the balls move. We may suppose the line extended indefinitely, and the motion of the cushion to be alternately forwards and backwards. While the adjacent ball impinges many times during each advance and retreat—during the former carrying forward a succession of small increments of *vis viva*, during the latter a succession of small decrements of *vis viva*—a pulse is formed, the intensity and duration of which depends on the motion of the cushion, but the velocity of propagation upon the motions of the balls, upon their common velocity.

Now suppose these balls reduced excessively in their dimensions, and to be *perfectly* elastic as well as the cushion, and we shall have obtained an idea of how pulses may be conveyed in a manner quite different from that depending upon a statical repulsive force between adjacent particles. Instead of such stupendous force, we have to substitute

molecules simply moving with the velocity of a cannon-ball, and assume the atmosphere to maintain its elasticity by its particles striking against each other with such velocity, which, viewed cosmically, by no means exceeds what is moderate, and even highly probable if heat is molecular motion.

Such a theory of elastic fluids was started by Mr Herapath so far back as 1821 in the pages of the *Annals of Philosophy*, and has more recently suggested itself to M. Krönig, as we find noticed by M. Clausius in his memoir "On the Nature of the Motion we call Heat" (*Phil. Mag.*, vol. xiv., p. 108).

The following is an extract from Mr Herapath's memoir, p. 278, vol. i., *Annals of Philosophy*, April 1821:—". . . . . if gases, instead of having their particles endued with repulsive forces, subject to so curious a limitation as Newton proposed, were made up of particles or atoms mutually impinging on one another, and the sides of the vessel containing them, such a constitution of aëriform bodies would not only be more simple than repulsive powers, but as far as I could perceive, would be consistent with phænomena in other respects, and would admit of an easy application of the theory of heat by intestine motion. Such bodies, I easily saw, possessed several of the properties of gases: for instance, they would expand, and if the particles be vastly small, contract almost indefinitely; their elastic force would increase by an increase of motion or temperature, and diminish by a diminution; they would conceive heat rapidly and conduct it slowly; would generate heat by sudden compression, and destroy it by sudden rarefaction; and any two having ever so small a communication, would quickly and equally intermix."

At p. 341, in paragraph beginning "These impulses," etc., and in those which succeed, ending "have from one another," we have a very clear announcement of the mode by which a static force of pressure is counterbalanced by a dynamic force of elastic impact.

At p. 345, Prop. 8. "The same things remaining, the elasticity of a gas under a variable temperature and compression is proportional to its numeratom (number of atoms in constant volume) and the *square of its temperature* conjointly; or the elasticity varies as the *square of the temperature* directly, and the simple of the space inversely.

Mr Herapath unfortunately assumed heat or temperature to be represented by the simple ratio of the velocity instead of the square of the velocity—being in this apparently led astray by the definition of motion generally received—and thus was baffled in his attempts to reconcile his theory with observation. If we make this change in Mr Herapath's definition of heat or temperature, viz. that it is proportional to the *vis viva* or square velocity of the moving particle, not to the momentum or simple ratio of the velocity, we can without much difficulty deduce, not only the primary laws of elastic fluids, but also the other physical properties of gases enumerated above in the third objection to Newton's hypothesis. In the *Archives of the Royal Society* for 1845-46, there is a paper "On the Physics of Media that consists of perfectly Elastic Molecules in a state of Motion," which contains the synthetical reasoning upon which the demonstration of these matters rests. The velocity of sound is therein deduced to be equal to the velocity acquired in falling through three-fourths of a uniform atmosphere. This theory does not take account of the size of the molecules. It assumes that no time is lost at the impact; and that if the impacts produce rotatory motion, the *vis viva* thus invested bears a constant ratio to the rectilineal *vis viva*, so as not to require separate consideration. It also does not take account of the probable internal motion of composite molecules; yet the results so closely accord with observation in every part of the subject, as to leave no doubt that Mr Herapath's idea of the physical constitution of gases approximates closely to the truth. M. Krönig appears to have entered upon the subject in an independent manner, and arrived at the same result; M. Clausius, too, as we learn from his paper "On the Nature of the Motion we call Heat" (*Phil. Mag.*, vol. xiv., p. 108).

The physics of such media is a study that must be ungenial, perhaps repulsive, to mathematicians brought up in the statical school. The fundamental hypothesis does not permit us at once to transfer the subject to the domain of pure mathematics, as Newton's hypothesis converted physical astronomy into a purely mathematical study. The *mode* of action by which certain phænomena make their appearance must be realized at each step in conformity with the conservation of force: the

causal relation must be ever present to the mind, or no true progress can be made. On the other hand, the mathematics required is simple, and almost every one of the applications of the theory admits of popular illustration. An attempt of this kind I have given above in reference to the conveyance of sonorous impulses, introducing a dynamic theory of sound. The strict demonstration of the velocity is given in the memoir above referred to. A few further illustrations may here be added to show in brief the capabilities of the theory; but it is difficult to limit these to one branch, all the physical properties of elastic fluids being so interwoven with each other; and it is an admirable instance of the simplicity of nature, that the cluster of elegant quantitative relations which the physics of gases present, should flow from the constitution assigned, which indeed is the simplest that it is possible to imagine.

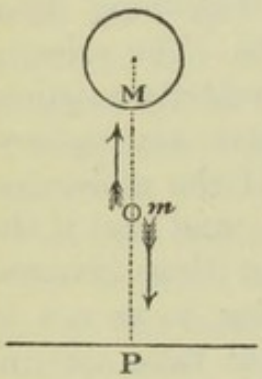


FIG. 1.

The velocity of sound is not affected by the height of the barometer, but it is sensibly influenced by a change of temperature. This latter is to be looked for; since the velocity of the particles of air increases with the temperature, the velocity with which they convey pulses must increase in the same proportion: but it is not so obvious that the height of the barometer or weight of the atmosphere should have no effect either to accelerate or retard.

Let $m$ be an elastic ball traversing the vertical PM backwards and forwards from the sphere, M, to the plane, P, the surfaces of $m$, M, and P being perfectly elastic. The condition of permanence in the mean distance of M from P requires that the impacts of $m$ upon M should have the effect of changing the velocity of M downwards into the same velocity upwards. Gravity affects M in the interval of time that elapses while $m$ descends from M to P and ascends from P to M; during half this time gravity is employed in destroying the upward motion of M, and during the second half in producing the velocity downwards with which it encounters $m$ on its return,—$m$ and M thus meeting each other, and

separating after impact with the same velocity, but with directions reversed.

The relation between the distance MP ($=\lambda$), the velocity of $m$ ($=v$), the weight of M $\left(=\frac{\mathrm{H}}{\lambda}m\right)$, and of $m$, is very simple, and enables us to compute the absolute value of $v$.

The time taken by $m$ to traverse MP is $\frac{\lambda}{v}$ part of a second; and in this time gravity communicates to M the velocity $\frac{\lambda}{v}g$.

From the law of elastic collision, two bodies impinging and reflected back in the direction they came with unaltered velocities, must have their velocities inversely proportioned to their masses, so that

$$\mathrm{M} : m :: v : \frac{vm}{\mathrm{M}} = \frac{\lambda}{\mathrm{H}}v = \frac{\lambda g}{v} \text{ as above.}$$

Thus we have $v^2 = \mathrm{H}g$, or the velocity is that produced by gravity in a body falling through $\frac{1}{2}$H. [Strictly, the square velocity of air-molecules must be six times this, because the above calculus only takes account of the action in one of the six rectangular directions of space.]

To trace the influence of the barometer or weight of the uniform atmosphere, we may suppose the weight of M doubled and $\lambda$ reduced one-half: this leaves H and $v$ unaltered; so that if the density of air increases as the weight of the uniform atmosphere, the velocity of sound is unaffected by the barometer. If with M constant the density represented by $\frac{1}{\lambda}$ diminishes, H must increase in the same ratio, and thence $v^2 \doteqdot \lambda$, or the volume under constant pressure as the square molecular velocity,—which conforms to Dalton and Gay-Lussac's law, if $v^2 \doteqdot$ temperature from zero of gaseous tension.

If we view these relations in another elastic fluid, where the weight of the molecule is twice that of air, M being unaltered, and the number of molecules in a unit volume also the same as with air, we have H inversely as $m$, or one-half the height of a uniform atmosphere of air, and $v^2$ reduced in the same proportion; also the velocity of sound reduced

inversely as the square root of the molecular weight or specific gravity of the gas.

To explain the increase of temperature that arises from suddenly condensing air, we may imagine an elastic ball traversing a vertical between two horizontal plates and striking alternately against them. Those plates being also considered as perfectly elastic, the velocity of the ball will continue uniform without its motion being impaired. If we now suppose the distance between the plates to be gradually diminished by one of them assuming a velocity incomparably less than that of the ball, the ball will, each time it strikes this advancing plate, receive an increment to its velocity, and thus to its *vis viva*.

Let $v$ represent the velocity of the ball, $\delta$ the distance between the planes, $\frac{v}{n}$ the velocity of the plane. The number of impacts upon the advancing plate in a unit of time is $\frac{v}{2\delta}$. The velocity after one impact has increased from $v$ to $v + \frac{v}{n}$, and the square velocity from $v^2$ to $v^2 + \frac{2v^2}{n}$, the increment being $\frac{2}{n}$ of the square velocity; at the same time the decrement of space is $\frac{v}{n}$ (the space moved over by the plate in a unit of time) divided by $\frac{v}{2\delta}$; this gives $\frac{2\delta}{n}$; and the ratio of this to $\delta$ is also $\frac{2}{n}$: thus the increment of *vis viva* is equal to the decrement of distance, irrespective of the velocities and distance, and is equal to the mechanical force exerted by the plate. In an elastic medium, the increment of absolute temperature is equal to one-third the decrement of volume, and the increment of temperature is the equivalent of the force expended in the act of compression. We thus gain a knowledge of the mechanical equivalent of heat, and further deduce the amount of *vis viva* in a gas to be equal to the work performed by its pressure acting through three times its volume.

The diminution of temperature found when ascending the atmosphere may be illustrated by supposing a series of elastic balls, *a*, *b*, *c*, *d*, etc., to be arranged in a vertical, and moving in the vertical so that those adjacent shall alternately strike against each other at the extremity of their up-and-down motion without any transference of *vis viva*, which requires that they should encounter each other with equal velocities, *e.g.*, *b* in its up-motion striking *c*, and in its down-motion striking *a*. Now we have to mark, that, between the upper and lower impact, *b* receives an accession of *vis viva* from the force of gravity which is proportional to the vertical distance traversed; so that comparing the *vis viva* of *b* with any other of the series, such as *z*, we shall find that the higher ball *z* has less *vis viva* than *b*, and the difference is equal to gravity acting through *bz*. Thus in the atmosphere we might expect the decrease of temperature to be uniform if its constitution agrees with this hypothesis.

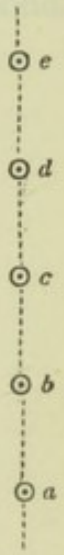


FIG. 2.

The strict demonstration represents this to be the case, and that the gradient of temperature is 1° in 319 feet; also that the absolute height of the atmosphere is six times the height of a uniform atmosphere, the density in a stratum as the fifth power of the depth of that stratum below the summit, and the elastic force or height of barometer as the sixth power of that depth.

Here we find that the diminishing temperature, in ascending the atmosphere, is represented as the natural condition of vertical equilibrium; and the question occurs, may not the increasing temperature found in descending through the earth's crust be also its natural condition of vertical equilibrium? This may be cited as one of many instances of the suggestive power of the *vis viva* theory, marking it as specially the natural introduction to the dynamic theory of heat, and as likely to promote a beneficial change in the application of mathematics to molecular physics generally. Upon this account it seems to merit the attention of the educational authorities

in the higher departments of scientific instruction. The subject, however, is so remote from practical application, that there is little hope of any impression being made in such quarters for many years. In the meantime, it seems the duty of those who have profited by it, to do what lies in their power to proclaim its merits and acknowledge the value of the idea first struck out by Mr Herapath, and perhaps saved from oblivion by the *Philosophical Magazine* of that period.

EDINBURGH, *Nov.* 6, 1858.

# III

# PAPERS ON LIQUIDS AND THEIR VAPOURS

# ON A GENERAL LAW OF DENSITY IN SATURATED VAPOURS *

*(Illustrated by Chart I.)*

*By* J. J. Waterston, Esq., *Communicated by* Lieut.-Col. Sabine, *V.P. and Treas.*

(From *Philosophical Transactions of the Royal Society*, 1852, Part I., p. 83.)

The relation between the pressure and temperature of vapours in contact with their generating liquids has been expressed by a variety of empirical formulæ, which, although convenient for practical purposes, do not claim to represent any general law. Some years ago, while examining a mathematical theory of gases, I endeavoured to find out from the French Academy's experiments, if the density of steam in contact with water followed any distinct law with reference to the temperature measured from the zero of gaseous tension. [By Rudberg's experiments, confirmed by Magnus and Regnault, this zero is $-461°$ in Fahr. scale, or $-273{\cdot}89°$ in the Centigrade scale. *Temperatures reckoned from this zero I shall call* G *temperatures to save circumlocution.*] If $t$ represents the G temperature, $\Delta$ the density of a gas or a vapour, and $p$ its elastic force, the equation

$$t\,\Delta = p \quad . \quad . \quad . \quad . \quad (1)$$

represents the well-known laws of Mariotte and of Dalton and Gay-Lussac. The function that expresses a general relation between $p$ and $t$ in vapours must include a more simple function, expressing a general relation between $\Delta$ and $t$. The proper course, therefore, seemed to be to tabulate the quotients $\frac{p}{t}$ from the experiments of the Academy and to project them

* Received June 19, 1851.—Read June 19, 1851.

into a curve. Now, for reasons connected with the *vis viva* theory of gases, which represents the G temperature as a square quantity, I projected these densities as ordinates to *the square root of the* G *temperatures as abscissæ*, and the curve traced out was of the parabolic kind, but of high power. To reduce this, because density is a cubic quantity, I tabulated their cube roots and set them off as ordinates to the same abscissæ. The result was gratifying, for the familiar conic parabola made its appearance. To ascertain whether this curve was exactly the conic parabola, I tabulated the square root of these ordinates, corresponding with the sixth root of the densities, and laid them off as new ordinates to the same abscissæ. The result is shown in the accompanying Chart, No. I., Plate VII., under the title *French Academy's Steam*. The observations are denoted by dots thus •, and it will be remarked that they range with great precision in a straight line, any slight divergence being sometimes to the right and sometimes to the left; precisely as might be expected from small errors of observation. Other series of experiments on steam were projected in a similar manner; and it was found that although no two exactly agreed with each other, yet that each set ranged in a straight line nearly. The vapours of ether, alcohol, and sulphuret of carbon were tried in the same way, and found to conform to the same law. I have since added to the Chart M. Avogadro's observations on the vapour of mercury, which will be found remarkably in accordance: also Dr Faraday's experiments on liquefied gases, given in the *Philosophical Transactions* for 1845. Of these, olefiant gas (No. 1, p. 160) is remarkably in accordance; also the nitrous oxide (No. 2, p. 168), ammonia, cyanogen, sulphurous acid, and carbonic acid at the upper part of its range. Muriatic acid, sulphuretted and arseniuretted hydrogen do not show the same regularity.

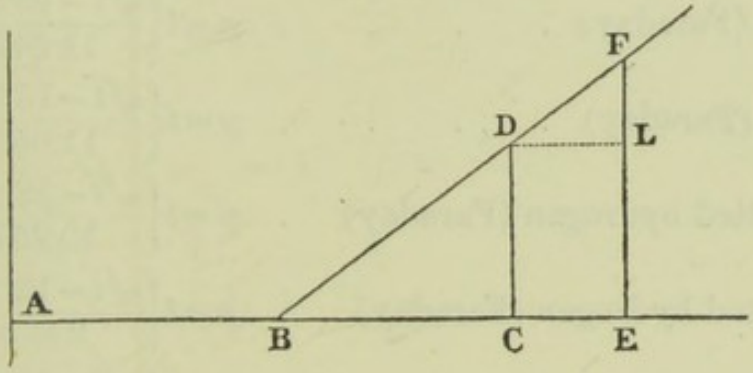
The co-ordinates of the points being the square root of the G temperature and the sixth root of the densities, the equation to the straight line that passes through the points expresses the sixth root of the density in terms of the square root of the G temperature.

Thus let $t_1$ = the G temperature corresponding to $\Delta_1$ density of vapour: set off AC = $\sqrt{t_1}$ and CD = $\sqrt[6]{\Delta_1}$. A second

observation treated in the same way gives AE, EF, which determines the position of the line of vapour BF. Thus let

$$\text{AC} = g, \text{ and } \cot \text{FBE} = h = \frac{\text{DL}}{\text{FL}} = \left\{ \frac{\sqrt{t_2} - \sqrt{t_1}}{\sqrt[6]{\Delta_2} - \sqrt[6]{\Delta_1}} \right\}$$

and $$g = \text{AB} = \sqrt{t_1} - h \sqrt[6]{\Delta_1}.$$




The constants $g$ and $h$ are thus determined from two observations, and the equation for the density $\Delta$ at any other G temperature is

$$\left\{ \frac{\sqrt{t} - g}{h} \right\}^6 = \Delta \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and for the pressure,

$$\left\{ \frac{\sqrt{t} - g}{h} \right\}^6 t = p \quad . \quad . \quad . \quad . \quad (3)$$

The following are the equations for the series of observations given on the Chart; the G temperatures are in degrees of Fahr. scale, and the values given to $h$ are made to give the pressure in inches of mercury.

Mercury (Avogadro) . . . . $p = t \left\{ \frac{\sqrt{t} - 22{\cdot}606}{20{\cdot}00} \right\}^6$.

Oil of turpentine (Ure).
Salt water saturated (Watt).

Water (French Academy and Southern) $p = t \left\{ \frac{\sqrt{t} - 19{\cdot}492}{10{\cdot}830} \right\}^6$.

Alcohol, sp. gr. ·813 (Ure) . . . $p = t \left\{ \frac{\sqrt{t} - 19{\cdot}287}{9{\cdot}800} \right\}^6$.

Sulphuret of carbon (Marx) . . $p=t\left\{\frac{\sqrt{t}-16{\cdot}254}{12{\cdot}76}\right\}^6$.

Sulphuric ether (Dalton) . . . $p=t\left\{\frac{\sqrt{t}-16{\cdot}860}{10{\cdot}990}\right\}^6$.

Sulphurous acid (Faraday) . . . $p=t\left\{\frac{\sqrt{t}-14{\cdot}667}{11{\cdot}194}\right\}^6$.

Cyanogen (Faraday) . . . . $p=t\left\{\frac{\sqrt{t}-13{\cdot}846}{11{\cdot}542}\right\}^6$.

Ammonia (Faraday) . . . . $p=t\left\{\frac{\sqrt{t}-13{\cdot}317}{11{\cdot}050}\right\}^6$.

Arseniuretted hydrogen (Faraday) . $p=t\left\{\frac{\sqrt{t}-12{\cdot}929}{10{\cdot}264}\right\}^6$ (?).

Sulphuretted hydrogen (Faraday) . $p=t\left\{\frac{\sqrt{t}-12{\cdot}957}{9{\cdot}878}\right\}^6$ (?).

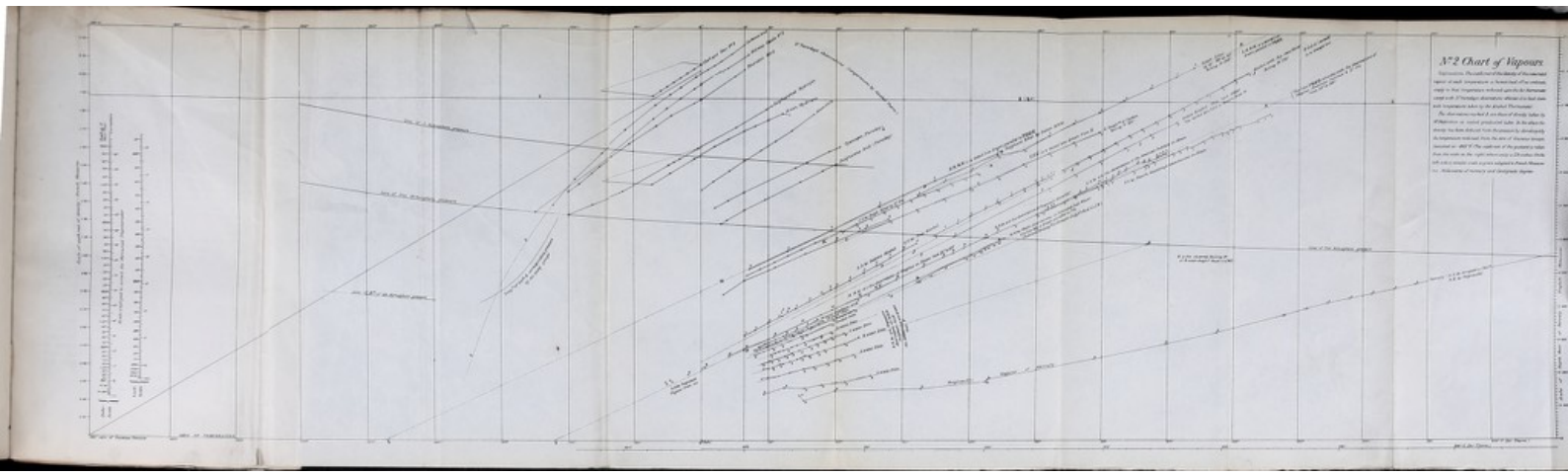
Muriatic acid (Faraday) . . . $p=t\left\{\frac{\sqrt{t}-12{\cdot}060}{9{\cdot}413}\right\}^6$ (?).

Carbonic acid (Faraday) . . . $p=t\left\{\frac{\sqrt{t}-11{\cdot}997}{8{\cdot}857}\right\}^6$.

Nitrous oxide (Faraday), No. 2, p. 168 $p=t\left\{\frac{\sqrt{t}-8{\cdot}936}{11{\cdot}604}\right\}^6$.

Olefiant gas (Faraday), No. 1, p. 160 . $p=t\left\{\frac{\sqrt{t}-10{\cdot}352}{10{\cdot}152}\right\}^6$.

While projecting different sets of observations of the same vapour, the attention is forcibly drawn to the tendency which the line shows to alter its inclination on the most trifling change of circumstances. This inclination depends on the value of the denominator $h$. It will be remarked that this constant is nearly the same for the vapours of ether and of water, also for a considerable number of the liquefied gases. In fact the difference is not greater than might be caused by a slight want of purity. Even the specific nature of the vessel in which the observations are made seems materially to affect the results. Dr Faraday employed tubes of from one-fifth to one-seventh inch bore. I believe it will be found that the density is considerably less in these tubes than in larger vessels; I have had proof of this in the case of ether heated in such tubes to 280°. The capillary attraction of the sides


N°2 Chart of Vapours

even at high pressures seems to affect the whole surface of the liquid, and to check the formation of vapour. We must not forget also, that Dr Faraday's temperatures are confessedly not so low as a correct thermometer would have indicated. The liquid he employed to measure the temperature was alcohol, and the degrees were graduated with the same capacity as between 32° and 212°. Now from the experiments that have been made on the expansion of alcohol by M. Gay-Lussac, and more recently by M. Muncke, it appears that the mean contraction between 32° and 212° is 6·2 thousandths of the volume at 173°, its boiling-point, and that this amount diminishes very sensibly with the temperature; and if it continued to do so at the same rate from 32° to −100°, we should have Dr Faraday's −100° equal to less than −200° by the air-thermometer. It is clear that until the value of these degrees are known with reference to the air-thermometer, the importance of Dr Faraday's observations cannot be fully estimated.

It will be observed from the Chart, that the observations on the vapour of water below 80° show a small excess of density above what is required by the line drawn through those at higher temperatures, and the contour of their projection is a curve convex to the axis, and very nearly the same as would be caused by the pressure of a very minute quantity of air; but the character of the late experiments of MM. Regnault and Magnus forbid this mode of explaining the slight divergence. It is probable that it is connected with the change of condition that takes place at 32°, and the effects of which may be sensible up to 80° or 90°.

It is a curious circumstance that the law of expansibility of water also becomes disturbed at about the same temperature. As this has not been remarked, I have given the proof of it by projecting M. Despretz's observations* on the Chart. If the volume is made ordinate to the square root of the G temperature as abscissa, these observations above 25° Centigrade or 77° Fahr., trace out *in the most exact manner a conic parabola.* The equation is

$$a(v - \theta) = (\sqrt{t} - \phi)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

* *Ann. de Chem.*, vol. lxx.

in which $v$ is the volume of the G temperature $t$ in terms of its volume unity at 39·2° Fahr., or 4° Centigrade (its point of maximum density), $a = 352{\cdot}38$, $\theta = {\cdot}99872$, and $\phi = 21{\cdot}977$ or $\phi^2 = 483°$.

In the Chart I have made $\sqrt{v - \theta}$ the ordinate, and $\sqrt{t}$ the abscissa. The proof of the position is, that in this mode of projection the observations range themselves exactly in a straight line from about 77° upwards,* and that below this temperature there is both less decrement of volume and less decrement of density than is required by the law that is followed above that temperature. Other liquids appear to follow the same rule of expansion, but the range of the observations is too small as yet to found upon them any general conclusion.

If these equations for the expansion of water and the density of its vapour hold good at high temperatures, they would have a common density at 1108° Fahr., its amount being $\frac{1}{1{\cdot}878}$.

The equation for the density of steam, at any G temperature under this, in terms of the density of water unity at 39·2, is

$$\Delta = \left\{\frac{\sqrt{t} - 19{\cdot}492}{22{\cdot}2745}\right\}^6 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

M. Cagniard de la Tour observed the sudden conversion of water into steam at a much lower temperature, but his observations are at variance with the laws of Mariotte, and Dalton and Gay-Lussac; and although a slight divergence from these laws has been discovered by M. Regnault, it is quite in the opposite direction to that which M. Cagniard de la Tour's observations require.

* If these ordinates are projected to ordinates $= t$ instead of $\sqrt{t}$, the line is a distinct and regular, though flat, curve convex to axis. This, if confirmed at high temperatures, will prove that the density of liquids as well as vapours has reference to the zero of gaseous tension.

[*Chart I. is not reproduced here: it is superseded by Chart II. of the following paper.*]

# ON A GENERAL LAW OF DENSITY IN SATURATED VAPOURS

(*Illustrated by Chart II.*)

*Submitted to the Royal Society*, 1852.

In a former paper I gave an account of what appeared to be a general law of density in saturated vapours, founded on the evidence of various authors, projected on a Chart. The mode of projection was to take as co-ordinates *the sixth root of density and square root of temperature* reckoned from −460° F., the zero of gaseous tension,—named the G temperature. The points for each vapour thus determined ranged so nearly in a straight line that there seemed little reason to doubt that a general law was indicated, notwithstanding a certain divergence that appeared in the lower part of the line of aqueous vapour.

Being desirous of tracing this law up to high temperatures, it occurred to me that it might be done simply by observing the change of liquid volume in two common glass tubes filled with the same liquid *in different proportions* and sealed at the lamp after being carefully graduated.

On examining the mathematical elements of this method of observation, it appeared that by means of a simple computation or graphical construction the increase of density in the vapour and of volume in the liquid could be obtained distinctly as separate phenomena independently of any theory, and that the number of tubes employed above two would have the effect of mutually checking the accuracy of their graduation and of testing the identity of physical influences to which they might be subjected.*

The first trial of the system was made upon a sample of

* See Appendix, § 1.

sulphuric ether in seven tubes up to 374° F. Four of these showed such accordance that I proceeded with confidence to compute the density of the vapour at each 10° and then projected the result on the Chart. The anticipated accordance, however, did not take place. The divergence from the straight line was not great but it was decisive. The points arranged themselves in a regular curve inclining upwards.

The method * of tube observations could hardly have a liquid better adapted to test its efficiency than ether. The ratio of the specific gravity of its vapour to that of the liquid is not only considerable, but its density increases so rapidly that at 370° it amounts to nearly one-fifth that of the liquid. Having no doubt of the theory, I lost confidence in the observations and fancied that capillary action or some other obscure cause might in tubes disturb the normal equilibrium between the liquid and its vapour.

The formulæ of computation as well as the method of reading and graduation were tested by observations on the expansion of water in four tubes carried up to 350°.† The results were entirely satisfactory in so far as the observations responded to the test of (8) [Appendix] and the dilatation computed by (13) agreed well with M. Despretz's results; indeed so well as to leave an impression that the system might be adopted with advantage even at low pressure temperatures.‡

The next trial was made on a sample of alcohol with improved apparatus. Six tubes were carefully graduated and filled in different proportions. The curves of expansion were established by repeated trials up to 420° and the two elements [$r_1$ and $f$] required for computing the density of the vapour and volume of the liquid were obtained by graphical construction that equalised small irregularities. The accordance of these tubes throughout was satisfactory, but again when projected on the Chart they failed to develop a straight line. The

* See Appendix, § 2. † See Appendix, § 3.

‡ By a modification of the graphical method described in Appendix 6, Fig. 9, I have discovered errors equal to half a Centigrade degree in recent observations where all the refinements that modern art and skill seem to have been applied. Even in M. Despretz's observations on Water one or two irregularities occur amounting to about 1/3rd of a degree.

temperatures were corrected for the unequal expansion of mercury in glass to bring them into accordance with the air thermometer; but this only made the difference greater.

The very exact manner in which the French Academy's observations on high pressure steam accorded with the theory, seemed to put its accuracy beyond question. The fault, therefore, I had no doubt, lay in the observations, and many laborious and unavailing attempts were made to discover it. When about to abandon the subject as hopeless, my attention was fortunately drawn to a remarkable equality in the differences of the sixth root of the densities, which had been drawn out in a table at intervals of 10° of the air thermometer.* This was so nearly perfect that it became obvious that the desired result would be obtained if *the* G *temperature simply and not its square root were taken as the abscissa.* This was accordingly done, and is shown on the accompanying Chart, marked Δ Δ, etc., at the upper part of the line J, J, J. The same treatment was successful in bringing the ether observations into a line. They are also exactly laid down on the Chart, and marked Δ Δ, etc.

The observations on steam were then adjusted to this new form of projection and the points laid down. They will be found along the line P, Q, Q, Q. It will be remarked that from about 50° F. to 212° the observations of M. Magnus almost perfectly coincide with a straight line which, on being produced upwards, agrees with the five highest observations of the American Institute and with four or five of the lower ones. Dr Ure's observations also agree extremely well with this line, the highest and some others being exactly upon it. To avoid confusion these have been omitted.

M. Regnault's low pressure observations agree nearly with those of M. Magnus, but his high pressure observations all lie below the line.† The deviation is such that if the correction that reduces the mercurial to the air thermometer were twice the amount that has been adopted, M. Regnault's observations would exactly coincide with the line.

All the observations of the French Academy have been projected on the Chart with temperatures corrected, and it

* See Appendix, § 4. † See Appendix, § 6.

will be remarked they entirely fail in conforming to the line. At the outset they coincide with M. Regnault's highest observations, and the divergence increases upwards to a formidable extent. It is obvious that if uncorrected for the air thermometer they would be still further out. I have been unable to test this by tube observations in consequence of the steam corroding the glass. This action begins at 350° and a small proportion of the water is absorbed, while an opaque white crust is formed that prevents anything being seen above 400°. In tubes of hard glass it is slower of making its appearance, but the absorption of liquid is fatal to accuracy.

Before entering upon further details, it must be premised that all temperatures above 212° have been corrected in order to reduce them to the standard of the air thermometer. The amount is shown by the two scales (Fahr. and Cent.) placed under the Chart. These have been constructed from the well-known observations of MM. Dulong and Petit taken at 0°, 100°, 200°, and 300° C.* The corrections of intermediate temperatures have been computed by formulæ that embrace the four observations exactly.†

It will be remarked that the scales show a certain small amount of correction to temperatures between 32° and 212°. This has not been applied in the Chart—where indeed it could hardly be recognised—as its existence is denied by the best authorities. It is difficult to see, however, that such a correction can be wanting if the law of expansion is continuous. Three formulæ have been tried on different hypothesis of expansion founded on the above observations, and they all give nearly the same amount.‡

* More recent researches on this subject differ sensibly in their results from those of MM. Dulong and Petit. The subject does not seem capable of a high degree of accuracy. Thus we may see from the details of these observations that an error of 1/1100th of an inch would produce an alteration of 1/3rd of a Cent. degree in the amount of correction at 300° C. M. Regnault's observations on low pressure steam from 50° to 100° agree to within 1/40th part with the correction interpolated from the formulæ of the standard observations. I would not therefore be justified in rejecting them and adopting the results of any more recent *single* authority however eminent. (See Appendix, § 8.)

† See Appendix, § 7. ‡ See Appendix, § 8.

About the maximum density of water the observations of Magnus and of Regnault rise a little above the line, the difference being about 1/30th of an inch at 32° F.: thus tension observed by M. R. 4·600 mm., by M. M. 4·525 mm., by Chart line 3·747 mm. There can be no doubt that, although minute, this is a real divergence either from the presumed law of density or from Dalton and Gay-Lussac's law of pressure. Smaller tensions were observed in the course of M. Regnault's hygrometrical researches (*Ann. de Chem.*, Aug. 1845), which arrange themselves in straight lines with surprising accuracy. I refer to the three first of the nine sets of observations on the vapours of several hydrates of sulphuric acid. All of these nine sets are projected on Chart II; and although the conformity of several series of observations to the straight line on both Charts ought to caution us against hasty inferences, it seems to be a circumstance conclusively in favour of the last, that *the lines of these observations upon it, all radiate from one point and that that is the point where the line of aqueous vapour meets the axis of temperature.* In the previous Chart these observations all described palpable curves. To test this further, I prepared some of the four water and ten water acid from a sample of the pure hydrate and tried their boiling-points with the barometer at 29·9 m. On laying off the corresponding points on the Chart and producing the lines that had been previously drawn through the observations, they were found to coincide. The effect of this singular and most unexpected relation is to make the proportion between their several tensions and also their several densities the same at all temperatures.

In the case of Dr Ure's alcohol below 173°, we have upon this Chart a very correct straight line which produced meets the axis of temperature at almost exactly the same point as the comparatively well-established line of aqueous vapour. At and above the boiling-point the same series falls considerably below this line. The sample of alcohol with which I made an elaborate series of observations in sealed tubes was far from pure—showing by its specific gravity 19 per cent. of water—and boiled at 178°. Nevertheless their line intersects the axis at the same point. To test this further, I tried another sample

of dilute alcohol with two tubes (62 per cent. water) and found the point of intersection still the same.

It thus appears that alcohol and water and sulphuric acid and water have a relation in the density of their vapours that may be termed *radiant* to distinguish it from another species of relation where the lines are *parallel*.

If the line of Dr Ure's ether *below* its boiling-point is examined, it will be found exactly parallel to the line of aqueous vapour. Above the boiling-point this parallelism is not maintained. Dalton's ether seems to have been pure: its line agrees nearly with this parallelism, but not exactly. My tube observations were made upon a very impure sample, yet at high temperatures — where only good results are to be expected—their parallel relation to water is almost perfect.

The same relation of parallelism seems to subsist between water and its saturated solution of common salt, judging from the observations of Watt (marked W).

The observations of Marx on the tension of the vapour of sulphuret of carbon (marked xx on Chart) conform with tolerable accuracy to a straight line which meets the axis of temperature somewhat below the line of Dalton's ether. I have made some observations with two tubes on a less pure specimen up to about 300°. They range very well in a line that meets the axis at the same point as Dalton's ether. It also appears to be parallel to Marx's sample.

M. Avogadro's observations on the tension of mercury have all been carefully laid down on the Chart. It will be observed that they conform to the line perfectly except the two lowest. Produced upwards, this line crosses the curve of 30 inch pressure at 639° F. air therm.—corresponding to 656½° of the merc. therm. This ought to be its boiling-point, but 662° or thereabouts is generally adopted, sometimes 670° or even higher.

A few other liquids were tried by the tube method, which also conform to the law, but the observations were not carried high enough to fix their lines with certainty.

A sample of Reef turpentine from its boiling-point 320° to 420° gave a line radiating exactly from the zero point of aqueous vapour: it is therefore probably correct.

Samples of the chloride and dichloride of sulphur* gave lines parallel to each other and to mercury nearly. The line of the chloride points to about the zero of gaseous tension. This also seems to be the case with chlorine, hydriodic acid, and fluoboron; judging from Dr Faraday's experiments on the condensible gases between 32° and 0°. But further observations are much required in all of these. A vapour thus situated has its density proportional to the *sixth* power of its G temperature, a result which it is interesting to compare with the law of gases subjected to pressure demonstrated in a previous paper, viz., that the *cube* of the G temperature of a gas during the process of condensation varies as its density.

The evidence thus far afforded by Chart II. seems conclusive as to this mode of projection being the exponent of a general law. Not only do sets of observations range themselves in straight lines, but those lines have a palpable relation to each other in their position. It seems mathematically impossible to realise even an approximation to these coincident relations upon any other system of projection.

When observation is extended to other vapours these mutual relations will no doubt become more intelligible and lead to higher generalisations. The attention of chemists has already been directed to a certain connection between the boiling-point and the chemical constitution of certain organic compounds (see M. Schroeder's paper quoted in *Ann. de Chim.*, Feb. 1845). The position of their lines of vapour cannot fail to throw some light on the subject. When we consider that two good observations at moderate temperatures and pressures are sufficient to determine a line by the ordinary barometric method, and that there are fully 200 bodies of determinate chemical constitution whose specific gravities in the state of vapour have been ascertained, we may indulge a hope that no long period will elapse before the necessary data are supplied.†

The mathematical definition of the law may thus be expressed: *The temperature by air thermometer and the sixth root of the density of a saturated vapour form the co-ordinates*

* Kindly prepared for me along with other specimens of pure liquids by Dr Haines, Professor of Chemistry, Grant College, Bombay.

† See Appendix, § 5.

*of a straight line*, or in other terms, *At the same temperature by air thermometer, or at the same interval of temperature, any two vapours in contact with their generating liquid have densities that bear a constant proportion to each other.*

The following is a list of the few vapours whose lines have been fixed with some degree of certainty. The first column contains the composition of a volume in volumes of its constituents. The second the specific gravity on the hydrogen scale, the value of C being 12. The third contains the value of the constant $g$ and the fourth of log $h$ in the general formula $\left\{\frac{t-g}{h}\right\}^6 t = p$, in which $t$ represents the G temperature on Fahr. scale, and $p$ the pressure in inches of mercury. To change the value of $h$ so that $\left\{\frac{t-g}{h_1}\right\}^6$ shall denote the density in terms of the liquid unity, see App., § 5.

| | 1. | 2. | $g$. | Log $h$. |
|---|---|---|---|---|
| | | | Deg. | |
| Water . . . . | $HO^{\frac{1}{2}}$ | 9 | 354·7 | 2·7281741 |
| Sulphuric Ether . . | $H^5O^{\frac{1}{2}}C^2$ | 37 | 228·7 | 2·72817 |
| Cyanogen . . . . | NC | 26 | 106·0 | 2·72817 |
| Ammonia . . . . | $NH^{1\frac{1}{2}}$ | $15\frac{1}{2}$ | 63·0 | 2·72817 |
| Aq. Sulphuric Acid . . | ... | ... | ... | 2·72817<br>3·10981 |
| Alcohol . . . . | $H^3O^{\frac{1}{2}}C$ | 23 | 354·7 | 2·66728 |
| Mercury . . . . | Hg | 100 | 463·0 | 3·06436 |

Dr Faraday's observation on the condensible gases are marked on the Chart with the temperatures as given by the author. In reference to the measurement of these, the following description is quoted: "*In order to obtain some idea of this temperature* (*i.e.*, the temperature of the liquid carbonic acid and ether bath) *I had an alcohol thermometer made of which the graduation was carried below* 32° F. *by degrees equal in capacity to those between* 32° *and* 212°." Was this thermometer employed in all the experiments, some of which were made at temperatures so high as 110°, or only at temperatures below the range of the mercurial thermometer? As to this we are not informed, the above being all that is said on the subject. Yet it is of importance inasmuch as it has been ascertained

that the mercurial thermometer requires little or no correction below 212°, but the alcohol thermometer a very large correction if constructed on the principle mentioned.

A comparison between such a thermometer and the common mercurial is on record by De Wildt, who constructed a table intended as a means of correcting Rutherford's thermometer, supposing the difference between air and mercury at low temperatures to be trifling. I drew out a scale of correction from De Wildt's table with the hope of bringing Dr Faraday's temperatures nearer to the standard, but the result is unsatisfactory. In the case of Olefiant Gas, of Arsen. Hydr. and of Sulphurous Acid the alteration is shown by dotted lines, and it is apparent that they conform less to the presumed general law than without the correction, and that from being concave towards the axis they have become convex. It seems probable that Dr Faraday has followed what I believe is the usual practice, and graduated his alcohol thermometer by a comparison with the mercurial at 32° and the adjacent temperatures. If such was the case, little or no correction would be required above 0°. As partly confirming this view, we find the lines of Cyanogen and Ammonia almost exactly parallel to that of water; also other relations both *radiant* and *parallel*. There is some indication, too, of a disposition in the lines to cluster, radiating from certain *equidistant* nodal points in the axis of temperature.

I have ventured to include Dr Faraday's Cyanogen and Ammonia in the preceding list.

## Appendix.

§ 1. The following is the demonstration of the formulæ of density both of the vapour and liquid, as they are mutually involved in this kind of observation.

Let
$T$ = the lower of two temperatures.
$t$ = the higher of two temperatures.
$D$ = density of liquid at T.
$d$ = density of liquid at $t$.
$\delta$ = density of saturated vapour at T.
$\delta + \epsilon$ = density of saturated vapour at $t$.

The volume of each tube taken as unity, let the proportion of the volume of No. 1 tube occupied with liquid at T be represented by $m$ and suppose this volume to become $m + \mu$ at $t$. The weight of the liquid and vapour taken together is the same at both temperatures, hence

$$m\mathrm{D} + \overline{1 - m}\delta = \overline{m + \mu}d + \overline{\delta + \epsilon}(1 - \overline{m + \mu}) \quad . \quad (1)$$

This supposes no change in the volume of the tube. The effect of expansion at the higher temperature is to diminish the relation between the weight of its contents and its volume considered as unity. The equation therefore holds good if we multiply each side by the volume of the tube at the respective temperatures. Let the volume at $\mathrm{T} = 1$ and at $t = \frac{1}{\rho}$, the above equation becomes

$$\rho(m\mathrm{D} + \overline{1 - m}\delta) = \overline{m + \mu}d + \overline{\delta + \epsilon}(1 - \overline{m + \mu}) \quad . \quad (2)$$

in like manner with another tube, No. 2:

$$\rho(n\mathrm{D} + \overline{1 - n}d) = \overline{n + \nu}d + \overline{\delta + \epsilon}(1 - \overline{n + \nu}) \quad . \quad (3)$$

comparing (2) and (3), we eliminate

$$d = \frac{\rho\mathrm{D}(m \,.\, \overline{1 - \nu} - n \,.\, \overline{1 - \mu}) + \rho\delta(\mu \,.\, \overline{1 - n} - \nu \,.\, \overline{1 - m})}{\overline{m + \mu} - \overline{n + \nu}} \quad (4)$$

$$\epsilon = \frac{\rho n\mathrm{D} - \overline{n + \nu}d + \nu\delta + [\overline{1 - n} \,.\, \overline{\rho - 1} \,.\, \delta]}{1 - \overline{n + \nu}} \quad . \quad . \quad (5)$$

transposing (2), we have

$$\mu = \frac{\overline{\rho m\mathrm{D} + \rho - \rho m - 1} \,.\, \delta - \epsilon}{d - \delta + \epsilon} - m \quad . \quad . \quad . \quad (6)$$

transposing (3)

$$\nu = \frac{\rho n\mathrm{D} + \overline{\rho - \rho n - 1} \,.\, \delta - \epsilon}{d - \delta + \epsilon} - n \quad . \quad . \quad . \quad . \quad (7)$$

subtracting (7) from (6) and dividing by $m - n$, we have

$$\frac{\mu - \nu}{m - n} = \frac{\rho(\mathrm{D} - \delta)}{d - \overline{\delta + \epsilon}} - 1 = \frac{\mu_1 - \nu_1}{m_1 - n_1} = \frac{\mu_2 - \nu_2}{m_2 - n_2} \,!= \mathrm{R}) \quad . \quad (8)$$

Thus it appears for each of any number of tubes if we set off its difference of liquid volume as ordinate to its original liquid

volume as abscissa the extremities of such ordinates should range in a straight line inclined to the axis at an angle whose tangent is R and crossing it where the *apparent* expansion is zero. That is to say, the liquid volume that corresponds to this point is such that the exact amount of its *true* expansion is abstracted to increase the density of the vapour from $\delta$ to $\delta + \epsilon$. Below this point the liquid volume *diminished* by an increase of temperature. Let $f$ = volume at this point; it being the value of $n$ when $\nu = 0$. Also let $r$ = the value of $\mu$ when $m = 1$. Substituting these values in (8), we have

$$\mathrm{R} = \frac{r}{1-f} = \left[\frac{m-\nu}{\mu-n}\right] \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In practice it is most convenient to project the ordinates of the several tubes and find $r$ and $f$ by linear construction. We have thus a most useful check on the observations and graduation of the tubes when there are more than two, and a test of their value by the exactness with which the ordinates range in a line. (See § 3.)

In (4) and (5) substituting $1 = m$, $r = \mu$, $f = n$, $0 = \nu$, we have

$$d = \frac{\rho\mathrm{D}\{1 + rf - f\} + \rho\delta(r - rf)}{1 + r - f} \quad . \quad . \quad . \quad . \quad (10)$$

$$\epsilon = \rho(\mathrm{D} - \delta)\frac{fr}{1 + r - f} = (\rho\mathrm{D} - d)\frac{f}{1 - f} \quad . \quad . \quad (11)$$

In reducing (5) to (11) it is assumed that $\rho$ as a factor of $\delta$ has no sensible effect on the result.

If the same ordinates are drawn to the volumes at the higher of the two temperatures as abscissæ the value of R is diminished to

$$\mathrm{R}_1 = \frac{\mu - \nu}{m + \mu - n + \nu} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The value of $f$ is unchanged but $r$ becomes $r_1$, being the value of $\mu$ when $m + \mu = 1$. Substituting these values of $\mu$ and $m + \mu$ in (4), we have

$$d = \rho\{\mathrm{D}1 - r_1 + \delta r_1\} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and for descending temperatures

$$\mathrm{D} = \frac{1}{\rho} \cdot \frac{d}{1 - r_1} - \delta \frac{r_1}{1 - r_1} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

The equation for augmentation of density in the vapour is

$$\epsilon = \rho(\mathrm{D} - \delta)\mathrm{R}_1 f = \rho(\mathrm{D} - \delta)\frac{r_1 f}{1 - f} \quad . \quad . \quad . \quad (15)$$

The formula that was generally employed in these researches is (15), and as $\delta$ had no sensible effect in any of the liquids that were tried it assumed the very simple form of $\epsilon = \rho \cdot \frac{r_1 f}{1 - f}$ in terms of D unity.

(Fig. 8). To obtain the several values of $r_1$ and $f$, a line 25 inches long was taken to represent the volume of a tube (BE, fig. 8), each $\frac{1}{100}$ of volume being $\frac{1}{4}$ of an inch. The liquid volume of each tube at the initial temperature T was marked off upon this line. Thus E$c$ the liquid volume of No. 1 tube, E$c'$ of No. 2, etc. The vertical scale of volume being double was $\frac{1}{2}$ inch to $\frac{1}{100}$ of volume and parallel lines $cb$, $c'b'$, $c''b''$, etc., were drawn through the starting-points of each tube making $ab = 2\,ac$, etc. The lengths of $ab$, $a'b'$, etc., were taken from the curve of each tube respectively (see Note B) at intervals of 10° or 20° and lines drawn through the simultaneous points $b$, $b'$, $b''$, etc., which ought to range in a straight line (see (8)). PAQ represents one of these lines for a temperature $t$; EA is the value of $f$ for this temperature, and BP the value of $r_1$. Since BE is unity it is apparent that $\mathrm{EQ} = \frac{r_1 f}{1 - f} = \epsilon$ in terms of D unity uncorrected for expansion of glass.

When carefully worked out this graphical method of obtaining the density is fully preferable to computation, inasmuch as we are not so liable to make a false estimate of the value of the results. I generally employed both methods and equalised the graphical values of $f$ and $r_1$ by differences.

If the graphical method of finding $r_1$ and $f$ is not employed, the expressions for $d$ and $\epsilon$ in terms of R and $f$ are preferable. (10) is thus changed to

$$d = \rho\left\{\frac{\mathrm{D}(1 - f\mathrm{R}) + \delta(\mathrm{R} - \mathrm{R}f)}{1 + \mathrm{R}}\right\} \cdot \quad . \quad . \quad . \quad (16)$$

and (11) into $\epsilon(\rho D - d)\frac{f}{1-f} = \rho(D - \delta)\frac{fR}{1+R}$ . . . . (17)

For descending temperatures setting out from $d$ to find D, we have

$$D = \frac{\frac{1}{\rho}d(1+R) - \delta(R - Rf)}{1 + fR} \quad . \quad . \quad . \quad . \quad (18)$$

$$R = \frac{\mu - \nu}{m - n} \quad . \quad . \quad . \quad . \quad . \quad f = m - \frac{\mu}{R}$$

The correction for the expansion of the glass tubes was made by means of the following table of differentials computed from a formula which embraces the four standard observations of MM. Dulong and Petit.

| 500° | 450° | 400° | 350° | 300° | 250° | 200° | 150° | 100° | 32° | Temperature Fahr. |
|---|---|---|---|---|---|---|---|---|---|---|
| 204 | 191 | 180 | 170 | 161 | 154 | 149 | 144 | 142 | 140 | Expansion of Glass per 10° Fahr. in ten thousandths of volume. |

According to MM. Dulong and Petit there is no sensible difference in the expansion of different kinds of glass. According to M. Pierres' late researches the difference may amount to one-fourth. This is not sufficient to affect the results of the tube experiments in any sensible degree. Indeed the whole expansion has scarcely any sensible effect on the results as to density of vapour.

Another cause of some uncertainty may be noticed here, viz., the distension of the glass by the great internal pressure at the higher temperatures. M. Grassi's experiments with the Piezometer afford some idea of its probable amount. (*Ann. de Chim.*, April 1851.) A glass cylinder 7 in. × 1 in., in thickness $\frac{1}{24}$ in., subjected to an internal pressure of from 1 to 7 atmospheres at the temperature of 0° C. showed a nearly uniform distensibility amounting to about $\frac{1}{14000}$ per atmosphere. The maximum pressure in the alcohol experiments was about 36 atmospheres. If the distension of the tubes corresponded with that of the cylinder, the amount which would have to be added to the value is $\frac{9}{3500}$ or about $\frac{1}{400}$; diminishing the density of the vapour to this amount and augmenting the

volume of the liquid. On the Chart this would diminish the highest ordinate of the alcohol series by $\frac{1}{2400}$, a quantity too small to be recognised. On examining the line of the series minutely, a very slight upward tendency may be discovered, which is perhaps due to this cause as it is likely the distensibility increases with the temperature. Thus M. Grassi's observations do not lead us to apprehend any serious effect from this cause on the results as to vapours. As to observations on liquids tried by this plan, there is no doubt they must be very sensibly affected. Probably the easiest way of ascertaining its amount would be to make observations with a sealed graduated tube, A, two-thirds filled with ether enclosed within another ungraduated sealed tube, B containing a portion of the same liquid. Comparing the readings of A at high temperatures with the readings at the same temperatures before it was enclosed within B, the difference would probably lead us to the value of the distension in terms of the pressure. Trying this with tubes of different thicknesses might afford us sufficient data to supply the correction generally.

§ 2. These observations were made with the most ordinary means, merely to indulge private curiosity on a point believed to have been established by the elaborate researches of the French Academy. They would not have been thought worthy of mention had they not been confirmed by others of established character and had they not led to an important modification of the general principle discussed in the previous paper accompanying the first Chart. My experience so far as it goes has impressed me more and more with a conviction of the efficiency of this kind of observation as a mode of inquiry at high temperatures. That it is convenient and simple will not be denied, but that it is also sufficiently accurate I long doubted until it was no longer possible to do so from the accumulated evidence in its favour. The following details will give an idea of the course of proceeding adopted and the apparatus employed.

Tubes were selected of even bore about two feet long, $\frac{1}{6}$ or $\frac{1}{7}$ inch internal diameter and $\frac{1}{50}$ inch thickness. They were generally of soft glass. After being cleaned and thoroughly dried by passing heated air through, one extremity was sealed.

A file mark was made in the centre as index. The tube was then weighed in an apothecary's balance. For graduating by mercury a series of weights were prepared, the unit being $3\frac{1}{2}$ grains, and this was divided into 20 parts of grains of rice so that the weight of mercury was ascertained in each case to about $\frac{1}{6}$ grain. The first measurement was made after pouring in just enough to fill up the conical space at the end. A twelve-inch ivory scale, divided 20 to the inch, was employed to measure the lengths from the index mark to the surface of the mercury, half-way down the convexity. By comparing several readings of the same, they were found to correspond within $\frac{1}{200}$ of an inch. The process of pouring in mercury, weighing and measuring, was continued and repeated at about every two inches along the tubes, they being nearly cylindrical. The weights and corresponding lengths were then set off on paper to a large scale as co-ordinates. Any change in the bore of the tube was thus distinctly represented to the eye. Sometimes it was found perfectly cylindrical throughout its length; generally it was only so in one or two parts, and *one of those parts* was selected for graduation.

The tube was now washed out with a portion of the liquid to be experimented upon and a mark made at the height proper to fill it up to so that the expansion might take place in the cylindrical part. The liquid was then poured in and the tube immediately sealed. This required some attention so that there should be neither concavity nor bulging. The tube was held over a small Argand lamp having a short burner and heated gradually till it bent over about an inch from the end. The flame was then increased so as to play on the glass, the tube held straight was gently pulled asunder (fig. 6), and slowly removed upwards from the flame so that the equilibrium of pressure should be maintained until the glass became rigid. The result was generally a fine conical termination as strong as any other part of the tube, without the inner surface having been exposed to the flame. With hard glass, a blowpipe had of course to be substituted for the lamp.

To ascertain the capacity of a tube, the distance of the liquid surface from the file mark was taken and laid off on the projection which is represented in fig. 12 on a small scale.

A*bcd*E is the line developed by the co-ordinates of length and volume, the lengths measured horizontally and volumes vertically. The surface of the liquid suppose at $\delta$ measures B$\delta$ from index mark B, the liquid volume is $\delta d$. The tube is now turned upside down and by tapping it the liquid descends

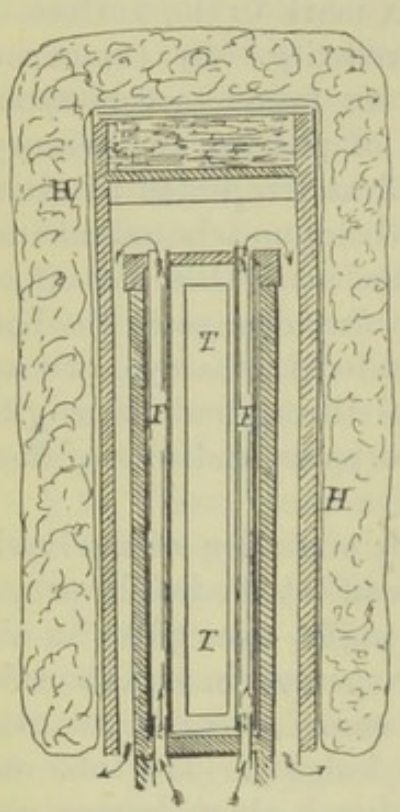



Fig. 1.

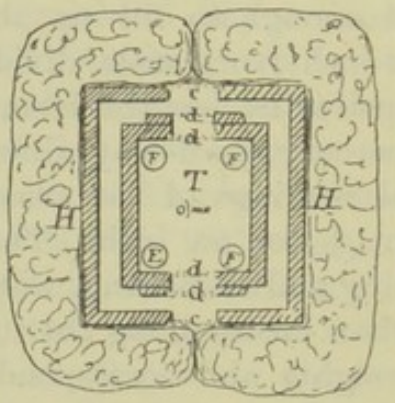



Fig. 2.

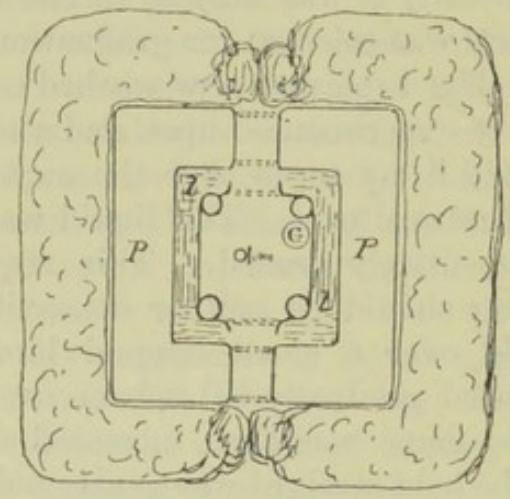



Fig. 3.

and the vacant space is dried by gentle heating. The surface of the liquid is now on the other side of B at $\beta$; the corresponding ordinate $\beta b$ is the volume unoccupied by the liquid. The volume of the tube is thus $\delta d + \beta b = \delta$D. This is supposed to be divided into 100 equal parts and the linear value of $\frac{1}{100}$ at the cylindrical part to be graduated is ascertained with great precision by extending the corresponding part of AE and

measuring off the subtense of a vertical equal to whole volume $D\delta$. This will of course be equal to 100 divisions. A certain

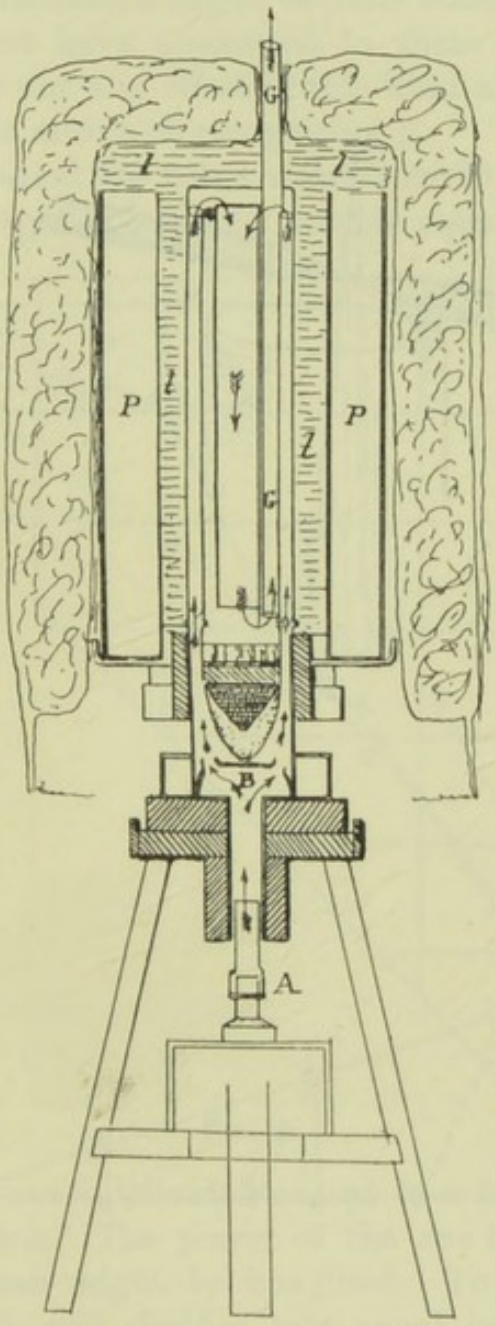



Fig. 4.

number of these corresponding to the probable amount of expansion is then accurately laid off upon a slip of paper.

The distance of the first $\frac{1}{100}$ division below the liquid surface from the index mark is accurately determined from the

position on AE; also its reading. The divided slip of paper is then laid along the tube and the divisions are *drawn on the glass* with Indian ink (see fig. 5).

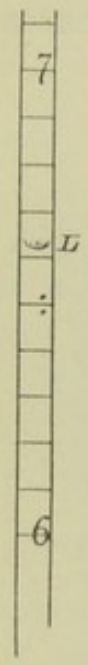



Fig. 5.

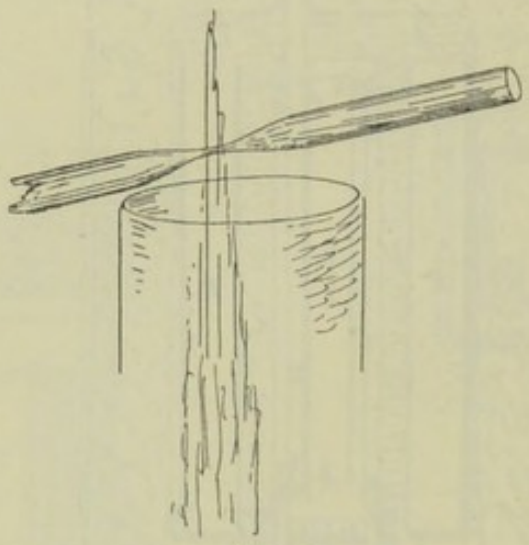


Fig. 6.

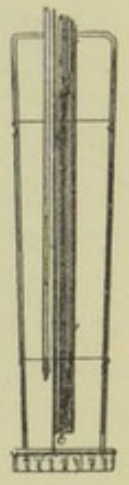


Fig. 7.

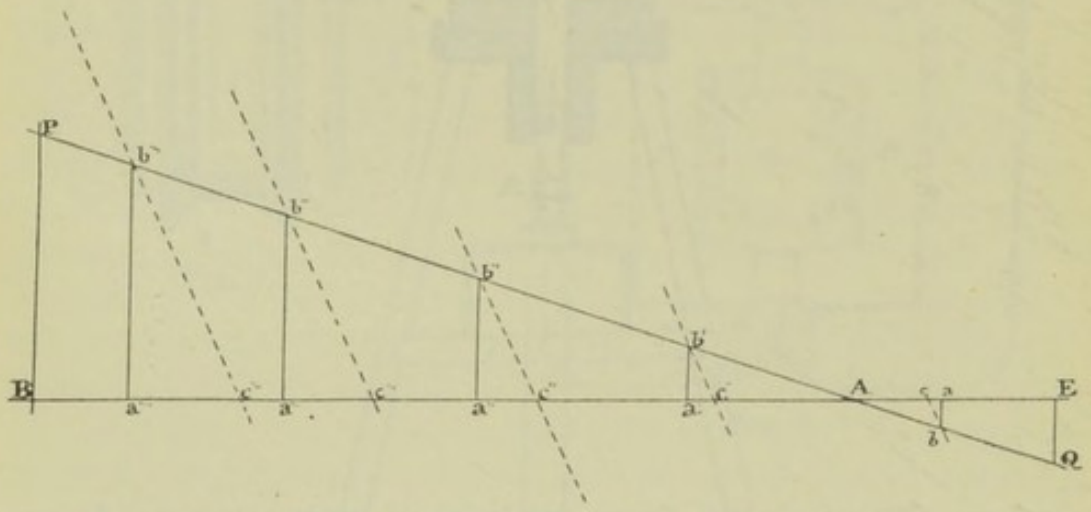



Fig. 8.

No means were taken to exclude the air while sealing the tubes except in one instance with ether; but at moderately high temperatures the liquid column separated into several parts even when the temperature at top was one degree higher

than the bottom of the heater. In extreme cases when the expansion of the liquid had nearly filled the tube the pressure of the air thus condensed might become sensible. In one case this pressure must have amounted to three atmospheres, but I could not make out any effect of this kind although the other tubes afforded the means of recognising it.

After being sealed the tubes were from 20 to 22 inches long and the linear magnitude of $\frac{1}{100}$ volume equal to about $\frac{1}{5}$ of an

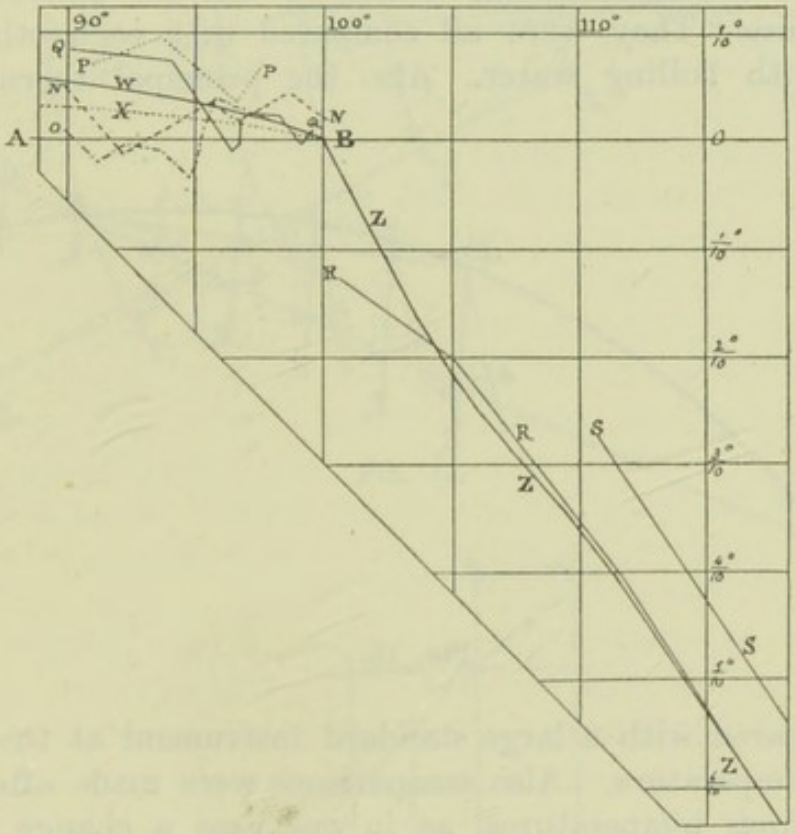



FIG. 9.

inch. This was not subdivided except in a few cases where it extended to $\frac{2}{7}$ inch. The power of the eye to divide spaces is much greater than might be imagined. To test this several lines were drawn and divided into equal parts of one inch; intermediate marks were then made at random and two parties were called upon to estimate their distance in hundredths of one division. Although quite unpractised, their readings never differed more than three hundredths from the correct amount, generally two, sometimes exact. I generally could read off to one hundredth; it requires a little time and attention. To read

off to one twentieth of a division on the tube is easy and certain.

The principal thermometer was divided on its thick stem to every 2° up to 440°. It had a conical bulb and a degree measured $\frac{1}{10}$ of an inch. A second with glass scale in tube had 30° to an inch. A third of the same kind had 40° to an inch. Other smaller thermometers were divided into spaces of 10° and could read to half a degree; they were placed at the top and bottom of the heater to test its uniformity of temperature. They were all compared with each other and tested with boiling water. Also the principal thermometer

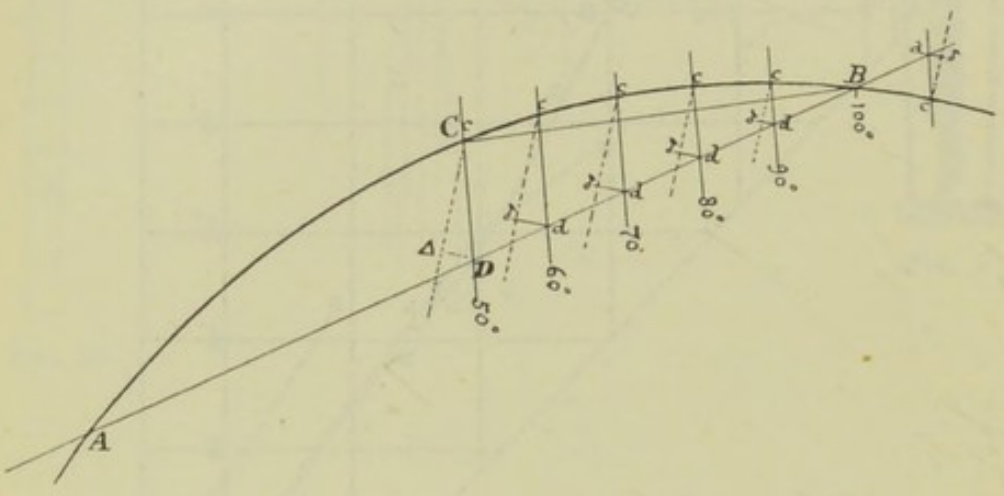



FIG. 10.

was compared with a large standard instrument at the atmospheric temperature. Also comparisons were made after each trial at high temperatures, as in one case a change in the standard points had taken place by incautiously exposing the upper part of the stem.

The heating apparatus employed for the first experiments on ether is represented in plan and section by figs. 1 and 2; that afterwards employed for alcohol, etc., by figs. 3 and 4. The lower part is the same in both. The heat was derived from one Argand lamp placed at A, fig. 4, the draft proceeding directly upwards first struck against an iron plate at B, from whence it was distributed through four vertical double iron flues, F, figs. 1 and 2, placed at the corners of the box, T, in the middle of which the thermometer and tubes were suspended.

This box was made of inch teak plank strongly screwed together and lined with iron plate. It was about 2 ft. high and at each side there were two glass partitions, *dd*, $2\frac{1}{2}$ ins. broad and extending nearly the full height of the box. Over it was placed a hood, HH, made also of inch plank

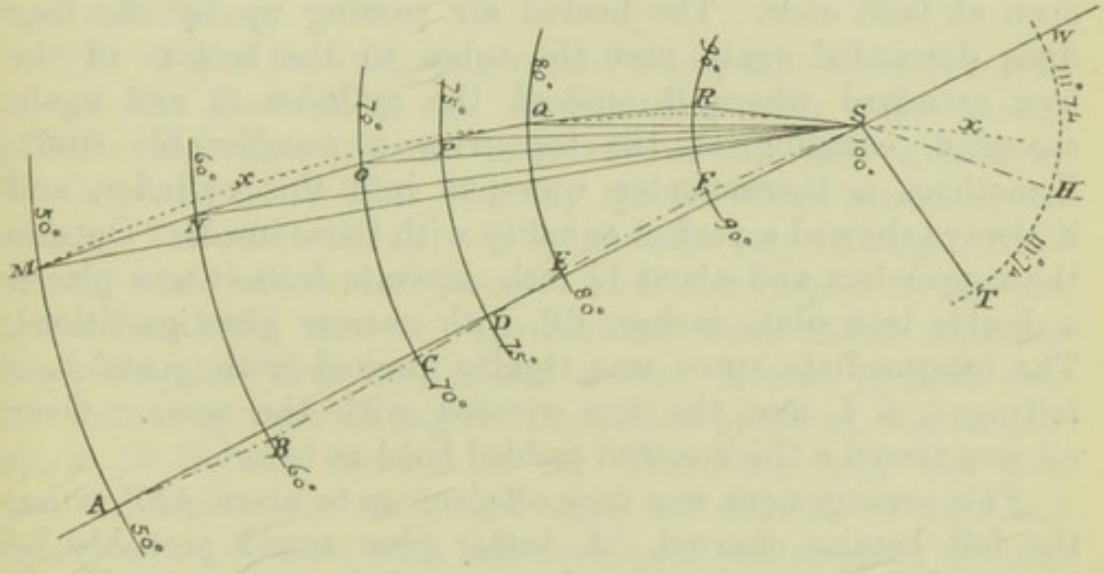



Fig. 11.

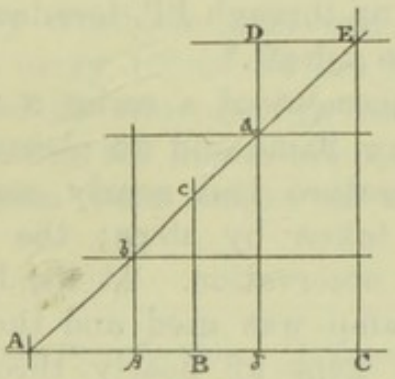



Fig. 12.

and with a glass partition on each side opposite *dd*. It caused the heated air to pass downwards along the outer surface of T and escape at the bottom. A clear view was thus obtained of the thermometers and tubes throughout their entire length. At the higher temperatures a thickly padded cotton hood was placed over all, which was opened only on each side while taking an observation. Figs. 3 and 4 represent

a similar apparatus with the inner box and flues made of copper. The thermometers and tubes were fixed upon an upright iron standard (fig. 7) that rested on a base of cork. The copper box being shut at top was placed over the standard after the tubes were adjusted. Through a hole in the upper surface a copper cylinder G was passed $1\frac{1}{2}$ in. diameter and open at both ends. The heated air passing up by the four flues descended again past the tubes to the bottom of the iron standard where it entered the cylinder G and again ascended, escaping at the top with a considerable draft. Sometimes a thermometer was put into this cylinder, and it always showed a perfect equality with those inside. Outside the copper box and about $1\frac{1}{2}$ inch separate from it was placed a double iron plate casing, PP, with narrow glass partitions. The intermediate space was tightly packed with goats' hair felting *l*, *l*, *l*, and the top covered with the same. Over all was placed a thick cotton padded hood as before.

This arrangement was very efficient up to about 420°, when the felt became charred. A better plan would probably be to cut off G close to the top of the copper box, then by means of a cover to bring the heated air down again through the space *l*, *l*, and then up through PP, terminating in a funnel of some length to make a draft.*

At the commencement of a series a small Argand lamp was used with a low flame and an observation was made as soon as the temperature had nearly ceased to rise. Thus observations were taken by steps; the flame being raised a little after each observation. At the higher temperatures a more powerful lamp was used and the rate of 1° rise in three minutes was kept up nearly throughout, this being found to give the same result as if the rise were slower indefinitely. At high temperatures conduction takes place so rapidly that an equilibrium of temperature is much more easily obtained than at the lower part of a series.

* My apparatus makers were a Hindoo blacksmith and Parsee carpenter. The thermometers, tubes, etc., had to be sent for to Europe, and by some inevitable fatality it was never possible to get an order executed correctly. The observations though in themselves very simple were thus made troublesome and difficult by local circumstances.

While getting acquainted with the working of the apparatus the same set of tubes were repeatedly tried, so as to make sure of the curves of each tube. These curves were drawn to a scale of 20° to the inch horizontal and $\frac{1}{100}$ of volume to the inch vertical. The observations were thus made in some degree to check each other and the results were generally very satisfactory.

§ 3. I find the following record in my note-book of a trial to test the graduation, etc., of five tubes with water having a little carbonate of soda dissolved in it. This is said by M. Cagniard de la Tour to delay the corrosive action of the steam. The effect, if any, was found to be very trifling. These several trials with water, though abortive in respect to vapours, showed that the rate of expansion of weak saline solutions is sensibly less than of distilled water. I must premise that the following were among the first tubes prepared, the longest about 19 in. and shortest 15 in. There was only one trial and the curves for each tube were not drawn. B tube had been previously used with another liquid and resealed without changing the graduation. Another unfavourable circumstance is, that these tubes were not so cylindrical as those afterwards employed. Generally available observations were obtained much closer, viz., at every 10° or 5°, the lower temperatures being first determined by two or three trials each occupying twelve or fourteen hours, the higher temperatures afterwards. Results as to expansion were sought as well as for density of vapour.

| No. | Time. | Temperature Fahr. | Tubes filled with Water + $\frac{4{\cdot}6}{100}$ Carbonate of Soda. | | | | | Thermometer. |
|---|---|---|---|---|---|---|---|---|
| | | | A. | B. | C. | D. | E. | |
| | H. M. | | | | | | | |
| 1 | 1 41 | 132 | 297 | 419 | 539 | 648 | 714 | Rising |
| 2 | 2 47 | 189½ | 302 | 426 | 548 | 659 | 726½ | ,, |
| 3 | 3 40 | 216 | 305 | 431 | 553 | 665½ | 734 | Steady |
| 4 | 4 47 | 242 | 307½ | 435 | 559½ | 672½ | 741½ | Rising |
| 5 | 6 4 | 286 | 314 | 444½ | 570 | 685 | 756½ | ,, |
| 6 | 7 19 | 300 | 315½ | 448 | 574½ | 690½ | 761 | ,, |
| 7 | 8 12 | 337 | 320½ | 456 | 584½ | 703 | 775 | ,, |
| 8 | 8 51 | 357 | 323½ | 461½ | 591 | 710 | 783½ | ,, |

The steam now corroded the glass.

B and E were legible up to 410°, but absorption took place = $\frac{1}{200}$ nearly.

Tried the differences of No. 4 and No. 1 and 2 (as shown in fig. 8). Found ACD perfect; E to be increased $\frac{1}{10000}$ and B $\frac{1}{4000}$.

Tried No. 2 and 3.—ACE in a line requires + $\frac{1}{2000}$ to D, and - $\frac{1}{3000}$ to B.

,, 1 and 3.—AC in a line requires - $\frac{1}{2000}$ to B, + $\frac{1}{5000}$ to D, and - $\frac{1}{5000}$ to E.

,, 3 and 6.—AD in a line requires - $\frac{1{\cdot}2}{1000}$ to B, - $\frac{1}{3500}$ in C, and + $\frac{1}{2500}$ in E.

,, 6 and 8.—B seems always $\frac{1}{12}$ in excess. The line of A, E requires a correction of - $\frac{1}{5000}$ in C, and of + $\frac{1}{5000}$ in D.

,, 3 and 8.—D, E in a line requires - $\frac{1}{1500}$ in A and - $\frac{1}{1000}$ in C.

,, 6 and 7.—ACDE perfectly in a line.

,, 1 and 8.—ADE in a line gives C, $\frac{1}{1500}$ in excess and B $\frac{3}{1000}$ in excess.

§ 4. The first three columns of the following are an exact copy of this table. First column contains the temperature by air thermometer. Second column the density of the vapour in terms of the liquid taken as 10,000 at 177°. Third column the sixth root of the numbers in column 2. Fourth column the differences of the numbers in column 3. Column 5 has been added to show what the numbers in column 3 ought to be to conform to the dotted line J, J, on the Chart. Their common difference is 0553·35, which makes the zero 354·7° G temperature Fahr. or −106·3° Fahr. same as the zero of aqueous vapour. In column 6 the errors of the densities in column 2 are given supposing the vapour to conform to the line J, J. The three first columns of this table are valuable as no suspicion of bias towards any particular theory can be attached to them. It was while making No. 4 that the general law suggested itself. After long groping in the dark it was from the equality of these differences the welcome light came that flashed con-

viction. Such was the impression at the time, although it may be said the equality does not appear very striking. An inspection of the 6th column will, however, show that considering the method of observation the accordance is really good. It certainly exceeds my expectations.

| 1. | 2. | 3. | 4. | 5. | 6. |
|---|---|---|---|---|---|
| Deg. | | | | | |
| 180 | 18·0 | 1·6189 | ... | 1·5844 | +2·3 |
| 190 | 22·0 | 1·6739 | 0550 | 1·6397 | +2·8 |
| 200 | 26·5 | 1·7267 | 0528 | 1·6950 | +3·0 |
| 210 | 31·8 | 1·7808 | 0541 | 1·7502 | +3·3 |
| 220 | 38·1 | 1·8344 | 0536 | 1·8056 | +3·7 |
| 230 | 45·3 | 1·8880 | 0536 | 1·8609 | +3·8 |
| 240 | 53·6 | 1·9418 | 0538 | 1·9163 | +4·1 |
| 250 | 63·0 | 1·9950 | 0532 | 1·9716 | +4·5 |
| 260 | 73·5 | 2·0467 | 0517 | 2·0270 | +4·2 |
| 270 | 85·4 | 2·0985 | 0518 | 2·0823 | +3·9 |
| 280 | 98·9 | 2·1505 | 0520 | 2·1376 | +3·7 |
| 290 | 114·2 | 2·2026 | 0521 | 2·1930 | +3·0 |
| 300 | 131·6 | 2·2553 | 0527 | 2·2483 | +2·5 |
| 310 | 151·6 | 2·3091 | 0538 | 2·3036 | +1·9 |
| 320 | 174·2 | 2·3633 | 0542 | 2·3590 | +1·7 |
| 330 | 199·6 | 2·4174 | 0541 | 2·4142 | +1·0 |
| 340 | 227·9 | 2·4715 | 0541 | 2·4696 | +1·0 |
| 350 | 259·5 | 2·5255 | 0540 | 2·5250 | +0·3 |
| 360 | 295·1 | 2·5803 | 0548 | 2·5803 | +0·0 |
| 370 | 334·9 | 2·6353 | 0550 | 2·6356 | −0·2 |
| 380 | 379·5 | 2·6907 | 0554 | 2·6910 | −0·2 |
| 390 | 429·2 | 2·7466 | 0559 | 2·7463 | +0·3 |
| 400 | 484·2 | 2·8022 | 0556 | 2·8016 | +0·5 |
| 410 | 545·5 | 2·8585 | 0563 | 2·8570 | +1·8 |

The numbers in column 2 are the values of $\delta + \epsilon$ (§ 1); the observations only give $\epsilon$, although after knowing the law it is possible to obtain $\delta$ independently if the series is carried sufficiently high. But it is easy to compute its value ($\Delta_1$) at the boiling-point of the liquid from the known specific gravity of the vapour, and this was always done first.

The formula is $$\Delta_1 = \frac{a}{\mathrm{A}st_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

in which A is a constant quantity found by multiplying the ratio of specific gravity of water to air at 60° F. by the ratio of air to hydrogen and dividing the product by 521°, the G temperature of 60°.

$a$ = specific gravity of the vapour on the hydrogen scale.
$t_1$ = the G temperature of the boiling-point.
$s$ = the specific gravity of the liquid at T, the initial temperature of a series of observations.

The next operation is to subtract the value obtained for $\epsilon$ at $t$, from all the other values of $\epsilon$, then add on $\Delta_1$ to each in place of it. The sixth root of these *specific* densities may now be computed and laid off as ordinates to the temperatures. The extremities of these ought to range in a straight line which on being produced meets the axis of temperature in a point that is the zero of vapour. Let $g$ = the G temperature at this point and $h_1 = \frac{\delta t}{\delta(\delta + \epsilon)}^{\frac{1}{6}}$ = the ratio of differences of temperatures to differences of ordinates, we have

$$\left\{\frac{t-g}{h_1}\right\}^6 = \Delta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

$\Delta$ being the density of the vapour at the G temperature $t$ in terms of density of liquid unity at T the initial temperature of a series of observations. On the general Chart $\frac{p}{t}$ is taken to represent $\Delta$, the tension of the saturated vapour being denoted by $p$. At the boiling-point $p = 30$ in., so that the quotient $\left(\frac{p}{t}\right)^{\frac{1}{6}}$ becomes known, and comparing it with $\Delta_1{}^{\frac{1}{6}}$ (19) we discover the ratio between the ordinates on the Chart and those derived from the *specific* densities $\left[= \frac{h}{h_1}\right]$ see (24). In this way the points on the Chart were determined from the tube observations. It is very simple in practice though rather tedious to explain. First $g$ is obtained from the sixth root of the specific densities; then suppose $p$ to be the height of the barometer when the boiling (G) temperature $t$ is observed, we have $\left(\frac{p}{t}\right)^{\frac{1}{6}}$ ordinate to $t$ abscissa. The line drawn through $g$ and the extremity of this ordinate is the *gradient of density*. Thus the accurate determination of the boiling-point is of vital importance.

The scales for the ordinates on the chart are drawn out for $\left(\frac{p}{t}\right)^{\frac{1}{8}}$ adapted to either French (left hand side) or English (right hand side) measures.

§ 5. So long as the laws of Mariotte and of Dalton and Gay-Lussac are maintained $\Delta \doteqdot \frac{p}{t}$ (in which $\Delta$ = the density of saturated vapour, $p$ the tension and $t$ the G temperature) and the general law of density (20) gives also the general law of tension

$$\left\{\frac{t-g}{h}\right\}^6 t = p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

The constants $g$ and $h$ may be computed from two good observations of tension by the following rule:

Let $p_1$ $p_2$ be the two pressures and $t_1$ $t_2$ the corresponding G temperatures (found by adding 461° to the temperature Fahrenheit or 273·9° to the temperature Centigrade):

$$h = \frac{t_2 - t_1}{\left(\frac{p_2}{t_2}\right)^{\frac{1}{8}} - \left(\frac{p_1}{t_1}\right)^{\frac{1}{8}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

$$g = t_2 - \left(\frac{p_2}{t_2}\right)^{\frac{1}{8}} h \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

The line ZZ on the Chart is drawn parallel to the axis of temperature and at a distance from it equal to unity on the scale $\left(\frac{p}{t}\right)^{\frac{1}{8}}$ English measures. Thus if on this line we lay off from the G zero the value of $(g + h)$, while on the axis of temperature we lay off from the same zero the value of $g$, joining the points gives us at once the Chart line of the vapour.

Comparing $h$ of (21) with $h_1$ of (20) and $\Delta_1$ of (19) we find $\frac{h}{h_1} = \left\{\frac{t_1 \Delta_1}{30}\right\}^{\frac{1}{8}}$ (24). Hence from the general formula (21) we may compute the specific gravity of a vapour at any temperature in terms of the specific gravity of its liquid taken as unity, by changing the value of $h$. The rule for doing so is as follows:

To the log. $h$ add the constant log. 0·47208 and from the sum subtract one-sixth of the log. of the quotient found by dividing the standard specific gravity of the vapour in terms of hydrogen unity by the specific gravity of the liquid in terms of water unity at 60° F.

§ 6. M. Regnault is so eminent an authority and his low pressure observations between 50° and 100° C. are so admirably in accordance with the general principle, that I have thought it worth while to analyse them graphically in connection with his high pressure observations immediately above 100° so as to ascertain distinctly if the law of continuity is observed or whether they afford internal evidence of a dislocation or break in the line at 100°. It is only by studying graphically the sequence of the observations that we can hope to discover this. The different series at the same temperatures are in excellent accordance with each other. The following is the method adopted and illustrated by fig. 9. The temperatures are not corrected (*Ann. de Chim.*, July 1844). From Table IV. series NOPQ of M. Regnault's observations from 90° C. upwards were taken and the sixth root of the quotient of the tension by the temperature plus 274° was computed for each. These call $\alpha$ ordinates. Next the same function of the equalised tensions at 75° and 100° were computed. A line AB was then supposed to be drawn joining the extremities of these latter considered as ordinates to 75° and 100°. The ordinates to AB at the preceding temperatures of Table IV. were then computed, which call ordinates $\beta$. Now the process adopted in constructing fig. 9 was to take the difference between the $\alpha$ and $\beta$ ordinates and lay them off as $\gamma$ ordinates to the same temperatures respectively. These differences projected on a large comparative scale give us an exact and extremely foreshortened view of the trend of the projected observations, NN, OB, PP, QQ, similar to looking along a line instead of down upon it. In fact it may be considered as an orthographic projection of the lines on a plane very nearly perpendicular to AB, the slope of this line to the axis of temperature being at the same time very small.

AB being supposed to be produced, a similar computation was made for the observations in Table V. series R and S up

to 120° RR, SS, fig. 9. The lines traced out by the respective formulæ of interpolation employed by M. Regnault are shown by the line W for (A) formula and ZZZ for (C) formula. They represent the observations very correctly. The dotted line X is the locus of the Chart line for steam on this projection. It will be remarked that this system provides us with a vertical scale of temperature to which the irregularities of the observations may at once be referred. Thus the greatest divergence from the line W below 100° is $\frac{1}{16}$ of a degree and above 100° about $\frac{1}{10}$.

We have now in fig. 9 the means of judging from the evidence afforded by the observations themselves independent of any theory, whether or not there is a latent cause of error increasing with the temperature above 100° which has escaped M. Regnault's notice. (See § 8 at end.)

Whether this mode of projection is natural or artificial does not affect the question. Whatever the system of projection the line must present continuity. The method employed is selected because it makes the continuity more obvious. Unless the natural law of tension changes suddenly at 100° C., I do not see how the accuracy of the observations can be maintained.

§ 7. It is remarkable that if we compute the elements of the conic hyperbola that passes through three of these observations on the expansion of mercury (the asymptotes being supposed parallel to the axis of temperature and of volume) it will be found to pass exactly through the fourth also. This singular fact may be a chance coincidence, or, as I am more inclined to think, may arise from the orbit of dilatation actually being a conic section in this as in other cases I have tried. At any rate it affords, when combined with the equation previously alluded to for the expansion of glass, a very convenient formula of interpolation to correct the mercurial thermometer. It is here annexed adapted to the Centigrade scale:

$$t_m = \frac{\mathrm{B}t_a}{\mathrm{A}t_a} - \frac{t^3{}_a}{\mathrm{C}^3} - \frac{t_a}{\mathrm{D}}$$

$t_a$ = temperature by air thermometer, Centigrade scale.
$t_m$ = temperature by mercurial thermometer, Centigrade scale.

log. B = 3·7145723.
A = 4539·617.
$C^3$ = 6·43303.
D = 0·78587.

It is more convenient to have $t_a$ in terms of $t_m$. The following gives a near approximation:

$$t_a = \frac{t_m}{\frac{B}{A - t_m} - \frac{t^2_m}{C^3} - \frac{1}{D}}$$

These formulæ exhibit the temperature by air thermometer in advance of the temperature by mercurial thermometer between 0° and 100° C. The amount of difference increases from 0° to 48°, where its maximum value is 0·513 and when the value of a degree on both is the same. At 0° the mercurial degree is $\frac{51}{50}$, the air degree at 100° is $\frac{49}{50}$. There is distinct evidence of this difference in M. Regnault's observations on low pressure steam. (See § 8.)

§ 8. Although of little practical importance, this difference below 100° C., if it exists, cannot safely be neglected in theoretical researches, inasmuch as the value of a degree of the mercurial thermometer must in such case be a variable quantity differing in the very sensible ratio of 23 to 24 from 100° to 0° C. The injurious effect of this latent distortion will be apparent when we consider the difficulty, if not impossibility, of detecting the nature of the most simple curve if that happens to be laid off from a slightly bent line instead of a straight line. This is precisely the difficulty we encounter in tracing the law of liquid dilatation. It is like computing the orbit of a planet from observations taken with unequally divided instruments. It is true the amount of difference is not great and may be compensated in a great measure by extending the range of observations; but it so happens that almost all the most delicate thermometric observations lie between these limits. They are therefore quite unavailable for theoretical purposes if the difference exists and is not allowed for. I have looked about anxiously for evidence on this point of sufficiently decisive character to carry conviction to the most sceptically disposed and have at last been successful in extracting it from

M. Regnault's observations on low pressure steam in connection with the law of density set forth in this paper.

That law has reference to the air thermometer, so that if a series of observations between 0° and 100° C. were *perfectly* correct they must, when projected on the Chart, exhibit the difference very clearly, if it really exists. The line of density would appear as a curve slightly concave towards the axis, and if the proper correction were made on the temperatures that curve would be converted into a perfectly straight line.

The diagram, fig. 10, has been drawn to illustrate this, the curvature caused by the supposed difference being very much exaggerated. ADB is the gradient of density in which the points range if the temperatures are by air thermometer. Through D, *d*, *d*, etc., corresponding to 50°, 60°, 70°, etc., lines of constant pressure DC, *dc*, *dc*, etc., are drawn and at distances D$\Delta$, *d*$\delta$, etc., equal to the respective computed differences, verticals $\Delta$C, $\delta c$, etc., are drawn intersecting the lines of constant pressure in C*c*, etc. The curved line passing through C*ccccc*B*c* is that in which the points of density range if laid off to the temperatures of the mercurial thermometer. At low temperatures the errors of observation are enormously magnified on this projection, so that we cannot expect to recognise the curvature of ACB below C. The only chance of doing so is by examining minutely the upper arc CB.

To realise this the values of the constants $g$ and $h$ of the general equation (21) were obtained from the equalised observation at 50° and 100°. This gives the equation of the line CB. The intermediate tensions at 60°, 70°, etc., were then computed as being in this line and the results compared with the equalised observations supposing these to be sufficiently represented by the table at the end of M. Regnault's Memoir.*

The differences were then divided by the differentials of tension at the respective temperatures so as to obtain the

* Computed from the formula of interpolation $\log e = a + b\alpha^t - c\beta^t$, the constants of which were determined from the graphically equalised observations at 0°, 25°, 50°, 75°, 100°. There is abundant evidence of the sufficiency of this equation to represent the observations within their very small limits of error.

intervals between the arc and chord CB, if any should exist. The result of these operations is shown in Table I.

TABLE I.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 1 | Deg. 50 | Deg. 60 | Deg. 70 | Deg. 75 | Deg. 80 | Deg. 90 | Deg. 100 |
| 2 | mm. 91·98 | mm 148·52 | mm. 232·22 | mm. 287·27 | mm. 353·06 | mm. 523·71 | mm. 760·0 |
| 3 | 91·98 | 148·79 | 233·09 | 288·50 | 354·64 | 525·45 | 760·0 |
| 4 | 0 | +0·27 | +0·87 | +1·23 | +1·58 | +1·74 | 0 |
| 5 | Deg. 0 | Deg. $+\left(\frac{40}{1000}\right)$ | Deg. $+\left(\frac{87}{1000}\right)$ | Deg. $+\left(\frac{102}{1000}\right)$ | Deg. $+\left(\frac{110}{1000}\right)$ | Deg. $+\left(\frac{87}{1000}\right)$ | Deg. 0 |

1 = Temperature by mercurial thermometer.
2 = Tensions computed.
3 = Tensions observed.
4 = Observed T., minus computed.
5 = Corresponding differences of ordinates.

The differences in the last line are nearly what might be expected as the intervals between an arc and its chord, except that the maximum is at 80° instead of 75°.

The same operations were next performed with the temperatures corrected by the formula ( ) so as to transform them into what is for the present assumed to be the temperatures by air thermometer. The results are contained in Table II.

The differences in the fifth line should all be zero if our anticipations were realised and if the quantities under the heading *Tensions observed* were *perfectly* exact; but this is not to be expected, as they are derived from a purely empirical formula bound down to the points 50°, 75°, and 100° only and agreeing with the other observations within small limits which in such formulæ are alternately positive or negative. The only real discrepancy is under 75° amounting to $\frac{8}{1000}$ of a Centigrade degree. If the line were drawn through 100° and 75° instead of 100° and 50° it may be seen from the seventh line of the table that the positive and negative differences exactly balance each other and that their maximum amount is only $\frac{1}{25}$ of a degree. This assumes M. Regnault's graphically equalised observation at 50 to be $\frac{1}{60}$ of a degree in error. On the other

hand, if that is perfect, the assumed difference between the air and mercurial thermometers is $\frac{1}{48}$ too great. In the eighth line of Table II. the differences are adjusted to this hypothesis. It will be remarked that their sum is still zero and the maximum only $\frac{1}{34}$ of a degree.

Thus M. Regnault's observations amply realise all that can be desired of them. They give evidence of their own great

TABLE II.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. |
| 1 | 50 | 60 | 70 | 75 | 80 | 90 | 100 |
| 2 | 50·512 | 60·481 | 70·413 | 75·366 | 80·310 | 90·171 | 100 |
| | mm. | mm. | mm. | mm. | mm. | mm. | mm. |
| 3 | 91·98 | 149·03 | 233·30 | 288·60 | 354·60 | 525·09 | 760·0 |
| 4 | 91·98 | 148·79 | 233·09 | 288·50 | 354·64 | 525·45 | 760·0 |
| 5 | 0 | −0·27 | −0·30 | −0·10 | +0·04 | +0·36 | 0 |
| | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. |
| 6 | 0 | $-\left(\frac{39}{1000}\right)$ | $-\left(\frac{30}{1000}\right)$ | $-\left(\frac{8}{1000}\right)$ | $+\left(\frac{3}{1000}\right)$ | $+\left(\frac{18}{1000}\right)$ | 0 |
| 7 | $+\left(\frac{16}{1000}\right)$ | $-\left(\frac{26}{1000}\right)$ | $-\left(\frac{20}{1000}\right)$ | 0 | $+\left(\frac{9}{1000}\right)$ | $+\left(\frac{21}{1000}\right)$ | 0 |
| 8 | $+\left(\frac{11}{1000}\right)$ | $-\left(\frac{29}{1000}\right)$ | $-\left(\frac{22}{1000}\right)$ | 0 | $+\left(\frac{9}{1000}\right)$ | $+\left(\frac{21}{1000}\right)$ | 0 |

1 = Temperature by mercurial thermometer.
2 = Temperature by air thermometer.
3 = Tensions computed.
4 = Tensions observed.
5 = Observed T., minus computed.
6 = ,, minus computed ordinates.
7 = ,, if line passed through 100° and 75° instead of 100° and 50°.
8 = ,, if difference between air and mercurial thermometer were reduced $\frac{1}{48}$ part.

accuracy, while they afford proof of the correctness of the thermometric formulæ of MM. Dulong and Petit's standard observations and of the general law of density set forth in this paper.

To bring out these facts into higher relief and present the deflections to the eye on a scale that should at once be relatively correct and very highly magnified, six values of $h$ (§ 5) were computed from the observations at 90 and 100 at 80 and 100, etc., without correcting the temperatures. These are given in

Table III. They are quantities proportional to the co-tangents of the inclination of the chords BC, B*c*, etc., to the axis of temperature.

The differences are regularly progressive. Now it is obvious that the relation of these differences to each other is the same very nearly as the relation of the corresponding differences of inclination of the chords. We may magnify them therefore to any extent, as in fig. 11, without sensibly affecting the accuracy of the comparative view. The scale of the figure is 10° of angular magnitude to unity in the column of differences and the lengths of the chords are made to correspond with the intervals of temperature. Thus $<$MSN = 1·71°, $<$MSO = 5·03°, etc., and MS = 50°, NS = 40°, etc. Joining

Table III.

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 1 | Deg. 50 | Deg. 60 | Deg. 70 | Deg. 75 | Deg. 80 | Deg. 90 | Deg. 111·74 | Temperature by mercurial thermometer. |
| 2 | 168·854 | 159·025 | 159·357 | 159·571 | 159·816 | 160·387 | 165·406 | Value of *h*. |
| 3 | 0 | +0·171 | +0·503 | +0·717 | +0·962 | +1·533 | +6·552 | Difference from *h* at 50° |

the points MNOPQRS we obtain a magnified or extremely foreshortened representation of the curve of the observations with temperatures uncorrected. If the temperatures required no correction it is obvious that the points N, O, P, etc., would lie in the line MN produced; always taking for granted the integrity of the law of density and the perfect accuracy of the observations.

The next part of the process was to perform the same computation with temperatures corrected. The resulting values of *h* are given in Table IV.

The difference between *h* at 50° in the two tables is 1·733 being 17·33° for the angle MSA. The differences in Table IV. are laid off from AS, thus $<$ASB = 1·38°, etc., but the lines SB, S*c*, etc., are omitted to save confusion.

Joining the several points, we have A, B, C, D, E, F, S, as the line of observations with temperatures corrected. If this coincided with AS we would have M, N, O, P, etc., coinciding with the faint dotted line M*x*S and the desired proof of the

thermometric formulæ would be obtained. Now the dotted sinuous line ABFS intersects the straight line AS in 100 in 50 and very nearly in 75° and diverges from it at a maximum which is only about $\frac{1}{25}$ of a degree, and the equation of this dotted sinuous line is the empirical formula of interpolation which is only bound down to these three points and at intermediate points cannot differ much more than $\frac{1}{25}$ of a degree from the truth.

The residual difference at D, which is all the amount of deviation from perfect accuracy, can well be afforded when we consider that the accordance we have found is dependent not only on the integrity of three general laws, viz., that of

TABLE IV.

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 1 | Deg. 50 | Deg. 60 | Deg. 70 | Deg. 75 | Deg. 80 | Deg. 90 | Deg. 111·74 | Temperature by mercurial thermometer. |
| 2 | 50·512 | 60·481 | 70·413 | 75·366 | 80·310 | 90·171 | 111·50 | Temperature by air thermometer. |
| 3 | 157·121 | 156·983 | 157·006 | 157·053 | 157·155 | 157·428 | 161·738 | Value of $h$. |
| 4 | 0 | −0·138 | −0·115 | −0·068 | +0·021 | +0·307 | +4·617 | Difference from $h$ at 50°. |

Mariotte, that of Dalton and Gay-Lussac, and this law of density, but it demands almost perfect accuracy in two series of observations, viz., that of M. Regnault on low pressure steam and that of MM. Dulong and Petit on the expansion of air, mercury, and glass.

It may be said that errors in one may compensate for errors in the other. Such might be the case in one out of many observations, but that two series should thus have *compensating* errors throughout is quite beyond the limits of probability.

It remains for us to notice the high pressure observation which appears in Tables III. and IV. It might be expected without reference to theory that PQRS in its continuation above 100 should not show any abrupt divergence from the regular curve M$x$S$x$. Nevertheless, from 100° to 111·74° the direction of the chord is ST and the continuation of EFS is SH

instead of keeping close to SW. There seems no other way of accounting for this angular break or dislocation of the line, unless by attributing it to a general fault in the observations above 100°. The divergence of these from the law of density has already been noticed and is shown in the Chart.

M. Regnault is so eminent an authority that I have thought it necessary to enter more minutely into this subject in § 6.

Bombay,
*Oct.* 15, 1852.

# ON CAPILLARITY AND ITS RELATION TO LATENT HEAT

(From the *Philosophical Magazine* for January 1858.)

In the following paper I offer an account of some experiments on capillarity, and certain inductions from them, suggested by the modern views of the dynamical or *work*-value of molecular force. The argument kept in view is, if the capillarity of a liquid is the exhibition of part of the cohesive force of the superficial stratum of its molecules, numerical relations with the latent heat of its vapour ought to be demonstrable if latent heat is the measure of liquid cohesion.

§ 1. As a general rule, the glass tubes and plates were washed first with sulphuric acid, then with nitric acid and with distilled water immediately previous to each set of observations. This was especially necessary when water was the liquid under observation, as it does not attach itself so readily to glass as the spirits and æthers; it is also apt to dry off the glass surface above the line of capillary adhesion. In all cases care was taken to ascertain that the surface of the glass was wet at this part. When by holding the tubes or plates vertical and gently swaying them up and down, the capillary column plays with a delicate independent motion, the results can always be depended upon if the glass surface has been perfectly cleaned with acid. When observing at temperatures higher than that of the atmosphere, this test had to be constantly applied, otherwise the column slowly descended below the point of capillary equilibrium, and no certain results could be obtained.

§ 2. The following mode of observation was employed with two tubes, A and B. The diameters were measured by a scale finely divided into hundredths of an inch, and the reading made with a watchmaker's lens.

$$\text{Tube A} = \frac{0{\cdot}242}{0{\cdot}270}\Bigg\}\ \text{The numerators express the internal diameters, the denominators the external diameters in decimals of an inch.}$$

$$\text{Tube B} = \frac{0{\cdot}137}{0{\cdot}152}\Bigg\}$$

The tube being firmly fixed in a vertical position, with its lower edge before the centre of the double lens of a photographic camera, an Argand lamp was placed in front, and the focal distance adjusted so that a distinct image of the tube magnified about 40 diameters was thrown back about 30 feet on paper stretched on a vertical board. A small capillary pipette was employed to pass distilled water upon the outer rim of the tube. By inclining the pipette at a greater or less angle, the column of water in the tube and its suspended drop could be increased or diminished at pleasure with the greatest precision.

Perfectly distinct, highly magnified profiles of the tube and pendent drop were thus obtained at different phases and carefully traced with pencil. The bottom of the cup formed by the upper surface of the liquid was also distinctly marked; the upper edge less distinctly, but the versed sine of the cup seemed constant under all circumstances, and was assumed to be so.

While the suspended volume was increased from the smallest beginning, the upper surface within the tube ascended to a certain maximum height, while the lower surface swelled out to the extent of about one-third the external diameter of the tube below the lower rim. The top surface then descended, and also the lower surface, in an increasing ratio until a certain maximum depression was attained, when the lower drop broke off.

The cycle of this action included first a *rise*, and then a *fall* of the centre of gravity of the liquid, and a contemporaneous closing and expanding of the liquid surface. The interior of the tube, being wet, is taken as *liquid surface augmented* when denuded by the descent of the column, and as *liquid surface diminished* when covered by the ascent of the column.

The question proposed for solution was,— Is the ratio between the amount of work performed (as measured by the descent of the centre of gravity) and the amount of liquid

surface developed or denuded, a constant ratio; and if so, to ascertain its numerical value, taking the inch as the standard unit of magnitude?

The liquid volume suspended by the capillary attraction being assumed to be enclosed within the surface of revolution defined by the magnified profile, it was easy by an obvious process of graphical integration to compute for each of the several contours delineated,—1st, the position of the centre of gravity in the vertical axis; 2ndly, the volume of liquid suspended; and 3rdly, the extent of liquid surface. The work performed from one to another of these contours was taken as the mean volume of the liquid descending through the distance of the centres of gravity, and the corresponding surface denuded was the difference of surfaces at those times.

The contours of tube A corresponding to the highest and lowest position of the centre of gravity thus integrated and computed, gave 132·3 as the value of the quotient of the surface denuded by work performed. Expressed by symbols, this quotient $Q = \frac{a^2}{b^3 c}$, in which $a$ is the square root of difference of surfaces, $b$ the cube root of mean volume of liquid, and $c$ the vertical distance between the centre of gravity, all expressed with reference to an inch as the unit.

Two adjacent contours of tube B, similarly computed, gave $Q = 126$. Two contours of B further removed, similarly computed, gave $Q = 134$.

The atmospheric temperature at which these experiments were made was 86° (the locality being within the tropics), but the temperature of the surface of the drop was somewhat uncertain; first, the lamp being within a few inches, tended to heat it; and secondly, the atmosphere being very dry, the evaporation from its surface, sensibly rapid, tended to cool it.

Thus far it seemed proved that the tensile force at the surface of water is uniform, and that the contour of the suspended volume is determined through its complete cycle of elegant curvatures by the quotient of the differential of surface (upper and lower inclusive), by differential of work being a constant quantity.

§ 3. The maximum volume suspended at the extreme development of the drop just before it separates, was found in the case of tube C of the dimensions $\frac{0{\cdot}083}{0{\cdot}129}$, by the same method of observation and graphical integration as employed with tubes A and B. The quotient of the sum of the outer and inner rims of suspension by the volume suspended was 129.

The same tube C held vertical was brought down until its lower rim just touched the water in a cup, and the volume suspended by the inner surface of the tube computed: the quotient of the length of the inner circumference by this volume was 127. The temperature was about 86°, as before.

§ 4. Suppose a strip of paper, one inch wide, immersed in water and its whole surface wetted. In pulling it out we develop or denude an aqueous surface of two square inches for each inch of vertical height; and the work required to effect this would be ascertained if we could weigh the water adhering to the lower edge of the paper at the instant of separation; for this weight descending through one inch, is the work that denudes two square inches of aqueous surface. The volume of water equivalent to this weight divided by 2, ought to be about $\frac{1}{132}$ of a cubic inch, if the quotient (Q) of surface developed by the work performed is the same as in the first series of observations.

To try this, a slip of paper 10 inches long and half an inch broad, with clean cut edges, was formed into a spiral having about a quarter of an inch between each convolution. It was then fixed with lac upon a thin piece of plate glass while heated. The lower edge of the paper was thus firmly established in a plane parallel to the plate, and the whole suspended from the pan of a delicate balance, and adjusted by means of a plate and spirit-levels, so as to hang with the lower edge of the paper perfectly level. The paper spiral was now immersed a quarter of an inch in a shallow vessel of distilled water and allowed to soak; the level of the surface of the water being adjusted so that the spiral edge of the paper should separate from it at the turn of the beam. Thus arranged, the difference of weight just at the separation and immediately after, was found by repeated careful observations to be exactly 38 grains.

This being the weight of 0·1505 cubic inch of water (at the temperature 86°), shows the volume raised by a water-line 20 inches long. For one inch the volume is $\frac{0{\cdot}1505}{20} = 0{\cdot}00752$, or $\frac{1}{132{\cdot}9}$ of a cubic inch. The value of Q thus determined is 132·9.

§ 5. The generally recognised fact, that the power of capillary suspension resides at the water-line — that the weight of the whole column hangs, as it were, upon the water-line, and is equilibriated by the cohering energies of one ring-line of molecules,—is clearly brought out by the following experiment:

A column of water in a capillary tube was drawn out of it by pressing the lower end on an absorbing surface until only a small portion was left. It was then brought down to touch a surface of alcohol. The column rose in the tube, having a small portion of air separating the water from the spirit; and the height attained by the capillary column exceeded that of the water alone exactly in the inverse ratio of the specific gravities of the two liquids. The spirit soon drew down the small heading of water over the wet surface of the tube; and it was interesting to observe, that, at the instant when it all disappeared, the column tumbled down to the level proper to spirit, which in fine tubes is only about four-tenths that of water. In the same way, when spirit forms the heading, the column sprung up at the instant that the upper line of spirit had disappeared. This experiment was repeated with other liquids, and the result could always be anticipated from the specific gravity and specific capillarity.

A water-line one inch long holds suspended by its capillary force $\frac{1}{Q}$ of a cubic inch of water.

§ 6. The quotient Q may be computed from the product of the diameter of a tube by the mean elevation of the capillary column, which in theoretical works on capillary attraction is assumed to be constant for the same liquid at constant temperature. Supposing the tube to be drawn up one inch, the capillary

column descends one inch, denuding 1 in. $\times \pi d$ of surface, and the work performed is the volume $\frac{1}{4}d^2\pi h$ descending one inch: we have therefore

$$Q = \frac{\pi d}{\frac{1}{4}\pi d^2 h} = \frac{4}{dh},$$

in which $d$ is diameter of tube, and $h$ the equalised height of the suspended column. The value of $dh$, computed from $Q = 132$ [§ 2], is $\frac{1}{33} = 0{\cdot}0303$, which agrees nearly with Dr Brewster's value given in Table, p. 279, of the *Report of the British Association*, 1834. In small tubes, where the abnormal tendency [§ 8] is manifested, the value of Q is about $\frac{2}{3}$ of 132 or 88, and the corresponding value of $dh$ is 0·045, which agrees nearly with that given opposite the names Martin and Morveau in the same Table. By similar reasoning, in the case of plates we have $Q = \frac{2}{DH}$, in which D is the distance of the parallel plates, and H the equalised height of the capillary column.

§ 7. The 32nd volume of the *Annales de Chimie* (1851) contains an extensive series of researches on capillarity by M. Simon. Water was the liquid chiefly employed, and the dimensions of the capillary column in tubes of different bores, and between plates at different distances, were measured with great minuteness. The most remarkable result of these observations was the discovery of the fact, that the column between plates is only about one-third the height of the column in tubes, instead of one-half, as theory had previously indicated. If we assumed this as a general law, and employed the Table of specific capillarities given in p. 15 for computing the values of Q for alcohol, æther, and carburet of sulphur, we should be led into serious error. The Table shows the comparative heights of the capillary columns in fine tubes only. Another table is required to show the comparative heights between parallel plates, in which, setting out with the height of water as unity, the numbers for carburet of sulphur, alcohol, and sulphuric æther must be multiplied by $\frac{3}{2}$. The temperature at which the

observations were made is not specified, which is an omission of some consequence, seeing that there is proof that the ratios of capillarity change with the temperature [§ 10].

§ 8. The existence and extent of this abnormal feature may be ascertained with very simple appliances. I employed a tube about 3 inches long and $\frac{1}{45}$ of an inch diameter, marked with divisions cut by a diamond-point at intervals of about $\frac{1}{14}$ of an inch, each line being a guide to the eye, and the exact length of a reading being ascertained by scale and lens. There were also several thermometer-tubes with flat bore and scale engraved upon them. The glass plates employed were kept separate and parallel by inserting a piece of thin tinned plate, beaten flat and coated with lac, between them when heated. One-half of their surface being thus cemented, the other half was left open for the capillary column, and their perfect parallelism could be easily tested. One of the plates was crossed with diamond lines as guides to the eye in observing, and afterwards for applying the scale and lens to obtain the exact reading.

The liquids employed were:—

| *1st Class.* | *2nd Class.* |
|---|---|
| Distilled water. | Alcohol. |
| Sulphuric acid. | Sulphuric æther. |
| Nitric acid. | Sulphuret of carbon. |
| Ammonia. | Dichloride of sulphur. |
| Saturated solution of common salt. | Acetic acid. |
| | Chloroform. |
| | Acetic æther. |
| | Acetone. |
| | Turpentine. |

The ratios of the capillary columns of these several liquids in the tubes and between the plates were compared, and it was found that the columns of the first five liquids (being of the nature of hydrous solutions) had a higher ratio to the others when measured in the tubes than when measured between plates. If the hydrous plate columns had changed in the same ratio as the plate columns of the other liquids, their height in tubes of small bore would have been about two-thirds the

height observed. Thus if Q were estimated from the capillarity of water in small tubes, the value is about two-thirds of 132 or 88 (Q being inversely as $h$, § 6).

The measure of the tensile force at the surface of water and hydrous liquids is thus not to be obtained from the apparent capillary force in small tubes. I found that so small a proportion as 12 per cent. of alcohol destroyed the abnormal tendency of water. I made no observations with tubes of from $\frac{1}{45}$ to $\frac{1}{12}$ of an inch diameter; but in tubes of $\frac{1}{12}$ of an inch and larger, the abnormal feature was found not to exist (see § 3). I found also that in *all* tubes that do not touch a water surface, but that receive their supply of water at the lower rim by capillary pipette, as in the observations described in § 2, when the upper surface is at its maximum elevation, the interior capillary volume estimated above the lower rim *has* the abnormal ratio to the internal water-line. When the tubes thus filled to the capillary maximum are brought to touch a water surface, the water in the wider tubes descends to the normal level, but in the finer tubes it does not. With alcohol and its class, in fine tubes thus supplied at their lower extremities, the maximum abnormal level is never attained, but in the wider tubes the phenomena occur precisely as with hydrous liquids.

§ 9. To find the capillarity of alcohol, I tried spirits of various strengths in a thermometer-tube; and by setting off the heights of the capillary column and the specific gravities as co-ordinates, and drawing the curve, it was easy to obtain the height corresponding to 0·782, the specific gravity of absolute alcohol at the temperature 86° F., according to Lowitz's Table. This, compared with the height of the water-column in the same tube, gave 228 as the value of Q for absolute alcohol, after making the reduction required by the abnormal height of water in fine tubes. The value of the ratio of reduction is mainly dependent on comparative plate-observations, which are not so precise as comparative tube-observations; but it is satisfactory that it agrees closely with the recent observations of M. G. Wertheim, noticed in the *Philosophical Magazine* for October last.

The following are the values of Q for sulphuric æther and some other liquids taken at 86° F.:—

| | |
|---|---|
| 228. Absolute alcohol. | 225. Acetone. |
| 261. Sulphuric æther (of commerce). | 288. Sulphuret of carbon. |
| 199. Camphene. (212). | 257. Chloride of sulphur. |
| 355. Chloroform. | 143. Ammonia. |
| 225. Acetic acid. | 263. Sulphuric acid. |
| 262. Acetic æther. | 270. Nitric acid. |
| | 132. Distilled water. |

The value of Q for turpentine within parentheses was determined by linear integration of the contours of two phases of the drop suspended in tube B, as described in § 2; but the circumstances were unfavourable. The capillary power of turpentine being small, the change in the position of the centre of gravity that could be observed before the drop broke off did not exceed $\frac{1}{30}$ of an inch. The evaporation between the observations must also have affected the result.

§ 10. A few observations were made with tubes and plates to ascertain the influence of heat in modifying capillary force.

In a glass retort, water mixed with small flowers was kept at a boiling temperature over an Argand lamp. The stopper being removed, a capillary tube filled with water was suspended by a brass wire within the retort; also a thermometer with its bulb close to the tube at the level of the top of the capillary column. The lower end of the tube was about half an inch above the suface of the water in the retort. The versed sine of the undeveloped drop on the lower surface of the tube was about one-fifth the external diameter.*

* The height of the column in a tube of small bore is but little affected by the size of the drop at the base. In a tube of $\frac{1}{45}$ of an inch internal diameter, and $\frac{1}{4}$ of an inch external diameter, the maximum height occurs when a drop has broken off, the versed sine of the spherical segment of water attached to the base being then about $\frac{1}{5}$ the external diameter. The following are a few of the observed heights and versed sines :—

| Height of capillary column above base of tube. | Versed sine of volume suspended at base. |
|---|---|
| in. | in. |
| 1·850 | ·025 |
| 1·856 | ·100 |
| 1·838 | ·125 |
| 1·825 | ·150 |
| 1·796 | ·200 (drop about to break) |
| 1·875 | ·050 (drop just broken) |

In this kind of observation the interior of the tube is apt to dry, and the column descends very gradually, much below the true capillary level. By tilting it up and down, it may be kept wet; and good observations may be made in this way, as the lower drop does not sensibly alter.

The temperature at the top of the column was kept at about 95° C., the atmospheric temperature being about 28° C. In a tube with round bore $\frac{1}{45}$ of an inch diameter, the column diminished $\frac{2}{19}$ for a difference of temperature of 66·5 Centigrade degrees. With thermometer-tubes of flat bore the decrement amounted to from $\frac{1}{11}$ to $\frac{1}{12}$, and with plates $\frac{1}{12}$. These ratios were confirmed by another set of observations, made by plunging the tubes into a vessel of hot water and taking the reading at the instant of withdrawal.

By a similar method observations were made on alcohol and on turpentine. Between 30° C. and 79° the column of alcohol descended $\frac{10}{113}$; the sample employed had 85 per cent. pure spirit.

Between 32° C. and 132° the column of turpentine descended $\frac{24}{77}$; between 32° and 165½° it descended $\frac{33}{77}$. The sample was prepared by a chemist, and believed to be pure camphene.

§ 11. M. Simon (de Metz) in his *Recherches sur la Capillarité* has observations of the influence of temperature on the capillarity of water in tubes of from $\frac{1}{50}$ to $\frac{1}{500}$ of an inch diameter: the details of the mode of observation are not stated, but the mean result given is a descent of one-fourth from 0° to 100° C.

The following is quoted from his paper in the *Annales de Chimie*, vol. xxxii., p. 17:—

"Ce rapport 4:3 ou 1·33 que donne l'expérience conduit à penser que la densité de l'eau est la cause qui produit cette différence d'ascension. En effet, un volume d'eau augmentant de 0·0466 pour 100 dégrés de température, il en résulte qu'une colonne d'eau étant 1 à 0 dégré il sera 1·36 à 100 dégrés: rapport très voisin du rapport moyen des expériences ci-dessus.

L'ascension est donc en raison direct de la densité." This conclusion is so manifestly discordant with the premises, that it requires to be noted. In the *Annales de Chimie* for February and March last (vol. xlix.), there is a paper by M. C. Wolf on the influence of temperature on capillarity in tubes of round bore. At p. 259 there is a table of observations taken throughout one year at atmospheric temperatures from 0° C. to 25·4°, with a tube of $\frac{1}{100}$ of an inch diameter, in which every desirable precaution seems to have been taken to ensure accuracy. From 0·35° to 25·45° the descent was observed $\frac{6{\cdot}31}{132{\cdot}15}$, and it is stated that this agrees with the previous observations of M. Brünner (Poggendorff's *Annalen*, 1847). If this rate is continued up to the boiling-point, the descent from 0° to 100° C. would be $\frac{1}{5{\cdot}25}$, the height at 0° and from $26\frac{1}{2}$° to $93\frac{1}{2}$° about $\frac{2}{15}$. In another set of observations with a smaller tube, the effect of temperature was found to be less by about $\frac{1}{10}$. It is probable that the abnormal feature in the capillarity of water in fine tubes affects these results in a greater or less degree. As the true measure of the capillarity of hydrous liquids can only be obtained from observations with parallel plates, it would be desirable to have a set of observations made with water between plates at atmospheric temperatures between summer and winter, in the manner adopted by M. Wolf.

That heat has less effect on the plate-column than on the tube-column, is the impression I have from many observations. When we consider that the effect of adding a very small proportion of alcohol to water is several times greater in tubes than between plates, it appears as if any cause that increased the limpidity of the water had more effect on the capillarity of tubes than of plates.

§ 12. In § 4 it was shown that the weight of 38 grains descending through one inch represents the work required to denude 20 square inches of aqueous surface at the temperature of 86° F., if the value of Q is 132·9. If it is 132, as assumed from the observations in § 2, the weight must be taken at

38·262 grains descending through one inch; and to denude one square inch, it requires 1·9131 grain descending through one inch vertical. This being the weight of ·007577 cubic inch of distilled water at the temperature of 86° F., this volume of water raised through one inch is equivalent to the work of denuding a superficial stratum of one square inch, of overcoming the integral cohesion force *on one side*—the outer side—of a superficial stratum of molecules; being one-sixth of the cohesion integral of all the molecules in the stratum.

A molecule at the surface of a liquid differs from a molecule below the surface, in having that part of its cohesion force which is directed outwards, unmated—disengaged; and the force that operates in disengaging it, is the weight of liquid held in capillary suspension. On applying heat to water to convert it into vapour, we overcome the cohesion force of all the molecules, and the quantity of work which this is equivalent to, we can readily compute from the data afforded by M. Regnault and Mr Joule, assuming the latent heat of steam to be the cohesion integral of all the molecules while in the liquid state. Thus, having the cohesion integral of *one* stratum of a cubic inch, and the cohesion integral of *all* the strata of a cubic inch, we obtain the number of strata in an inch, and hence the absolute volume of an aqueous molecule.

With similar data for other liquids we may compute the absolute volume of their molecules.

Now the quotient of atomic weight by specific gravity of one liquid compared with the similar quotient of another, gives us the *ratio* of the absolute atomic volumes.

We have thus two distinct modes of computing this ratio,—1st, from the absolute volumes; 2nd, from the relative volumes. Do they give the same result?

§ 13. From Regnault's empirical formula, the latent heat of aqueous vapour at 86° F. is 1054. One cubic inch of water thus heated is equivalent to one cubic inch of water at temperature 60°, raised through $1054 \times 772 \times 12$ inches vertical, or 9,764,256 cubic inches raised through one inch.

From the capillary force we have, as above, ·007577 cubic inch raised through one inch as the measure of one-sixth the

cohesion of one layer of molecules, and ·045462 cubic inch raised through one inch as the integral of one layer of the cubic inch. Hence,

$$\frac{9{,}764{,}256}{\cdot 045462} = 214{,}778{,}500$$

is the number of layers of molecules in one cubic inch, and the cube of the reciprocal of this is the absolute volume of one molecule of water at the temperature 86°.

The process expressed by symbols is $m^3 = \left(\frac{6}{QL}\right)^3$, in which L is the product of the latent heat by 12 times 772, Q the quotient of capillarity, and $m^3$ the molecular volume expressed with reference to a cubic inch as unit.

The similar data for alcohol are as follows:—

The latent heat of absolute alcohol at 174° F. is 376, according to MM. Fabre and Silberman (*Ann. de Chim.*, vol. xxxvii., 1853, p. 466). The specific heat is 0·64. The ratio of latent to sensible heat in alcohol is about the same as with water, according to M. Despretz, hence 415° may be taken as the latent heat of alcohol at 86°,

[174° − 86° = 88°, 88° × 0·64 = 56·3°, 56·3° × 0·695 = 39°,
39° + 376 = 415°],

and 415 × 772 × 12 = 3,844,560 cubic inches of alcohol raised through one inch vertical. From Q = 228 we have $\frac{1}{Q}$ = volume raised by a water-line one inch long: $\frac{6}{Q}$ = ·026316 cubic inch raised through one inch as the cohesion integral of one layer of molecules; hence

$$\frac{3{,}844{,}560}{\cdot 026316} = 146{,}092{,}000$$

is the number of layers of molecules of alcohol in one cubic inch, and the cube of the reciprocal of this is the absolute volume of a molecule of alcohol at the temperature 86°.

Comparing this absolute volume with that of water, we have the ratio of molecular volume of water and alcohol equal to 1 : 3·177.

To compute this same ratio from the relative volumes, we have the following data:

| | | |
|---|---|---|
| Specific gravity of | water at 68° F. | 1·000 |
| ,, ,, | water at 86° F. | ·998 |
| ,, ,, | alcohol at 86° F. | ·780 |
| ,, ,, | steam | 9 |
| ,, ,, | alcohol vapour | 23·16 |

The quotient of relative volumes gives 1 to 3·294 as the value of the above ratio.

To find the change in Q that brings the first value of the ratio to an equality with the second, let $r = 3{\cdot}294$, and

$$\delta r = 3{\cdot}294 - 3{\cdot}177 = 0{\cdot}117.$$

It is easy to see by differentiating the equation for $r$, that $\frac{\delta r}{3r} = \frac{\delta Q^*}{Q}$, hence $\frac{0{\cdot}117}{3 \times 3{\cdot}294} = \frac{1}{84} = \frac{\delta Q}{Q}$, and $\delta Q = 2{\cdot}7$ if the error is in the capillary quotient for alcohol only, or $\delta Q = 1{\cdot}5$ if the error is in the quotient for water only. In short, an error of $\frac{1}{84}$ in either of the capillary constants is sufficient to account for the discrepancy. Or if these were perfectly correct, then an error of $\frac{1}{84}$ in either one of the latent heats would make the difference.

§ 14. The data for sulphuric æther are less certain. The specific gravity of the æther of commerce employed in determining the capillary quotient 261 was ·7365 at 86° F., but pure æther is only ·7063 at that temperature. The latent heat of the pure liquid, as determined by MM. Fabre and Silberman, is 163·8° (F.) at its boiling-point. The latent heat of the æther of commerce, according to Dr Ure, is 302°. It is this last, which, combined with the above value of Q, gives the absolute volume of a molecule of æther in the correct ratio to those of water and alcohol. The difference between the numbers for latent heat is certainly remarkable, and marks this liquid as one not adapted to test the theory. We have also to note, that the rapid evaporation of æther must keep the superficial

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* $\left(\frac{ql}{QL}\right)^3 = r$, $\left\{\frac{(q+\delta q)l}{QL}\right\}^3 = r + \delta r$, $r + \delta r = \left(\frac{ql}{QL}\right)^3 + 3\left(\frac{ql}{QL}\right)^2 \frac{\delta ql}{QL}$

$$\frac{1}{3}\delta r = \left(\frac{ql}{QL}\right)^2 \frac{\delta ql}{QL} = \frac{r^{\frac{2}{3}} r^{\frac{1}{3}} \delta q}{q} = \frac{\delta q}{q} r, \quad \frac{1}{3}\frac{\delta r}{r} = \frac{\delta q}{q}.$$

stratum of molecules probably at a much lower temperature than the rest of the liquid; and as it is the surface particles alone that influence the capillary force, the observed value of Q may be much too small.

With reference to turpentine, Dr Ure and MM. Fabre and Silberman agree nearly as to its latent heat; but the molecular volume deduced from its capillarity is not in accordance with that deduced from the specific gravity of the liquid and of the vapour; the latter would require to be twice the observed density.

With reference to acetic æther, it appears that the acetic acid separates from the æther while rising into vapour, as the specific gravity of its vapour is one-half what it would be if a volume contained a volume of each of the constituents. If we assume them to be combined to form a single molecule, the absolute volume assigned to it by capillarity is in the proper ratio. And this agrees also if we consider the liquid merely as a mixture with no greater integral of cohesion between a molecule of æther and of acid than between molecules of the æther or of the acid when separate.

§ 15. The absolute volume of a molecule must augment with the temperature according to the expansion of the liquid. Now the capillary force and the latent heat both diminish as the temperature rises. Thus all the elements of the equation [§ 13] $m^3 = \left(\frac{6}{QL}\right)^3$ are observed to vary with the temperature. Is the equation maintained at all temperatures from the point of liquefaction up to the point of transition when the liquid condition suddenly terminates?

With reference to the point of transition, first observed by M. Cagniard de la Tour in liquids subjected to high temperatures in sealed tubes, I have made a considerable number of observations in graduated tubes filled with the same liquid in different proportions, so as to deduce both the density of the liquid and of the vapour when the liquid state terminates. I found that the cup-shape of the upper surface of the liquid caused by its capillarity, ceased at a temperature considerably under the point of transition. In alcohol the surface (examined closely with a watchmaker's lens) became quite flat at 422° F.,

and the point of transition occurred at 540°, the density of the vapour at that point being 0·222, and of the liquid 0·480, the density at 85° being unity. In sulphuric æther the surface became flat at 385°, and the point of transition at 437° F., when density of vapour was 0·230, and of liquid 0·434.

M. Wolf enclosed a capillary tube in a larger tube with æther, and, raising its temperature, observed the capillary column disappear at 190° C., and the transition of the liquid at 200° C.

The disappearance of the capillary column indicates that the liquid has lost the power of wetting the glass. Liquid cohesion must nevertheless exist up to the point of transition, and ought to give the top of the liquid a convex form; like mercury in glass and in capillary tubes the liquid should descend beyond the outer level.

This was actually remarked by M. Wolf (*Ann. de Chim.*, p. 270); I have also observed the convexity. It quickly assumes a sugar-loaf aspect. It requires a steady temperature to show the surface a distinct line.

M. Wolf further observed, that the *rate* at which the capillary column of æther descended, increased at the higher temperatures (p. 271).

It seems to be almost certain that the rate at which latent heat diminishes also augments with the temperature. The following is the evidence on this point.

M. Regnault's empirical formula for the latent heat of aqueous vapour is $\lambda_w = 606{\cdot}5 - 0{\cdot}695t$ (Centigrade). If the ratio ·695 applies to alcohol, which it does nearly according to M. Despretz, then $\lambda_a = 209 - (t - 78)\ (0{\cdot}695)\ (0{\cdot}64) = 244 - 0{\cdot}45t$, in which 209° is the latent heat of alcohol at the boiling-point, and 0·64 its specific heat, according to MM. Fabre and Silberman's observations (*Ann. de Chim.*, vol. xxxvii., p. 466). If the ratio 0·45 did not increase, transition could not take place until 542° C., instead of 260° or 280° C.

That the specific heat increases with the temperature, is proved by the quotient of the latent heat by the specific heat being about 400° C., showing that the quantity of heat required to boil off a measure of alcohol is sufficient to raise it to 400° C. if its specific heat were constant.

The same remark applies to water; its latent heat alone would carry it far beyond the point of transition. But it is otherwise with æther, if we take MM. Fabre and Silberman's data. The latent heat could only raise it to 220° C., whereas transition takes place at 200°. The difference, even including the probable amount of sensible heat, gives little room for increase in the specific heat, or for acceleration in the diminution of latent heat.

The same remark applies to turpentine, which I found to undergo a chemical change before it reached the point of transition. It is probable that a portion of the latent heat is absorbed in partially overcoming the chemical forces that keep the elements of each compound molecule together. Thus we have nitrous, formic and acetic æthers boiling at low temperatures, and rising with the æther separated from the acid. Oxalic and succinic æthers, on the other hand, boil at high temperatures, and rise unchanged, the molecule of sulphuric æther still clinging to the acid in the state of vapour. Even with regard to such permanent liquids as water, we must be prepared to view as possible a certain absorption of heat in partially separating, not only the molecules, but their constituent chemical elements, because the application of extreme heat alone is capable of effecting their complete separation.

§ 16. The work expended (and its equivalent of heat made latent) in causing the *same* absolute increment of volume, diminishes as the volume increases. The evidence in favour of this is as follows:—At the boiling-point water expands ·0008 in volume for 1° C., and $\frac{7}{10}$ of a degree are rendered latent—are expended in performing the work of expansion; hence if the same rate holds good up to the transition point, when the liquid volume is at least double, the quantity of heat required to effect such expansion would be 875°. But the latent heat is only 537°.

At its boiling-point alcohol expands ·0013 per 1°, and $\frac{7}{10}$ of a degree are rendered latent. At transition-point the volume is doubled. This, at the rate of ·0013 per 0·7°, would require 540 C. degrees of the specific value of the heat of alcohol, or 345 if of the value of the specific heat of water. The latent heat is only 209. Thus it appears that the

increment of volume corresponding to constant decrement of latent heat must increase as the liquid expands.

§ 17. I may here remark, that the curve traced out by the expansion of alcohol from $-32°$ C. (observed by M. Pierre, *Ann. de Chim.*, vol. xv., p. 354) up to the point of transition agrees remarkably well with the hyperbola, so as to admit of its equation being employed as an empirical formula. This is also the case with sulphuric æther, turpentine, sulphuret of carbon, and some other anhydrous liquids, including mercury.

The form of the equation is $(v - b)(a - t) = c^2$; in which $v$ is the volume, $t$ the temperature by air-thermometer reckoned from the zero of gaseous tension, $b$, $a$, and $c^2$ constants that may easily be computed from three observations.

§ 18. There are hardly sufficient data at present fully to test the equation $m^3 = \left(\frac{6}{QL}\right)^3$ [§ 13] for the same liquid at different temperatures; but it may be of advantage to follow the analysis, and trace out what are the relations amongst the data that are required by it.

As the volume of the molecule must be assumed to increase in correspondence with the volume of the liquid, the expansion of the liquid supplies the exact value of the differential of the left side of the equation at any temperature within certain limits.

At a given temperature, and for a constant increment of temperature, we have

$$\frac{\delta m}{m} = \frac{\delta\left(\frac{6}{QL}\right)}{\frac{6}{QL}} = \frac{\delta L}{L} - \frac{\delta Q}{Q}.$$

In § 6 it was shown that $Q = \frac{4}{dh}$; and $d$ being constant, $h$ is observed to diminish with the temperature [§ 10], hence $\frac{\delta Q}{Q} = \frac{-\delta h}{h}$. Also the latent heat has been observed to diminish with the temperature, so that $\delta L$ requires the negative sign; and the differential equation, which expresses the relation of molecular volume, latent heat, and capillarity

of the same liquid while being acted upon by heat, is $\frac{\delta m}{m} = \frac{\delta h}{h} - \frac{\delta \mathrm{L}}{\mathrm{L}}$.

It is obvious from this that the first condition required is that $\frac{\delta h}{h}$ should exceed $\frac{\delta \mathrm{L}}{\mathrm{L}}$, because $\frac{\delta m}{m}$ is positive. The *absolute* value of the difference of the decrements of capillarity and latent heat to fulfil the equation is also indicated.

The most precise observations on the decrement of capillarity are probably those of M. Wolf [§ 11]. The range of temperature was 25° C., and the middle temperature 13°: the value of $\frac{\delta h}{h}$ is ·0477: the value of $\frac{\delta m}{m}$ is ·00089 from M. Despretz's expansion of water, and $\frac{\delta \mathrm{L}}{\mathrm{L}} = ·0293$ from Regnault's empirical formula. Thus we have

$$-·0477 - ·0293 = ·0184 \text{ instead of } ·0009.$$

The first condition, viz. that $\frac{\delta h}{h} > \frac{\delta \mathrm{L}}{\mathrm{L}}$, is satisfied, but the absolute values do not fulfil the equation. We must keep in view that Regnault's formula assumes that the latent heat diminishes uniformly with the temperature. I have not seen the details of the experiments upon which it is founded, but it is unlikely that they can have indicated more than an average result.

At 5° C. the point of maximum density of water $\frac{\delta m}{m} = 0$ and $\frac{\delta \mathrm{L}}{\mathrm{L}} = \frac{\delta h}{h}$. It appears certain from M. Wolf's observations, that the decrement of capillarity is very uniform, and in tubes of not less than $\frac{1}{100}$ of an inch diameter is not sensibly different from the amount put down. Between plates the decrement is probably less, on account of the abnormal feature in the capillarity of water in tubes; but it cannot be less than two-thirds of the above, or ·0318. The evidence of an increase of the decrement of latent heat and of the specific heat at high temperatures has been stated [§ 15]; there is consequently no reason to suppose that the value of $\frac{\delta \mathrm{L}}{\mathrm{L}}$ can be greater than

·0293, unless the peculiarity in the expansion of water affects it in an abnormal way.

Between 28° and 95° C. I observed the capillary column in flat tubes and plates to descend $\frac{1}{12}$, the height at 28° or $\frac{1}{11\frac{1}{2}}$, the height at middle temperature $61\frac{1}{2}°$ [§ 10]; hence $\frac{\delta h}{h} = ·087$. The value of $\frac{\delta L}{L}$ at this temperature is from Regnault's formula ·0785, and $\frac{\delta m}{m}$ from Despretz is ·0117. These give ·087 − ·0785 = ·0085 instead of ·0117. If we assume the decrement of capillarity $\frac{1}{11}$, we should have ·0909 − ·0785 = ·0124, which is very close to the observed value of $\frac{\delta m}{m}$. The value of this $\frac{\delta h}{h}$, computed from M. Wolf's empirical formula, is $\frac{1}{7}$. This formula is derived from observations made in a tube of round bore $\frac{1}{100}$ of an inch in diameter from 0° to 25°.

Dr Young in his essay on the cohesion of liquids, casually mentions that he observed the capillary column descend at the rate of $\frac{1}{1000}$ for each degree of Fahrenheit's scale above 50°. This would give $\frac{\delta h}{h} = \frac{2}{17}$. In a tube of $\frac{1}{45}$ of an inch round bore I found it to be $\frac{2}{19}$ [§ 10]. The equation requires it to be $\frac{2}{21}$.

§ 19. In the cases of alcohol, æther and turpentine, we have no direct information as to the value of $\frac{\delta L}{L}$; but we may compute it for each of these liquids by means of the equation from their differentials of volume and capillarity. Future observation must decide as to their accuracy. Enough for the present if they are such as may be deemed probable.

The data for alcohol (85 per cent. pure spirit) are, from 30° to 79° C. column descends from 113 to 103 or $\frac{\delta h}{h} = \frac{10}{108}$,

$\frac{\delta m}{m} = \frac{184}{9716} = \cdot 0189$. Hence $\frac{\delta L}{L} = \cdot 0737$, and $L = 253°$ (allowing for the 15 per cent. water); hence $\delta L = (253 + \frac{1}{2}\delta L)\,\cdot 0737 = 19\cdot 3°$, and the specific heat of alcohol being ·64, we have $\frac{19\cdot 3}{49 \times 0\cdot 64} = 0\cdot 61$ as the proportion of each degree that goes to diminish the latent heat, while alcohol is heated from 30° to 79° C. The corresponding proportion in water is, according to Regnault, 0·695. This proportion may be called $\delta\lambda$, it being understood that the latent heat is expressed in degrees, which have a mechanical equivalent corresponding with the specific heat of the liquid.

The data for sulphuric æther, are, from 14·79° to 34·78° C. column descends from 35·44 to 32·14 (M. Wolf, *Ann. de Chim.*, vol. xlix., p. 269), hence $\frac{\delta h}{h} = \frac{3\cdot 30}{33\cdot 79} = \cdot 09766$, $\frac{\delta m}{m} = \cdot 01066$ (M. Pierre, *Ann. de Chim.*, vol. xv., p. 361); hence $\frac{\delta L}{L} = 0\cdot 0870$, and $L = 92°$, hence $\delta L = 8\cdot 4°$; and specific heat of æther being 0·504, we have $\frac{8\cdot 4°}{20 \times 0\cdot 504} = 0\cdot 83 = \delta\lambda$.

The data for turpentine are—

1. From 132° to 165½° C. the column descended from 53 to 44 [§ 10], hence $\frac{\delta h}{h} = \frac{9}{48\cdot 5} = 0\cdot 1856$, $\frac{\delta m}{m} = \frac{\cdot 0144}{1\cdot 1288} = \cdot 0127$,

$$\frac{\delta L}{L} = \cdot 1856 - \cdot 0127 = \cdot 1729,\ L = 69°$$

(MM. Fabre and Silberman, *Ann. de Chim.*), and $\delta L = 13\cdot 1°$. The specific heat of turpentine being 0·467, we have

$$\frac{13\cdot 1}{33\cdot 5 \times \cdot 467} = 0\cdot 837 = \delta\lambda.$$

2. From 32° to 132° C. the column descended from 77 to 53 [§ 10]; hence

$$\frac{\delta h}{h} = \frac{24}{65} = 0\cdot 370,\ \frac{\delta m}{m} = \frac{\cdot 0358}{1\cdot 060} = \cdot 0337, \text{ and } \frac{\delta L}{L} = \cdot 336;$$

$L = 69 + 13 = 82°$, and $\delta L = 33\cdot 1$. Specific heat (assumed

same as last, though probably it is actually less) 0·467, we have

$$\frac{33{\cdot}1}{100 \times {\cdot}467} = {\cdot}709 = \delta\lambda.$$

§ 20. The relation between capillarity and latent heat—assuming it to be proven—enables us to compute the latent heat from the capillarity, and *vice versa*, *e.g.*, mercury. M. Gay-Lussac determined the constant product of the capillarity of this metal to be $\cdot 0201 = \frac{4}{Q}$ [§ 6], hence $Q = 200$. From the general equation $m^3\left(\frac{6}{QL}\right)^3$ [§ 13], we have $m^3Q^3L^3 = m_1{}^3Q_1{}^3L_1{}^3$ * and

$$L_1 = \frac{mQL}{m_1Q_1} = \sqrt[3]{\frac{9}{7{\cdot}44}} \times \frac{132 \times 1072}{200} = 754^\circ \text{ [Fahrenheit's scale].}$$

This heat applied to the metal would raise it 25133°.

The weight of a molecule of mercury being $11\frac{8}{9}$ times that of water, the cohesion integral of a molecule is $\frac{8373}{1072}$ times that of a molecule of water, while its bulk is less in the proportion of 7·44 to 9.

* The product $m$QL has the same constant value for all liquids at any temperature.

Its numerical value $\begin{cases}\text{for Fahrenheit's scale is } 294330\\ \text{for centesimal scale } 163516\end{cases}$, and $m^3$ is expressed as quotient of atomic weight (on hydrogen scale) by specific gravity, L is latent heat expressed in degrees, and Q, as described in §§ 2 and 6, has reference to the inch as unit.

22 London Street,
EDINBURGH, *November* 2, 1857.

# ON THE EVIDENCE OF A GRADUATED DIFFERENCE BETWEEN THE THERMOMETERS OF AIR AND OF MERCURY BELOW 100° C.

*Derived from* M. REGNAULT'S *Observations on the Tension of Aqueous Vapour*

(From the *Philosophical Magazine* for March 1858.)

The comparison between the thermometers of air and of mercury made by M. Regnault, and of which an account is given in the *Ann. de Chim.*, vol. v., 3rd series, may seem, from the eminent character of the observer, to justify the withdrawal of our confidence from the results of MM. Dulong and Petit's previous inquiry on the same subject (*Ann. de Chim.*, 1817): nevertheless, before yielding to this conclusion, there is certain evidence in favour of the accuracy of MM. Dulong and Petit's deductions, which appears to me to merit attention, inasmuch as it is derived from the observations of M. Regnault himself in another field of research. Hoping that you may consider the subject of sufficient importance, I have drawn up the following paper for the *Philosophical Magazine*, giving an account of this evidence, and an explanation of the processes of computation by means of which it was obtained. The graphical projection into which the results are cast (fig. 2) enables the proofs of the graduated difference in question to appeal to the eye with but little demand on the attention of the reader. Nevertheless, all the details necessary to enable him fully to check those results will, I trust, be found set forth distinctly and without reserve or bias.

§ 1. In M. Regnault's *Études sur l'hygrométrie* (*Ann. de Chim.*, vol. xv.) we are presented with observations on the tensions of the vapour given by six mixtures of sulphuric acid and water in different proportions, at temperatures ranging between 4° and 52°. Each of these observations I subjected to the following process of computation. Adding 274 to the temperature, I divided the corresponding tension by the sums and extracted the sixth root of the quotient. This was laid off by convenient scale as ordinate to the temperature. The points thus obtained ranged distinctly in straight lines; each mixture presenting a gradient of density. These gradients were found all to converge to one point, which was situated on the base-line upon which the temperatures were marked off, and from which the ordinates were measured (*i.e.* the axis of temperature). The position of this central point corresponded to the temperature −77°.

M. Regnault's observations on the tension of the vapour of pure water below 100° being treated in the same way, presented also a straight line directed to the same point. (See Appendix I.)

The same system of projection was applied to observations on the vapours of other liquids collected from scientific journals and publications. In all cases they seemed to range in straight lines, some of which were parallel to others, and some converged towards a point in the axis of temperature, *e.g.* the gradients of sulphuric ether, cyanogen, and ammonia vapour, show parallelism to that of water. Alcohol radiates from the same point on the axis as water.

§ 2. The chart containing the observations thus projected was submitted to the Royal Society in 1852. It will not be necessary to reproduce that chart here, as I shall show independently how exactly M. Regnault's observations on steam conform to the straight line; and the further proof of what has been stated with respect to the vapours of the mixtures of sulphuric acid and water, may be obtained by inspecting the Table of comparative tensions given by M. Regnault at pp. 179, 180, 181, vol. xv., *Ann. de Chim.* The convergence of gradients of density obviously implies a constancy in the ratios of tension at the same temperature (see Appendix II.), and in

the above-mentioned Table we have columns in which this ratio has been computed for each degree.

Beginning with the last mixture, in which there are 18 equivalents of water, the ratio, it will be remarked, varies irregularly between ·828 and ·841, and the greatest divergence from the mean value is equal to an error of $\frac{1}{500}$ of an inch of the column of mercury. In the next, the ratio varies between ·677 and ·701 and the implied maximum error, about $\frac{1}{400}$ of an inch. The same slight divergence characterises them all; and although the first and second show a considerable diminution of the ratio at the higher temperature, yet the observed tensions are so small, that an error of not more than $\frac{1}{700}$ of an inch of mercury would account for the difference.

§ 3. The chart above referred to as being in the archives of the Royal Society, contains observations on the density of the vapour of alcohol, sulphuric æther, and sulphuret of carbon, carried up to about 200°. They were made in sealed graduated tubes, so that the results should be uninfluenced by a deviation from Mariotte's law, which must in some degree affect all densities derived from measurements of tension. In projecting these, the temperatures were reduced to the standard of the air-thermometer by a formula derived from MM. Dulong and Petit's observations on the expansion of mercury and glass from 0° to 300°. (See Appendix III.) These represent mercury in glass to be 3° in advance of the air-thermometer at 200°, and I felt certain that with such corrections the points conformed more nearly to a straight line than if they were uncorrected. M. Regnault does not admit of any correction being required up to 200°, and further, that at about 50° the air-thermometer is behind the mercury.

If the expansibility of mercury increases with the temperature, it is plain that it must be behind the air-thermometer at temperatures between 100° and 0°, unless there is compensation from glass diminishing in its expansibility to the same amount as mercury increases in its rate of expansion. But M. Regnault and M. Pierre both agree with MM. Dulong and Petit, as to glass experiencing an augmentation in its coefficient of dilatation. Hence, if there is continuity in the law of expansion of mercury, the conclusion is inevitable that the mercury is behind

the air between the fixed points of the scale. The amount computed by the formula is 0·512 at 50°, 0·481 at 60°, 0·413 at 70°, 0·31 at 80°, and 0·171 at 90°.

§ 4. Let us first proceed on the hypothesis that the thermometer employed by M. Regnault in his researches on the tension of aqueous vapour from 100° to 50°, corresponded with the formula, and was actually behind a perfect air-thermometer exactly the amount specified at the respective temperatures. Let us also, for the present, assume that the gradient of density with the temperatures thus corrected is exactly a straight line. The projection of M. Regnault's observations with the temperatures *uncorrected* will not range exactly in a straight line, but in an arch concave towards the axis of temperature. Thus, *e.g.* 50° by the mercury indicates 50·51° by the air: we have therefore, by disregarding the correction, applied to abscissa 50° the ordinate of density that belongs to 50·51°; to abscissa 60° the ordinate of density that belongs to 60·47°; to abscissa 70° the ordinate that belongs to 70·42°, etc.

Suppose the scales of projection on the chart to be so arranged that the line that approximately represents M. Regnault's observations is inclined at an angle of 45° to the axis of temperature, the distance of the points above the straight line measured in the vertical is equal to 0·51 at 50°, to 0·48 at 60°, to 0·42 at 70°, etc. It is hardly possible to recognise this deflection on the chart, however large the scale employed, but it may be made quite apparent to the eye by projecting the points orthographically with the distances extremely foreshortened in the direction of the gradient.

The points computed from M. Regnault's observations with temperatures uncorrected being laid down as before described on a plane surface; suppose we look along the line of these points with the eye close to the plane on which they are marked, we shall see inequalities of position under a highly exaggerated aspect, in consequence of the extreme foreshortening of the longitudinal intervals between them. Suppose the same observations with M. Regnault's temperatures corrected by the formula are also projected in this way; we obtain the means of effectively comparing their positions, and thus of testing the value of the assumptions made with

respect to the correction of the thermometer and the gradient of density.

§ 5. Let S (fig. 1) be the standard point on the chart corresponding to the density of steam at 100°, and $m$ the point corresponding to density at 50°. Join S$m$ (in the figure, to save confusion, the lines joining the several points and S are not drawn), and draw $m$G parallel to the axis, and SG perpendicular to it. In $m$G, take $m$a equal to the assumed cor-

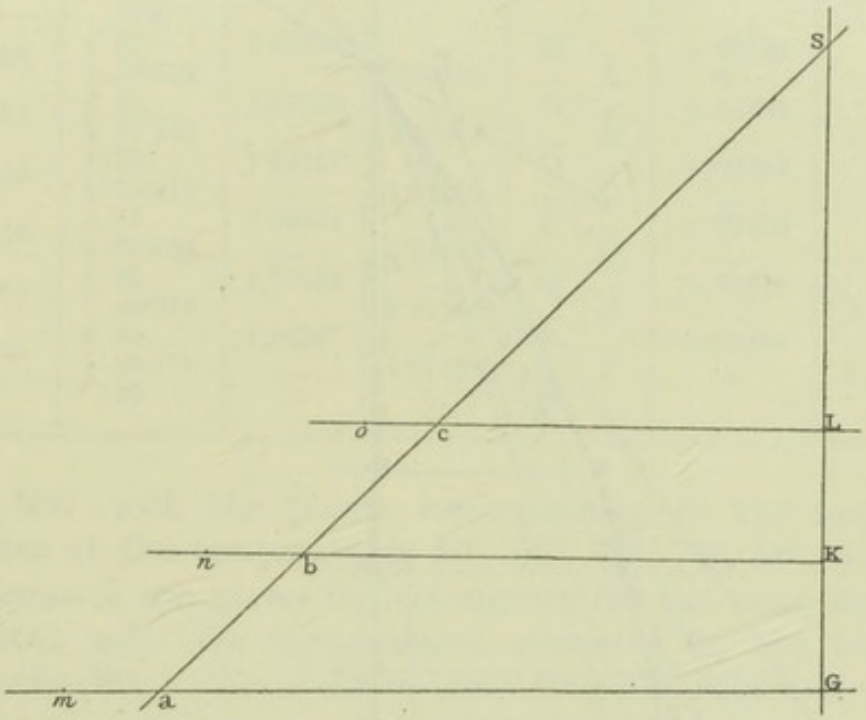



Fig. 1.

rection of the mercury thermometer at 50°, viz. 0·512°. We have $\frac{m\text{G}}{\text{SG}}$ = cotangent inclination of $m$S to axis; $\frac{\text{aG}}{\text{SG}}$ = cotangent inclination of aS to axis; and $m$a = difference of cotangents × SG. In the same way we have $n$b = difference of cotangents of inclination of $n$S and bS to axis × SK = the assumed correction of the mercury thermometer at 60°, and so on for the other points.

Let the orthographical projection be made in the direction Sa inclined to the plane of the paper about 1°. The effect will be to reduce distances in the direction aS to one-sixtieth, and maintain them in the same relative proportion: thus SB to SA

(fig. 2) is equal to bK to aG (fig. 1) = (100° − 60·481°) to (100° − 50·512°). The differences *m*a, *n*b, etc. (fig. 1) are

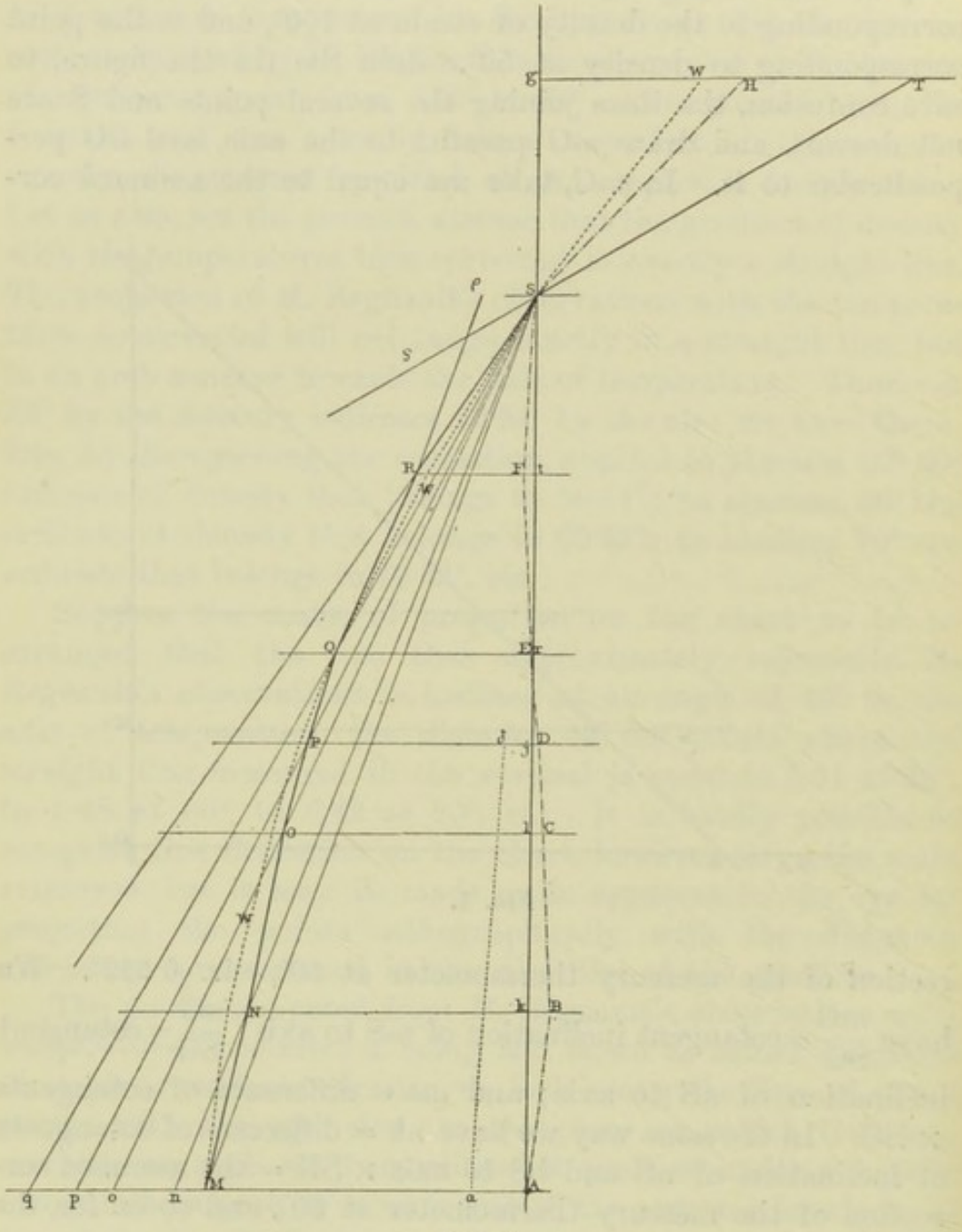



FIG. 2.

reduced in the ratio of $\sqrt{\frac{1}{2}}$, the scales of the chart being supposed to be adjusted so as to make SaG = 45°. Also *m*a (fig. 1) becomes MA (fig. 2), nearly perpendicular to AS; the

abscissa of M from S upon SA being 50, while SA = 48·488 and the ordinates are the difference of cotangents.

The following Table contains in column 3 the cotangents of the inclination of the straight lines that connect the standard

| Tensions. | Tempera-tures. | Cotangents. | | Points on Fig. 2. | Difference of Cotangents. | |
|---|---|---|---|---|---|---|
| 1. | 2. | 3. | 4. | 5. | 6. | 7. |
| mm. | Deg. | | | | | |
| 91·98 | 50 | 1·58854 | ... | M ... | + ·01733 | |
| | 50·512 | ... | 1·57121 | ... A | ·0 | ·0 |
| 148·79 | 60 | 1·59025 | ... | N ... | + ·01904 | |
| | 60·481 | ... | 1·56983 | ... B | ... | − ·00138 |
| 233·09 | 70 | 1·59357 | ... | O ... | + ·02236 | |
| | 70·413 | ... | 1·57006 | ... C | ... | − ·00115 |
| 288·50 | 75 | 1·59571 | ... | P ... | + ·02450 | |
| | 75·366 | ... | 1·57053 | ... D | ... | − ·00068 |
| 354·64 | 80 | 1·59816 | ... | Q ... | + ·02695 | |
| | 80·310 | ... | 1·57155 | ... E | ... | + ·00034 |
| 525·45 | 90 | 1·60387 | ... | R ... | + ·03266 | |
| | 90·171 | ... | 1·57428 | ... F | ... | + ·00307 |
| 760 | 100 | ... | ... | S | | |

point 100° with the points corresponding to the respective densities at the temperatures 50°, 60°, 70°, 75°, 80°, and 90°. In column 4 are given the cotangents for the same densities computed with the temperatures corrected by the formula. (Appendix III.) The mode of computing the values of ($h$) the cotangents is explained in Appendix III. The equation is

$$h = \frac{t_1 - t}{\sqrt[6]{\frac{e_1}{t_1}} - \sqrt[6]{\frac{e}{t}}}$$

in which $t$ is the temperature reckoned from the zero of gaseous tension (found by adding 273·89° to the temperature by thermometer), $e$ the tension in millimetres. The tabular numbers are the numbers resulting from computing the above formula divided by 100. The letters refer to fig. 2, and the columns (6, 7) of differences show the differences between the cotangent at A, and the others respectively.

The mode of drawing fig. 2 will now be apparent. MA, NB, OC, etc., are parallel; the distance between AM and

NB = (60·481 − 50·512); the distance between AM and CO = (70·413 − 50·512), etc., upon one scale, and by another scale the cotangent differences are laid off on AM: those with positive signs on one side of A towards M; those with negative signs on the other side of A towards L. Lines are then drawn to S, and the points of the projection are found at the intersection of these lines with their respective parallels. Thus,

AM = ·01733 by scale 1 inch to ·01 = 1·733 inch.
An = ·01904 ,, ,, = 1·904 ,,
etc.

Sn intersects NB in NS which is the point corresponding to M. Regnault's observations at 60°, and So intersects CO in O, which is the point of the same for 70°, etc.

§ 6. Thus M N O P Q R S represent M. Regnault's observations with temperatures uncorrected, and A B C D E F S the same with temperatures corrected by the formula. If this latter coincided with the straight line AS, we should have the desired proof at once of the thermometric formula, of the law of density, and of the extreme accuracy of M. Regnault's observations.

With reference to the slight apparent deviation, we must keep in view that the tensions employed are, strictly speaking, not those actually observed, but those given in the Table at the end of M. Regnault's memoir computed from the empirical formula $\log e = a + ba^t - c\beta^t$, the constants of which were determined from the graphically equalized observations at 0°, 25°, 50°, 75°, and 100°. There is abundant evidence in the memoir of the remarkable efficiency of this formula to represent the observations within their limits of error. It does not profess to represent the true line, but it must coincide with it at the five points, and at intermediate temperatures cannot be more than a small fraction of a degree removed from it.

The line A B C D E F S thus represents the formula of interpolation, and its relation to the straight line A S, it will be remarked, is precisely what might be expected to subsist between an empirical and a true curve of tension. It intersects, and *intersection*, not *contact*, is the character of such formulæ, as proved by the differences between the

computed and observed quantities being alternately of different signs.

If we suppose the point B shifted to $k$, C to $l$, etc., and the points N, O, P shifted the same respective amounts, these points, N, O, P, etc., trace out the curved line M $w$ $w$ $w$, which conforms nearly to a circular arc. There is little doubt that this curve represents the true equalized densities observed by M. Regnault, while M, N, O, etc., represents the densities by the empirical formula. We have thus a semi-arch and semi-chord defining the graduated difference between the air- and mercury-thermometers below 100°.

§ 7. The only evidence of real discrepancy is to be found at D, which ought to coincide with $j$. If we take $\mathrm{A}a = \frac{1}{11}$ AM and $\mathrm{D}\delta = \frac{1}{11}$ DP, then $a\delta$S is a straight line. Thus the difference $j$D, which does not amount to more than $\frac{1}{100}$ of a degree, may indicate that the formula gives the correction $\frac{1}{110}$ in excess throughout. Otherwise it may be caused by it not perfectly representing the curve of expansion of mercury in glass between 50° and 100°. Otherwise M. Regnault's graphical curve of equalized observations may thus far be removed from perfect accuracy, or it may be caused by the deviation from Mariotte's law affecting the air-thermometer. (See § 18.)

That so small an adjustment should bring M. Regnault's observations into *perfect* harmony both with the law of density and with the thermometric formula constructed upon MM. Dulong and Petit's data, cannot be viewed as otherwise than remarkable. Can such a coincidence be accidental? Is it illusory?

§ 8. The coefficient of the expansion of mercury in glass must change with the quality of the glass. M. Pierre found the coefficient of glass between 0° and 100° to vary from ·0026 to ·0019,* which is from about $\frac{1}{6}$ to $\frac{1}{10}$ the coefficient of mercury; hence the absolute expansion of mercury in glass may vary to the extent of $\frac{1}{22}$ part.

How this may influence the comparison of thermometers, we may perhaps most easily understand by means of a diagram

* *Ann. de Chim.*, vol. xv., p. 335.

such as fig. 3. Let A D represent the curve of expansion of mercury in glass with the greater coefficient, and A C the same with the lesser coefficient. Let $DC = \frac{1}{22} DB$, and $AB = 100°$. If the arch A C were flatter than the arch A D in the proportion of B C to B D, there could be no difference between the thermometers, and the formula of correction for one would apply to the other. The subtense of the arch of expansion represents the uniform rate of the air-thermometer, and its extension beyond 100° is situated below the curve of expansion, showing the air-thermometer to be behind the mercury. If the *absolute* amount of this difference in the two thermometers

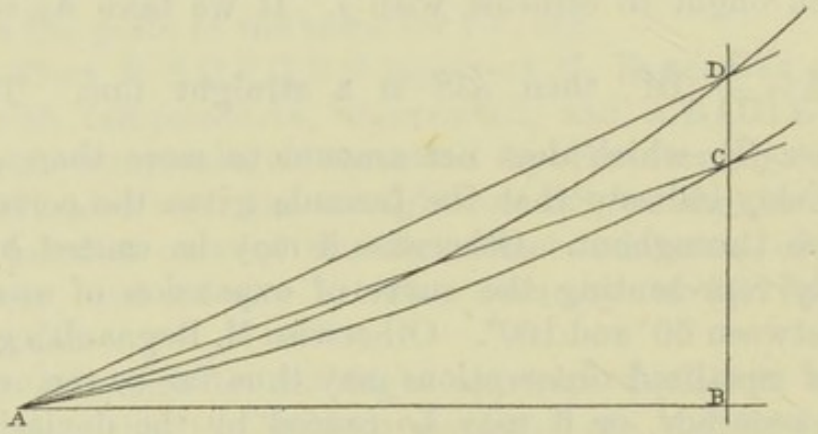



Fig. 3.

continued to have the same ratio C B to C D above 100° as below, the *relative* amount would be the same, and thermometers of different kinds of glass would keep pace with each other.

§ 9. The evidence on this point is rather conflicting. M. Regnault states (*Ann. de Chim.*, vol. v., p. 102) that the different kinds of glass not only have coefficients of dilatation absolutely different, but they all follow different laws in the dilatation. The glasses which have the *smallest* coefficient appear to experience a *less* rapid augmentation of dilatation with the temperature. This is precisely what is required to admit of thermometers constructed of different kinds of glass keeping pace with each other. On the other hand, M. Pierre states (*Ann. de Chim.*, vol. v., p. 438) that the thermometer

with crystal reservoir is constantly in advance upon the thermometer with common glass reservoir, and the difference increasing generally with the temperature, may extend to several degrees while the two are being heated to 250° or 300°. Another adverse statement advanced by M. Pierre, is that exactly the same coefficient of dilatation for the same glass apparatus is not always found, although the circumstances are apparently identical. If such uncertainty is the normal state of thermometric measurement, how are we to account for the results shown in fig. 2? In the face of them, it seems impossible to accept the comparative thermometric observations of MM. Pierre and Regnault as conclusive.

§ 10. Besides the difference in the march of the air- and mercury-thermometers, there must be a difference in the value of the individual degrees marked on the two thermometers. Employing the formula [Appendix III.] to obtain an approximate value of the mercury degrees in terms of the degrees of the air-thermometer, we find that at 0° the value of a degree, or $\frac{1}{100}$ of the distance between the freezing and boiling temperatures, is $\frac{24}{23}$ the value at 100°. At the lower part of the scale, the value exceeds the mean by $\frac{1}{48}$ part, and at the higher point it is $\frac{1}{48}$ part less than the mean. This inequality may at times have a sensible effect on the quantitative determinations of physicists. Thus the mechanical equivalent of heat is, according to Mr Joule, 772 for 1° F., at temperature about 60° F. If this were reduced to the standard value of a degree of the air-thermometer, we should have to reduce it by $\frac{1}{60}$ part, viz. to 759.

§ 11. There is another point referred to by M. Regnault as a source of uncertainty in measurements by the air-thermometer. The deviation from the primary laws of elastic fluids which he discovered must have some effect in disturbing that ratio of equality between the increments of volume and temperature, or of pressure and temperature, upon which the value of the instrument depends.

The air-thermometer may indicate temperature by change of volume under constant pressure, or by change of pressure with volume constant. M. Regnault's observations make the proportionate change of volume slightly larger than the change

of pressure between the standard points of the thermometer; the difference being perhaps within the limits of error.

With increased densities these proportionate changes sensibly augment at the rate of about $\frac{1}{261}$ per atmosphere, and Messrs Joule and Thomson's experiments with plugs, indicate a deviation from the law of Mariotte of about $\frac{1}{272}$ per atmosphere at ordinary temperatures, and a probability that this amount increases in descending the scale of temperature. [See paper on "Deviation from the Primary Laws of Elastic Fluids," *Phil.*

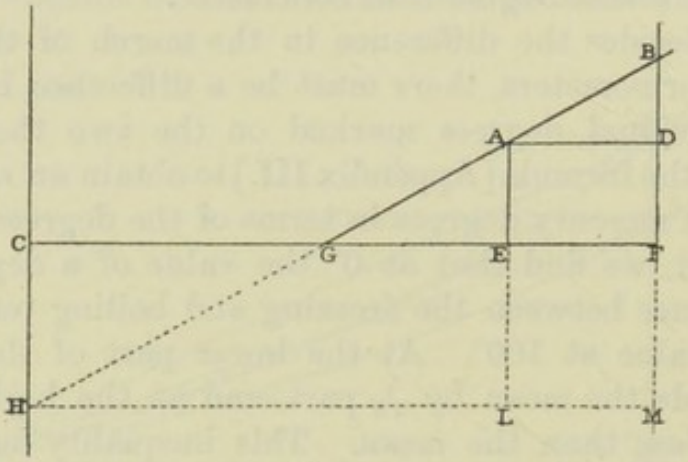



Fig. 4.

*Mag.*, Oct. 1857.] The deviation consists in the elastic force being *less* than the inverse of the density by $\frac{1}{272}$ per atmosphere at ordinary temperatures. How the measurement of temperature is likely to be affected by these, we may perhaps appreciate most distinctly by means of a diagram (fig. 5).

§ 12. Let C Q represent the interval of temperature between the freezing and boiling of water, C D and Q P the corresponding volumes of air under constant pressure at those temperatures. Join P D, and extend it to meet C Q produced in A. We have (P Q – C D) : C D : : C Q : C A, or according to Regnault, 0·36706 : 1·00000 : : 100° : 272·44°.

Suppose the temperature to descend from C to B, the corresponding volume would be B F if the primary laws of gases were entire. But the deviation, although as yet not

perfectly known in a case such as this, points to a lesser volume B E as that which the thermometer would actually exhibit under the influence of the augmented density and diminished temperature, both of which unite in affecting the relation between tension and density.

§ 13. The locus of the point E is unknown, but it must be curvilinear, as both D and P are to be found in it; and its curvature is convex upwards, showing that between any two points D and P the air-thermometer is in advance of true temperature. As the amount of this difference depends on

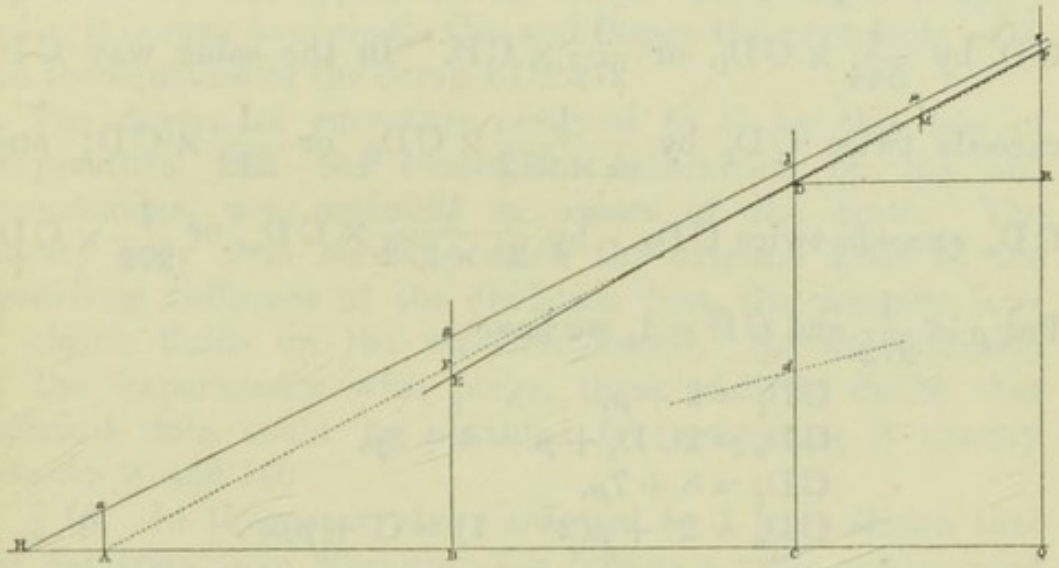


Fig. 5.

the degree of curvature, it may be worth while to inquire as to its probable maximum between 0° and 100°. Certainty is of course not to be expected, but I think it can be shown that the complete solution of the question is possible by employing the mode of experiment with plugs, suggested by Professor Thomson, and carried out to a certain extent by that gentleman and Mr Joule under the auspices of the Royal Society.

§ 14. Suppose the air that occupies the volume D C is compressed into half the space ($d$ C); we know, if it passed through a plug and were allowed to expand from the greater to the lesser density without performing work, that a thermal depression will be exhibited which indicates that the pressure

at the double density is *less* than two atmospheres by about $\frac{1}{272}$. (See paper in *Philosophical Magazine* above referred to.) If $d\,C$ represents the volume at two atmospheres, we should have the point $d$ situated $\frac{1}{272} \times \frac{D\,C}{2}$ below the middle point between D and C. We have now to suppose the air represented by D C to pass through a plug into air of half an atmosphere tension, *i.e.* to have its volume changed to $C\,D_1$. The absolute amount of the deviation is $\frac{1}{2} \times \frac{1}{272}$, that is, $C\,D_1$ exceeds twice C D by $\frac{1}{544} \times C\,D_1$, or $\frac{1}{272} \times C\,D$. In the same way $C\,D_2$ exceeds twice $C\,D_1$ by $\frac{1}{4 \times 272} \times C\,D_2$, or $\frac{1}{272} \times C\,D$; and $C\,D_n$ exceeds twice $C\,D_{n-1}$ by $\frac{1}{2^n \times 272} \times C\,D_n$, or $\frac{1}{272} \times C\,D$.

Put $\rho = \frac{1}{272}$ and $C\,D = 1$, we have

$$C\,D_1 = 2 + \rho,$$
$$C\,D_2 = 2C\,D_1 + \rho = 4 + 3\rho,$$
$$C\,D_3 = 8 + 7\rho,$$
$$C\,D_n = 2^n + \rho(2^n - 1) = (1 + \rho)2^n,$$

when $n$ is indefinitely great. If we now return from $C\,D_n$ with a gas that strictly conforms to the law of Mariotte, and suppose the volume to be diminished until the tension of one atmosphere is regained, it is evident that at this point the volume, instead of being $C\,D = 1$, is $C\delta = (1 + \rho) = 1 + \frac{1}{272}$; and generally for any pressure $m$ atmospheres at this temperature, the ratio of *non-deviation* to *deviation* volumes is $\frac{1 + m\rho}{1}$, and the *absolute* value of the difference of these volumes in terms of volume unity at one atmosphere is $\frac{1}{272}$, a constant absolute magnitude.*

* This result is dependent on the assumption that the thermal effect is, at constant temperature, proportional to the difference of pressures.

§ 15. Thus far, while confining our view to one temperature, we require to make no assumption in advance of the results strictly derived from Messrs Thomson and Joule's experiments, but the influence of a change of temperature has not as yet been distinctly made out.

In passing carbonic acid through plugs at temperatures up to nearly 100°, the thermal effect seemed to vary in the inverse ratio of the square of (A Q) the G temperature. (*Phil. Trans.*, 1854, p. 336.)*

If this is the case with one gas, it is probably a general law applicable at least approximately to all. Assuming it to apply to air, it is easy to compute $Q\pi$, and thence the asymptote $\pi\delta H$, and the equation of the curve EDP.

The degree of curvature assigned to it by this law of temperature, and the consequent aberration of the air-thermometer, are probably in excess of the truth. The inquiry may thus serve to mark the extreme limit of the disturbing influence of the deviation from the primary laws of elastic fluids on the air-thermometer. By an extension of the experiments with plugs, there is little doubt that sufficient data could be obtained for computing it exactly between 0° and 100°.

§ 16. In the paper above referred to, I have shown that the deviation from Mariotte's law follows a ratio compounded of the thermal effect directly, and the G temperature inversely; hence the thermal effect being assumed to vary inversely as the square of the G temperature, the deviation must follow the inverse cube of the G temperature thus,

$$\frac{\delta D}{CD} : \frac{\pi P}{PQ} = \frac{1}{CD^3} : \frac{1}{PQ^3},$$

or

$$\delta D : \pi P = \frac{1}{CD^2} : \frac{1}{PQ^2} = \frac{1}{C\delta^2} : \frac{1}{\pi Q^2} = \frac{1}{CH^2} : \frac{1}{QH^2} \text{ nearly.}$$

* This law is indicated by Prof. Thomson's equation at p. 337, *Phil. Trans.*, 1854, which is constructed on a formula of Mr Rankine. *This formula is palpably erroneous, nevertheless Prof. Thomson's computed results agree exactly with experiment!* See paper on Deviation referred to in § 11.

We thus obtain the position of $\pi$. Next, by joining $\pi$ and $\delta$ and producing to meet the axis of temperature in H, we determine the *non-deviation* zero of gaseous tension, which thus computed is 1·80° below A, the *deviation* zero derived from the expansion of air between 0° and 100°.

We thus fix the ratio HC to AC, and the equation of the curve is found by assuming E$\beta$ to follow the ratio of the temperature given above, viz. $\left(\frac{HC}{HB}\right)^2$. The equation for the locus of E, the curve EDP, thus determined is

$$(E\beta) = x = \delta D \cdot \frac{HC^2}{y^2} = \frac{1}{272} \cdot \frac{HC^2}{y^2},$$

in which $y$ is the temperature reckoned from the non-deviation zero H, and $x$ the portion of the ordinate to $y$ intercepted between the asymptote H$\pi$ and the curve.

§ 17. We have now obtained the means of computing the versed sine of arch DP, and thence the correction required upon the air-thermometer at 50°.

| | |
|---|---|
| The value of M$\mu$ thus computed is . | ·002594 |
| The mean between D$\delta$ and P$\pi$ is . . | ·002821 |
| The difference is . . . . . | ·000227 |

which is the versed sine at 50°. Comparing this with ·36706, which represents 100°, we deduce 0·0618°, or about $\frac{1}{16}$ as the fraction of a degree which the air-thermometer is in *advance* of true temperature at 50°.

At 200°, by a similar computation, it is $\frac{1}{4}$ of a degree *behind* the true temperature.

§ 18. It will be remarked that the differences are of contrary signs to the differences between the mercury- and air-thermometers, and that between 0° and 200°; a reduction of $\frac{1}{8}$ in the correction of the mercury-thermometers derived from the formula [Appendix III.] would compensate almost exactly for this slight aberration from uniform expansion.

## Appendix.—I.

This expressed by symbols is $\sqrt{\frac{e}{t}}$ [= A E, fig. 4], in which $e$ is the tension and $t$ the temperature [= C E], by air-thermometer reckoned from [C] the zero of gaseous tension.

The ranging of the points in a straight line or gradient is shown by the quotient of the difference of ordinates by difference of abscissæ being constant: thus

$$\left[\frac{\text{CF} - \text{CE}}{\text{BF} - \text{AE}}\right] = \frac{t_1 - t}{\sqrt[6]{\frac{e_1}{t_1}} - \sqrt[6]{\frac{e}{t}}} = h = \text{a constant ratio} = \text{cotangent}$$

of the inclination of the gradient to the axis of temperature $\left[= \frac{\text{AD}}{\text{BD}}, \text{ fig. 4}\right]$.

The value of $t_0$ when $e = 0$ is thus $t_0 = t_1 - h\sqrt[6]{\frac{e_1}{t_1}} = g =$ the temperature of the zero of tension of aqueous vapour [ = C G].

These constants, $h$ and $g$, may thus be obtained from two observations of tension, and the general equation for the tension in terms of the temperature is

$$e = \left\{\frac{t - g}{h}\right\}^6 t.$$

Assuming A S (fig. 2) to be the true gradient of steam, we have

$$\left.\begin{aligned} g &= 197{\cdot}05^\circ \\ \log h &= 2{\cdot}1962446 \end{aligned}\right\} \text{ French measures,}$$

$$\left.\begin{aligned} g &= 354{\cdot}7^\circ \\ \log h &= 2{\cdot}7281741 \end{aligned}\right\} \text{ English measures.}$$

This gradient coincides nearly with the observations of Dr Ure and the American Institute at high pressures, but it is too high for those of the French Academy; also for those of M. Regnault above 100°. These last were taken by a process different from that employed for the observations below 100°. On examining them minutely I find that they are not continuous; there is a dislocation at 100°, which can only be accounted for by some fault in the observations. Take the tension at 111·74°, as given by M. Regnault, and compute

its projection on fig. 2. The cotangent is 1·65406 (see Table, *ante*), and the difference ·08285. This gives T as the position of this point (fig. 2), and P Q R S T as the line of M. Regnault's observations. It will be remarked that the law of continuity is manifestly broken at S, the critical point at which the *mode* of observation underwent a change. Compare R S T with Q R S, or F S H with E F S; also the angle $\rho$ R S with $\varsigma$ S R.

## II.

When the gradients radiate from the same point, it is evident that in the general equation $e = \left\{\frac{t-g}{h}\right\}^6 t$, the value of $g$ is the same for all. Hence comparing the densities of two vapours thus related, it is obvious that at the same temperature they must vary inversely as the sixth power of $h$: also the tensions must be in this proportion. Thus between pure alcohol and aqueous vapour the ratio at the same temperature will be found to be 2·3192.

When two gradients are parallel, the value of $g$ differs by a constant amount, and $h$ is the same for both: hence on comparing the densities of two vapours thus related, we find equal densities not at the same temperatures, but at the same constant interval of temperature $(g - g')$. As an example, the vapour of sulphuric æther taken at 69·55° below the temperature at which the density of the vapour of water may happen to have been observed, will be found at all parts of the scale to have $4\frac{1}{8}$ times its specific gravity, being the relative proportion of their chemical equivalents. This applies to Dalton's observations only.

## III.

This formula is

$$t_m = \frac{\mathrm{B}t_a}{\mathrm{A} - t_a} - \left(\frac{t_a}{\mathrm{D}} + \frac{t_a^3}{\mathrm{C}^3}\right),$$

in which

$t_a$ is temperature by air-thermometer,
$t_m$ temperature by mercury-thermometer,
log B = 3·7145723 log D = 0·78587
log A = 4539·617° log $\mathrm{C}^3$ = 6·43303.

The data upon which it is constructed are taken from the original memoir of MM. Dulong and Petit, "On the Expansion of Mercury and Standard of Temperature" (*Ann. de Chim.*, vol. vii., 1817, p. 136). They are as follows:—

Between 0° and 100° the rate of expansion of mercury is $\frac{1}{5550}$.

Between 0° and 200° (by air-thermometer) the rate is . $\frac{1}{5425}$.

Between 0° and 300° (by air-thermometer) the rate is . $\frac{1}{5300}$.

The denominators of these coefficients being in arithmetical progression, indicates that the curve of expansion approximates closely to a hyperbola, with the axes of co-ordinates parallel to the asymptotes. We find, accordingly, by drawing that curve through the co-ordinate points at 0°, 100°, and 200°, that it will be found also to pass within $\frac{1}{500}$ of a degree of the point at 300°.

The data as to the expansion of mercury in glass are as follows:—

Between 0° and 100° the rate of expansion is $\frac{1}{6480}$.

Between 0° and 200° ,, ,, $\frac{1}{6378}$.

Between 0° and 300° ,, ,, $\frac{1}{6318}$.

From these we obtain the following volumes of glass:—

| | |
|---|---|
| 0° . . . . | 1·000000 |
| 100° . . . . | 1·002586 |
| 200° . . . . | 1·005508 |
| 300° . . . . | 1·009120 |

By considering the volume to increase, partly as the temperature and partly as the cube of the temperature, we may represent the curve of expansion exactly by an equation of the form

$$\frac{t}{D} - \frac{t^3}{C^3} = v - 1.$$

By combining this with the equation for the hyperbola in which mercury expands, the above formula for $t_m$ in terms of $t_a$ was constructed.

The original equation for the hyperbola is

$$\overline{a-t}\,\overline{v-b} = c^2,$$

in which the constants $a$, $b$, and $c^2$ are determined from three observations. When $t = 0$, we have $v = 1$, and the equation becomes $\overline{a1-b} = c^2$. Let $m$ = absolute increase of volume from 0°; then $v-m=1$, $v-b=1+m-b$, and $c^2=\overline{a-t}(1+m-b)$, and

$$m = \frac{c^2 - \overline{1-b}\,(a-t)}{a-t} = \frac{\overline{1-b}\,t}{a-t}.$$

In the formula, B represents $1-b$, and A represents $a$.

The numerical values of B and A are fixed, because $t_m$ coincides with $t_a$ at 100°.

The expansibility of the glass corresponds with the maximum observed by M. Pierre (*Ann. de Chim.*, vol. xv., p. 335). The minimum is about three-fourths the maximum. If we adjust the formula to the minimum expansion of glass, and compute the value of the correction at 50°, we shall find that it does not sensibly differ from the correction with thermometer glass of maximum expansion, the amount of difference being less than $\frac{1}{100}$ of a degree.

The alteration of the formula to conform to a change in the quality of the glass requires a change in the first term, which contains the part due to the expansion of mercury, as well as the two last terms, which contain the part due to the expansion of glass.

Let $r$ = the ratio in which the coefficient of glass has to be reduced, and $x$ the ratio of the change that has to be made simultaneously in the first term. We have at 100°, $t_m = t_a$; and by division,

$$\frac{\mathrm{B}x}{\mathrm{A}-100} - \left(\frac{10000}{\mathrm{C}^3} + \frac{1}{\mathrm{D}}\right)r - 1 = 0;$$

$$x = \frac{\mathrm{A}-100}{\mathrm{B}}\left\{1 + r\left(\frac{10000}{\mathrm{C}^3} + \frac{1}{\mathrm{D}}\right)\right\}.$$

The coefficient of glass being taken at M. Pierre's minimum, fixes $r = \frac{3}{4}$, and $\log x = 9{\cdot}9841437$, and the formula becomes

$$x\frac{Bt_a}{A - t_a} - \left(\frac{{t_a}^3}{C^3} + \frac{t_a}{D}\right)r = t_m.$$

If we now compute the difference between this thermometer and the former, we shall find that it is $\frac{1}{100}$ of a degree behind at 50°, and 1·52° in advance at 300°.

This seems to prove that the influence of a difference in the quality of the glass of which thermometers are composed can have no sensible effect on the graduated difference brought out by the projection (fig. 2).

In conformity with what M. Regnault remarks as to glasses with the lesser coefficient experiencing a less rapid augmentation of dilatation with the temperature, it is probable that the term involving the cube should be changed to a lower power of the temperature when the coefficient $\frac{1}{D}$ is diminished.

22 London Street, Edinburgh,
*January* 7, 1858.

# ON A LAW OF LIQUID EXPANSION

*That Connects the Volume of a Liquid with its Temperature and with the Density of its Saturated Vapour.*

(*Illustrated.*)

(From the *Philosophical Magazine* for June 1861.)

§ 1. In the archives of the Royal Society for 1852, there is an account of observations on the density of liquids and their superjacent vapours at high temperatures, made in sealed graduated tubes filled with the same liquid in different proportions of their volumes. The general law of density in saturated vapours deduced from these observations, and from the various observations of vapour-tension already published by other physicists, is therein set forth, with the assistance of a chart (No. 2 chart) in which the observations are all projected, and lines drawn, that enables the eye to judge of the accordance between theory and observation. (See Note A.)

An account of this general law is also given in the *Philosophical Magazine* for March 1858, in a paper entitled "On the Evidence of a Graduated Difference between the Thermometers of Air and Mercury between 0° and 100° C."

By the same mode of observing in sealed graduated tubes, I afterwards extended the observations up to the transition-point of three of the liquids, viz. alcohol, ether, and sulphide of carbon, and found that the law of vapour-density was maintained in them without deviation to their extreme limiting temperatures. As these extensive series of observations supplied the curves of expansion of the three liquids, I have occasionally tried by means of them to discover a general law of liquid expansion. I submit the following account of the last attempt of this kind, as it appears to be successful.

§ 2. The curves of expansion, drawn to a large and distinct scale, were examined by the following graphical process:—At four or five points nearly equidistant, tangents were drawn (carefully judging of the direction to be given to the straight edge by the sweep of the curve to the right and left of the point of contact). Thus were obtained several values of the quotient of the differential of volume by the volume or proportionate differentials for constant element of temperature $\left[\frac{dv}{dt \,.\, v}\right]$. These were set off as ordinates to the temperatures, and the curve drawn through the points appeared to be the common equilateral hyperbola, having one asymptote coinciding with the axis of temperature and the other perpendicular to it, and intersecting it at a temperature $[\gamma]$ that evidently was above the transition-point. If this were the case, the product of the co-ordinates to each point of the curve, reckoning from the point $\gamma$ as origin, ought to be constant $[=\rho]$; and accordingly it was found that when the inverse of the quotients were projected as ordinates to the temperatures, the points ranged in a straight line, which being produced, cut the axis in the point $\gamma$. The differential equation is thus

$$\frac{dv}{dt} \cdot \frac{\gamma - t}{v} = \rho,$$

the integration of which is

$$v = \left\{\frac{k}{\gamma - t}\right\}^{\rho},$$

in which $k = (\gamma - t)$ when $v = 1$. (See Note B.)

§ 3. There is a relation between this expression and that for saturated vapour-density which seems to prove that it is not empirical, but the true exponent of the physical condition of the molecules of a body in the liquid state. The following is a statement of it.

In the papers above referred to, there will be found an account of the law of saturated vapour-density, and the proofs on which it rests. It is expressed by the equation

$$\left\{\frac{t - g}{h}\right\}^{6} = \mathrm{D}.$$

If we put $\Delta = \frac{1}{v}$, the law of liquid density is

$$\left\{\frac{\gamma - t}{k}\right\}^{\rho} = \Delta.$$

On comparing the constants for different liquids, I find as a general rule that the quotient $\frac{h}{\rho}$ is a constant quantity [E].

Thus for Alcohol my observations give E = 1717
" Ether " " E = 1739
" Sulphide of carbon " E = 1725

M. Muncke's observations on sulphuric acid from 50° to 230° C. E = 1600 to 1800. (See Note C.). MM. Dulong and Petit's observations on the expansion of mercury between 0° and 300° C. give $\rho = \frac{1}{1{\cdot}489}$. The line passing through M. Avogadro's observations on the vapour of mercury from 230° to 300° C. gives $h = 1160$. These combined give E = 1727. I have adopted 1717 as the nearest probable value at present, because the most labour was bestowed on the alcohol series of observations.

These values of E are derived from English measures of pressure and temperature, viz. inches of mercury and degrees of Fahrenheit scale. The value of this constant derived from French measures is F = 504·44, which corresponds with E = 1717. The ratio of reduction is $a = \frac{5}{9}\left\{\frac{\frac{5}{9} \times 29{\cdot}922}{760}\right\}^{\frac{1}{6}}$, so that Ea = F,

$$\left[\begin{array}{l} \log F = 2{\cdot}70282 \\ \log \frac{1}{a} = 0{\cdot}53195 \end{array}\right]$$

§ 4. M. Regnault's observations on the tension of the vapour of mercury from low temperatures up to 200° C., respond to the same value of $h$ as those at the higher temperatures by M. Avogadro, but with $g$ augmented 12 degrees, showing a boiling-point 12 degrees higher. It is remarkable that M. Regnault's observations on the expansion of liquid mercury differ so far from those of MM. Dulong and Petit as to be

represented with $\rho = \frac{4}{5}$ and E = 870. At 300° C. this difference amounts to about the equivalent of $3\frac{1}{2}$° C. The acceleration of the rate of expansion is in M. Regnault's observations only one-half what is shown by those of MM. Dulong and Petit. (See Note D.) This is a remarkable discrepancy, both being so eminent in this class of observations.

§ 5. In all cases I have worked with temperatures reduced to the air-thermometer by scales of correction computed from the formula given in Appendix III. to the paper in the *Philosophical Magazine* for March 1858 above referred to [p. 446 above].

The formula is founded on MM. Dulong and Petit's observations. I annex exact tracings of these scales (see Plate). In two cases (petroleum and sulphuric acid) the temperatures were taken uncorrected and compared with the results when corrected. In both, the differences between theory and observation were less when the temperatures were corrected.

It is not absolutely necessary to correct the temperatures in order to recognize these laws of density, and for practical purposes may not be required; but it seems best to accustom ourselves to do so, in order to be prepared for the recognition of any other relations of harmony that may exist in the thermo-molecular physics of different bodies.

§ 6. Water is, as might be expected, an exception to the law of liquid density, as it is to the law of capillarity and compressibility (see papers by M. Grassi and M. Simon in the *Annales de Chimie*). I have traced its curve of expansion by observations in sealed tubes up to 210° C. air-thermometer (see Note E), and projected the densities to the value of $\rho$ required by its vapour-gradient; also those of M. Despretz from 0° to 100° C.; but they do not conform to the line required at any point of its range even at the highest temperature. These abnormal features in this first of liquids have had a prejudicial effect on the progress of science in this department. There is no other liquid as yet found with such point of maximum density that remains a liquid under its maximum; yet such a point seems invariably to be sought for. M. Muncke and M. Pierre have bestowed much unavailing labour on this question. (See Note F.)

§ 7. To determine the constants of these two equations for the density of the liquid and of its vapour, not more than four exact observations are strictly required; two of the vapour, and two of the liquid.

If the series of observations on the dilatation of a liquid extend over a considerable range of temperature, and have had their inequalities equalised by graphical processes equivalent to weighing by the method of least squares, the three constants of the equation may be directly determined.

Thus let $t_0$, $t_1$, $t_2$ be the three temperatures, and $v_0$, $v_1$, $v_2$, the corresponding volumes observed, to find $\rho$ and $k$ we have

$$\frac{t_1 - t_0}{\left(\frac{1}{v_0}\right)^{\frac{1}{\rho}} - \left(\frac{1}{v_1}\right)^{\frac{1}{\rho}}} = \frac{t_2 - t_0}{\left(\frac{1}{v_0}\right)^{\frac{1}{\rho}} - \left(\frac{1}{v_2}\right)^{\frac{1}{\rho}}} = k,$$

which may be solved by trial and error. But few observations as yet published will stand this test, the range of temperature being too small, and the irregularities proportionably too great.

§ 8. A simple and satisfactory way to test both of these laws of density by published observations, is to take two of the vapour-tensions not far from the boiling-point and compute the value of $h$. *Ex. gr.*, let $e_0$, $e_1$, be the two observations of the pressure of vapour in contact with its generating liquid; $T_0$, $T_1$ the corresponding temperatures by air-thermometer reckoned from the zero of gaseous tension [ $-461°$ F. or $-273{\cdot}89°$ C.]; then we have

$$h = \frac{T_1 - T_0}{\left(\frac{e_1}{T_1}\right)^{\frac{1}{6}} - \left(\frac{e_0}{T_0}\right)^{\frac{1}{6}}};$$

and since $\rho = \frac{h}{E'}$, we obtain the index of the power of the density of the liquid, which being set off as ordinates to the temperature, ought to range in a straight line; and this line produced, cuts the axis of temperature at $\gamma$. If, when these points are connected by distinct lines, a general convexity in the range can be discovered, viewing it foreshortened with the eye close to the plane of projection, then we may infer

that $\rho$ requires to be diminished if the convexity is directed upwards from the axis of temperature, and *vice versa*.

§ 9. Having found $\rho$ and $k$ and $\gamma$, the next step is to compute the values of $t$ from the volumes by the equation, and tabulate the differences between the computed and observed temperatures. This will be found attended with but little additional labour. If we now project these differences as ordinates on an exaggerated scale to the temperatures, we obtain a distinct impression of how far theory and observation accord.

§ 10. Mercury and alcohol being the most important liquids for thermometric purposes, may serve as examples of the mode of computation.

### I. *Mercury.*

M. Avogadro's observations on the tension of the vapour of mercury:—At 260° C. the observed tension was 133·62 millims., at 290° it was 252·51 millims. The correction to reduce the temperatures to the air-thermometer from the scale is 5·60° at 260°, and 6·91° at 290°. Hence—

$$260 - 5{\cdot}60 + 273{\cdot}89 = 528{\cdot}29 = T_0, \quad 133{\cdot}62 \text{ millims.} = e_0,$$
$$290 - 6{\cdot}91 + 273{\cdot}89 = 556{\cdot}98 = T_1, \quad 252{\cdot}51 \text{ millims.} = e_1;$$

and we arrive by computation at $\log h = 2{\cdot}54796$ and $g = 247{\cdot}45°$.

[I have computed the temperatures for M. Avogadro's other six observations. The computed, minus the observed, is, at

$300° = + 0{\cdot}15°$
$290° = \quad 0°$
$280° = + 0{\cdot}42°$
$270° = - 0{\cdot}26°$
$260° = \quad 0°$
$250° = - 0{\cdot}26°$
$240° = - 1{\cdot}90°$
$230° = - 4{\cdot}56°$

= difference in tension amounting to one-third inch mercury.]

This value of $h$ being derived from French measures, is to be applied to F to find $\frac{1}{\rho}$, which thus comes out 1·4284.

The following are MM. Dulong and Petit's volumes of mercury computed with this index:—

| Temp. Cent. Air. | Volumes. | Inverse of Volumes raised to the Power $\frac{1}{\rho} = 1{\cdot}4284$. | First Differences. | Second Differences. |
|---|---|---|---|---|
| Deg. | | | | |
| 0 | 1·000000 | 1·000000 | | |
| | | | ·025185 | |
| 100 | 1·0180180 | ·974815 | | ·000028 |
| | | | ·025213 | |
| 200 | 1·0368664 | ·949602 | | ·000023 |
| | | | ·025236 | |
| 300 | 1·0566037 | ·924366 | | |

The second differences indicate a slight convexity in the line upwards. This shows that $\frac{1}{\rho}$ requires augmentation. The following is the result of computing the same observations with $\frac{1}{\rho} = 1{\cdot}489$:—

| | | First Differences. | Second Differences. |
|---|---|---|---|
| Deg. | | | |
| 0 | 1·000000 | | |
| | | ·026240 | |
| 100 | ·973760 | | ·000001 |
| | | ·026239 | |
| 200 | ·947521 | | ·000005 |
| | | ·026234 | |
| 300 | ·921287 | | |

To find $\gamma$, we have $1 - 0{\cdot}947521 : 200^\circ :: 1 : 3811{\cdot}05^\circ = \gamma$, and $\left\{\frac{3811{\cdot}05 - t}{3811{\cdot}05}\right\}^{\frac{1}{1{\cdot}489}} = \Delta = \frac{1}{v}$, or $(3811{\cdot}05 - t)v^{1{\cdot}489} = 3811{\cdot}05$.

This expresses MM. Dulong and Petit's observations with a difference at 100° amounting to $\frac{1}{1000}$ of a degree, and at 300° the difference is $\frac{1}{50}$ of a degree.

§ 11. To bring the D of the vapour formula to the same standard as the $\Delta$ of the liquid formula, it is requisite to change the value of $h$ in the one and $k$ in the other, so that the weight in grains of a cubic inch of either may be indicated.

Let $\eta = \cdot 0216216$ = weight in grains of a cubic inch of hydrogen at the temperature 0° C. and pressure 760 millims.

$\delta$ = vapour-density of the body on the hydrogen scale.

$\tau$ = temperature (reckoned from the zero of gaseous tension) at which the pressure of the saturated vapour is 760 millims.

$\mu$ = required factor

$$\left\{\frac{\tau - g}{\mu h}\right\}^6 = \frac{273{\cdot}89}{\tau} \cdot \eta\delta = \beta^6.$$

By the formula,

$$g = 247{\cdot}45°,$$
$$\tau = 611{\cdot}28°,$$

also $\delta = 101$; hence $\mu h = \dfrac{\tau - g}{\beta}$, and $\log \mu h = 2{\cdot}56247$. Thus we obtain

$$\left\{\frac{\mathrm{T} - 247{\cdot}45}{[2{\cdot}56247]}\right\}^6 = \omega,$$

the general expression for the weight in grains of a cubic inch of the saturated vapour of mercury at T° (C.A.G.), temperature reckoned by Centigrade Air-thermometer from the zero of Gaseous tension.

§ 12. The weight of a cubic inch of mercury at 0° is 3754·4 grains, hence

$$\frac{3754{\cdot}4}{v} = \left\{\frac{3811{\cdot}05 - (\mathrm{T} - 273{\cdot}89)}{\mathrm{M} \times 3811{\cdot}05}\right\}^{\frac{1}{1{\cdot}489}};$$

and $\mathrm{M} = \left(\dfrac{1}{3754{\cdot}4}\right)^{1{\cdot}489}$; hence

$$\left\{\frac{3811{\cdot}05 - (\mathrm{T} - 273{\cdot}89)}{[8{\cdot}25857]}\right\}^{\frac{1}{1{\cdot}489}} = \mathrm{W},$$

the weight of a cubic inch of mercury in grains at T° (C. A. G. temperature).

We may thus find the temperature at which $\omega = \mathrm{W}$, or that at which liquid and vapour would be of equal density if the laws were maintained.

With alcohol, ether, and sulphide of carbon, transition occurs a few degrees below the theoretical temperature of equal density.

## II. *Alcohol.*

§ 13. M. Regnault's observed tensions of saturated vapour:—At 40° C. the tension is 134·1 millims.; at 70°, 539·2 millims.

$$\left.\begin{array}{l} 40 + 0{\cdot}48 + 273{\cdot}89 = 314{\cdot}37 \text{ C. A. G.} = T_0 \\ 70 + 0{\cdot}41 + 273{\cdot}89 = 344{\cdot}30 \qquad\quad = T_1 \end{array}\right\} \log \begin{array}{l} h = 2{\cdot}15392 \\ g = 190{\cdot}70 \end{array}$$

$$\frac{1}{\rho} = \frac{F}{h} = 3{\cdot}5392$$

This gives 78·83° C. A. as boiling-point at pressure 760 millims. M. Pierre's observations on the expansion of alcohol were made on a specimen that boiled at 78·63° C. A. under pressure 758 millims., and its specific gravity at 0° was 0·8151.

The following are his second series of observations computed with the above index, 3·5392:—

| | C. C.A. | V. | $\left(\frac{1}{V}\right)^{3\cdot5392}$ | Computed temp. minus observed temp. |
|---|---|---|---|---|
| | Deg. | | | Deg. |
| (1) | 33·46 + 0·47 = 33·93 | 1·03714 | ·87890 | +0·21 |
| (2) | 47·52 + 0·51 = 48·03 | 1·05356 | ·83140 | 0 |
| (3) | 50·33 + 0·51 = 50·84 | 1·05676 | ·82248 | −0·19 |
| (4) | 56·26 + 0·50 = 56·76 | 1·06416 | ·80245 | −0·25 |
| (5) | 60·41 + 0·48 = 60·89 | 1·06989 | ·78736 | +0·03 |
| (6) | 73·70 + 0·38 = 74·08 | 1·08780 | ·74238 | 0 |
| (7) | 76·73 + 0·35 = 77·08 | 1·09168 | ·73310 | −0·27 |

The computed densities (·87890, etc.) set off as ordinates to the temperatures, show a trend without any appearance of curvature. The straight line seems to pass exactly through the points of the second and fifth and sixth. Assuming it to pass through the second and sixth, we have

$$\cdot83140 - \cdot74238 = \cdot08902 : 74{\cdot}08° - 48{\cdot}03° = 26{\cdot}05° :: \cdot74238 : 217{\cdot}24,$$

and $74{\cdot}08 + 217{\cdot}24 = 291{\cdot}32° = \gamma$. This line gives unity volume at $-1{\cdot}30°$, hence $k = 292{\cdot}62 = \gamma = t$ when volume equal unity, and the equation is $(291{\cdot}32° - t)v^{3\cdot539} = 292{\cdot}62$. The differences between the observed temperatures and those computed from the observed volumes by this equation are given in the last column.

§ 14. This equation answers well to the observations of M. Pierre above 10°; also to those of M. Muncke (*St Petersburgh Memoirs*) above the same temperature; but in both the trend of the points below this lies in a line inclined to that of the equation. The divergence is the greatest in M. Pierre's. In neither is it a general convexity, but distinctly the contour shows two lines diverging from about 15° to 20° C. This is most distinct in M. Muncke's observations. I have computed them by the above equation, and tabulated the differences between the observed and computed temperatures, which are set off in the Plate, fig. 3, as ordinates to the temperatures on ten times the natural scale. Above these, in fig. 2, M. Pierre's differences are set off to the same scale, and in fig. 4 the differences in my series of observations on alcohol, described in the paper above referred to as being in the archives of the Royal Society. This alcohol was not absolute; it had 19 per cent. water, and the index of its power derived from its line of vapour-density was 3·60; also $\gamma = 290{\cdot}89$, $k = 209{\cdot}81$, and its equation (the volume being reckoned as unity at the boiling-point)

$$(290{\cdot}89^\circ - t)\, v^{3{\cdot}60} = 209{\cdot}81^\circ \text{ C. A. G.}$$

§ 15. The deflection in M. Pierre and M. Muncke's observations, it will be remarked, occurs in those below atmospheric temperatures, where the reduction of temperature had to be artificially produced by mixtures of broken ice and muriate of lime, and it represents the temperature of the mercury to be higher than that of the alcohol. This is precisely what took place in some observations I made on the contraction of ether about 20° below the atmospheric temperature. A similar deflection, but in a greater degree, appears in the ether observations of the same authors. They are projected in figs. 5 and 6 to the same scale as the others. (See Note G.)

The application of cold to maintain a constant temperature is by no means under the same command as the application of heat; and, besides, conductibility is very much reduced at low temperatures. There is an evident dislocation, the law of continuity is broken, but it is at the part of the scale where the *mode* of observation underwent a change. I submit, therefore, that the verdict should be against the observations at

the lower temperatures, not against the law of expansion, which, if in fault, would cause the trend of the points to have a general curvature throughout the range.

In judging of the evidence afforded by these graphical projections, it should be kept in view that the vertical scale magnifies the amount of the differences tenfold. The accordance of theory with observation is in some cases remarkable. Thus, for 40° C., Muncke's alcohol and Pierre's ether do not show a difference greater than one-sixth of a degree. We have also to keep in mind that the power $\frac{1}{\rho}$ that reduces the densities to a straight trend, is not arbitrarily assumed to suit a particular series of observations, but that it is determined *à priori* from the vapour. If we take any other value of $\frac{1}{\rho}$ but that which is thus determined, the graphical projection of the computed densities shows a general bend. If $\frac{1}{\rho}$ is too great, the bow is turned downwards; if too small, the bow is turned upwards. The string of the bow only makes its appearance when the value of $\frac{1}{\rho}$ is that deduced from the gradient of vapour-density above described.

§ 16. The time has not perhaps yet arrived for deducing these laws of density from the dynamical theory of heat; but if we are ever to arrive at a conception of the true ultimate nature of molecular force, it seems clear that the inductive path of least difficult approach (if not the only one) is that which sets out from the study of the gaseous state, and proceeds by way of that of the equilibrated condition of saturated vapours in communication with their generating liquids, to the molecular condition of the liquids, where the dynamic condition of the chemical element is constrained by the cohesive force; and the struggle in which this dynamic force is gradually subdued by the increase of temperature is as now ascertained, represented by one quantitative relation throughout, that seems to indicate a certain simplicity in the ultimate recondite principle on which molecular force is based.

We know that the physics of gases conform to the physics of media that consist of perfectly free elastic projectiles.* Their free concourse and perfectly elastic recoil determines the resolution of their *vis viva* into the six rectangular directions of space; and it is this number that probably fixes the ratio of the proportionate increment of density in a saturated vapour to the corresponding proportionate increment of temperature reckoned from the fixed limit *g*. But the *absolute* increment of density corresponding to constant increment of temperature differs in different vapours, being ruled by a gradient, the sixth root of which has a constant ratio to the index of density of the generating liquid expressed as a function of the temperature.

The next step that seems within reach, if we had a few more observations to work from, is the discovery of the relation which no doubt exists between the increase of volume and decrease of latent heat or capillarity regarded as the integral of cohesion. The density and the capillarity both diminish as the temperature rises. (See Note H.) If there is a simple law of quantitative relation between them, its discovery would supply all that is now wanting to bring the dynamical theory of heat to bear upon the molecular physics of liquids.

## Notes.

*Note* A. § 1.—The title of the paper is "On a General Law of Density in Saturated Vapours," illustrated by Chart No. 2. In the *Philosophical Transactions* for 1852 there is a paper with the same title, illustrated by a Chart No. 1. (This paper was originally sent to the British Association.) In Chart No. 1 the sixth root of density is laid off as ordinate to the square root of the temperature reckoned from the zero of gaseous tension. In Chart No. 2 the sixth root of density is laid off as ordinate to the temperatures simply. In Chart No. 1 the lines appear straight at the upper part of their course, but with an increasing flexure at the lower part of the range convex to

* See paper "On the Physics of Media that consist of perfectly elastic Molecules in a state of Motion," in the *Archives of the Royal Society*, 1845-46.

axis of temperature. Also there is no relation of harmony apparent between them. In No. 2 Chart (of which a tracing is to be found in the *Archives of the Royal Society* for 1852-53) the lines are straight throughout, and relations of parallelism appear; also several radiate from the same point in the axis of temperature, showing that, as a general law, *vapours in contact with their generating liquids have, at the same temperature, or at the same constant difference of temperature, densities that have a constant ratio.*

*Note* B. § 2.—This mode of graphical analysis seems a natural mode of operating when a law of nature has to be unmasked. If the proportionate differentials of volume had been laid off as ordinates, not to the temperature, but to the volume, the result would be the logarithmic curve, which might not be so easy to recognize with only a few points to lead from. We must be guided in the selection of the co-ordinate axis by the causal relation of dependent phenomena. Heat being, as it were, the instrument of action in molecular physics, claims the preference as a standard by which to measure the proportionate differentials, and to which other variables may be referred to as co-ordinate axis. As an example, the following is the analysis of the law of saturated vapours by this process.

The vapour-tensions being divided respectively by the corresponding temperatures reckoned from the zero of gaseous tension, the quotients represent densities of saturated vapour. Setting off these quotients as ordinates to the temperatures, we next draw the curve, and equalize the irregularities as far as possible; then take off the ordinates of the finished curve at equal intervals of temperature, say 10° or 5°; next take the differences of adjacent ordinates and divide each by the intermediate ordinate. These quotients, $\frac{d\mathrm{D}}{dt\mathrm{D}}$, are to be laid off as ordinates to the temperatures. The points appear to range in a conic hyperbola, having the axis of temperature as an asymptote. This conjecture is to be tested by laying off the inverse of these quotients as ordinates to the temperatures. The conjecture is confirmed by the points ranging in a straight

line which cuts the axis at a certain temperature $g$. Hence $(t-g)f = \frac{dt\mathrm{D}}{d\mathrm{D}}$ and $\frac{d\mathrm{D}}{\mathrm{D}} = \frac{dt}{f(t-g)}$. The integration of this gives $\mathrm{D} = (t-g)^{\frac{1}{f}} \times \frac{1}{\mathrm{H}}$, in which $\mathrm{H} = (t-g)^{\frac{1}{f}}$ when $\mathrm{D}$ = unity, or let $h' = \mathrm{H}$, then $\mathrm{D}^{f} = \left(\frac{t-g}{h}\right)$. Comparing the value of $f$ in different vapours, it is found to be constant for all and equal to $\frac{1}{6}$.

As another example, but unconnected with heat, we may inquire as to the possibility of ascertaining the law of gravitation from the changes in the moon's apparent size and motion, its actual distance and the earth's radius being supposed unknown, but assuming that the difficulty caused by an unknown parallax and augmentation of diameter might be evaded by taking lunar distances at equal altitudes on both sides of the meridian.

By observations on consecutive nights, while the diameter is increasing or diminishing at the maximum rate, we might obtain two angular velocities and two measurements of diameter; hence the proportionate differential of the diameter and the correction to be applied to the angular velocities to reduce them to the same radius. We might thus obtain the velocity, the increment of velocity, and increment of distance expressed in terms of $r$, the radial distance of the moon from the earth at the middle epoch. Now since $\frac{dr}{r^2} = 2vdv$, if we had these same quantities for different values of $\frac{1}{\mathrm{D}}$ or $r$, and projected the different values of $\sqrt{\frac{1}{2vdv}}$ as ordinates to the corresponding values of $r$, the points would converge in a straight line to the zero of $r$; and if an approximate parallax was obtained, the point corresponding to the value of $2vdv$ at the earth's surface would fit in and confirm the propriety of the projection. If it is a question what should direct us to this particular projection, it might be answered the increment of square velocity is a square quantity, and the inverse form of function is applicable to a power depending on distance.

*Note* C. § 3.—The longest series of observations on the expansion of a liquid that I have met with is that of M. Muncke, on sulphuric acid from $-30°$ to $+230°$ C. I have been enabled to put them to the test by the following equation for the tension of its vapour, viz. $\left(\frac{t - 354{\cdot}7}{1288}\right)^6 t = p$ (English measures). In this the value of $g = 354{\cdot}7$ is assumed to be the same as that for steam, and for the vapours of several hydrates of sulphuric acid observed by M. Regnault, and referred to in § 1 of paper in the *Philosophical Magazine* for March 1858. The value of $h(= 1288)$ is derived from the boiling-point. The value of $\frac{1}{\rho} = \frac{E}{h}$ is thus 1·33. The inverse volumes being computed to this power, and laid off as ordinates to the temperatures, were found to range well in a straight line above 30°. The line drawn through 45° and 220° is expressed by the equation

$$(1433{\cdot}2° - t)v^{\frac{1}{3}} = 1436{\cdot}1° \text{ C. A.}$$

The differences between the temperatures computed from the volumes by this equation and those observed are laid off in fig. 1 (see Plate) as ordinates to the temperatures, the scale vertical to horizontal being 10 to 1.

The law of continuity is evidently broken at about 40°, the deflection being similar to the other cases referred to in §§ 14, 15, and probably due to the same cause.

Fig. 7 (see Plate) represents the differences of Muncke's observations on petroleum projected in the same way. The equation is

$$(489{\cdot}5° - t)\, v^{2{\cdot}14} = 489{\cdot}5° \text{ C.},$$

in which $\frac{1}{\rho} = 2{\cdot}14$ has been deduced from Ure's observations on the vapour of petroleum. It is unlikely that the liquids were exactly the same. A slight convexity directed upwards is apparent in the trend of the points, which a small augmentation in $\frac{1}{\rho}$ would correct.

*Note* D. § 4.—M. Regnault stands so high as an authority, that an error in his observations that can be clearly demon-

strated from internal evidence is of importance to science. Erroneous observations from eminent observers are serious obstacles to progress, as are unsound deductions from eminent men of science. They are weeds difficult to root up, and the attempt to do so is a task so ungracious and so irreverent as to incur every discouragement.

The projection of M. Regnault's observations on the tension of steam above and below 100° C. is given in fig. 8. The dotted line represents the empirical formula which had to be altered at 100°, the point at which the method employed in making the observations was changed. They are projected

| C. A. | Volume. | C. A. | Volume. | C. A. | Volume. | C. A. | Volume. |
|---|---|---|---|---|---|---|---|
| Deg. | | Deg. | | Deg. | | Deg. | |
| −10 | 1·00184 | 55 | 1·01413 | 100 | 1·04333 | 155 | 1·09847 |
| − 5 | 1·00068 | 60 | 1·01668 | 105 | 1·04743 | 160 | 1·10456 |
| 0 | 1·00013 | 65 | 1·01940 | 110 | 1·05172 | 165 | 1·11083 |
| + 5 | 1·00001 | 70 | 1·02230 | 115 | 1·05620 | 170 | 1·11731 |
| 10 | 1·00025 | 75 | 1·02538 | 120 | 1·06086 | 175 | 1·12400 |
| 15 | 1·00083 | 80 | 1·02863 | 125 | 1·06570 | 180 | 1·13093 |
| 20 | 1·00168 | 85 | 1·03205 | 130 | 1·07073 | 185 | 1·13811 |
| 25 | 1·00280 | 90 | 1·03563 | 135 | 1·07593 | 190 | 1·14553 |
| 30 | 1·00416 | 95 | 1·03939 | 140 | 1·08130 | 195 | 1·15323 |
| 35 | 1·00575 | 100 | 1·04333 | 145 | 1·08684 | 200 | 1·16124 |
| 40 | 1·00755 | | | 150 | 1·09257 | 205 | 1·16958 |
| 45 | 1·00955 | | | | | 210 | 1·17829 |
| 50 | 1·01175 | | | | | | |

with temperatures uncorrected, in the manner described in § 1 of paper in the *Philosophical Magazine* for March 1858, and they are orthographically foreshortened as described in the latter part of § 4. See also § 6, and Appendix I of the same paper.

The question to ask ourselves when looking at the fig. 8 is, Do the points conform to the law of continuity? Is their trend not clearly broken at 100°? To put a series of observations to the test of this law can always be done, but it is attended with considerable labour, and seems to require a speciality different from that which characterises the eminent observer and experimentalist.

*Note* E. § 6.—The above are those observations in series along with those of M. Despretz, both equalised graphically

by elaborate processes, and the temperatures corrected and reduced to the air-thermometer. My observations from 100° up to 212° F. agree so well with M. Despretz's, that those at the higher temperatures will, I think, be found nearly correct, although there was some uncertainty in consequence of absorption by corrosion of glass.

The following is an extract from note-book of the experiments:—

"The observations on pure distilled water could only be made up to 305° F., in consequence of the glass being corroded and becoming opake above that temperature. At the higher temperatures, five tubes were employed with water having $\frac{1}{213}$ of carbonate of soda in solution. Two only of these five were sufficiently transparent up to 413°. But on examining them next day, $\frac{1}{200}$ of the volume of liquid was absorbed. This allowed for.

"The expansion of the solution rather less than pure water. The corrosion of the glass began immediately above the surface of the liquid. The vapour was computed from formula, assuming the law of vapour-density maintained."

*Note* F. § 6.—M. Muncke and M. Pierre have employed the general formula $1 + \Delta_x = 1 + ax + bx^2 + cx^3$, etc., to represent their observations, and have computed the constants for each series. They have also sought, by means of the roots of this equation, to find points of maximum density of each liquid beyond the range of their observations. Thus M. Pierre, at p. 358, vol. xv. *Ann. de Chim.*, expresses himself as follows:—". . . puisque l'équation $\frac{d(1+\Delta_x)}{dx} = 0$, dont les racines *doivent* donner la température de ce maximum, a ses deux racines imaginaires."

I have traced graphically the curve of the equation and of the observations, and find that its course through them is similar to fig. 8, interlacing at the fixed points, and departing altogether from the line of observation beyond the extreme points to which it is bound down. The positive and negative differences at the loops sometimes amount to $\frac{1}{2}$ degree. A conic section may be drawn to represent almost perfectly a

series of observations if the range is not great. The hyperbola answers well, and can be simply applied as the increasing rate of expansion adapts itself to the curve, referred to an asymptote parallel to the axis of temperature.

*Note* G. § 15.—The value $\frac{1}{\rho} = 3{\cdot}28$ is taken from Regnault's observations on the tension of its vapour at 0° and 20° C. The observations at 0° and 30° represent $\frac{1}{\rho} = 3{\cdot}25$. Dalton's observation on the vapour give it equal to 3·2108, which is probably the most correct, as $h$ is thus represented to be the same for sulphuric ether and water, their lines of vapour-density being parallel.

*Note* H. § 16.—In a paper on Capillarity in the *Philosophical Magazine* for January 1858, the proofs are given in detail of a law that connects molecular volume with capillarity and latent heat. It is expressed by the equation $m \div \frac{p}{\mathrm{L}}$, in which $m$ is the cube root of the molecular volume of a liquid, $p$ the height of the same in a capillary tube of constant bore, and L the latent heat of the vapour of the same, all taken at the same temperature $t$. According to Wolf and Brunner (*Ann. de Chim.*, vol. xlix.), $p = \mathrm{A} - bt$ is the empirical equation for the capillary height in terms of the temperature. According to the law of expansion, $m = \sqrt[3]{\left(\frac{k}{\gamma - t}\right)^{\rho}}$. Hence

$$\mathrm{L} \div \frac{p}{m} \div (\mathrm{A} - bt)\sqrt[3]{\left(\frac{\gamma - t}{k}\right)^{\rho}}$$

is the equation which, at present partly empirical, it is so desirable to convert into one wholly expressive of a general natural law of quantitative relation between L and V.

Edinburgh, 6*th May* 1861.

# ON THE EXPANSION OF WATER AT HIGH TEMPERATURES

(*Illustrated with three Plates*).

(From the *Philosophical Magazine* for August 1863.)

In the account given of a law of liquid expansion in the *Philosophical Magazine* for June 1861, it was stated that water alone, of all the liquids examined, did not conform to the law in the lower part of the scale of temperature, and that even up to 200° * it showed no tendency to do so. My attention has lately been drawn to this subject again by having occasion to graduate a water-thermometer. The inequality in the rate of expansion of water is so great that, to obtain even moderate accuracy in the scale, it was requisite to find the law of the differences. To do this a scale of equal parts was attached to the stem, and $\lambda$, the length between $t_0$ and $t_1$ (two near temperatures), was divided by $t_1 - t_0$, and the quotient set off as ordinate to $\frac{1}{2}(t_0 + t_1)$, the middle temperature; the points were found to range in a straight line that came down and crossed the axis at 4° nearly. It would thus appear that the rate of expansion increased exactly in the simple ratio of the distance from 4°, and it would seem that the curve of expansion was the conic parabola having its vertex at the point of minimum volume.

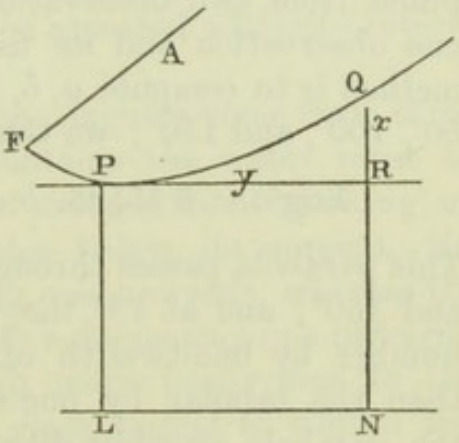
Let P L represent unity or volume at 4°, and N Q = $v$ the volume at $t$. Then

$$t - 4 = \mathrm{LN} = \mathrm{PR} = y,$$

and $\mathrm{R\,Q} = x = v - 1$. If the curve of expansion P Q is a parabola, $x \propto y^2$, and $\frac{dx}{dy} \propto y$. I had previously examined the observations of M. Despretz both above and below the minimum point, and found that from 50° downwards to as far below 0° as the liquid condition can (by perfect stillness of the water)

* The temperatures are Centigrade.

be maintained, they could be very accurately represented, not by a parabola, but by an ellipse F P Q, the curvature of expansion below P being greater than that above, and F A, the axis of the ellipse, being inclined to L N, the axis of temperature. On this account the curve was to be considered as only empirical, and therefore of no value in advancing the thermo-molecular physics of water. But now the question occurred, after seeing how the ordinates of graduation ranged, might not the curve above 50° conform to the parabola? might not the small deflection from it in the lower part of the scale be the abnormal effect of the crystallising force gradually increasing towards the lower temperatures?




§ 2. To test this, reference was made to the Table of the expansion of water up to 210°, given in note E of the paper "On Liquid Expansion" above referred to; and since, if the curve was a parabola, $dx$ or $dv \propto y$ or $d^2v$ constant, it was easy to test the point in question by examining the second differences of the volumes in that Table, which had been carefully reduced to intervals of 5° of the air-thermometer (C. A.).

The following is the Table with first and second differences:—

| $t$. | $v$. | $dv$. | $d^2v$. | $t$. | $v$. | $dv$. | $d^2v$. | $t$. | $v$. | $dv$. | $d^2v$. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Deg. | | | | Deg. | | | | Deg. | | | |
| −10 | 1·00184 | | | 65 | 1·01940 | 272 | | 140 | 1·08130 | 537 | |
| | | 116 | | | | | 18 | | | | 17 |
| − 5 | 1·00068 | | 61 | 70 | 1·02230 | 290 | | 145 | 1·08684 | 554 | |
| | | 55 | | | | | 18 | | | | 19 |
| 0 | 1·00013 | | | 75 | 1·02538 | 308 | | 150 | 1·09257 | 573 | |
| | | | | | | | 17 | | | | 17 |
| 5 | 1·00001 | | | 80 | 1·02863 | 325 | | 155 | 1·09847 | 590 | |
| | | 24 | | | | | 17 | | | | 19 |
| 10 | 1·00025 | | 34 | 85 | 1·03205 | 342 | | 160 | 1·10456 | 609 | |
| | | 58 | | | | | 16 | | | | 18 |
| 15 | 1·00083 | | 27 | 90 | 1·03563 | 358 | | 165 | 1·11083 | 627 | |
| | | 85 | | | | | 18 | | | | 21 |
| 20 | 1·00168 | | 27 | 95 | 1·03939 | 376 | | 170 | 1·11731 | 648 | |
| | | 112 | | | | | 18 | | | | 21 |
| 25 | 1·00280 | | 24 | 100 | 1·04333 | 394 | | 175 | 1·12400 | 669 | |
| | | 136 | | | | | 16 | | | | 24 |
| 30 | 1·00416 | | 23 | 105 | 1·04743 | 410 | | 180 | 1·13093 | 693 | |
| | | 159 | | | | | 19 | | | | 25 |
| 35 | 1·00575 | | 21 | 110 | 1·05172 | 429 | | 185 | 1·13811 | 718 | |
| | | 180 | | | | | 19 | | | | 24 |
| 40 | 1·00755 | | 20 | 115 | 1·05620 | 448 | | 190 | 1·14553 | 742 | |
| | | 200 | | | | | 18 | | | | 28 |
| 45 | 1·00955 | | 20 | 120 | 1·06086 | 466 | | 195 | 1·15323 | 770 | |
| | | 220 | | | | | 18 | | | | 31 |
| 50 | 1·01175 | | 18 | 125 | 1·06570 | 484 | | 200 | 1·16124 | 801 | |
| | | 238 | | | | | 19 | | | | 33 |
| 55 | 1·01413 | | 17 | 130 | 1·07073 | 503 | | 205 | 1·16958 | 834 | |
| | | 255 | | | | | 17 | | | | 37 |
| 60 | 1·01668 | | 17 | 135 | 1·07593 | 520 | | 210 | 1·17829 | 871 | |
| | | 272 | | | | | 17 | | | | |
| 65 | 1·01940 | | 18 | 140 | 1·08130 | 537 | | | | | |
| | | | | | | | 17 | | | | |

It will be remarked that from 50° to 150° the second differences are sensibly uniform, their exact mean being 17·5. Now the parabola being defined by the equation $ax = y^2$, to find $a$ we have the differential equation $2(dy)^2 = ad^2x$, in which $dy = 5°$, $d^2x = \cdot 000175$, hence $a = 285714$; and since $x = v - b$ and $y = t - c$, in which $v$ is the volume at $t$ temperature, we have $285714(v - b) = (t - c^2)$, in which $b$ and $c$ may be computed from two observations, or they may be computed from one observation and its first difference. But the most exact method is to compute $a$, $b$, and $c$ directly from the volumes at 50°, 100°, and 150°; we thus obtain

$$\text{Log } a = 5\cdot 45196,\ b = \cdot 99710, \text{ and } c = -14\cdot 4025.$$

This parabola passes through the tabular volumes at 50°, 100°, and 150°; and at 75° the volume is smaller than the tabular number by one-twelfth of a degree, and at 125° it is larger than the tabular by one-sixteenth of a degree. Above 150° the parabola sensibly diverges from the tabular volumes; at 200° the difference corresponds to 1°. But, as stated in Note E, these higher observations were of inferior value. The tubes were unfortunately of soft glass, which are corroded by steam under 200°. It was thus still an open question whether water expanded in a parabola. The simplicity of the curve was inviting, although, if confirmed at the higher temperatures, it would be fatal to the generality of the law of expansion. I therefore endeavoured to obtain observations at such high temperatures as would settle the point.

§ 3. This has proved a much more difficult task than was anticipated, and has occupied several months.

Below 200° it is comparatively easy to obtain good observations in tubes 20 inches long. In previous researches I employed concentric boxes or hoods, in which a current of heated air, after ascending on the outside of the inner one (which contained the tubes and thermometers), was directed downwards upon the tubes before escaping by the funnel, which also passed upwards through the inner hood. Some attempts to follow this method on a smaller scale having failed, I resorted to an open tube held over Bunsen-flames, and after a good deal of labour and many failures was at last successful

in obtaining good observations up to 335°, which afford distinct proof that the curve of expansion above 200° conforms to the general law.

To establish this decisively, requires observations above 300°; and unfortunately it is just at this temperature that difficulties accumulate on all sides. To overcome these, required a kind of apprenticeship to be served.

I shall now describe the method finally adopted and employed in the last series, so that they may be repeated without obstacle to success.

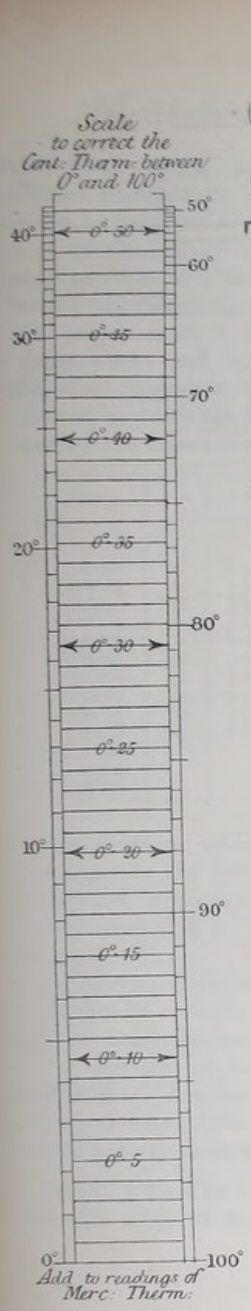
§ 4. The difficulties that beset the inquiry come from three sources. 1. The glass tube that contains the water must be vertical; and, to prevent the liquid column from breaking up by the formation of vapour-bubbles below its summit, the temperature should be an increasing one upwards, whereas the natural gradient of a simple heater is a decreasing one upwards. 2. The hard German glass, though more impervious to the solvent power of water than soft glass, begins to whiten and cloud below 300°, and generally becomes mottled with opake patches just at the very part of the tube where clear vision is indispensable. There is also a certain amount of the liquid absorbed, apparently, but which may be partially recovered by heating. 3. To obtain certainty as to the mean temperature of the liquid column, it was thought proper to use as thermometer a tube of the same length as the water-tube, filled with a liquid of great expansibility and high boiling-point. To graduate such thermometers by comparison with ordinary bulb-thermometers, it was necessary to have an open bath of a similar liquid and transparent in a glass vessel sufficiently deep to contain the tube held vertical. The fumes of such liquids heated towards 300° become overpowering, and reliable comparisons can hardly be obtained above 250°.

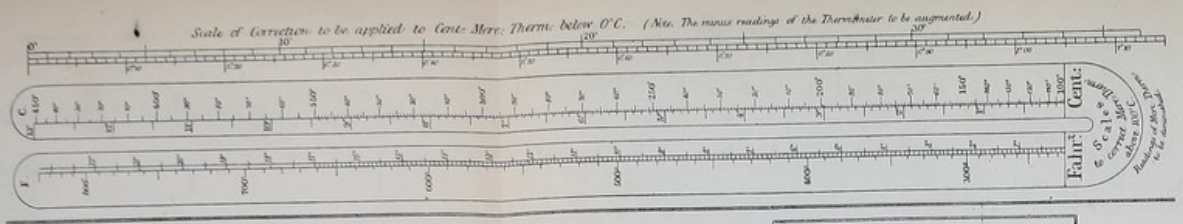
§ 5. The ordinary chemical thermometers, having a vacuum at the top of the stem, are not to be depended upon at temperatures near the boiling-point of mercury, as vapour is sometimes generated below. Besides they are graduated so as to require the stem to be of the same temperature as the bulb. I therefore had one made with short stem and bulb strong enough to withstand high pressures, containing air over the mercury. It was

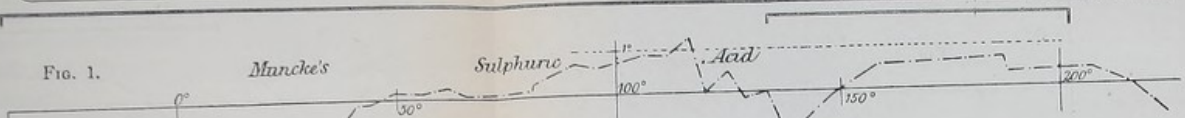
about the same length as the water-tubes, and 100° measured about $\frac{5}{4}$ of an inch. After being heated to 400°, it was graduated and marked off in divisions of 10°, and did not alter in its reading afterwards. It is represented by Plate II., fig. 7. Another two of a similar kind were also provided: and, each being graduated by itself and afterwards compared with the others, the highest temperatures would have been well determined if it depended only upon thermometers.

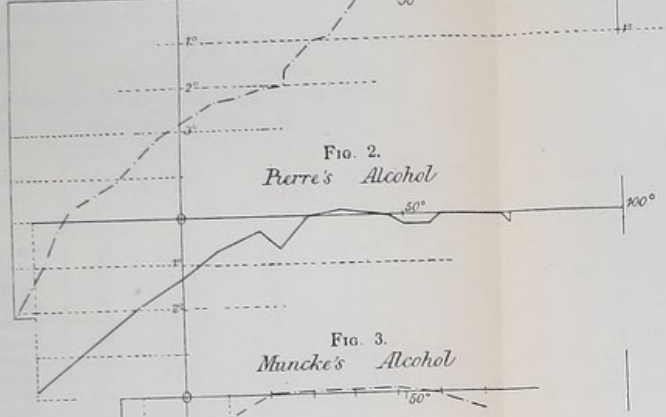
§ 6. *Graduating tubes.*—Fig. 3 (Plate I.) is a full-sized representation of the last of the six water-tubes employed. It was prepared by first cleaning out, before sealing, with sulphuric acid, then by distilled water, then dried by heating. The bottom was then closed at the flame, and allowed to remain in it until the glass thickened. The top was then sealed before filling, employing a very small Bunsen-flame. In operating with small flame, the tube after a pull will contract of itself without being drawn; the glass is thus kept thick and strong; and the flame being slowly withdrawn while the tube is kept revolving, a strong conical point may be obtained. This is filed down until a small needle-point opening appears. Before filling the tube, a scale of twenty divisions to the inch is finely and carefully marked upon the glass, as shown in fig. 3. The mode of doing so is shown in fig. 6. The feather edge of the ivory scale resting on the glass, the lines were drawn with a fine-pointed lithographic steel pen, using a watchmaker's lens. The ink was of gamboge, rubbed down in water and darkened with black. When dry, the marked portion of the tube was passed through a small flame, gradually heating until, when near red heat, the lines are observed suddenly to turn black; it is then to be quickly withdrawn. The lines are thus ingrained upon the glass, and cannot be easily rubbed off. The tube is then filled with distilled water to about the proportion required. The small hole is then sealed by holding the extreme point only in the flame for a few minutes.

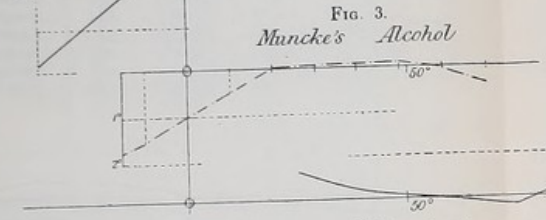
§ 7. *To find total contents of the tube and the proportion occupied by the liquid.*—At a small Bunsen-flame the upper part of the tube is dried, beginning at the top and working downwards; then, immersing in a water-bath of known atmospheric temperature, the scale-reading of the lower part of

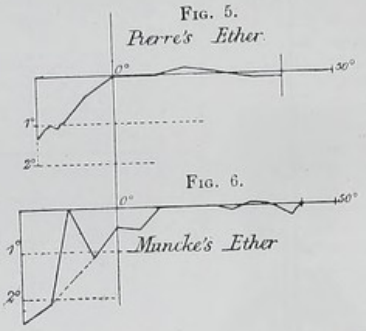


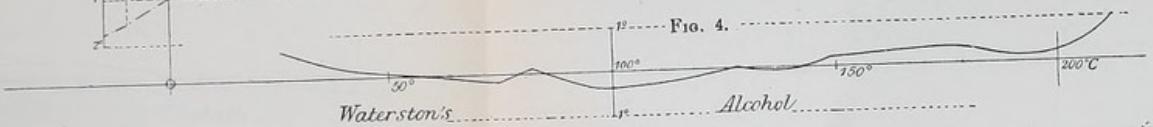



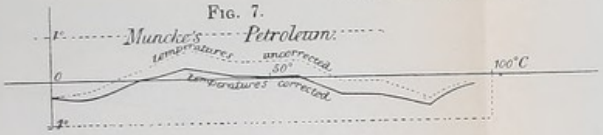



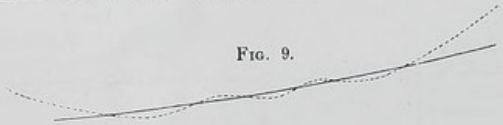



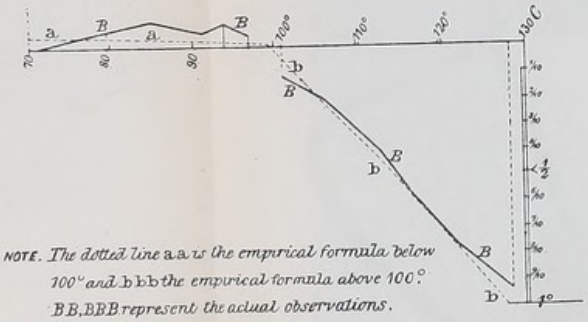







NOTE. The dotted line a a a is the empirical formula below 100° and b b b the empirical formula above 100°. B B, B B B represent the actual observations.

Fig. 8 —M. Regnault's Observations on Steam from 70° to 130° C.

the capillary cup is noted. The liquid is then shaken to the other end of the tube and a similar reading noted, taken by applying the ivory scale. Halfway between these is the middle point. Add to this the distance between this middle point and the zero of scale, the sum is the value of zero of scale, to which we have to add the scale-reading of an observation, and half the depth of the capillary cup is taken by estimation as the volume to be added to reading of bottom of cup, which is always a very distinct line by transmitted light.

Next shake a small part of the liquid into the conical top; then dry, and take readings. Thus, and by allowing for capillary cup, we obtain the contents of half the tube, and thence that of the liquid with great exactness. By further operations of the same kind we may discover any tapering of the tube. This was slight, and omitted in the reductions.

§ 8. *Graduating the tube thermometers.*—The liquid employed for these thermometers was burnt linseed oil. After the tubes were marked with scale divisions and sealed, they were subjected for an hour to a temperature of 350° before graduation. Three were constructed (the least of them is shown in fig. 3, lashed with copper wire to the water-tube). In the top of the funnel F, fig. 1, a large test-tube was fixed and filled with lard. In this bath the standard mercurial thermometer, fig. 7, and an oil-tube were placed. The heating took place by means of two Bunsen-burners placed below, as shown in fig. 1. At the top of each Bunsen-tube there was fixed a spiral wire (fig. 5), which effectually prevented the flame from jumping downwards when at its lowest. With these and a lever with graduated arc applied to the gas-cock *s*, fig. 1, the temperature could be gradually raised and held in command if the day was calm. In repeating such experiments it would be well to have a gas-cock made specially, so as to be able to regulate the supply by uniform motion.

In the lard-bath the convection currents were very rapid, and the temperature throughout nearly the same. The standard thermometer was kept moving up and down for some time before making the comparison (which requires a perfectly steady temperature to be maintained).

The value of the scale-readings of the oil-tubes were thus

determined, and their curves of expansion drawn to a large scale up to about 280°. Beyond this the curve was carried on the hypothesis that it was an arc of a hyperbola, which from previous experience I had found to answer well as an empirical curve in liquid expansion.

§ 9. Another method was employed to measure the higher temperatures of the last water-tube. A tube thermometer containing a 5½-inch column of sulphuric acid was prepared. The value of its scale-readings above 210° depended on a very good observation in acid-bath at 203° (the temperature having remained steady for half an hour). To this and the reading at atmospheric temperature the formula derived from the law of expansion was applied, and the value of the higher scale-readings computed. It was then lashed to the oil thermometer, and simultaneous readings taken on both up to 350°. On comparing results, it was found that 330° by the acid-tube was 343° by the oil. This corresponds to the highest observation of the last water-tube. A comparison was finally made with acid-tube and standard in acid-bath at 270°, but this did not help to a nearer accordance.

§ 10. Granting that the higher temperatures are uncertain and doubtful in their exact position on the scale of temperature, the method of curves generally employed will cause the divergence to be gradual, so that the law of expansion, if it existed, would not by this cause be masked.

§ 11. Sulphuric acid has little more than half the expansibility of oil, but has the advantage of being colourless and easier managed. Glycerine was not employed, although it may be the best liquid upon the whole, being only one-fifth less expansible than oil. In using oil-tube thermometers it is not proper to regard observations under 100°.

§ 12. *Mode of conducting the observations.*—The figures 1, 2, 3, and 4 (Pl. I.) show the arrangements adopted. The heater, C C, is a thin brass tube 2 inches diameter and 27 inches long, held vertieal over Bunsen-flames by screws as shown in figs. 1 and 2. To a length of 8½ inches down from the top two slits were cut one inch wide and exactly opposite. To these were applied slips of plate glass (G G, fig. 2). One inch lower down a plate of talc was fixed to intercept the

upward current of heated air from the lower part and direct it upon the upper end of the tubes. An inspection of figs. 2, 3, and 4 will show the size and position of this plate, without which the observations would have been impracticable.

The tubes of oil and water O, W were suspended as shown, having a small cork stuck on at the lower end of the water-tube. The additional glasses on the slit near which the tubes hung were required to prevent loss of heat by radiation. A lens was used in reading the position of the bottom line of the capillary cup of the liquid on the scale. It is a very distinct object by transmitted light.

The only way to avoid the difficulty caused by corrosion of the glass, is to make the preliminary arrangement so that a single series of observations shall complete the range of temperature in as short a time as possible without expecting a second from the same tube. The temperature must always be kept ascending, and is regulated by the stopcock (*s*, fig. 1), which ought to be made, if possible, so that the angular motion should increase the flame at a uniform rate. The readings are taken when, although the temperature is still ascending, it is going very slow. The gas-cock is then touched slightly to increase the flame a little, then another observation, and so on. A windy day is to be avoided.

| Oil. | Water. | Temperature C.M. by | | Oil. | Water. | Temperature C.M. by | |
|---|---|---|---|---|---|---|---|
| Inch. | Half Inch. | Acid-tube. | Oil-tube. | Inch. | Half Inch. | Acid-tube. | Oil-tube. |
| | | Deg. | Deg. | | | Deg. | Deg. |
| −0·035 | 0·89 | 10 | 10 | +0·960 | 3·48 | 297·7 | 303·7 |
| +0·650 | 2·23 | 225·3 | 224 | 0·950 | 3·40 | 295·3 | 301·3 |
| 0·750 | 2·58 | 250 | 251 | 1·030 | 3·90 | 311·7 | 320·7 |
| 0·800 | 2·73 | 262 | 263·5 | 1·050 | 3·96 | 315·8 | 325·5 |
| 0·840 | 2·97 | 271 | 273·8 | 1·060 | 4·07 | 317·5 | 327·9 |
| 0·850 | 3·00 | 273·5 | 276·3 | 1·075 | 4·17 | 320·5 | 331·4 |
| 0·900 | 3·15 | 284·6 | 288·9 | 1·100 | 4·27 | 325·5 | 337·4 |
| 0·925 | 3·30 | 290 | 295·1 | 1·115 | 4·47 | 328·3 | 341·0 |
| 0·950 | 3·43 | 295·3 | 301·3 | 1·120 | 4·48 | 329·3 | 342·0 |

§ 13. The above series was completed in less than an hour; the tubes *in situ* are shown in fig. 3, and the top surface of liquids at the beginning and end are marked at

$\beta$ and $\epsilon$. The glass of the water-tube appeared but slightly acted on, and there was no sensible absorption when afterwards examined. No liquid had collected in the top cone, and the highest observation was quite distinct and unexceptionable.

§ 14. These observations will be found projected in fig. 8 (Pl. II., the readings of the water-tube being co-ordinate with the C.M. (Centigrade mercury) temperatures by acid-tube thermometer. The thick dotted line is drawn by means of a thin flexible straight edge bent round and held so as to suit the general trend of the points. The vertical dotted lines intercepted by the thick dotted line represent the ordinates to each 10° of the air-thermometer (C.A.), which were read off by scale as follows:—

| Deg. | | Deg. | |
|---|---|---|---|
| 320 C.A. | 4·486 | 260 C.A. | 2·835 |
| 310 „ | 4·068 | 250 „ | 2·660 |
| 300 „ | 3·745 | 240 „ | 2·490 |
| 290 „ | 3·455 | 230 „ | 2·350 |
| 280 „ | 3·230 | 220 „ | 2·222 |
| 270 „ | 3·023 | 210 „ | 2·102 |

The value of zero of scale is 6·36, and ·04 is half the depth of capillary cup, so that 6·4 added to each ordinate gives absolute liquid volume at each 10° of the air thermometer. The reading at 4°, the point of unit or minimum volume, is 0·89, to which adding 6·4 gives 7·29, the absolute liquid volume at 4°. If there was no correction required for expansion of glass and the density of vapour in the upper part of the tube, the quotient $\frac{6{\cdot}4 + 4{\cdot}486}{7{\cdot}29} = \frac{10{\cdot}886}{7{\cdot}29}$ would represent the volume of water at 320° C.A. in terms of volume unity at 4°. If the glass that contained the 7·29 of water were to assume the volume it has at 320°, the apparent volume 7·29 would be diminished ·047, $[\pi]$ being at the rate of $\frac{1}{5\,0\,0}$ per 100°, which has been assumed as a probable value. Again, if we find the volume that the steam in the upper part of the tube would obtain if condensed to water at 4° and call this $y$, then $\frac{10{\cdot}886}{7{\cdot}29 - (\pi + y)}$ is the true volume at 320°. To find $y$, the whole contents of the tube is 12·33, from which taking 10·89, leaves 1·44. The density of

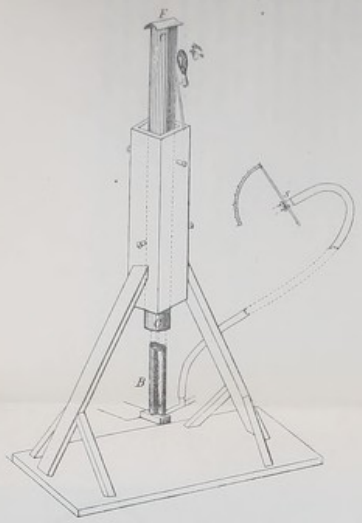



FIG. 1.—General View of Heating Apparatus.

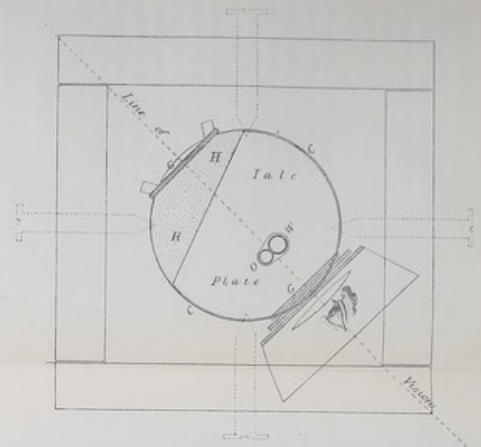



FIG. 2.—Plan of upper part of Heater. (Full size.)

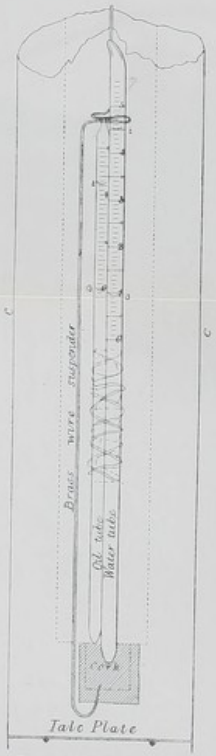



FIG. 3.—Front View of Tubes.
(Full size.)

FIG. 4.—Section showing effect of Talc Plate on Heating Current.
(Scale one-half.)

FIG. 5.—Top of Bunsen.

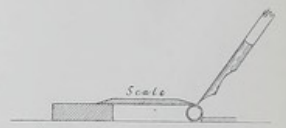



FIG. 6.—Side View of Scale Marking.

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steam at $t$ is expressed by the formula $\left\{\frac{t+76{\cdot}8}{h_1}\right\}^6 = \mathrm{D}$, which rests on Regnault's observations and the law of vapour-density (see *Phil. Mag.*, March 1858, Appendix I., and June 1861, § 11) [pp. 445, 456]. The density of steam at 100° in terms of water-unity at 4° being ·00061337, we have $\left\{\frac{100+76{\cdot}8}{h_1}\right\}^6 = {\cdot}00061337$, and hence $\log h_1 = 2{\cdot}78296$.

Thus D for $t = 320$ is $\left\{\frac{320+76{\cdot}8}{[2{\cdot}78296]}\right\}^6 = 0{\cdot}109$, and this, multiplied by 1·44 the volume of top space, gives $y = 0{\cdot}157$, and $\pi + y = {\cdot}204$, which, subtracted from 7·29, gives 7·086 as corrected denominator, and $\frac{10{\cdot}886}{7{\cdot}086} = 1{\cdot}5363$, the volume at 320° C.A.

§ 15. The second column of the following Table was thus computed. In the third column are the differences for 1°. These are projected as ordinates to the temperatures in fig. 9, then equalised as shown by the *small* dotted curved line. The ordinates to this line are read off by scale and set down in the fourth column as the equalised differences. These are values of $\frac{dv}{dt}$. Now, since the law of liquid expansion requires that these differences, divided by their respective volumes, should, when projected as ordinates to the temperatures, range in a conic hyperbola, the reciprocals, if the observations were perfect, ought to range in a straight line.

| 1. | 2. | 3. | 4. |
|---|---|---|---|
| Deg. | | | |
| 320 C.A. | 1·5363 | | |
| 310 ,, | 1·4742 | ·00621 | ·00592 |
| 300 ,, | 1·4280 | ·00462 | ·00486 |
| 290 ,, | 1·3856 | ·00424 | ·00410 |
| 280 ,, | 1·3518 | ·00338 | ·00356 |
| 270 ,, | 1·3204 | ·00314 | ·00313 |
| 260 ,, | 1·2912 | ·00292 | ·00292 |
| 250 ,, | 1·2645 | ·00267 | ·00267 |
| 240 ,, | 1·2385 | ·00260 | ·00243 |
| 230 ,, | 1·2171 | ·00214 | ·00220 |
| 220 ,, | 1·1978 | ·00193 | ·00198 |
| 210 ,, | 1·1800 | ·00178 | ·00178 |

The first reciprocal is $\frac{1{\cdot}5052}{{\cdot}00592} = 254{\cdot}3$, which in fig. 10 is set off as ordinate to 315°, and so on down to $\frac{1{\cdot}1890}{{\cdot}00178} = 668$, which is set off to 215°.

These points are distinguished by small circles. It will be remarked that they range in a flat ogee curve. The strong broken *straight* line is drawn through them and transferred to fig. 9, showing that if the equalised differences had been taken from the strong dotted line instead of the faint dotted line, the points in fig. 10 would have ranged exactly in a straight line, as theory requires. The difference this involves in the curve of observations (fig. 8) would hardly be sensible.

§ 16. Such accordance is indeed much greater than at first sight might be expected from the observations, which in fig. 8 are plotted in the rough from the note-book. But the method of drawing the curve through them by means of a flexible straight edge held in one position for the whole length accounts for this. If the law of continuity is maintained, the true line must be one of a similar kind with regular curvature; and if not exactly coincident with this, must intersect it with flat loops of very limited divergence, or be nearly parallel to it, because the straight edge is fixed from end to end of the range of points, and is assisted by the observations quoted in the previous paper; one of which, viz. that at 200°, is computed backwards, and marked off on fig. 8. In this figure a faint dotted line has been drawn below the one now discussed. It coincides with certain of the observations, which, from some recurring cause (connected with the increasing of the temperature by steps), appear on a lower level than the others. I have carefully analysed this curve in the same way as the first, and find the projection of the points in fig. 10 is so close to those given that it would only confuse to add them.

§ 17. The question now occurs, what is the value of $\frac{1}{\rho}$ given by the line A A? (see § 2 in original paper on Liquid Expansion, *Phil. Mag.*, June 1861) [p. 451 above].

The general equation that expresses the law is $v^{\frac{1}{\rho}} = \frac{k}{\gamma - t}$.

To find $\gamma$, we produce A A to meet the axis of temperature, which it does at 389°. This therefore is the value of $\gamma$ indicated by these observations. The value of $k$, which is equal to $\gamma - t$ when $v = 1$, must not be taken from the temperature 4° at which the volume of water is unity, because the curve of expansion departs from the theoretical curve at the lower part of its range. We must compute it from a scale-reading of an ordinate of the line A, as $n\,n$ at 245°, which is $517 = \frac{dt\,.\,v}{dv}$; and since $\frac{dt\,.\,v}{dv(\gamma - t)} = \frac{1}{\rho}$ and $\gamma - t = 144$, we have $\frac{1}{\rho} = \frac{517}{144} = 3{\cdot}59$.

What is the value of $\frac{1}{\rho}$ required by the theory that connects the expansion of a liquid with the density of its saturated vapour? By referring to § 3 of original paper, the rule is that $h \times \frac{1}{\rho}$ is a constant quantity F (French measures) for all bodies; and referring to the value of $h$ derived from M. Regnault's observations given in Appendix I. to paper in *Phil. Mag.*, March 1858, we find the theoretical value of $\frac{1}{\rho} = 3{\cdot}21$.

§ 18. The results hitherto have been with temperatures by the curve of the acid tube thermometer produced. I have drawn similar curves with the temperatures by the curve of the oil tube thermometer produced, and have projected the results on fig. 10, the points being shown by crosses and the straight line B B drawn through them. We have here a similar flat ogee range, and the value of $\frac{1}{\rho}$ that comes out by a process of computation similar to the above is 2·69. The mean is 3·14, which is remarkably close to 3·21.

§ 19. That the deviation of the acid-tube curve should be so nearly the same as that of the oil-tube and in the opposite direction, is a curious coincidence if the true line really lies, as it appears to do, halfway between A A and B B. While suspecting that there may be a latent cause for this (though each curve was as carefully determined as if the results depended on it alone), it cannot of course be said that $\frac{1}{\rho}$ has

been fully brought out. There may be a deviation in its value corresponding to the deviation in expansion that undoubtedly exists from about 185° downwards.

§ 20. With the view of obtaining further evidence on this point, I have examined by the differential process (detailed in § 27) the results of three series of observations of three of those tubes first experimented upon in which the temperature was carried up to from 260° to 280°. Assuming $\frac{1}{\rho} = 3{\cdot}21$, the values of $\gamma$ obtained by computing several of the values of $\frac{dv}{dt}$ at the higher temperatures accord with 411·6, varying from 400° to 430°. The thermometers employed with these water-tubes were linseed-oil tubes, but different from the last (represented in fig. 3), and somewhat longer and wider.

§ 21. To arrive at the probably true value of the highest temperature observed, we may divide the difference 12·3° (between the scale of acid- and oil-tubes) in the ratio of the values of $\frac{1}{\rho}$. Thus $3{\cdot}59 - 2{\cdot}69 = 0{\cdot}90$, and $3{\cdot}59 - 3{\cdot}21 = 0{\cdot}38$; so $\cdot 90 : \cdot 38 :: 12{\cdot}3° : 5{\cdot}2°$, which, added to 320°, gives 325·2° C.A. as the temperature at which the volume is 1·5363. We have to draw the curve $v^{3{\cdot}21} = \frac{k}{\gamma - t}$ through this point, touching or cutting the lower branch or verging into it. If we arrange the terms so as to make the theoretical curve touch the parabola which answers so exactly between 50° and 150°, we find that contact is impossible. This shows that there is a gradual rise from the parabola to the theoretical. Judging from the curve of differences, the deflection from theory commences about 210°; and taking 1·18 as the volume at this temperature, and drawing the curve (by computation) through this and the preceding observation at 325·2, we find $\gamma = 411{\cdot}6$, $k = 342{\cdot}8$, and

$$v^{3{\cdot}21} = \frac{342{\cdot}8}{411{\cdot}6 - t} \quad . \quad . \quad . \quad (Q)$$

is the equation for the volume of water in terms of its temperature from 210° upwards.

This cuts the lower parabola at about 185°. The intersection

is shown upon a large scale in fig. 11. Also the line of observations, which coincides with the parabola at about 175° and with the normal theoretical at about 190°.

§ 22. The annexed Table contains the general results of the inquiry.

Column 1 is the C.A. temperatures.

,, 2 is the volumes computed from (Q).

,, 3 is the observed volumes. Above 210° the mean of the oil and acid thermometers is taken.

,, 4 is the observed minus the computed volumes.

| 1. | 2. | 3. | 4. | 1. | 2. | 3. | 4. | 5. | 6. | 7. |
|---|---|---|---|---|---|---|---|---|---|---|
| Deg. | | | | | | | | | | |
| 411·6 | | | | 200 | 1·1622 | 1·1612 | − ·0010 | | | − ·0060 |
| | | | | | | | | ·0009 | | |
| 400 | 2·8715 | | | 190 | 1·1456 | 1·1455 | − ·0001 | | ... | − ·0049 |
| | | | | | | | | ·0012 | | |
| 390 | 2·3660 | | | 180 | 1·1299 | 1·1309 | + ·0010 | | ... | − ·0036 |
| | | | | | | | | ·0012 | | |
| 380 | 2·1014 | | | 170 | 1·1151 | 1·1173 | + ·0022 | | ... | − ·0020 |
| | | | | | | | | ·0013 | | |
| 370 | 1·9286 | | | 160 | 1·1011 | 1·1046 | + ·0035 | | ... | − ·0002 |
| | | | | | | | | ·0013 | | |
| 360 | 1·8038 | | | 150 | 1·0878 | 1·0926 | + ·0048 | | ... | + ·0017 |
| | | | | | | | | ·0013 | | |
| 350 | 1·7069 | | | 140 | 1·0752 | 1·0813 | + ·0061 | | ... | ·0037 |
| | | | | | | | | ·0014 | | |
| 340 | 1·6287 | | | 130 | 1·0632 | 1·0707 | + ·0075 | | ... | ·0058 |
| | | | | | | | | ·0017 | | |
| 330 | 1·5637 | | | 120 | 1·1517 | 1·0609 | + ·0092 | | ... | ·0079 |
| | | | | | | | | ·0018 | | |
| 320 | 1·5084 | 1·5098 | + ·0014 | 110 | 1·0407 | 1·0517 | + ·0110 | | ... | ·0103 |
| | | | | | | | | ·0021 | | |
| 310 | 1·4606 | 1·4584 | − ·0022 | 100 | 1·0302 | 1·0433 | + ·0131 | | ... | ·0128 |
| | | | | | | | | ·0024 | | |
| 300 | 1·4185 | 1·4181 | − ·0004 | 90 | 1·0201 | 1·0356 | + ·0155 | | ... | ·0154 |
| | | | | | | | | ·0027 | | |
| 290 | 1·3810 | 1·3804 | − ·0006 | 80 | 1·0104 | 1·0286 | + ·0182 | | | ·0183 |
| | | | | | | | | ·0030 | 3333 | |
| 280 | 1·3475 | 1·3479 | + ·0004 | 70 | 1·0011 | 1·0223 | + ·0212 | | | ·0213 |
| | | | | | | | | ·0034 | 2941 | |
| 270 | 1·3171 | 1·3182 | + ·0011 | 60 | 0·9921 | 1·0167 | + ·0246 | | | ·0247 |
| | | | | | | | | ·0036 | 2778 | |
| 260 | 1·2893 | 1·2896 | + ·0003 | 50 | ·9835 | 1·0117 | + ·0282 | | | ·0283 |
| | | | | | | | | ·0042 | 2381 | |
| 250 | 1·2640 | 1·2645 | + ·0005 | 40 | ·9752 | 1·0076 | + ·0324 | | | ·0325 |
| | | | | | | | | ·0047 | 2128 | |
| 240 | 1·2405 | 1·2397 | − ·0008 | 30 | ·9671 | 1·0042 | + ·0371 | | | ·0371 |
| | | | | | | | | ·0052 | 1923 | |
| 230 | 1·2190 | 1·2186 | − ·0004 | 20 | ·9594 | 1·0017 | + ·0423 | | | ·0423 |
| | | | | | | | | ·0060 | 1667 | |
| 220 | 1·1986 | 1·1986 | 0 | 10 | ·9519 | 1·0002 | + ·0483 | | | ·0483 |
| | | | | | | | | ·0072 | 1389 | |
| 210 | 1·1799 | 1·1800 | + ·0001 | 0 | ·9446 | 1·0001 | + ·0555 | | | ·0555 |
| | | | | | | | | ·0087 | 1149 | |
| 200 | 1·1622 | 1·1612 | − ·0010 | − 10 | ·9376 | 1·0018 | + ·0642 | | | ·0644 |

These differences, it will be observed, increase regularly from 190° downwards. We have here presented to us the effect of the abnormal cause which constrains the molecules of water and raises them to a greater volume than they would assume at the respective temperatures if they had retained the character of a perfect liquid. Do the numbers in this column indicate any quantitative law of divergence? This has been tested by tabulating the differences of these numbers in column 5, and the reciprocals of these differences in column 6. These reciprocals have been laid off as ordinates to the temperatures

in fig. 13 (Plate III.). It will be observed that the points range in the straight line $s\ s$ nearly. This line meets the axis of temperature at $-47{\cdot}8°$. Let $w$ represent a number in column 4 at temperature $t$. The values of $\frac{dw}{dt}$ are given in column 5, and of $\frac{dt}{dw}$ in column 6. Fig. 13 represents $\frac{dt}{dw} \propto (t + 47{\cdot}8)$. If this were a governing law, we should have $w \propto \log (t + 47{\cdot}8)$, which affords the numerical equation

$$c\left\{\log \frac{t + 47{\cdot}8}{47{\cdot}8}\right\} = {\cdot}0555 - w,$$

in which $\log c = 8{\cdot}94037$. The numbers in column 7 have been computed from this equation: on comparing them with those in column 4, it will be remarked that it exactly represents the curve of observed expansion below 100°, but that above that the observed volumes gradually depart from it and enter the curve of normal liquid expansion at about 190°. The curve of column 7 crosses the normal at 159°, and at this temperature the observed volume is ·0036 above the point of intersection, an amount which represents 3°.

§ 23. It may be that the equation for column 7 is only to be considered as empirical; yet it may be remarked that if it expresses a physical law, that law is very simple, and has reference to a limiting temperature, as do also the laws of saturated vapour and normal expansion. The equation $\frac{dt}{dw} \propto (t + 47{\cdot}8)$ means that the *absolute* increment of divergence from the normal liquid volume increases with a descending temperature in the inverse ratio to the distance of that temperature from the limiting point $-47{\cdot}8$. The normal law of expansion is, that the *proportionate* increment of volume at a given temperature increases in the inverse ratio of the distance of that temperature from the upper limit $\gamma = 411{\cdot}6°$.

§ 24. Since the law of liquid expansion has all the appearance now of being general, and assumes the character of the quantitative exponent of perfect liquidity, it may be as well, before concluding this paper, to describe the method of projecting

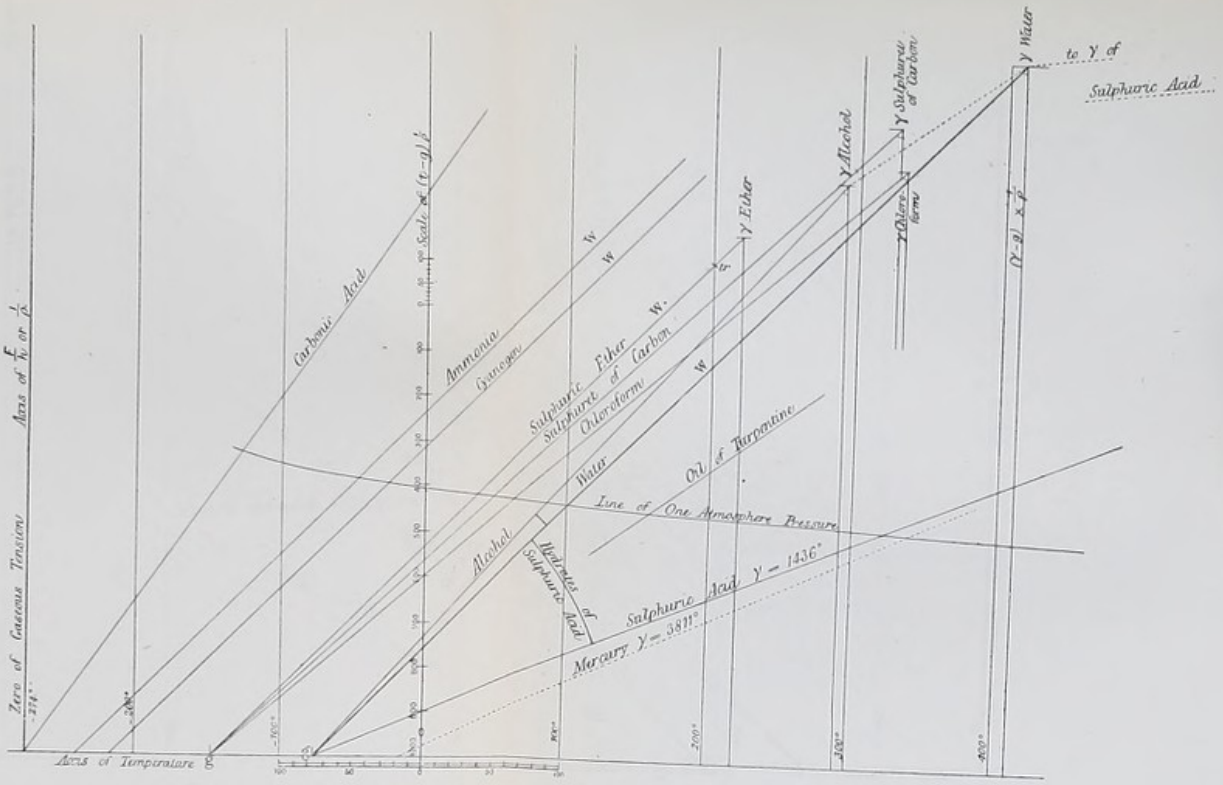
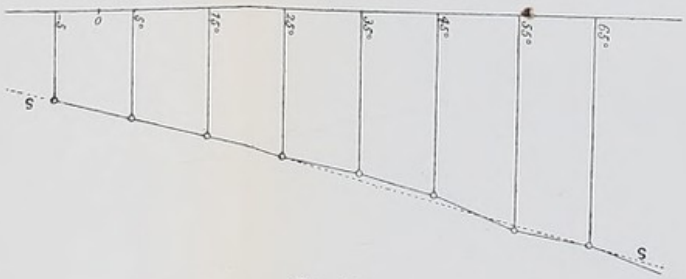

Zero of Gaseous Tension
Axis of $\frac{F}{R}$ or $\frac{1}{\rho}$
Axis of Temperature
Scale of $(t-g)^2$
Carbonic Acid
Ammonia
Cyanogen
Sulphuric Ether
Sulphuret of Carbon
Chloroform
Water
Oil of Turpentine
Alcohol
Hydrates of Sulphuric Acid
Line of One Atmosphere Pressure
Sulphuric Acid $\gamma = 1436°$
Mercury $\gamma = 5811°$
$\gamma$ Ether
$\gamma$ Alcohol
$\gamma$ Sulphuret of Carbon
$\gamma$ Chloroform
$\gamma$ Water
to $\gamma$ of Sulphuric Acid
$(\gamma - g) \times \frac{1}{\rho}$

FIG. 12.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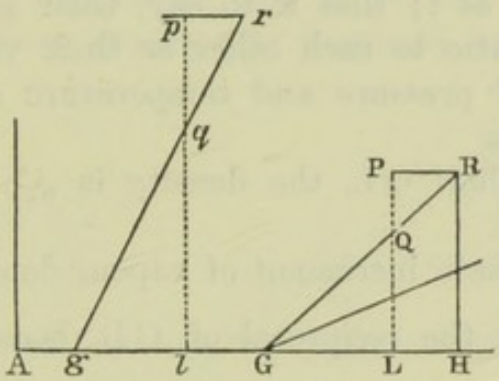
S

FIG. 13.

on a general chart the line which represents both the vapour and liquid condition of a body throughout its range of temperature.

The law of density of the saturated vapour is $\left\{\frac{t-g}{h}\right\}^6 = \Delta$, and of the liquid $\left\{\frac{\gamma - t}{k}\right\}^\rho = \mathrm{D} = \frac{1}{v}$, or $v^{\frac{1}{\rho}} = \frac{k}{\gamma - t}$. The connexion between them is that $h \propto \rho$, or $\frac{1}{h} \propto \frac{1}{\rho}$.

On the axis of temperature A H take A G $= g$ and A H $= \gamma$, reckoning all temperatures $t$ as well as $\gamma$ from A the zero of




gaseous tension. Draw R H $\perp$ to A H, and set off H R $= (\gamma - g)\frac{1}{\rho}$. Join G R and draw R P $\parallel$ to axis. At a point L corresponding to a given temperature $t =$ A L draw the $\perp$ L Q P, cutting G R in Q. We have P Q a constant function of the density of the liquid in terms of the temperature, and Q L a constant function of the density of the saturated vapour in terms of the temperature. P R is $(\gamma - t)$, and P Q is $(\gamma - t)\ \frac{1}{\rho} = \frac{vdt}{dv}$; so that the inverse of P Q represents the proportionate increment of volume or decrement of density for 1 degree. Again, we have G L $= (t - g)$, and Q L $= (t - g)\frac{1}{\rho}$; and since $\frac{h}{\rho} = \mathrm{F}$ (the constant 504), we have $\frac{1}{h} = \frac{1}{\rho \mathrm{F}}$ and $\frac{t-g}{h} = (t-g)\frac{1}{\rho \mathrm{F}} = \frac{\mathrm{Q\,L}}{504}$; and since the sixth power of $\frac{t-g}{h}$ is equal to density $\Delta$, we

have $\frac{\mathrm{QL}}{504}$ to represent the sixth root of the density of the saturated vapour—taking the word density to mean the quotient of the pressure or tension by the temperature reckoned from the zero of gaseous tension (A).

Suppose $g\,r$ to represent the chart-line of another body, say cyanogen, and $p\,q\,l$ to be the vertical of $t_1$. If $p\,q = \mathrm{P\,Q}$, then the value of the proportionate increment of volume of cyanogen at $t_1$ would be the same as the proportionate increment of water at $t$, and so on; if it is double or half, the proportionate increment is double or one-half. If $q\,l = \mathrm{Q\,L}$, the gaseous density of cyanogen vapour at $t_1$ is equal to the gaseous density of water vapour at $t$; that is to say, their absolute densities bear the same ratio to each other as their vapours do when *in æquilibrio* of pressure and temperature at 15° separated from their liquids.

If $q\,l$ is one-half Q L, the density is $\frac{1}{64}$, being the sixth power of one-half.

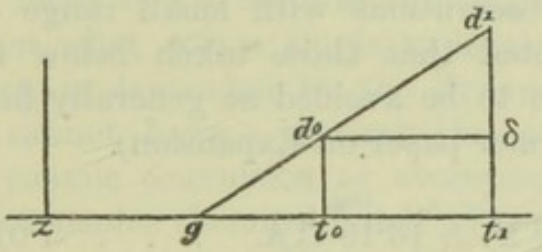
The proportionate increment of vapour-density for 1 degree is represented by the reciprocal of G L, because $\frac{d\Delta}{\Delta} = \frac{6dt}{t-g}$. Thus, suppose $l\,q = \mathrm{L\,Q}$, but $g\,l = \frac{1}{2}\mathrm{G\,L}$, the proportionate increment of density in the vapour represented by the line $g\,l$ for rise of 1° is double that of the vapour represented by G L, although their densities are equal.

§ 25. The simplicity of the relation between the proportionate increments of density in the vapour and in the liquid is noteworthy. In the vapour the proportionate increment of density is $\frac{6}{t-g}$; in the liquid it is $\frac{1}{(\gamma-t)\frac{1}{\rho}}$. A numerical example will illustrate this. Let $g = 0°$ and $\gamma = 100°$, and $\frac{1}{\rho} = 3$, and suppose $t = 50°$. The proportionate increment of vapour-density for 1° is $\frac{6}{\rho}$, or eighteen times that of the liquid; the first being $\frac{6}{300}$, and the second being $\frac{1}{150}$. When they are equal, $\frac{6}{\rho}(\gamma - t) = (t - g)$, or $t = 94{\cdot}8°$. Thus, to find

the point when these proportionate increments are equal, the rule is to divide $(\gamma - g)$ by $\left(\frac{6}{\rho} + 1\right)$ and take the quotient from $\gamma$.

It is remarkable that in alcohol and ether this corresponds very exactly with the point of transition. Is the temperature of this point always to be thus found? If so, what is the transition-point of water? $\gamma - g = 489$, $\frac{6}{\rho} + 1 = 20{\cdot}26$; so $\frac{489}{20{\cdot}26} = 24$, and $\gamma - 24^\circ = 388^\circ$ C.A. $= 399^\circ$ C.M.

§ 26. As an example of the mode of laying down a line upon the chart, chloroform may be taken. We have observations of its vapour-tensions by Regnault, and of its expansion by M. Pierre.




At $60^\circ$ C.M. $= 60{\cdot}48$ C.A. $= t_0$ its tension is 738 millims.
At $100^\circ$ C.M. $= 100^0$ C.A. $= t_1$ its tension is 2354·6.

$$\left\{\frac{738}{274 + 60{\cdot}48}\right\} = 1{\cdot}1410, \quad \left\{\frac{2354{\cdot}6}{274 + 100}\right\}^{\frac{1}{8}} = 1{\cdot}3588.$$

The difference is 0·2178, which, divided by $t_1 - t_0$, gives $7{\cdot}74124 = \log \frac{1}{h}$, which, added to log F = 2·70285, gives

$$0{\cdot}44406 = \log \frac{1}{\rho},$$

and thus $\frac{1}{\rho} = 2{\cdot}758$. This, multiplied by $t_1 - t_0$ or $d_0\delta$, gives $\delta d_1$; and since

$$\delta d_1 : \delta d_0 :: d_0 t_0 : t_0 g,$$

we have $t_0 g = 207$; and since $t_0 z = 274 + 60{\cdot}48$, we have $zg = 127{\cdot}5$ and $gt_1 = 246{\cdot}5$. This gives the position of $g$ on the axis of temperature. To find $t_1 d_1$, we multiply $gt_1 = 246{\cdot}5$

by $\frac{1}{\rho} = 2{\cdot}758$, and set off the product 685 from the vertical scale of the chart as the length of $t_1 d_1$. Having thus found $g$ and $d$, the chart line is drawn.

Densities deduced from high tensions are generally less than the true, in consequence of deviation from Mariotte's law, the pressure increasing in somewhat less proportion than the density. On this account the vapour-tensions taken below the boiling-point are to be preferred in fixing the *direction* of the chart line.

§ 27. To find the upper terminal $\gamma$ of the chloroform line, we have M. Pierre's observations (*Ann. de Chim.*, October 1851, p. 208) to apply to the equation $(\gamma - t)\frac{1}{\rho} = \frac{v\,dt}{dv}$, in which $\gamma$ is the unknown quantity. This equation is adapted to a series of dilatation observations with small range of temperature. It is to be noted that those taken below the atmospheric temperature are to be avoided as generally faulty (see figures appended to former paper on Expansion).

| Deg. | Deg. | | |
|---|---|---|---|
| 15·90 C.M. | = 16·19 C.A. | . . . | 1·0187 |
| 23·40 | 23·79 | . . . | 1·0276 |
| | 7·60 = $dt$ | | ·0089 = $dv$ |
| | | | 1·0231 = $v$ |

Hence

$$\gamma = \frac{vdt\rho}{dv} + t = 334°.$$

The two extreme observations at 0° and 62·72° being computed in the same way, give $\gamma = 329°$. So this value is probably not far from the truth.

§ 28. In fig. 12 (Pl. III.) is presented a small chart with the lines of a few bodies computed and laid down as above, chiefly from the observations of Regnault. The parallelism of certain lines (as those marked W), and the convergence of others to the same point in the axis of temperature, is well marked out.

§ 29. It would be well if the constants $g$, $\gamma$, and $\frac{1}{\rho}$ or $\frac{F}{h}$ were fully determined for the various bodies in a state of

purity where it is practicable. They are supplementary to specific gravity and boiling-point; and when tabulated both numerically and graphically, would perhaps carry us a step further on the road that connects the physical with the chemical.

The description of a body with regard to its quantitative physical characteristics embraces:—(1) specific gravity; (2) boiling-point; (3) constants of its chart line; (4) constitution of a gaseous volume in volumes or fractional parts of a volume of its constituents [*e.g.* $HO^{\frac{1}{2}} = 9$, which at once exhibits its chemical constitution and specific gravity in the gaseous or vaporous form; so also $H^{1\frac{1}{2}}N^{\frac{1}{2}} = 8{\cdot}5$ (ammonia), $OS^{\frac{1}{3}} = 32$ (sulphurous acid), $O^{1\frac{1}{2}}S^{\frac{1}{3}} = 40$ (sulphuric acid), $O^{\frac{1}{2}}N = 22$ (protoxide of nitrogen), $O^{\frac{1}{2}}N^{\frac{1}{2}} = 15$ (binoxide of nitrogen), etc.

A fractional index to a symbol as showing that in the act of chemical union what was a single molecule has separated into two or more, is demanded by the dynamical theory of gases; and we cannot have a distinct idea of the potential nature of the dynamic convulsion or evolution of a dynamic integral that accompanies chemical union without keeping both the symbolic letters and their indices united in idea. No other indices can be substituted; for although others may define the *chemical* constitution, they do not define the *molecular* constitution, which involves the antecedent separation as well as consequent union.

MM. Deville and Troost have by heat alone separated S into $S^{\frac{1}{3}}$; we are therefore justified in entertaining the idea of the ultimate molecule as an integral part of a gas or vapour being capable of subdivision. How far this may go, it would be improper to limit, as the forces at the command of the chemist are so insignificant in comparison to the heat and pressure with which the elements of matter have to contend in the body of the sun or even below the surface of our planet. But it is daily becoming more manifest that the elementary molecule, although minute beyond conception, is to be studied as a microcosm essentially dynamical in its internal constitution, its apparent statical condition being simply the antagonism of transcendant *vis viva* potents]; (5) molecular volume;

(6) other points with reference to specific heat, latent heat, combining-force, compressibility, capillarity, which may be expected gradually to assume a more simple aspect as materials for comparison accumulate.

*June* 15, 1863.

*Appendix.*

I. To compute the weight of a cubic inch of a liquid and of its saturated vapour at any temperature within range from the constants of its chart-line.

The general expression for the weight $u$ (in grains) of one cubic inch of saturated vapour at $t$ (C.A. temp.) is

$$\frac{m\delta}{\tau + 274}\left\{\frac{t + 274 - g}{\tau + 274 - g}\right\}^6 = u,$$

in which $\log m = 0{\cdot}77262$, $\delta$ = vapour-density of the body on the hydrogen-scale (*e.g.* if the body is water, $\delta = 9$; if mercury, $\delta = 101$); $\tau$ = the C.A. boiling temperature under pressure 760 millims. $\left[\text{obtained from } \frac{760}{\tau + 274} = \left\{\frac{t + 274 - g}{h}\right\}^6\right]\left[\frac{1}{h} = \frac{1}{504\rho}\right]$.

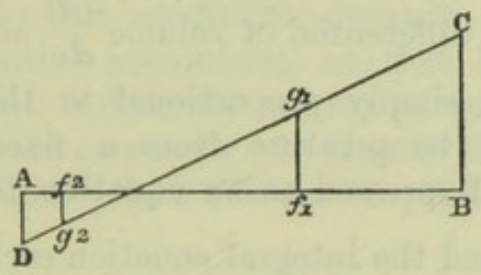
The general expression for the weight W (in grains) of one cubic inch of a liquid is $N\left\{\frac{\gamma - t}{\gamma}\right\}^\rho = W$, in which N is the weight of a cubic inch at 0° C.

The details of the above may be found in § 11 of previous paper on liquid expansion (*Phil. Mag.*, June 1861) [p. 456 above].

II. The complete method of observing in sealed tubes.

If two or more tubes are filled with the same liquid in different proportions of their volume and then sealed, the increase of density in the vapour and of volume in the liquid throughout the range of temperature may be obtained distinctly as separate phenomena, independent of any theory, from readings of their apparent liquid volume. Also the number of tubes employed above two affords the means of checking their general accuracy. *E.g.*, the liquid volume in No. 1 tube (given in decimals of the volume of the whole tube considered

as unity) at the lower temperature being $m_1$, and at the upper temperature $n_1$, on the line A B reckoned as unity mark off $Af_1 = n_1$, and the ordinate $f_1g_1 = n_1 - m_1$. In the same way for No. 2 tube, lay off $Af_2 = n_2$ and $f_2g_2 = n_2 - m_2$, which, if a negative quantity, is marked off below A B. The points $g_1$, $g_2$, etc., for any number of tubes 1, 2, etc., range in a straight line. Draw perpendiculars at A and B meeting the line $g_1, g_2$, etc., in D and C. It may be proved that (the expansion of glass being allowed for) $\frac{A\,D}{A\,B}$ = difference of vapour-density at upper and lower temperature in terms of the density of




the liquid at lower temperature considered as unity. Also $\frac{A\,B - B\,C}{A\,B}$ = density of the liquid at the upper temperature in the same terms.

The curves of the readings of several such tubes about 4 inches long provide the ordinate $fg$; and if extended to sufficiently high terms, afford data to compute the chart-line independently of boiling-point or any observation of tension. A perfectly unknown liquid treated in this way can be established on the chart.

(See observations on the density of vapours and their liquids at the point of transition, *British Association Report*, 1853. Also paper in *Royal Society's Archives* for 1853) [p. 371 above].

# ON LIQUID EXPANSION

(From the *Philosophical Magazine* for May 1864.)

In the *Philosophical Magazine* for June 1861 there is an account of a presumed law of liquid expansion which represents the proportionate differential of volume $\frac{vdt}{dv}$ at a given temperature $t$, as being simply proportional to the inverse of the distance of that temperature from a fixed upper limiting temperature $\gamma$. Expressed as an equation, this presumed law is $\frac{vdt}{dv} \propto \gamma - t$; and the integral equation corresponding to this differential showed a seeming relation to the law of vapour-density by an apparent constancy in the value of F (see above-mentioned paper, § 3), the product of the coefficient of saturated vapour density of a body by the exponent of the liquid density of the same body. I have for some time past been employed in testing this by means of M. Pierre's extensive series of experiments on liquid dilatation, taken in conjunction with those on vapour-tension by M. Regnault, lately published in his second volume of *Memoirs*. A large proportion of the liquids were the same in both, and chemically pure, so that the data are valuable for testing the law supposed to connect the volume of a liquid with the density of its saturated vapour at different temperatures, although not sufficient otherwise to test the law of liquid expansion by itself. But if F maintained its value at the lower range (that is, *under* the boiling temperature of the liquids), to which M. Pierre's researches were confined, as well as at the upper range, to which my observations in sealed tubes were chiefly directed, the integrity of the presumed law at all temperatures might with some certainty be inferred. Now, on projecting the co-ordinates of M. Regnault's vapour-

tensions below the boiling-points of the respective liquids, according to the mode previously described (viz. $\left(\frac{p}{t+274}\right)$ laid off as ordinate to $t$), I find that the points for the most part range admirably in a line, and that a large proportion of those lines (including those of mercury and sulphur), when produced downwards, meet (below the axis of temperature) at the zero of gaseous tension $-274°$ (the apparent parallelism previously noted being actually a convergence).

This so far confirms the law of vapour-density, and gives a very accurate value for the coefficient $h$ of each vapour respectively. The exponent $\rho$ of the liquid, that ought to have a constant ratio to this coefficient, depends on the value of the second differential expansion; so that if the series of observations are not sufficiently correct or regular to give a tolerably approximate constancy of value to the second difference, they will fail to afford the test required.

The equation is

$$\frac{1}{\rho} = \frac{1}{t_1 - t_0}\left(\frac{v_1 dt}{dv_1} - \frac{v_0 dt}{dv_0}\right) \text{ and } \frac{1}{\rho} \times h = \mathrm{F} = 504.$$

But instead of this value of F being maintained, after examining graphically and otherwise a considerable number of M. Pierre's tables of observations, I find that it varies from 280 to 380, and that on an average it is less at the lower than at the higher temperatures.

The discrepancy being so great, and always in one direction, has gradually undermined my confidence in the presumed law of liquid expansion.

M. Drion's researches on the dilatability of volatile liquids, published in the *Annales de Chimie* for May 1859, I had unfortunately previously overlooked entirely, although two of the liquids chosen, viz. sulphurous acid and muriatic ether, are perhaps the best that could be selected for testing a theory, as they expand upwards of one-third of their volume on being heated to about 130°, and below this limit of temperature the difference in the march of the thermometers of air and mercury is too small to cause embarrassment. Also they are specifically distinct in their chemical relations—one having its elements

O and $S^{\frac{1}{2}}$ in a burnt condition, and the other, $H^2C.H^{\frac{1}{2}}Cl^{\frac{1}{2}}$, having one part burnt ($H^{\frac{1}{2}}Cl^{\frac{1}{2}}$), and the other part ($H^2C$) unburnt.

On projecting these observations graphically, they are found to be wonderfully free from irregularities; so I trust to not having over-estimated the importance of the results obtained from their graphical analysis, which is presented in the accompanying chart.

Instead of the *proportionate* expansion for 1° being inversely as the temperature below an upper fixed limit, it will be remarked that the evidence is in favour of the *absolute* expansion for 1° having this ratio. Both values are laid down, the inverse of the *proportionate* expansion for 1° ranging along *a a* and *c c*, the inverse of the *absolute* expansion for 1° ranging along *b b* and *d d*. By employing a straight edge, the exact accordance or discordance of the observations with either hypothesis may be ascertained.

According to the previous hypothesis (the proportionate), the points in *a a* should not only range in a straight line, but that line ought to be S S, or at least parallel to it, if F held the constant value 504. Also the points in *c c* should range in M M. They do so approximately, it will be remarked, in the upper part; and this perhaps is the cause of the mistaken induction, as it was the upper part that was chiefly relied upon in my sealed-tube observations.

The following are M. Drion's observations from which the muriatic ether points were computed (*Ann. de Chim.*, vol. lvi., p. 21):—

| | Temperature. | Volume. | | Temperature. | Volume. |
|---|---|---|---|---|---|
| | Deg. | | | Deg. | |
| No. 1 | 0 | 1·0000 | No. 8 | 83 | 1·1647 |
| 2 | 12·6 | 1·0195 | 9 | 93·5 | 1·1946 |
| 3 | 26·4 | 1·0428 | 10 | 103 | 1·2248 |
| 4 | 35·0 | 1·0586 | 11 | 112 | 1·2567 |
| 5 | 45·5 | 1·0787 | 12 | 120·5 | 1·2908 |
| 6 | 59·0 | 1·1067 | 13 | 128 | 1·3249 |
| 7 | 71·5 | 1·1354 | 14 | 133 | 1·3507 |

The differentials are computed from the couples 1 and 4, 2 and 5, 3 and 6, etc.; *e.g.* for the first couple $dt = 35°$, $dv = \cdot0586$, and $t$, the middle temperature, is 17·5°; so $\frac{dt}{dv} = 597$, which is the ordinate to 17·5 for the first point in the line $d\,d$. The corresponding first point in the line $c\,c$ is obtained by multiplying the last, viz. 597, by 1·029, the mean volume between Nos. 1 and 4. This gives $\frac{vdt}{dv} = 615$ as the ordinate to be laid off to 17·5, the mean temperature as before. The following Table is thus computed:—

Muriatic Ether.

| Couples. | $t$. | $\frac{dt}{dv}$. | $\frac{vdt}{dv}$. |
|---|---|---|---|
| | Deg. | | |
| 1 and 4 | 17·5 | 597 | 615 |
| 2 ,, 5 | 29·0 | 559 | 587 |
| 3 ,, 6 | 42·7 | 509 | 545 |
| 4 ,, 7 | 53·3 | 474 | 520 |
| 5 ,, 8 | 64·2 | 436 | 488 |
| 6 ,, 9 | 76·2 | 392 | 450 |
| 7 ,, 10 | 87·2 | 350 | 413 |
| 8 ,, 11 | 97·5 | 315 | 381 |
| 9 ,, 12 | 107·0 | 281 | 348 |
| 10 ,, 13 | 115·5 | 250 | 319 |
| 11 ,, 14 | 122·5 | 223 | 290 |

The following are M. Drion's observations from which the points for sulphurous acid were computed (*Ann. de Chim.*, vol. lvi., p. 35):—

| | Temperature. | Volume. | | Temperature. | Volume. |
|---|---|---|---|---|---|
| | Deg. | | | Deg. | |
| No. 1 | 0 | 1·0000 | No. 8 | 82·5 | 1·2058 |
| 2 | 12·6 | 1·0230 | 9 | 91·0 | 1·2393 |
| 3 | 26·4 | 1·0509 | 10 | 100·0 | 1·2796 |
| 4 | 35 | 1·0698 | 11 | 108·5 | 1·3238 |
| 5 | 49·5 | 1·1048 | 12 | 115·5 | 1·3664 |
| 6 | 62·5 | 1·1403 | 13 | 122·0 | 1·4133 |
| 7 | 72·5 | 1·1710 | 14 | 126·8 | 1·4538 |

The computed differentials are in the following Table:—

Sulphurous Acid.

| Couples. | $t$. | $\frac{dt}{dv}$ | $\frac{vdt}{dv}$. |
|---|---|---|---|
| | Deg. | | |
| 1 and 4 | 17·5 | 498 | 518 |
| 2 ,, 5 | 31·0 | 452 | 481 |
| 3 ,, 6 | 44·5 | 402 | 442 |
| 4 ,, 7 | 53·7 | 371 | 416 |
| 5 ,, 8 | 66·0 | 327 | 377 |
| 6 ,, 9 | 76·7 | 288 | 342 |
| 7 ,, 10 | 86·2 | 254 | 312 |
| 8 ,, 12 | 99·0 | 205 | 264 |
| 9 ,, 14 | 108·9 | 167 | 225 |

By laying a straight edge along the line $b\,b$, it will be found that all the points except the lowest and two highest are very exactly in a straight line, which, produced downwards, crosses the zero vertical at 564 on the scale, and which, produced upwards ($\beta\,\beta$), intersects the axis of temperature in 157·5, which is $\gamma_s$, the upper limiting temperature of sulphurous acid. Now

$$\frac{564}{157{\cdot}5} = 3{\cdot}581 = \mathrm{P}_s,$$

and the integral of the differential equation gives

$$\text{hyp. log } \gamma_s - \text{hyp. log } (\gamma_s - t) = \mathrm{P}_s(v - 1),$$

which is reduced to common logarithms by dividing $\mathrm{P}_s$ by 2·3026.

A straight edge laid along $d\,d$ shows that the points range tolerably in a line $\delta\,\delta$ that passes through 663 on scale of the zero vertical and 187° of the axis, which is $\gamma_m$, the upper limiting temperature of muriatic ether, and $\frac{663}{187} = 3{\cdot}545 = \mathrm{P}_m$.

The faint dotted lines $a\,a$ and $\epsilon\,\epsilon$ are the computed *loci* of $\frac{vdt}{dv}$, corresponding to $\beta\,\beta$ and $\delta\,\delta$ straight lines, assumed to be the *loci* of $\frac{dt}{dv}$ at the extreme upper part of the range.

It will be remarked that $\mathrm{P}_s = \mathrm{P}_m$ very nearly in these two liquids; but this must be an accidental coincidence, as the

numerical value of P obviously varies inversely as the numerical value which may be arbitrarily given to the volume at 0°.

There are various considerations with respect to molecular volume, and the relation of $h$ and $\gamma$ reckoned from the zero of gaseous tension, that it would be proper to enter upon if it were decided that this is the true general law of liquid expansion; but, as in the former case, we may be too hasty in this. I only hope that what is here set forth may lead to further inquiry. The simplicity of the new differential equation is greater than the previous one—a material point in its favour, since true progress and simplicity seem always to go together.

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*Note.*

If the true theory of liquid expansion has now been discovered, other symmetrical relations are likely to appear, and more especially in those bodies of similar composition, as the hydrochloric, hydrobromic, and hydriodic ethers, which have fortunately been experimented upon both by M. Regnault and by M. Pierre.

The following will serve to show how far their observations strictly respond to certain relations presumed to exist between the constants of the thermomolecular lines of those bodies.

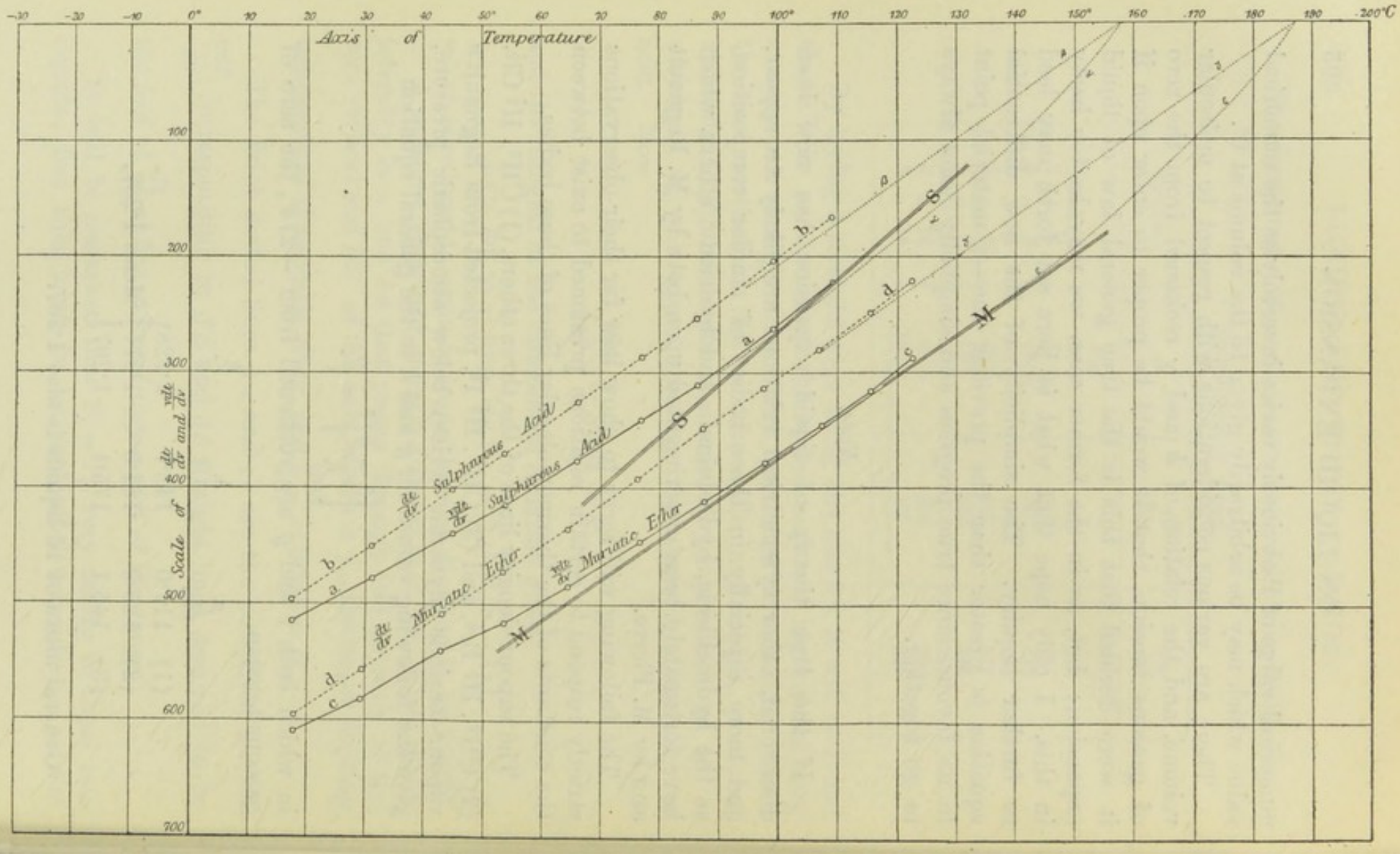
The vapour-density lines of the three ethers, (1) $CH^2 . H^{\frac{1}{2}} Cl^{\frac{1}{2}}$, (2) $CH^2 . H^{\frac{1}{2}} Br^{\frac{1}{2}}$, and (3) $CH^2 . H^{\frac{1}{2}} I^{\frac{1}{2}}$, projected from Regnault's vapour-tensions (dynamic series) below atmospheric pressure, give the following values for $g$ and $h$ in the general equation

$$\left\{\frac{t-g}{h}\right\}^6 = \frac{p}{t}.$$

in which both $t$ and $g$ are reckoned from $-274°$, the zero of gaseous tension.

| | $g$. Deg. | $h$. | $\frac{h}{g}$. | |
|---|---|---|---|---|
| (1) | 117·0 | 143·7 | 1·228 | Mean 1·237. |
| (2) | 125·9 | 161·0 | 1·277 | |
| (3) | 145·1 | 175·1 | 1·207 | |

General mean of 16 liquids is also 1·237.


-30 -20 -10 0° 10 20 30 40 50° 60 70 80 90 100° 110 120 130 140 150° 160 170 180 190 200°C
Axis of Temperature
100
200
300
400
500
600
700
Scale of $\frac{dt}{dv}$ and $\frac{vdt}{dv}$
$\frac{dt}{dv}$ Sulphurous Acid
$\frac{vdt}{dv}$ Sulphurous Acid
$\frac{dt}{dv}$ Muriatic Ether
$\frac{vdt}{dv}$ Muriatic Ether
a
b
c
d
S
M

It will be observed that the numbers in the two columns follow the same ratio very nearly, showing that the lines all meet at the zero-point of gaseous tension,—a very significant fact in molecular physics. To this central point or ether-node (as it may be termed) converge the lines of the greater number of the vapours experimented upon by M. Regnault. But water, alcohol, and sulphuric acid hydrates are notable exceptions; and there are others, viz. pyroxylic spirit, pyroxylic ether, sulphurous acid, and oxalic ether, which seem to trend towards the water-node.

The liquid expansion lines $\left(\text{as those on chart marked } \frac{dt}{dv}\right)$ of the same three ethers, projected from M. Pierre's observations of expansion, give the following values for G, the upper limiting temperature reckoned from the zero of gaseous tension (or $G = 274 + \gamma$), and the molecular volumes at the same zero $\mu = \frac{\text{V.D.}}{\text{sp. gr.}}$, or quotient of the vapour-density of the ether by its specific gravity at $-274°$, computed by means of the equation

$$\{\log \gamma - \log (\gamma + 274)\} \frac{2{\cdot}302}{P} = (v - 1).$$

| | G. | $\mu$. | $\frac{G}{h}$. | $\frac{h}{\mu}$. | |
|---|---|---|---|---|---|
| (1) | 460·0 | 26·0 | 3·22 | 55·3 | |
| (2) | 534·4 | 27·8 | 3·32 | 57·9 | Mean 56·5. |
| (3) | 597·4 | 31·1 | 3·41 | 56·3 | |

It will be remarked that the numbers in columns G and $\mu$ are nearly proportional to those above in columns $h$ and $g$. The same equality of ratio is found in other cases.

There are also certain apparent relations of harmony in the absolute measure of liquid cohesion of a molecule as deduced from the latent heat of its vapour. Thus the latent heat of a *molecule* of water is exactly the same as that of a molecule of alcohol at 0°.

The latent heat of a molecule of hydrochloric ether is to that of hydriodic ether as the initial molecular volumes at $-274°$?

EDINBURGH,
*March* 16, 1864.

# ON CERTAIN THERMOMOLECULAR RELATIONS OF LIQUIDS AND THEIR SATURATED VAPOURS.

(From the *Philosophical Magazine* for February 1868.)

§ 1. In the *Annales de Chimie* for May 1859 there is an account of some experimental researches on the dilatability of volatile liquids by M. Drion; and in the *Philosophical Magazine* for May 1864 I have shown, by a graphical analysis of M. Drion's results, that they indicate, as a general law of liquid expansion, that the inverse of the *absolute* increment of volume diminishes regularly as the temperature ascends. For example, if a liquid expands $\frac{1}{1000}$ at 0°, and $\frac{1}{950}$ at 10° per 1° rise of temperature, it will be found that at 20° the expansion is $\frac{1}{900}$, at 30° it is $\frac{1}{850}$, at 40° it is $\frac{1}{800}$, and so on to 200°, where the rate would be $\frac{1}{0}$, considered purely as a mathematical law. The unit is taken as the volume at a certain fixed temperature, say 0° C. Thus in the above example the expansion of $\frac{1}{800}$ at 40°, and $\frac{1}{750}$ per 1° at 50° means the $\frac{1}{800}$ and $\frac{1}{750}$ of the volume it had at 0°, *not* the $\frac{1}{800}$ and $\frac{1}{750}$ of the volume at the respective temperatures 40° and 50°. The first is the *absolute* increment, and the second is the *relative* increment. M. Drion's observations on muriatic ether and sulphurous acid were so regular that it was distinctly brought out by them that the law has reference to the absolute increment of volume, not to the relative increment, as I had at first deduced from my own observations in sealed tubes at high temperatures.

§ 2. This was a fortunate discovery, because on extending the inquiry to the numerous chemically pure liquids experimented upon by M. Pierre,* other relations make their appearance that confirm the general law, and further develop

* *Annales de Chimie et de Physique*, vols. xv., xix., xx., xxi., xxxi.

an interesting connection between molecular volume, liquid cohesion, and saturated vapour-density, which we are enabled to trace distinctly by means of M. Regnault's great and admirable labours published in the twenty-first and twenty-sixth volumes of the *Memoirs* of the French Academy.

§ 3. Although a good deal of labour is required to bring out the numerical results from the observations, the thermo-molecular relations are simple enough in their nature. I will endeavour to explain them concisely by diagram before proceeding to give the evidence upon which they are founded, and for simplicity will confine the attention to two liquids chemically analogous,—the hydrobromic and hydriodic ethers.

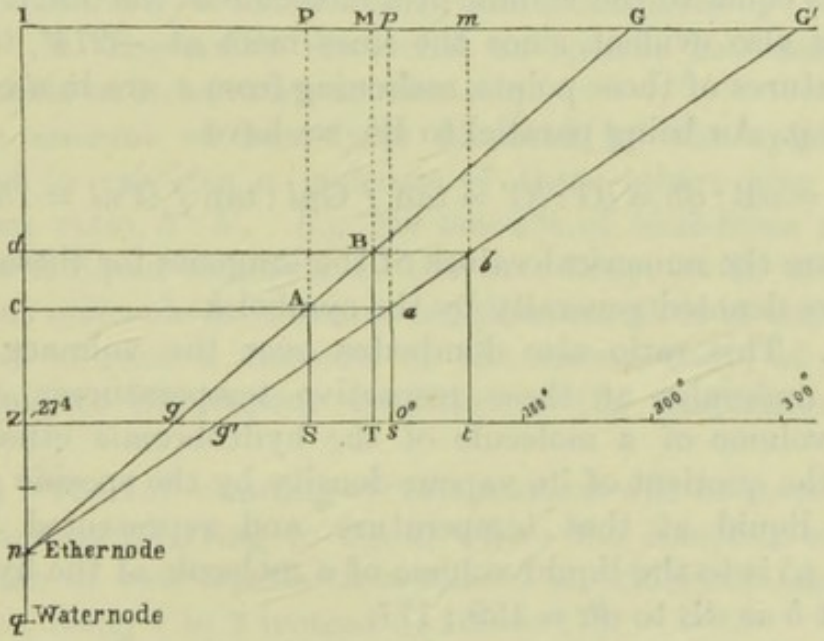



Fig. 1.

In fig. 1 the lines *n*ABG, *nab*G', that represent both the saturated-vapour density and the liquid expansion of these bodies, are obtained from M. Regnault's observations of pressure under the boiling-point (those taken by the dynamic method are preferred). A temperature $t$, reckoned from $-274°$, the zero of gaseous tension, is represented by $z$S, and SA is the *sixth root* of the quotient of the corresponding pressure by $z$S. The other observations of pressure (under the boiling-point) of hydrobromic ether treated in the same way give points which lie in a straight line AB. Similarly the pressure-

observations of hydriodic ether give points which lie in a straight line $ab$; and these lines produced *meet at* $n$, which is in the line $nzl$, a perpendicular to $z$S (the axis of temperature) passing through $z$.

§ 4. In a chart thus constructed, the pressure corresponding to a given point B is equal to the sixth power of BT (the ordinate of that point) multiplied by $z$T its abscissa, and, comparing two points B and $b$ equidistant from the axis, the density is the same at each if the vaporous body is the same; or the number of molecules in a volume is the same if the vaporous body is different. Hence if, in two lines AB, $ab$, points are taken equidistant from $z$T, the *volume* of a molecule at one is equal to the volume of a molecule at the other.

It is also evident, since the lines meet at $-274°$, that the temperatures of those points, reckoning from $z$, are in a constant ratio: *e.g.*, A$a$ being parallel to B$b$, we have

$$c\mathrm{A} : ca = a\mathrm{B} : db = l\mathrm{G} : l\mathrm{G}' = \tan \angle \mathrm{G}nl : \tan \angle \mathrm{G}'nl = 159 : 177,$$

which are the numerical values of the tangents for these ethers. They are denoted generally by the symbol $h$.

§ 5. This ratio also dominates over the volumes of the *liquid* molecules at these respective temperatures; *e.g.* the liquid volume of a molecule of the hydrobromic ether at B (being the quotient of its vapour-density by the specific gravity of the liquid at that temperature, and represented by the symbol $\mu$) is to the liquid volume of a molecule of the hydriodic ether at $b$ as $d$B to $db = 159 : 177$.

§ 6. It has now to be explained how these lines represent the expansion of the respective liquids throughout their range of temperature.

Suppose we had complete Tables of the expansion of these two liquids, the first having reference to a volume considered as a unit at A, and the second having reference to a volume considered as unity at $a$; also let the volumes for each degree in the case of the first liquid be given, and in the second the volumes for each $\frac{177}{159}$ degree be given. The Tables will be found to be *identical*; and computing the upper limit of temperature for each, it is found that, reckoned from $-274°$, they are in the ratio of $159 : 177 = h : h'$.

§ 7. It follows also that if $\frac{1}{\mathrm{PA}}$ represent the rate of expansion per degree at A, $\frac{1}{\mathrm{MB}}$ is the rate at B, and $\frac{1}{pa} = \frac{1}{\mathrm{PA}}$ is also the rate at $a$ of the second liquid; but we have to keep in mind that the unit of volume, as well as the increment of temperature, is $\frac{177}{159}$ time the absolute value of the unit and increment of the first.

Thus the behaviour of the two liquids and their saturated vapours are precisely the same at absolute temperature proportional to their liquid molecular volume.

The evidence is nearly complete that in the same category are to be found hydrochloric ether and chloroform.

§ 8. M. Regnault's researches on specific heat and latent heat enable us to advance another step.

The amount of heat-force (denoted by the symbol $\mathrm{C}\omega$) required to vaporise *a molecule* of these ethers also follows the same ratio, $h : h'$. *E.g.* the amount of heat-force required to raise a liquid molecule of the first body, at B, to become a vapour molecule at the same temperature $z\mathrm{T}$ is to the amount required to raise a molecule of the second body, at $b$, from the liquid to the vapour condition at the temperature $zt$ as 159 : 177.

§ 9. The full meaning of this relation will be more clearly made out by referring to fig. 2, where the complete range of expansion of two liquids thus related is exhibited,—the ratio, however, being 1 to 2 instead of 159 to 177.

It is assumed that the law of expansion extends downwards to $-274°$ at A. At this zero we start with the molecular volume of one double that of the other [$\mathrm{AB} = 2ab$]. In the axis of temperature take $\mathrm{AC} = 2ac$, the corresponding molecular volumes are still in the ratio of 2 to 1; and this is the case all the way up to E, $e$, the points of transition, where the maximum volume, when liquid condition terminates, is nearly three times the initial volume at $z$ computed from theory. From F and $f$ draw $\mathrm{F}\Delta'\Delta\mathrm{G}$, $f\delta'\delta g$ parallel to axis. If $\mathrm{D}\Delta$ is the value of $\mathrm{C}\omega$ of the larger molecule at C, $\mathrm{D}'\Delta'$ is the value of the same at C'; $d\delta$ is the value of $\mathrm{C}\omega$ of the smaller molecule at $c$, and $d'\delta'$ the value of the same at $c'$.

The ratio of 2 to 1 is common to all these; $D\Delta = 2d\delta$, etc.; and the thermomolecular lines of these two liquids will be represented on fig. 1 if we suppose $n$G to be drawn so that $l\mathrm{G} = \frac{1}{2}l\mathrm{G}'$.

The remarkable point to keep in view is that C$\omega$ and $\mu$

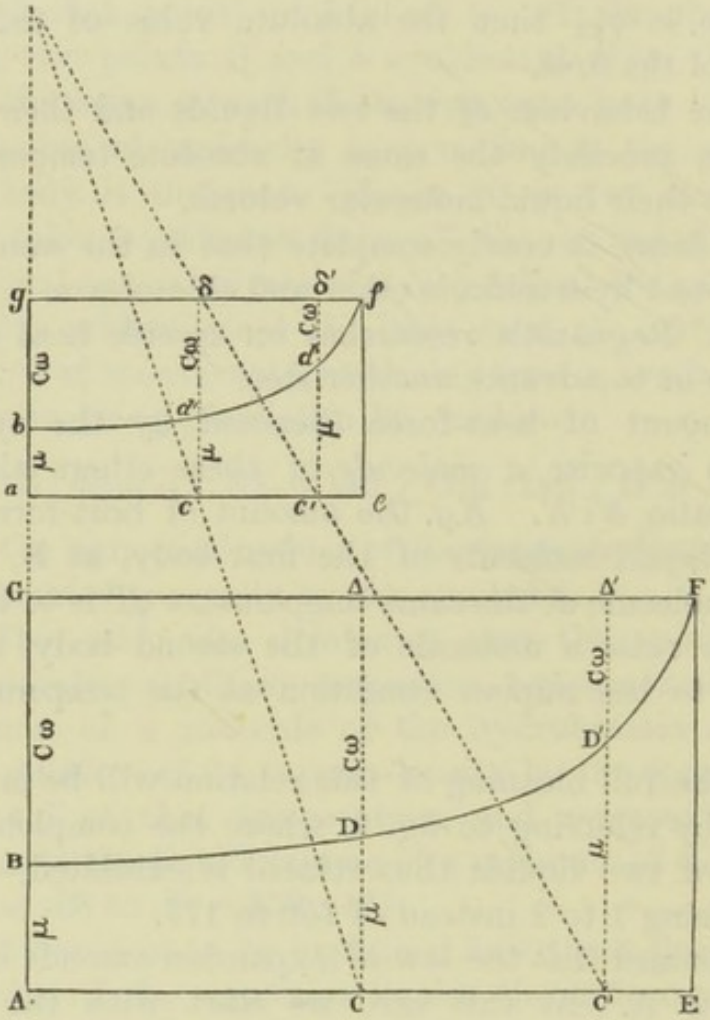



Fig. 2.

(molecular volume) follow the same ratio. The same *absolute* increment of $\mu$ is accompanied with the same *absolute* decrement of C$\omega$ at whatever part of the range it may be measured, either in the liquid with the small molecules, or in that with the large.

Take, for example, 300 cubic inches of muriatic ether at $t$ temperature, and apply heat so that its volume may increase to 301, and let the quantity of heat absorbed or rendered

latent be denoted by $k$. Take 30 cubic inches of the same liquid at temperature $t$, and apply heat so that its volume may increase to 31. The quantity of heat absorbed or rendered latent is still $k$. Take 278 cubic inches of chloroform at temperature T, and apply heat so that its volume may increase to 279, the quantity of heat absorbed is still $k$.

In another cluster of liquids the value of $k$ is the same for each, but it is different from the $k$ of the first set.

§ 10. The tangent of angle $Gnl$ (fig. 1), termed $h$, is proportional to $C\omega$ generally in all liquids that possess $n$, the ether-node, when $C\omega$ is taken at absolute temperatures respectively proportional to $h$. Most of the bodies treated by Regnault have their vapour-lines directed to this point. Even mercury and sulphur exactly conform to it. We may thus infer that the vapour-lines of most of the bodies not experimented upon are also related to the same node, but not all.

§ 11. Water, whose vapour-line is very exactly determined by the different series of observations published in the twenty-first volume of the *Memoirs*, cuts $zn$ produced in $q$, and $zq = \frac{3}{2}zn$. To this point, called the water-node, converge the vapour-lines of pyroxylic spirit and methoxylic ether; there is also evidence that other bodies of a special character belong to it.

§ 12. There are sixteen bodies, chemically pure, whose lines of expansion, as well as of saturated vapour-density, have been determined. In nine the upper limiting temperature (G), derived from the expansion-observations being laid off on the vapour-line, are found to range parallel to the axis of temperature. In three of the sixteen the observations are confined to too small a range and are too irregular to give G with certainty; but the average of several trials makes it likely that they also conform. Of the remaining four, mercury has its upper limit four times as far removed from the axis as the preceding.

§ 13. The line of alcohol-vapour does not trend to either node; it is specially related to the line of water-vapour by crossing it near where it meets the axis. At this point also the lines of sulphuric-acid vapour and hydrates intersect;

hydrated alcohol also. These are the only cases of such axial relation yet developed. The position of the upper limit of expansion of these bodies has not yet been determined.

§ 14. It is remarkable that *zl* (fig. 1) is just three times *zn*. What recondite principle determines this ratio of 1 to 3? Why is the vapour-line determined by the sixth root of density? Space has *three* rectangular *dimensions* and *six* rectangular *directions*; and these ratios continually make their appearance in the dynamical theory of gases, in consequence of the relation of space to *resolved forces*. It is natural to think that this is the parent of the triad in both cases.

§ 15. Another relation, differing from the above, is exemplified in two analogous chlorides. The vapour-line of the trichloride of phosphorus, $Cl^{1\frac{1}{2}}\ P^{\frac{1}{2}}$ (boiling-point 74°), trends to the ether-node (Plate III.). If the vapour-line of the trichloride of arsenic, $Cl^{1\frac{1}{2}}\ As^{\frac{1}{2}}$ (boiling-point 134°), trends to the same, we have two lines related as those in fig. 1; but the volume of the liquid molecule of one measured at A is *equal* to that of the other measured at *a* instead of being as $h : h'$.

### *Evidence of the Law of Saturated-vapour Density.*

§ 16. The law has reference to density, not pressure, and does not appear subject to deviation. To obtain density from pressure we have to divide the pressure by the temperature reckoned from the zero of gaseous tension, −274°; that is, in cases where the deviation from the laws of Mariotte, Dalton, and Gay-Lussac is too small to be noticed, as it is assumed to be in Regnault's observations of pressure under the boiling-point. Each observed pressure in millims. was divided by its temperature reckoned from −274°; the sixth root of the quotient was computed and laid off by scale as ordinate to the temperature: *e.g.* steam at 100°, pressure 760 millims., $\left\{\frac{760}{374}\right\}^{\frac{1}{6}} = 1{\cdot}1254$ is the ordinate which in Plate I. is laid off to a scale of $13\frac{1}{3}$ inches to unity, the scale of temperature being 10 inches to 100°.

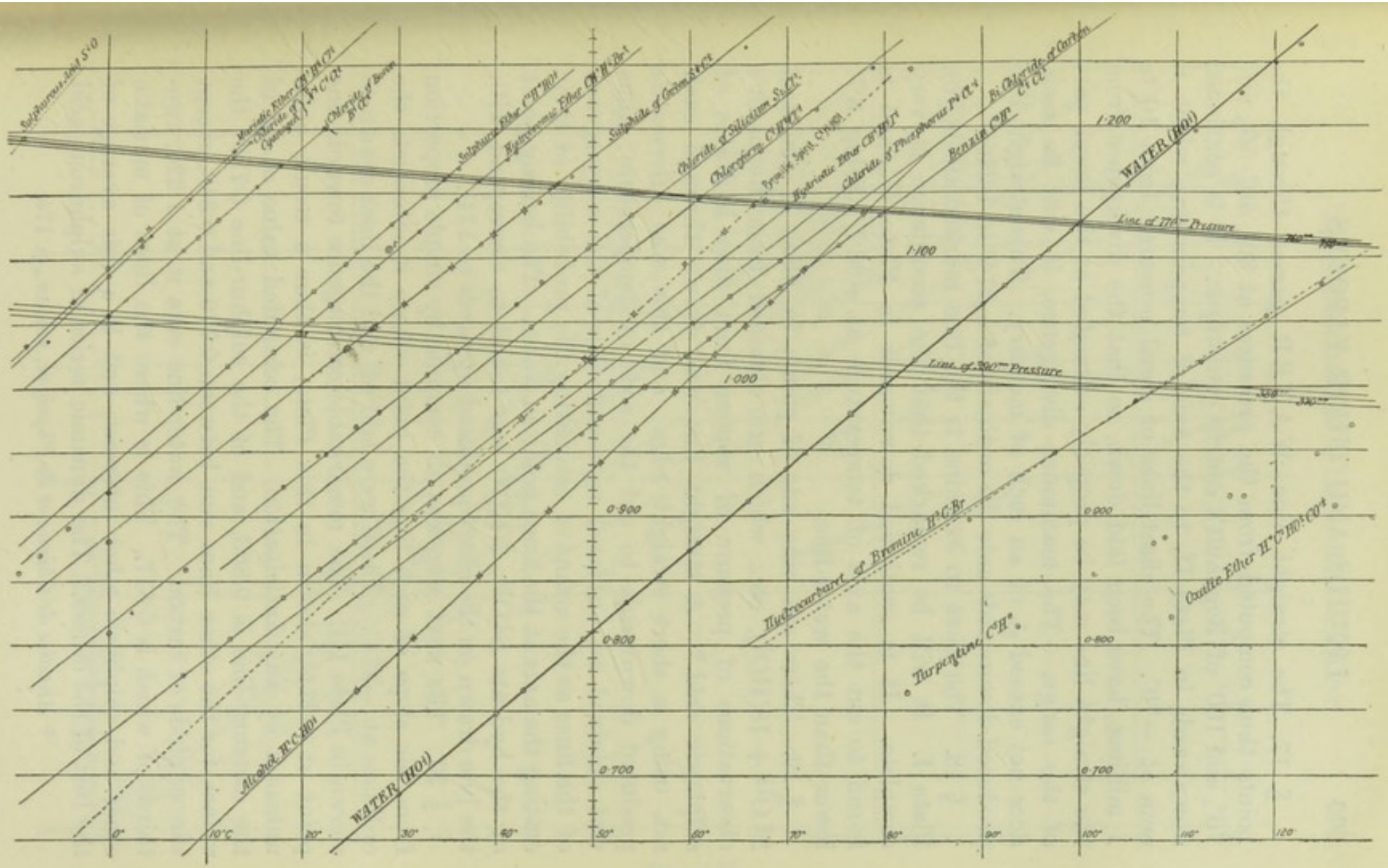

Sulphurous Acid $S^1O^2$
Muriatic Ether $C^4H^{10}Cl^2$
Chloride of Carbon (Quadruple) $C^1Cl^4$
Chloride of Boron $B^1Cl^3$
Sulphuric Ether $C^8H^{10}O^1$
Hydrocyanic Ether $C^3H^3Ae^1$
Sulphide of Carbon $S^1C^2$
Chloride of Silicium $Si^1Cl^4$
Chloroform $C^1H^1Cl^3$
Pyro-Ac. Spirit $C^3H^6O^1$
Hydriodic Ether $C^4H^{10}I^2$
Chloride of Phosphorus $P^1Cl^3$
Benzole $C^6H^6$
Bi-Chloride of Carbon $C^2Cl^4$
1·200
WATER (HO)$^1$
Line of 770$^{mm}$ Pressure
1·100
Line of 500$^{mm}$ Pressure
1·000
0·900
0·900
Hydrocarbure of Bromine $H^4C^4Br^1$
Oxalic Ether $H^{10}C^6O^4$
0·800
0·800
Turpentine $C^{10}H^8$
Alcohol $H^6C^4HO^1$
WATER (HO)$^1$
0·700
0·700
0°
10°C
20°
30°
40°
50°
60°
70°
80°
90°
100°
110°
120°

PLATE I.

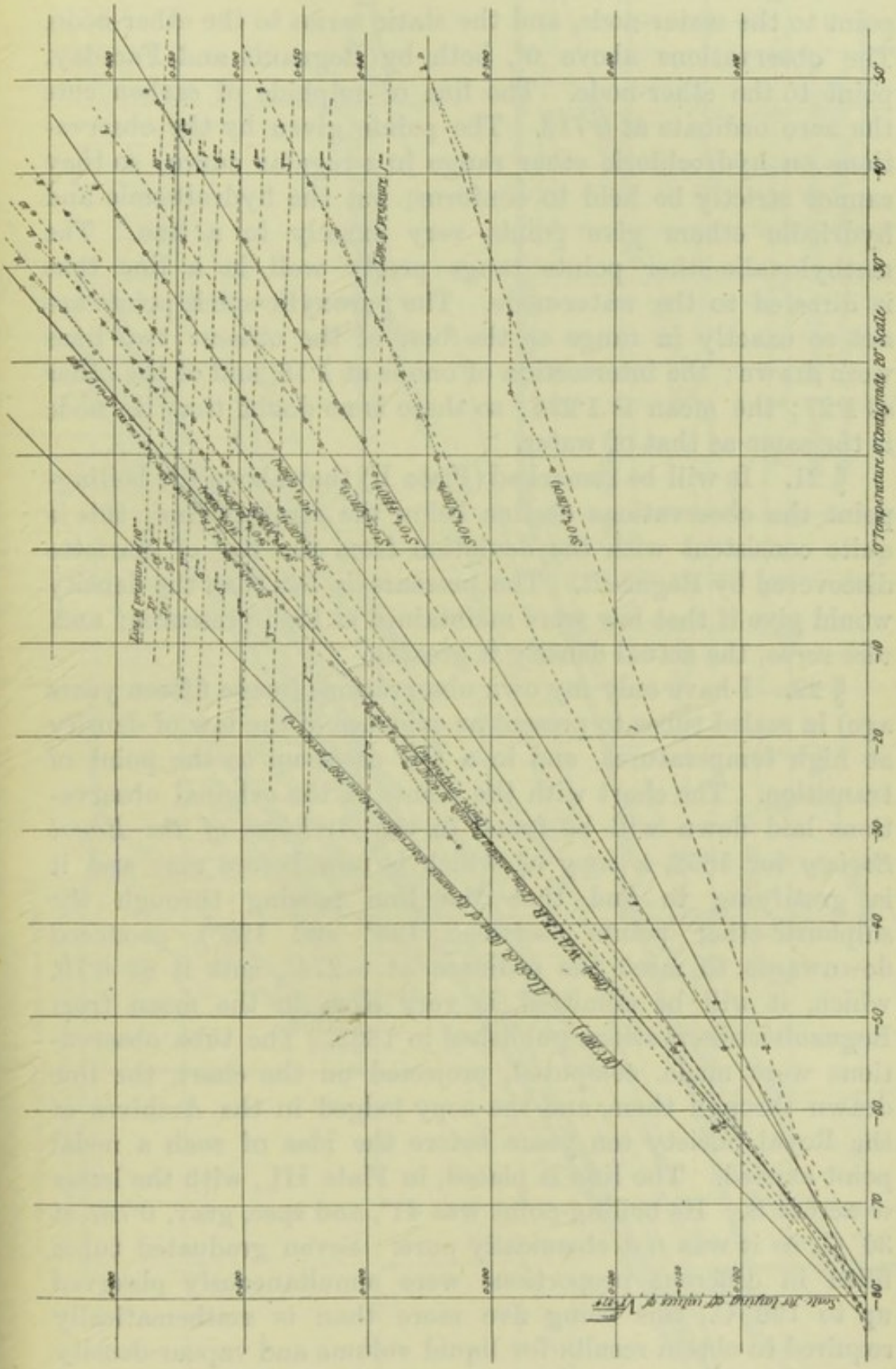
§ 17. The straight line WATER passes through the points thus computed from the pressures at 30°, 40°, 50°, 60°, 70°, and 100° of Regnault's *courbe graphique*. It is continued downwards in Plate II. to the axis of temperature, which it cuts at −79°. The chart-lines of equal pressure, from 10 to 1 millims., have been laid down, so that the divergence from the straight line may be properly estimated at the lower part of the range. The maximum divergence, it will be seen, does not exceed $\frac{1}{50}$ of an inch of mercury. This straight line produced downwards cuts the ordinate of −274° in 1·226.

§ 18. The next to examine is the line marked alcohol on Plate I. It will be remarked that the accordance is very complete. It is continued downwards in Plate II., and is found to cut the axis of temperature at −82·7°, being 3·7° lower than the water-line.

§ 19. There are nine sets of points in Plate II., marked $S^{\frac{1}{3}} O^{1\frac{1}{2}} + 18(HO^{\frac{1}{2}})$, etc., which are obtained from Regnault's observations of pressure of vapour from nine hydrates of sulphuric acid.* A straight line was drawn through each set, using a short straight edge; and this was *afterwards* produced downwards to cut the axis of temperature: thus was obtained an unbiased representation of the convergence of the lines as the means of estimating the probability of their cutting the axis at the same point as water. The boiling-point of the hydrate $S^{\frac{1}{3}} O^{1\frac{1}{2}}$, $3(HO^{\frac{1}{2}})$ was found to correspond with the line drawn on the chart produced upwards to 171°.

§ 20. The next step was to compute by simple proportion from the slope of each line the point where it intersects the ordinate at −274°. These were laid off and the lines drawn as shown in Plate III. The remarkable convergence towards one point at −274°, shown in this Plate, is thus a result quite unbiased by any anticipation. The computed value of $zn$ of the mercury-line is 0·803, and of the sulphur-line 0·799; the values for the other lines run between 0·776 and 0·843, except the sulphide of carbon. The water-line cuts it at 1·226, two-thirds of which is 0·817. This is where the lines of muriatic ether and sulphuric ether intersect, and is a fair average of the best of the others. The dynamic series of sulphurous acid

* *Annales de Chimie et de Physique*, vol. xv., p. 174.


Centigrade Scale
Temperature
Loss of pressure
Loss of pressure
Loss of pressure

point to the water-node, and the static series to the ether-node. The observations above 0°, both by Regnault and Faraday, point to the ether-node. The line of sulphide of carbon cuts the zero ordinate at 0·712. The points given by the observations on hydrochloric ether range in a regular curve; so they cannot strictly be held to conform; but the hydrobromic and hydriodic ethers give points very exactly in a line. The methyloxalic-ether points range pretty well in a line that is directed to the water-node. The pyroxylic-spirit points are not so exactly in range as the best of the others: two lines were drawn; the intersection of one is at 1·18, and of the other at 1·27; the mean is 1·225; so there is no doubt that its node is the same as that of water.

§ 21. It will be remarked (Plate I.) that above the boiling-point the observations decline below the straight line; this is quite consistent with the deviation from the law of Mariotte discovered by Regnault. The pressure is *less* than the density would give if that law were maintained at high pressures; and, *vice versa*, the actual density is greater.

§ 22. I have only my own observations (made fifteen years ago) in sealed tubes to present as evidence of the law of density at high temperatures, and in a few cases up to the point of transition. The chart with the points of the original observations laid down will be found in the *Archives of the Royal Society* for 1852, a copy of which is now before me; and it is gratifying to find that the line passing through the sulphuric-ether points (between 125° and 190°), produced downwards to meet the ordinate at −274°, cuts it at 0·79, which, it will be admitted, is very close to the mean from Regnault's observations published in 1862. The tube observations were made, computed, projected on the chart, the line drawn through them, and the copy lodged in the Archives of the Royal Society ten years before the idea of such a nodal point existed. The line is placed, in Plate III., with the letter *w* across it. Its boiling-point was 41°, and spec. grav. 0·737 at 30° C., so it was not chemically pure. Seven graduated tubes filled in different proportions were simultaneously observed up to 190° C., this being five more than is mathematically required to obtain results for liquid volume and vapour-density

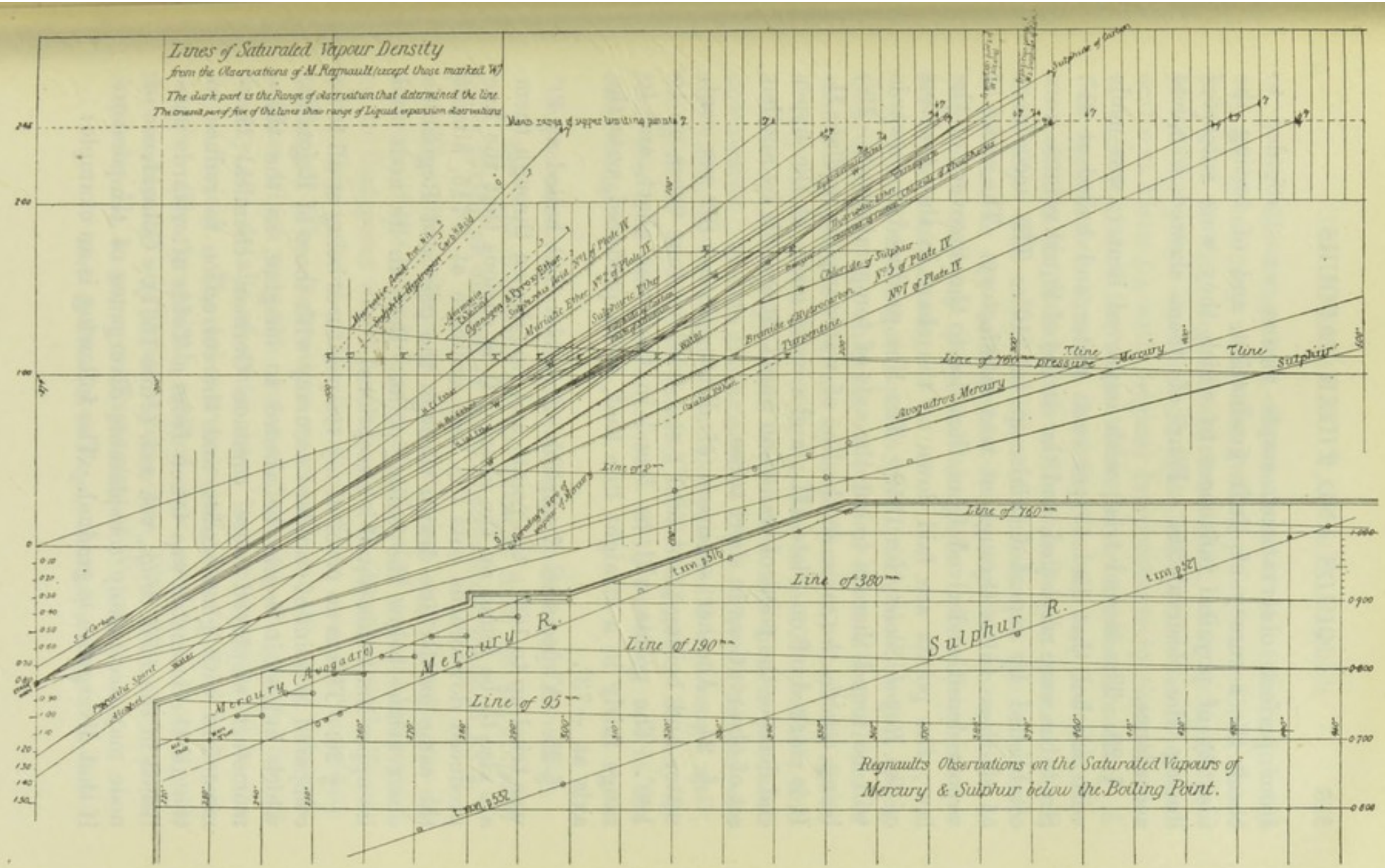

Lines of Saturated Vapour Density
from the Observations of M. Regnault (except those marked W)
The dark part is the Range of observation that determined the line.
The onward part of five of the lines show range of Liquid expansion observations
Line of 760mm
Tline Pressure
Tline Mercury
Tline Sulphur
Avogadro's Mercury
Line of 0mm
Line of 760mm
Line of 380mm
Line of 190mm
Line of 95mm
Mercury R.
Sulphur R.
Mercury (Avogadro)
t xxvi p 516
t xxvi p 527
t xxvi p 532
Regnaults Observations on the Saturated Vapours of
Mercury & Sulphur below the Boiling Point.

PLATE III.

from perfect observations, ample means were afforded to check the accuracy of their graduation, and of testing the identity of physical influences to which they were subjected during the observations. Four of them showed complete accordance.

§ 23. The second trial, with improved heater, was made with alcohol that had 19 per cent. water and boiled at 81°. Six tubes were employed, and the curves of liquid volume were established by repeated trials up to 216°. The accordance of all these tubes throughout was satisfactory. The series of vapour-densities derived from them led to the discovery of the law; the points are laid down on the chart in the *Archives of the Royal Society* for 1852. The line produced downwards will be found thereon to cut the axis of temperature at −79°, being *exactly* the same as the line of water-vapour in Plate II. It is marked off on Plate III.; and a cross mark at the upper end denotes the point of transition found afterwards by another set of observations in short tubes.

§ 24. Another specimen of dilute alcohol (62 per cent. water and boiling-point 86·5°) was tried in four tubes up to 190°. The points will be found in the same chart, and to range fairly in a straight line that, when produced, cuts the axis at −73°.

§ 25. A specimen of sulphide of carbon that boiled at 49° was tried up to 160°; the points and line drawn through them are also to be found on the chart. It is curious that the line produced downwards cuts the zero ordinate at 0·715, almost the same as the line drawn through the points of Regnault's observations below the boiling-point. Both in its node and its upper limit this liquid is exceptional.

§ 26. This is no proof of my temperatures being at all to be compared in point of standard accuracy with those of Regnault, which indeed it would be absurd to imagine, as the thermometers employed were common German chemical thermometers with glass scales, and the correction to reduce to the air-thermometer was taken from a Table afterwards published in the *Phil. Mag.*, vol. xxi. (June 1861). Coincidence of node may accompany considerable divergence of temperature if that divergence is gradual. The following is an example:

§ 27. M. Avogadro's observations of the tension of the vapour of mercury under the boiling-point * are to be found projected on the original chart of 1852. I have again projected the points on chart R, Plate III. (which has the same scales as Plate I.), with original temperatures by thermometer of mercury, and the same corrected to air-thermometer. A line was drawn through the latter as shown; and this line was afterwards transferred to the small-scale general chart, Plate III. The exact direction of this line to the ether-node is curious, as the boiling-point is nearly 20° below that of Regnault's mercury projected alongside. The temperatures are by air-thermometer.

§ 28. A few other liquids were tried, each in two tubes. One of these, the chloride of sulphur (a small specimen of which was prepared for me by a professional chemist), shows a very exact conformity with the law from 160° to 210°; and a curious anomaly presents itself when this line is produced downwards. It cuts the axis at −274°. The dichloride was also tried, in two tubes; but the points did not range well; the general trend was parallel to the chloride. It may be that the true line converges to −274°, certainly not to the ether-node.

*Vapour-density and Liquid volume at Point of Transition.*

§ 29. The following are the extracts from an unpublished paper giving an account of these observations, made in 1852.

"The chief object of the following experiments was to ascertain if the law of density of saturated vapours holds good up to that point where, according to M. Cagniard de la Tour's interesting researches, the liquid condition seems to terminate suddenly. The observations were made on the same principle as those which were the means of detecting the general law of density, the details of which have been communicated to the Royal Society. The tubes were mostly from 2 to 3 inches in length. The graphical method was employed to compute vapour-density and liquid-volume (see *Phil. Mag.*, vol. xxvi., Appendix II., to paper on Expansion of Water).

* *Ann. de Chim. et de Phys.*, vol. xlix., p. 369.

"The following was the method employed to heat the tubes and take the temperatures. A glass funnel, about 3 feet long and 1 inch in diameter, was fixed vertically over a large Argand oil-lamp. One of the prepared tubes was placed in a small brass wire frame arranged and fitted so as to slip into the top of the funnel and hold the tube fixed steadily at the centre of the current of heated air about 4 or 5 inches below the top. By increasing the flame of the lamp gradually, the transition temperature was attained, and by careful adjustment was kept nearly steady. The liquid volume being noted, the tube was taken out and a thermometer put exactly in its place. The mercury, quickly rising, became steady, the temperature was noted, and thermometer removed. A second tube, having a different proportion of its volume filled with the same liquid, was slipped into the same place; then a third, and a fourth.

"The state of the liquid in the tube was closely examined through a watchmaker's lens by transmitted light. The tubes were generally of hard glass $\frac{1}{8}$ inch bore and $\frac{1}{50}$ inch thick. When carefully sealed they did not burst at the transition-point of any of the liquids, except water, which burst the tube at about 330°, first corroding the glass in a ring at the top of the liquid.

"The liquids observed by this method were ether, alcohol, sulphide of carbon, water, chloroform, glacial acetic acid, dichloride of sulphur, turpentine, and sulphuric acid.

"*Ether.*—Boiling-point 41°; four tubes; transition-point 221°; density of vapour 0·19 to 0·23; density of liquid 0·43 by third and fourth tubes (unity at 29·5°). By two of the tubes the density of vapour was found to be 0·23, which is exactly the value computed from the line of density.

"*Alcohol.*—Boiling-point 79·5; four tubes; transition-point 281°. Five series of observations in good accordance. Density of liquid 0·48, and of vapour 0·222. The same computed from gradient is 0·245.

"*Sulphide of Carbon.*—Boiling-point 49°; five tubes; point of transition 302°. The second, fourth, and fifth tubes give density of vapour 0·305, and of liquid 0·45. The vapour-density computed from the gradient is 0·305.

"*Chloroform.*—Boiling-point 60°; three tubes; transition-point 266°; density of liquid 0·42, of vapour 0·24.

"*Glacial Acetic Acid.*—Three tubes; no distinct point of transition. The liquid surface gradually fades away at about 315°, but density of lower part greater than upper; no activity manifested in the tube; density of liquid about 0·40 to 0·44, and of vapour from 0·14 to 0·18.

"*Dichloride of Sulphur.*—Four tubes; transition temperature estimated at about 500°, at which the liquid and the vapour are of the *same* density, nearly 0·35 (the liquid unit density at 29·5°).

"Turpentine shows singular phenomena of change of colour (becoming scarlet before transition) and decomposition. On cooling, after transition, the liquid was thick and oily, deep green by reflected light, and reddish brown by transmitted. On opening the point of the tube there was a violent rush of gas and effervescence of the liquid, which had first the odour of pitch, then of creosote. and then naphtha, but had lost that of turpentine.

"It was remarked in these observations that near the transition-point the rate of apparent liquid-expansion becomes so very great that it is difficult to keep the liquid surface steady; it rises and falls like a sensitive air-thermometer. The capillary attraction ceases, and from being concave the surface becomes first quite flat, then convex like mercury."

§ 30. The law of saturated vapour-density thus appears to be strictly maintained from the zero of vapour up to the transition-point. It is not altered by the liquid becoming solid (Regnault), and is uninfluenced by the abnormal expansion of water. The liquid molecules of water are abnormal in their change of volume, while the vapour-molecules in contact with them behave as if they were subject to the influences common to all the other liquids expanding in the normal manner.

*Evidence as to the Law of Liquid Expansion.*

§ 31. This is given in Plate IV., where the values of $\frac{dt}{dv}$ are projected to $t$ in thirteen cases, which include the most accordant, Nos. 1 to 8, and the least accordant, Nos. 12 to 13.

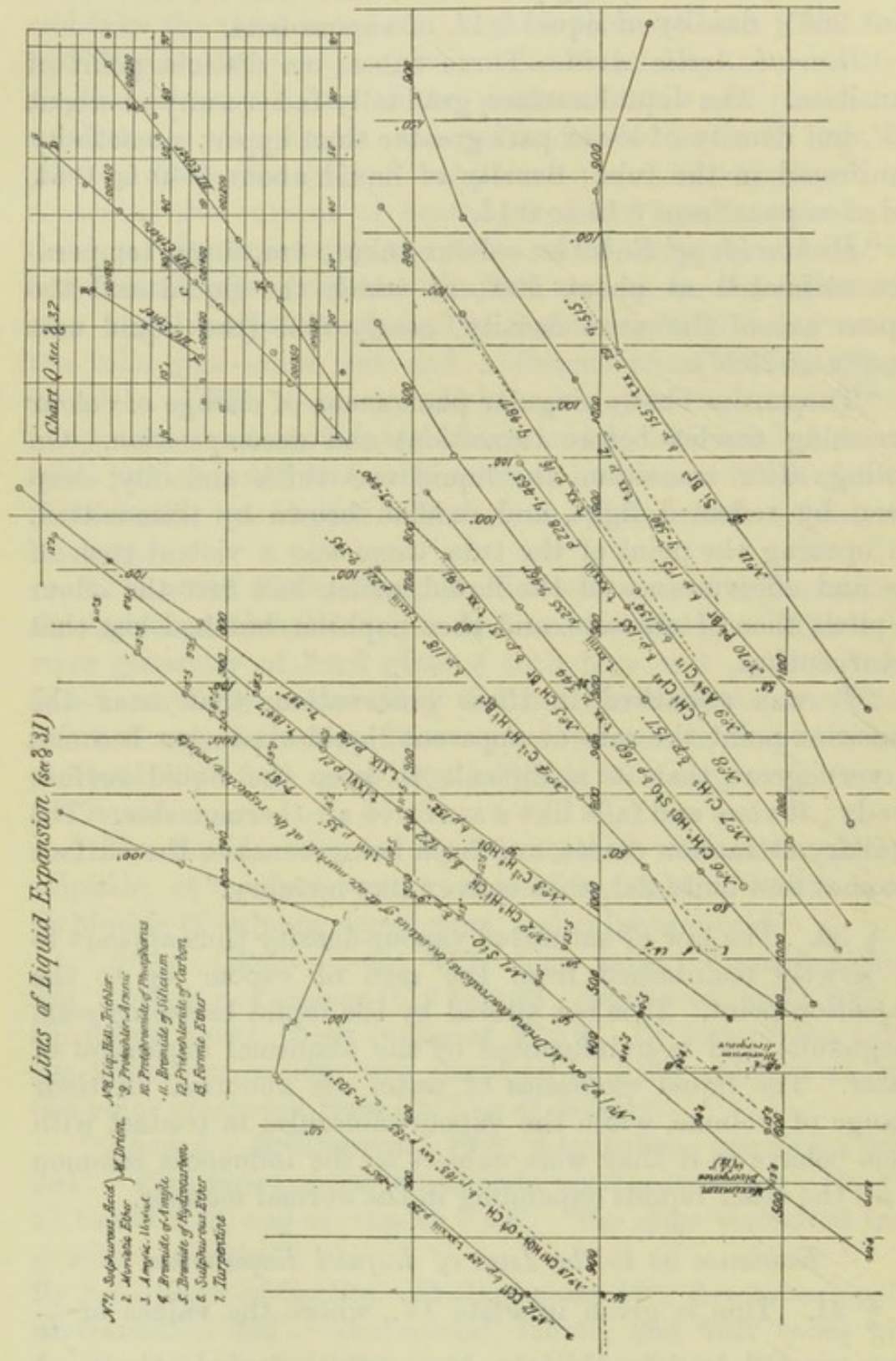



PLATE IV.

The most remarkable is sulphurous ether, No. 6. The first series of five observations are in perfect accordance with the theory, and prove the extreme accuracy of M. Pierre in this and several others, which are almost as good. In other cases, as Nos. 11, 12, 13, where the law of continuity is not maintained, it appears as if some chemical disturbance had taken place in the liquid during the process of heating.

M. Pierre has submitted forty-four chemically pure liquids to thermometric trial from 0° up to their boiling-point, and several of them from 0° down to −30°. I have graphically examined and analysed all but a very few. Some of them with high boiling-points present a clear range of about 100°; and the straightness of the line of $\frac{dt}{dv}$, in which the presumed law of expansion consists, is well developed (see Nos. 3, 4, 5, 6, 7, 8, 9).

Those with boiling-point under 100°, when regular, afford the means of computing the constants $\gamma$ and $P_0$ of the equation $\frac{dt}{dv} = P_0(\gamma - t)$,—although the range is too small to prove the law, except through the value they give to $\gamma$ being in accordance with the principle shown by Plate III., that causes the upper limiting temperature, $\gamma$, of many of the liquids whose lines of vapours trend to the ether-node to range in a line parallel to the axis of temperature.

§ 32. In most of these M. Pierre has given for each observation the value of $a$, the mean expansion from 0°. Thus, 1 being the volume at 0° and $v$ the same at $t°$, $a = \frac{v-1}{t}$. The projection of these values of $a$ as ordinates to $t$ ought to present a series of points in a curve convex to the axis of temperature: chart Q, Plate IV., is an example of this projection in the case of the three ethers—hydrochloric, hydrobromic, hydriodic. The first has too small a range; but it will be remarked that the upper four points keep well in a line, and that three are exact. This makes it probable that they are sufficiently accurate. Accordingly, the two observations at the points marked A, B are selected to compute $\gamma$ and $P_0$, as follows:— Let $t_0t_1$, $v_0v_1$ be the respective temperatures and volumes at

those points having reference to the unit volume at 0°. We have

$$\frac{t_0}{v_0-1}=\frac{dt}{dv}=P_0(\gamma-\tfrac{1}{2}t_0)$$

= the reciprocal of the absolute increment of volume at $\frac{1}{2}t_0$, and

$$\frac{t_1-t_0}{v_1-v_0}=P_0[\gamma-\tfrac{1}{2}(t_1+t_0)]$$

= the same at $\frac{t_1+t_0}{2}$; hence we obtain $\gamma=186{\cdot}4°$, and $P_0=3{\cdot}485$ ($t_0=14{\cdot}51°$, $t_1=26{\cdot}41°$, $v_0=1{\cdot}02326$, $v_1=1{\cdot}04386$). The previous values by M. Drion's observations were $\gamma=187°$ and $P_0=3{\cdot}545$.

The next line (DC) on chart Q was drawn through the $a$ points of M. Pierre's hydrobromic ether by means of a thin flexible steel straight edge. To judge the value of such a set of points, a light pencil-line is drawn in the general trend, and then the middle points are examined to see if there are any parallel to this line; if so, the straight edge (set on its edge) is brought to them and bent up a little on each side so as to pass over or through the extreme points if possible. The result, it will be observed, is favourable; the points C and D are selected for computation, from which is obtained $\gamma=253°$ and $P_0=2{\cdot}97$; the greatest discrepancy is at 28°, where the divergence is 0·18°.

In this way the points for hydriodic ether were also treated, and E, F selected, from which $\gamma=312°$ and $P_0=2{\cdot}75$. The greatest divergence is at 58°, where it amounts to about 0·2°.

A similar projection was made with the short series of observations on sulphuric ether*: the points ranged well; the two selected were 23·36°, ·03657; 38·14°, ·06216, which give $\gamma=211°$, $P_0=3{\cdot}21$. Others give $\gamma$ various up to 250°.

§ 33. The series on chloroform† was graphically examined, and the three highest were found very exactly in a line, giving $\gamma=334°$, $P_0=2{\cdot}52$; but the lower observations gave points which were quite out of range, and showed that the law of

* *Ann. de Chim. et de Phys.*, vol. xv., p. 361.

† *Ibid.*, vol. xxiii., p. 208.

continuity was not maintained; the divergence amounts to nearly 1°.

The series on chloride of silicium and chloride of phosphorus are also wanting in regularity.

§ 34. The series on the *bichloride of carbon* are good; they give $\gamma = 318°$, $P_0 = 2{\cdot}73$. The *bromide of hydrocarbon* is No. 5 of Plate IV. On extending the general line it cuts the axis at $\gamma = 440°$. If the two lower points are preferred, the line passes through the upper point and cuts the axis at 423°.

*Turpentine* is No. 7 of Plate IV.; the line cuts the axis at 461°.

The vapour-lines of all these liquids have been fixed by Regnault's observations, which are projected on Plate I., and they all trend to one point at the absolute zero of temperature, called the ether nodal-point, shown in Plate III.

§ 35. The next step is to lay off these values of $\gamma$ on the respective lines. This has been done on Plate III. and the points marked $\gamma$. Considering that the distance of these points from 0° depends on the value obtained for $P_0$, which depends on the integrity of the second differences of expansion, its mathematical expression being $\frac{d^2v}{(dv)^2}$, we have to judge whether, if perfect exactness had been attained in it and direction of line of vapour-density, they would have ranged in one line parallel to the axis of temperature, except sulphide of carbon. In favour of this there is to be added that if we compute $\gamma$ from Pierre's observations on a liquid (*e.g.* bromine, methyle-bromide, etc.) whose vapour-line has not yet been determined, but which we may judge from analogy is likely to trend to the ether-node, and which we assume to do so by drawing a line from this node through the boiling-point of the liquid and extending it upward to $\gamma$, we find invariably that this point ranges along with the others. They are marked $\overset{|}{\circ}\gamma$ on Plate III.

§ 36. The dotted line *l*G is drawn parallel to the axis at a distance, *zl*, equal to three times the distance of the ether-node, *zn*. It will be remarked that it coincides with the average range of the upper limiting points $\gamma$, $\gamma$, etc.

This closes the evidence of the law of saturated vapour-density and of liquid expansion,—also of the ether nodal-point, and of the line of the upper limiting temperatures $\gamma$. We now come to the

*Evidence as to Relations of Molecular Volume in different Liquids.*

§ 37. If we draw a line parallel to the axis of temperature intersecting the vapour-lines in the respective points $\tau$, $\tau$, etc. (Plate III.), it is evident that the molecular density of the respective saturated vapours at those points is the same in all (§ 4); *i.e.* the number of molecules in a cubic inch of each is the same in all; in other words, the volume of the *aëriform* molecule is the same in all. If we now compute the volume of the *liquid molecule* at these points or $\tau$ temperatures, we shall find that they also are quantitatively related to each other in a very interesting way.

§ 38. The vapour molecular volume of a body is the vapour-density (*i.e.*, hydrogen 1, oxygen 16, water 9, alcohol 23, etc.) divided by the specific gravity of the saturated vapour at the respective temperatures. The liquid molecular volume is the same vapour-density of the body divided by the specific gravity of the liquid at the respective temperatures. It has now to be shown that in certain chemically allied bodies these latter volumes, measured at $\tau$ temperatures, are proportional to the $h$ tangents (§ 4) respectively.

§ 39. To find this tangent, we take two points in the line of vapour (Plate I.), and measure by scale, first the difference $\eta$ of the ordinates at those points, and second the difference $\theta$ of the temperatures. The quotient $\frac{\theta}{\eta}=h$. Thus the easy plan is to select the points so that $\eta=1$, which gives $\theta=h$, the constant of that particular line of vapour.

§ 40. As a convenient point through which to draw the line parallel to the axis upon which $\tau$ temperatures have to be measured, we may take the intersection of the muriatic-ether-line with 0° C. This is drawn on Plate III. It intersects the hydrobromic-ether-line at 27·4°. The vapour-density of this

ether is 54·5 (on hydrogen scale), and spec. grav. at 0° 1·473; the volume at 27·4° by Pierre's observations is 1·0387 (volume at 0° unity). Hence, to find the liquid molecular volume at 27·4°, the $\tau$ temperature, we have to compute $\frac{54{\cdot}5 \times 1{\cdot}0387}{1{\cdot}473}$, which comes out $38{\cdot}43 = \mu$ of hydrobromic ether.

The hydriodic-ether-line intersects the $\tau$ line at 62·4°, at which temperature the volume is 1·082; the vapour-density is 78, and spec. grav. at 0° 1·975; we have $\frac{78 \times 1{\cdot}082}{1{\cdot}975} = 42{\cdot}73 = \mu$ of hydriodic ether.

Now $h$ of the first is 158·6, and of the second 177 (as may be computed from the chart, Plate I.). *These are in the same ratio as the respective values of* $\mu$. Thus $\frac{158{\cdot}6}{38{\cdot}43} = 4{\cdot}12$, and $\frac{177}{42{\cdot}73} = 4{\cdot}14$, that is, $\frac{h}{\mu}$ = a constant.

The $\tau$ temperature of the muriatic ether is 0°, spec. grav. 0·920, vapour-density 32, and $h = 144$: hence to find $\mu$ we have $\frac{32}{0{\cdot}92} = 34{\cdot}78$, and $\frac{h}{\mu} = \frac{144}{34{\cdot}78} = 4{\cdot}14$, the same as the preceding.

Chloroform intersects the $\tau$ line at 51·4°, where the volume by Pierre is 1·0664, vapour-density 60, spec. grav. at 0° 1·525, $h = 171$: to find $\mu$, there is

$$\frac{60 \times 1{\cdot}0664}{1{\cdot}525} = 41{\cdot}96, \text{ and } \frac{h}{\mu} = \frac{171}{41{\cdot}96} = 4{\cdot}08.$$

The mean of these four values of $\frac{h}{\mu}$ is 4·11.

§ 41. By inspecting fig. 1 it will be seen that measuring the temperatures on the $\tau$ line implies that the absolute temperatures, *i.e.* the temperatures reckoned from −274°, the zero of gaseous tension, are taken in the same ratio as the respective values of $h$, the tangents or exponents of the thermomolecular lines.

§ 42. The next point to consider is, that if the above ratio of molecular volume holds good throughout the whole range of expansion, the *proportionate* dilatation at the $\tau$ temperatures ought to follow the same ratio of $h$ inversely, or, expressed by

symbols, $\frac{vdt}{dv} \propto h$ (in which $dt = 1°$), because, if we imagine the $\tau$ line to move upwards parallel to itself through a small increment of its distance from the axis of temperature, it cuts off at each intersection with a vapour-line an increment of temperature proportional to $h$, and, to maintain the same ratio in the molecular volumes *at the end as at the beginning of these increments*, the *proportionate dilatations* should be all equal to each other—which can only take place if the proportionate increment corresponding to a constant value of $dt$ or 1° is inversely as $h$. This increment, therefore, multiplied by $h$, should be a constant quantity in all these four liquids. This product being a fractional quantity, we compute its reciprocal $\frac{vdt}{dv} \cdot \frac{1}{h}$ from the constants of the three ethers found in § 32.

*Muriatic Ether.*—The $\tau$ temperature is 0°, at which $v = 1$, hence

$$\frac{vdt}{dv} = \frac{dt}{dv} = \mathrm{P}(\gamma - t) = \mathrm{P}\gamma = 3{\cdot}48 \times 186{\cdot}4 = 649\,;$$

and $h$ being 144, we have $\frac{649}{144} = 4{\cdot}50$.

*Hydrobromic Ether.*—The $\tau$ temperature is 27°, at which $v = 1{\cdot}0385$*, P = 2·97, $\gamma = 253$, and $\mathrm{P}\overline{\gamma - t} = \frac{dt}{dv} = 671$, which, $\times$ by $v$ and $\div$ by $h = 158{\cdot}6$, gives 4·39.

*Hydriodic Ether.*—The $\tau$ temperature is 62·4°, at which $v = 1{\cdot}0782$, P = 2·75, $\gamma = 312°$, $\mathrm{P}\overline{(\gamma - t)}v = 742$, which, divided by $h = 176{\cdot}9$, gives 4·19. A previous result by another computation is 4·32.

*Chloroform.*—The $\tau$ temperature is 51·4°, at which $v = 1{\cdot}0660$,† P = 2·52, $\gamma = 334$, $h = 171$. These give the quotient 4·48. The mean of the four is $4{\cdot}38 = \frac{vdt}{dv} \cdot \frac{1}{h}$ for these four liquids.

§ 43. The product of this by the previous constant $\frac{h}{\mu}$ gives $\frac{vdt}{dv} \cdot \frac{1}{\mu} = 18{\cdot}00$, which is also a constant at the respective $\tau$ temperatures of these four liquids. This third constant is

* *Ann. de Chim. et de Phys.*, vol. xv., p. 369.

† *Ibid.*, vol. xxxiii., p. 208.

independent of $h$; it expresses merely the ratio of molecular volume to proportionate expansion at $\tau$, and is found to have the same value in the bromide of hydrocarbon and some others where the first two constants do not conform. *E.g.*:—

*Bromide of Hydrocarbon.*—The $\tau$ temperature is 127°, at which $v = 1{\cdot}1180$,* vapour-density 94, $P = 2{\cdot}61$, $\gamma = 423$, $h = 211{\cdot}2$, $\mu = \frac{1{\cdot}118 \times 94}{2{\cdot}207} = 47{\cdot}62$, $\frac{h}{\mu} = 4{\cdot}43$ (instead of 4·11), $\frac{vdt}{dv} \cdot \frac{1}{h} = 4{\cdot}09$ (instead of 4·32). The product of these is 18·11, almost the same as the preceding mean.

The same is the case with chloride of hydrocarbon and protochloride of arsenic. They would conform to the first group entirely, if the values of their $h$ tangents were reduced about $\frac{1}{14}$ part.

§ 44. A large proportion of the liquids examined by M. Pierre have not their vapour-lines established; but if we assume them to issue from the ether-node, and join that point and their boiling-point on the general chart (Plate III.), we may then lay off the upper limit $\gamma$ upon this line produced; and if it range near to the dotted line, it is probably correct. This is the case with the hydrobromate, hydriodate, and probably the hydrochlorate of methyle, which have the quotients $\frac{h}{\mu}$ etc. alike.

The bichloride of tin and bichloride of silicium are also thus united. Other symmetric relations are indicated in the Table before me, which includes most of Pierre's liquids; but it would be hazardous to proceed further in this direction without the complete data.

*Evidence of Relations with respect to the Cohesion-Integral of a Molecule in different Liquids.*

§ 45. Let Q = quantity of heat given out by a pound of a certain liquid in descending from $t°$ to 0°, and X = quantity of heat given out by a pound of the same body in the condition of saturated vapour at $t$ in passing into a liquid state and descending to 0°. Then $X - Q = C$, the quantity of heat

* *Ann. de Chim. et de Phys.*, vol. xx., p. 41.

required to change the condition of a pound of the same body from liquid to saturated vapour without performing work—in other words, the integral of liquid cohesion at $t$.

Regnault has determined X and Q for a number of bodies through a considerable range that includes the $t$ temperatures of some of our liquids whose thermomolecular lines are well established. We have thus the data for computing C at such temperatures, which are near the respective boiling-points. By multiplying this value of C by $\omega$, the vapour-density or *molecular* weight (not atomic weight) of the body, we obtain a number that represents the cohesive integral of *a molecule* of that body at the $\tau$ temperature.

Performing this computation for several bodies and comparing the numbers with the respective values of $h$ and of $\mu$, if it is found that they follow the same ratio, that $\frac{C\omega}{\mu}$ is a constant quantity at the $\tau$ temperatures of the several liquids, the legitimate inference to be drawn is that the cohesive force of each molecule is proportional to its volume $\mu$ when in the liquid state. This is equal to $\frac{\omega}{s}$, the quotient of vapour-density by spec. grav. at $\tau$, and hence $\frac{C\omega}{\mu} = Cs$ is constant in the group of liquids. The following is the computation.

*Muriatic Ether.*—At pp. 912, 914 of Regnault's second volume we have two values of X: one at $+12{\cdot}2°$ is 97·70; another at $-7{\cdot}2°$ is 95·88. The $\tau$ temperature of this body being 0°, we have X at $0° = 96{\cdot}60 = C$ in this case, as $Q = 0$. The vapour-density is $32\frac{1}{4} = \omega$ and $C\omega = 3115$, which, divided by $h = 144$, gives 21·63 as the value of the quotient $\frac{C\omega}{h}$.

*Hydriodic Ether.*—$X = 53{\cdot}20$ at $-5°$, and 58·9 at 71°. The $\tau$ temperature is 62·4°, at which (p. 278) $Q = 10{\cdot}4$, and $X = 58{\cdot}3$; hence $C = 47{\cdot}9$, $\omega = 78$, $h = 177$, $\frac{C\omega}{h} = 21{\cdot}11$.

*Chloroform.*—At p. 836, $X = 74{\cdot}00$ at 51·4°, the $\tau$ temperature, and $Q = 12{\cdot}07$ (p. 276); hence $C = 61{\cdot}9$. And since $\omega = 60$, $h = 171$, we have $\frac{C\omega}{h} = 21{\cdot}71$.

*Sulphuric Ether* has $\frac{h}{\mu} = 3{\cdot}00$ instead of 4·11, the value common to the three preceding liquids; nevertheless the cohesion-integral of its liquid molecule has the same ratio to $h$. Its $\tau$ temperature is 24°, at which Q = 12·86 (p. 275) and X = 105·2 (p. 820); hence C = 92·3 and $\omega = 37$, $h = 156{\cdot}7$ gives $\frac{C\omega}{h} = 21{\cdot}79$. The mean of the four values is 21·56.

In the same way the quotient was computed for the bichloride of carbon 20·2, sulphide of carbon 21·1, turpentine 19·1, chloride of phosphorus 20·0.

§ 46. It seems probable that this value of $\frac{C\omega}{h}$ is the same for all liquids which belong to the ether-node. I find that by taking C from Professor Andrews's observations on latent heat at the boiling-point, which corresponds nearly to the $\tau$ temperature, and computing therewith $\frac{C\omega}{h}$, a value of about 21 always appears in those cases that are likely to belong to the ether-node class.

§ 47. A higher value seems to belong to liquids that belong to the water-node; thus $\frac{C\omega}{h}$ for water is 30·4, and for pyroxylic spirit 29·25. For alcohol a still higher value is found, viz. 35·7; and its line cuts the zero-line *zq* (fig. 1) still further down than the water-node.

§ 48. The latent heat of chloroform, of sulphuric ether, and of some other bodies have been determined by Regnault up to high temperatures, so that we are enabled to test the presumed law at a higher value of $\tau$. I have computed the values of C corresponding to all the values of X (making use of the empirical formula for Q), and laid them off as ordinates to the respective temperatures; then, having drawn the curve by flexible straight edge, the value of C at the respective $\tau$ temperatures is read off and $\frac{C\omega}{h}$ computed.

Let 120° = the new $\tau$ temperature of sulphuric ether; a parallel to the axis drawn through this point (Plate III.) cuts the chloroform line in 157°. At these temperatures the

respective values of C are 73·2 and 49·4, which give 17·23 and 17·31 as the respective values of $\frac{C\omega}{h}$, an accordance that is surely very remarkable.

§ 49. Let us now compare C at different temperatures of the *same* liquid with the volume $v$ at those temperatures. First, sulphuric ether, the expansion constants of which are determined from Pierre to be in conformity with the general law ($\gamma = 224$, $P_0 = 3{\cdot}01$). I take from the curve of C 91·7 at 20°, and 78·0 at 100°,—the volumes at these temperatures being respectively 1·031 and 1·199. Hence we have 13·7, the difference of C, equivalent to 0·168, the corresponding difference of $v$. In the same proportion, 13·7 : 0·168 = 78·0 : 0·957, the augmentation from 100° up to the maximum volume at transition, if $\frac{-\delta C}{\delta v}$ is constant. This gives 2·156 as that maximum when C is reduced to zero. Now the temperature corresponding to this volume, computed from the above value of $\gamma$ and P, is 217°. The observed temperature of transition was 221° (§ 29), and maximum volume 2·44. I may add that the volume computed for −274° is 0·828, nearly one-third of the maximum.

Chloroform treated in the same way (but leaving out the highest observation of X, which is quite out of the line of the others) gives 268° as the temperature when C = 0. The computed volume at that point is 2·02. The transition temperature observed was 266° (§ 29), and maximum volume 2·43. The volume computed for −274° is 0·847.

Sulphide of carbon treated in the same way gives transition-volume 2·15, the observed being 2·22, and at −274° the computed is 0·852.

The computations all tend to show the probably simple relation between liquid molecular volume and cohesion-integral, —*the absolute value of the increment of the one having always* the same ratio to the absolute value of the decrement of the other in the same liquid and in the same class of liquids (where $\frac{h}{\mu}$ has the same value).

§ 50. In liquids, such as chloroform and sulphuric ether,

which are not related in molecular volume but have $\frac{C\omega}{h}$ common to both, the following computation suggests the question as to whether the force required to effect the *same absolute increase of volume* (or, in other words, the heat-force rendered latent) is proportional to the molecular weight of vapour-density. One ounce of *ether* absorbs 16·25 unities of heat in expanding from 0° to 100°; 1 cubic foot of it weighs 736 oz., and thus absorbs 11,960 unities in expanding from 0° to 100°, during which its volume is augmented from 1 to 1·2 cubic foot.

One ounce of *chloroform* absorbs 13·25 unities of heat in expanding, so as to augment its volume from 1 to 1·2. 1 cubic foot of chloroform weighs 1525 oz., and thus requires

$$1525 \times 13{\cdot}25 = 20{,}206 \text{ unities}$$

to increase its liquid volume from 1 to 1·2 cubic foot. Now mark the ratio 11,960 : 20,206 = 37 : 62·5, which is nearly the ratio of the respective values of $\omega$, viz. 37 : 60; that is, nearly, considering the difficulty of obtaining $\frac{\delta C}{\delta v}$ exactly.

A cubic foot of the sulphide of carbon at 0° requires $15{\cdot}5 \times 1293 = 20{,}041$ unities to become 1·2 cubic foot. This is nearly the same amount as chloroform and the three halogen ethers, although the relation of molecular volume is not so simple.

§ 51. The Table on p. 526 contains the thermomolecular constants of a few liquids belonging to the ether-node, brought together as an example of the mode of arrangement adopted after some experience.

*Note.*—The number in line 12 under Turpentine is rather small, to be in accordance with the law of uniformity. The paucity of observations on this liquid perhaps may account for the want of precision. The constants of expansion for the hydrochloric ether were taken from M. Drion's observations, and the others from Pierre; hence perhaps the inequality at 16, 19, 20.

§ 52. The relation between liquid molecular volume and liquid molecular cohesion (line No. 11) strikes the attention as being undoubtedly *causal*. Also the relation exhibited in

line 12 shows cohesion of a molecule as the potential antecedent of the inclination of its thermomolecular line.

§ 53. M. Pierre has subjected four liquids to examination which are closely related to each other by chemical substitution.

| No. | | Hydrochloric Ether, $H^2C.H^{\frac{1}{2}}Cl^{\frac{1}{2}}$. | Hydrobromic Ether, $H^2C.H^{\frac{1}{2}}Br^{\frac{1}{2}}$. | Hydriodic Ether, $H^2C.H^{\frac{1}{2}}I^{\frac{1}{2}}$. | Chloroform, $ClC^{\frac{1}{2}}.H^{\frac{1}{2}}Cl^{\frac{1}{2}}$. | Ether, $H^4C^2.HO^{\frac{1}{2}}$. | Turpentine, $H^8C^5$. |
|---|---|---|---|---|---|---|---|
| 1 | $\omega$ weight of molecule | 32¼ | 54½ | 78 | 60 | 37 | 68 |
| 2 | Boiling-point . . | 12·2° | 38·5° | 71° | 60·8° | 35° | 157° |
| 3 | Spec. grav. at 0° . | 0·920 | 1·473 | 1·975 | 1·525 | 0·736 | 0·872 |
| 4 | $h$ . . . . | 144 | 158·6 | 177 | 171·1 | 156·7 | 226 |
| 5 | $\tau$ . . . . | 0 | 27·4° | 62·4° | 51·4° | 24° | 155·4 |
| 6 | $v$ at $\tau$, $v$ at 0° being 1 | 1·000 | 1·038 | 1·082 | 1·066 | 1·038 | 1·176 |
| 7 | $\mu$ at $\tau$ . . . | 34·74 | 38·31 | 42·67 | 41·96 | 52·15 | 91·7 |
| 8 | $\frac{h}{\mu}$ at $\tau$ . . . | 4·14 | 4·14 | 4·10 | 4·08 | 3·00 | 2·46 |
| 9 | C at $\tau$ . . . | 96·6 | ... | 47·9 | 61·9 | 92·3 | 63·3 |
| 10 | C$\omega$ at $\tau$ . . . | 3115 | ... | 3736 | 3716 | 3415 | 4305 |
| 11 | $\frac{C\omega}{\mu}$ at $\tau$ . . . | 89·5 | ... | 86·5 | 88·6 | 65·5 | 47·0 |
| 12 | $\frac{C\omega}{h}$ at $\tau$ . . . | 21·63 | ... | 21·11 | 21·71 | 21·79 | 19·1 |
| | ,, at $\tau_1$ . . . | ... | ... | ... | 17·23 | 17·31 | |
| | hyp. $\log\left(\frac{\gamma}{\gamma - t}\right) = P_0(v-1)$ | | | | | | |
| 13 | $\gamma$ . . . . | 185° | 254° | 312° | (282°) | 224° | 461° |
| 14 | $P_0$ . . . . | 3·65 | 2·95 | 2·89 | (2·97) | 3·01 | 2·34 |
| 15 | G . . . . | 459° | 528° | 586° | (556°) | 498° | 735° |
| 16 | $\frac{G}{h}$ . . . . | 3·19 | 3·32 | 3·31 | (3·26) | 3·18 | 3·26 |
| 17 | $\frac{vdt}{dv}$ at 0° . . | 673 | 749 | 887 | 839 | 614 | 1080 |
| 18 | $\frac{vdt}{dv}$ at $\tau$ . . . | 673 | 694 | 764 | 759 | 562 | 843 |
| 19 | $\frac{vdt}{dv} \cdot \frac{1}{h}$ at $\tau$ . . | 4·67 | 4·38 | 4·32 | 4·43 | 3·59 | 3·74 |
| 20 | $\frac{vdt}{dv} \cdot \frac{1}{\mu}$ at $\tau$ . . | 19·36 | 18·11 | 18·00 | 18·09 | 10·77 | 9·19 |
| 21 | $\frac{\delta C\omega}{\delta\mu}$ . . . | 91 | ... | 95 | 97 | 61 | 42 |

These are the Dutch liquid $H^2C\,Cl$ and $H^{1\frac{1}{2}}Cl^{1\frac{1}{2}}C$, $HC\,Cl^2$, $H^{\frac{1}{2}}C\,Cl^{2\frac{1}{2}}$. The boiling-points are 85°, 114·2°, 138·5°, and 153·5°; and if their lines issue from the ether-node, the respective values of $h$ are 187, 198, 212, 220; and $\frac{G}{h}$ is constant

for all, being the same as in line No. 16 of the above Table; so it is satisfactory evidence of their belonging to the ether-node. But $\frac{h}{\mu}$ varies from 4·4 to 3·1; so there is no relation between molecular volume and slope of their lines. They are, however, connected by the proportion which holds good between their tangents $h$ and their proportionate dilatations at the respective $\tau$ temperatures. This ratio, $\frac{vdt}{dv} \cdot \frac{1}{h}$ (of line No. 19, Table), is the same in all, and equal to 4·00 very nearly. If $\frac{C\omega}{h}$ is constant, as in line 12 of Table, then $\frac{vdt}{dv} \cdot \frac{1}{C\omega}$ is also constant; so that in these substitutional liquids the proportionate expansion for increments of temperature respectively proportional to $h$ or $C\omega$ is the same in all. Thus, while the integral cohesion of a molecule determines $h$ as usual, the *proportionate* expansion for intervals of $\tau$ temperature is the same in all, although there is no apparent relation between their *absolute* molecular volumes.

§ 54. In employing chemical symbols in this inquiry, it is necessary to express the *molecule* of a compound in terms of the *molecules* of its constituents, not of the *atoms*. This is done by simply dividing the usual atomic formulæ by 2, unless when arsenic, phosphorus, selenium, or sulphur is involved—the divisors being then 4 for the first three, and 6 for the last.

It would be a boon to molecular physics if chemists could do without the word *atom*, and substitute in its place *molecule*, *half-molecule*, *quarter-molecule*, etc. Thus an atom of mercury is a molecule; an atom of cadmium is a molecule; an atom of oxygen is half a molecule; an atom of phosphorus is one-fourth of a molecule, etc. An atom of ether is a molecule, which denote by the letter E. An atom of sulphurous acid is a molecule, which denote by the letter U. An atom of sulphurous ether is a molecule, which denote by the letter Q.

$Q = UE = O^2S . C^4H^8 . H^2O$, in which the letters signify *atoms*, some of which are *half* molecules, some *whole* molecules, and one represents the *sixth* part of a molecule.

$Q = U \,.\, E = OS^{\frac{1}{3}} \,.\, C^2 H^4 \,.\, HO^{\frac{1}{2}}$, in which the same letters, *all without any exception*, signify *molecules*, each of which when it stands by itself, occupies a volume. U E indicates two volumes changed into one. $C^2 H^4 HO^{\frac{1}{2}}$ indicates $2 + 4 + 1\frac{1}{2} = 7\frac{1}{2}$ volumes changed into one, and $OS^{\frac{1}{3}} \,.\, C^2 H^4 HO^{\frac{1}{2}}$ indicates $8\frac{4}{6}$ volumes changed into one.

Applications of this system to express chemical reactions are given in the *Philosophical Magazine* for April 1864. It renders unnecessary the tedious ever recurring verbal description of constitution by volume given by chemical writers; and while thus saving space, would, I am fully convinced, put an end to the confusion which is still admitted to exist by M. Wurtz (see last sentence of the first part of the recently published *Introduction to Chemical Philosophy*), in spite of the new system of notation which he proposes.

1 Ardross Street, Inverness,
*August* 13, 1867.

# IV

# MISCELLANEOUS PHYSICAL AND CHEMICAL PAPERS

# EXPOSITION OF A NEW DYNAMICO-CHEMICAL PRINCIPLE

(From the *Philosophical Magazine*, 1831.)

"The new discoveries, in short, reveal to us the world of secret motions, whose laws are probably analogous to those of the universe, and which deserve to be the subject of our most earnest meditations."—Œrsted on "Thermo-Electricity" (*Edinburgh Encyc.*).

When we reflect on the progress which has been made, and is still making, in the physical sciences, and more especially in those which investigate the active properties of matter, when we behold that insatiable thirst after discovery, that enlightened spirit of inquiry, which so universally pervades the philosophic world, it becomes a source of exalted gratification to trace the steps which have led to so many brilliant results, and in contemplation of the future to look forward to that period when all that is now concealed under the veil of mystery shall finally be exposed in the sublime grandeur and simplicity which so eminently characterises the works of nature. The illustrious example which Newton held forth to posterity, of a philosopher who applied mathematical reasoning with so much success in explaining the grander phenomena of the universe, introduced the same system amongst those who succeeded him; which, joined to experimental analysis, have unfolded a series of the most splendid discoveries in every department of natural philosophy. Heat, electricity, magnetism, and light, are the principal fields in which the powers of induction have been most conspicuously displayed. The late discoveries and researches of Young, Œrsted, Seebeck, etc., have shown that those sciences are intimately connected, and that the actual principles of nature, if we except perhaps gravitation, interfere with each other in such a manner as to

lead us to conjecture they may all be particular modifications of one agent. If we, however, consider the numerous insulated facts which experimental investigation is so fertile in producing, that cannot even be generalised under any special laws, or included under any common analogy, we must be sensible that a vast distance yet separates us from the primary causes of all those phenomena.

Experiment, however ably conducted, has as yet shown nothing in heat, electricity and magnetism, but simply and exclusively the existence of force: and it seems doubtful if it will ever lead us directly to the knowledge of the essential nature of those powers. Heat is an example of a repulsive energy existing between the constituent atoms of bodies, and all the different situations in which it and various substances are placed in relation to each other serve only to exhibit instances in which the quantity and intensity of the repulsive power varies, thereby enabling experimentalists to deduce general laws which govern a diversified series of phenomena. Electricity and magnetism, on the other hand, present still more curious instances of invisible forces exercising functions which rival gravitation in the important parts they sustain in the œconomy of nature. Modern discoveries have developed the intimate nature of their connections; and the display of polar forces being their most remarkable feature, is so peculiar to them alone, that we are induced to look upon them as modifications of the same elementary principle. These powers are likewise connected with heat and light, which latter are in most cases co-existent. Electricity is the basis of chemical affinity, and heat is either absorbed or evolved in chemical action; the former is in most cases accompanied with a simultaneous change in the latter, whilst the latter in peculiar circumstances induces electrical phenomena. Magnetism is weakened or destroyed by an excess of heat; whilst the more refrangible rays of light possess evident magnetic properties.

Thus are all those subtle agents connected together in the different effects which they communicate to matter, and in the variety of forces which harmonise the routine of natural phenomena. Gravitation alone appears to be excluded from this system of interference; no difference in its intensity,

as far as experiment has yet shown, being consequent upon any change which may take place either in temperature or in electric state. Are we then to conclude that heat and electricity are removed from the sphere of its action? that they act independently of its general influence? If such is their relation to each other, our conceptions of relation and quantity are violated, and the nature of their existence must not only be different but contradictory to everything of which we can form the remotest conception. Experiment, however, warrants no such conclusion; the process appears too delicate, and the magnitude of the results may, like the parallax of the fixed stars, be placed far beyond the range of our means of observation, without at the same time justifying doubts which may be entertained either of the intimate connection of gravitation and caloric, or of the real magnitude of these heavenly bodies. Is it not more consonant to reason to consider them as inseparably combined in their operations? We know that both powers surround every particle of matter, exist in the same space, and generate motion, at the same time and in the same place; may we not therefore with justice conclude, that so far from acting independently, gravitation may communicate to heat all the properties by which it is distinguished, and that they may both be examples of the same elementary force exhibited through different media?

Such conjectures, although derived from an extensive and minute survey of the facts elicited by experiment, are yet prevented from ever becoming of practical utility by our ignorance of motion. How does it originate? In what manner are its effects so complex, and its development so varied? These are questions which first naturally arise, and indeed comprehend all that can form the object of philosophic inquiry. The first, relating to its origin, has often been investigated both by physical and metaphysical authors: many abstract speculations have engaged the attention of the latter, but they universally partake of the usual defects of that science, referring every effect to a primary cause which in whatever way it may be defined in words, fails to convey any definite conception or satisfactory meaning to the mind. Disgusted with such idle and inconclusive reasoning, and

despairing of being practically useful to science by prosecuting such inquiries, it is not surprising that philosophers have never devoted themselves to investigate this subject on simple physical principles, by the application of which alone we may expect to further the progress of natural philosophy.

Two opinions are entertained by philosophers relating to this subject: the first, that the absolute quantity of motion in the universe is always the same, suffering neither the smallest increase or diminution; the second, that "motion is much more apt to be lost than got," * and that therefore "some other principle is necessary for conserving it," to supply the continued loss incurred "by reason of the tenacity of fluids and attrition of their parts, and the weakness of elasticity in solids." The former doctrine was maintained by the Cartesians, who defended their opinions by the aid of such extravagant hypotheses, that Newton and his followers, by showing the obvious absurdity of their demonstration, adopted the contrary belief, not so much from the satisfactory proofs brought forward in support of it, as because it was contradictory to the great principle of Des Cartes, which was naturally supposed to have partaken of the general fallacy of his vortical system. Later philosophers, although ardent admirers of Newton and his philosophy, have yet rejected his doctrine, and in refutation of it have brought forward mathematical proofs of its fallacy, which if not conclusive, are at least plausible and ingenious. To prove either hypothesis, however, involves reasoning distinct from the abstract comparison of quantities. An intimate acquaintance with nature is requisite. The constituent principle of the attractive and repulsive powers, and their mode of operation, may differ from all we can deduce in comparison. When we therefore perceive matter, once in a state of motion, gradually arrive at a state of rest, without any visible transference of its power, having no direct proof to the contrary, we are induced to consider it absolutely lost. Still the following simple analogy appears to afford evidence of the contrary, and authorises the conclusion, that momentum-like matter cannot by natural means be annihilated, the existence of both being of equal importance in the œconomy

* 30th Query, Newton's *Optics*.

of nature. A body when falling towards the earth, gradually accumulates a quantity of momentum, which is visibly lost when it arrives at the ground. In this instance the momentum, before it is transferred to the falling body, is invisible; why may not, therefore, the same momentum after collision, be again reduced to the same invisible state without being actually destroyed? The manner in which it appears and disappears is certainly different, but the latter may be governed by laws as unalterably fixed as those of the former, although from the complexity of the attending circumstances their influence cannot so readily be appreciated. Thus after collision, in the above example, undulations or vibratory motions are always observed to take place. These changes are influenced by the nature of the composing substance, which again is an immediate consequence of the peculiar molecular forces of the ultimate constituent particles. Since we have this reason to suppose that their molecular forces are, like gravitation, subject to fixed laws and are every way of like importance and universality, it becomes highly probable that they are alone the invisible agents which abstract this momentum of collision, without any evidence of its existence being afterwards perceived.

These views of the transference of motion are further deserving of attention by their accordance with the simplicity of nature, and by tending to clear science of all those auxiliary causes, the introduction of which, though necessary to explain the contrary hypothesis, has yet proved a serious obstacle to the progress of true philosophy. If founded on truth, they induce a lively hope that matter and motion alone will be found sufficient to explain all the phenomena attending the grand cycle of nature's operations, and that that system of unity and simplicity which the advancement of discovery is always bringing further into view, will at length be completely unfolded, and all the physical sciences eventually traced to the varied development of these two principles.

Two opinions are at present entertained of the origin and nature of gravitation. In the first, no intermedia are deemed necessary to convey its influence; whilst in the second, direct impulsion is considered essential, and a subtle fluid or ether is supposed to transmit the power from one region of space

to another. This last doctrine has been reckoned by some unphilosophical, by introducing a clumsy mode of explaining that, which certain refined metaphysical speculations on causality do not require to be explained. But although the cause which is sought may not on these metaphysical principles be necessary, yet it will always remain inconceivable how two bodies in an absolute vacuum will move towards each other in accordance with the laws of gravitation; and it is certainly preferable to adopt the contrary opinion, more especially if we discover a certain arrangement of the fluid which will explain the development of an attractive and repulsive energy on the most simple and evident mechanical principles.

In the following three articles it has been attempted to show how an attractive force may exist between two particles proportional to the quantity of matter in each, and which is in every other respect subject to laws similar to those of gravitation. As it is intended at present to introduce and explain the general principle alone, without entering into mathematical details, the systematic arrangement which would otherwise become necessary, will not be so particularly attended to as briefness in demonstrating what will be sufficient to convey a distinct notion of the system; reserving for a future opportunity its mathematical elucidation and further extension in explaining a diversified series of chemical and electrical phenomena. The following particulars define what is intended to be understood as properties of matter co-existent with perfect solidity, and are the foundation of all the reasoning afterwards made use of.

*Postulates.*—Let it be granted that,

(1). Perfect solidity is accompanied with an inseparable union of parts.

Many may deny this as an unwarrantable assumption; but although hypothetical, it is but a corollary to the doctrine which is at present supported by the most enlightened and distinguished philosophers, who have inferred from the combining ratios of the simple chemical elements, that matter is divisible to a certain extent only, after which no force is capable of effecting any change in the relative situation of its

parts; and that when a plenum exists within the surface of the ultimate particle, no disunion of parts can be effected.*

(2). Perfect solidity is accompanied with perfect elasticity.

This proposition, although inserted in the form of a postulate from being dependent on the foregoing, is yet capable of being demonstrated. If after collision two elastic bodies recover their shape with a force equal to that by which they have been compressed, they will recoil from each other without any alteration being effected in the sum of their motions. This will likewise happen, however much we consider the extent of compression to be enlarged or diminished; perfect elasticity being always consequent to an equality existing betwixt the force of the contracting and that of the dilating vibration. This vibration may be therefore conceived to be infinitely reduced; the body will then be *perfectly* solid, and this finally becomes the limit of perfect resiliency.

*Exposition of a principle by which it is proposed to explain the manner in which a mutual attraction may exist between two particles of matter by the direct impulse of an intermedium.*

(1). Let there be an infinite number of particles of *cylindric form*, the length of each being indefinitely greater than its breadth; and let them be extended through space at finite equal distances from each other; and let an indefinite velocity be then communicated to each, which may cause them to assume a rectilineal motion in different directions. What will be the after state of the medium so constituted?

Each rigid line will pursue an undeviating course, until it meets with another moving in a contrary direction, when a collision will take place, and by 2nd postulate a perfect reflexion, without the sum of their motions being diminished, although the whole momentum will then be stored up in the particles in a different manner, *a considerable portion being gradually abstracted to effect a rotatory movement*, whilst the

* See *Phil. Mag.*, vol. lxii., p. 360; lxiii., p. 372.—Edit.

rectilineal velocity becomes greatly diminished. (Fig. 1.) Thus let the line $ab$, having at first a motion in the direction $cr$, impinge against another $df$, having likewise a rectilineal motion in a contrary direction $es$. Let $p$ be the point of concourse: then by dynamical formula $\frac{fd^2\ 6{\cdot}2832}{12\ ep}$ and $\frac{ab^2 \times 6{\cdot}2832}{12\ cp}$ are the spaces which would be afterwards described by $e$ and $c$ respectively during one revolution of the line $fd$, $ab$; and the ratio of the rotatory to the rectilineal momentum, will therefore be $\frac{1{\cdot}5708\ ep}{{\cdot}5236\ df}$ and $\frac{1{\cdot}5708\ cp}{{\cdot}5236\ df}$ if no elastic force is supposed to be exerted. Since perfect resiliency however occurs, the force

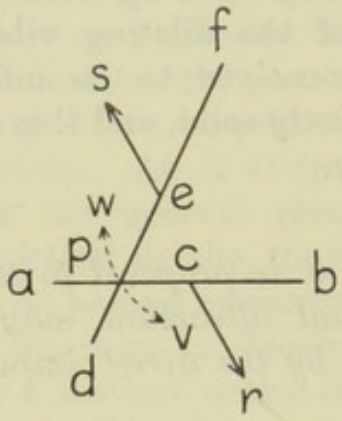



Fig. 1.

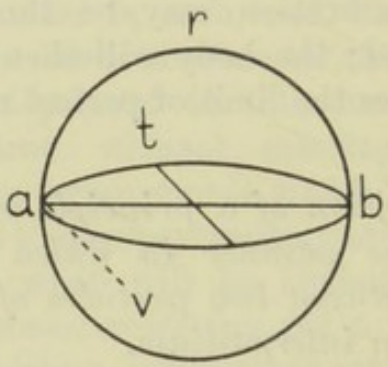



Fig. 2.

of impact will be reflected in contrary directions, and the supplementary momentum will be exerted at $p$ on $fd$ in the direction $pv$, and on $fd$ in the direction $pw$ in both lines; it will therefore tend to augment the rotatory, and simultaneously lessen the rectilineal motion. The actual ratio which these quantities will have to each other, after the condition of the medium is established, will be influenced by the following circumstances: (1) It is equally probable that the point of concourse $p$ may be anywhere situated in the lines $ab$, $df$; the extreme cases are, when it coincides with their extremities or centres of gravity; in the former by the above formula the rotatory motion is $\frac{3}{5}$ of the whole; in the latter it is 0; the mean quantity or $\frac{3}{10}$ will, therefore, show the ratio of the whole quantity of rotatory momentum generated in the

medium by this cause singly, and $\frac{7}{10}$ that of the remainder, by which the lines continue their rectilineal motions. (2) (Fig. 2.) The rotatory motion by the diversified concourse of the particles will be performed simultaneously in planes perpendicular to each other. For while *ab* revolves in the plane *arb*; another particle may communicate a rotatory impulse in any other direction *va*, which by a well-known principle in dynamics will cause the line *ab* to revolve at the same time in the planes *atb*, *arb*, perpendicular to each other. In this manner the whole quantity of rotatory momentum effected by the first cause will be nearly doubled, whilst the elastic recoil will tend further to diminish the rectilineal motion of the particles. Before ascertaining exactly this ratio, the principles of chance require to be employed in estimating the frequency of peculiar modes of concurrence, and thus discovering the mean results of the combined action of the whole medium. It is not intended, however, to enter upon this investigation at present, as it is unnecessary to prove the truth of what has been advanced, that the *primitive momentum is separated into two parts, one of which is employed in sustaining a rectilineal and the other a rotatory motion.*

(2). Let a rigid plane be introduced into this medium. What will be the corresponding change in its relative density?

(Fig. 3.) Let the line *ab*, having a rectilineal motion in the direction *de*, and a rotatory motion *bg*, impinge against the plane of which AB is a section; the whole momentum in the line will immediately after the first collision be separated into (1) a reflecting impulse in the direction *af*; and (2) the influence of the remaining force in continuing the motion towards AB, and bringing the other extremity *b* likewise in contact with it, thus inducing a contrary rotatory motion, which by encountering and balancing the former will concentrate the whole force in the centre of gravity, and thus cause the line to shoot forwards from AB in radiating lines *wx*; nearly the whole momentum being now exerted in a rectilineal direction. Thus the lines after collision with the plane AB will convey a newly acquired impulse radiating from every point of its surface, and the rectilineal force thus generated will be

communicated from one part of the medium to another. For we may suppose the fluid surrounding the plane to be divided into concentric films, and the primary rectilineal impulse occasioned by the introduction of the plane to be communicated from the first to the second, from the second to the third, and outwards successively. After the mutual reflection which takes place between the first and second, the former will have given away a portion of its rectilineal force, whilst the supplementary portion is converted into a rotatory motion. The

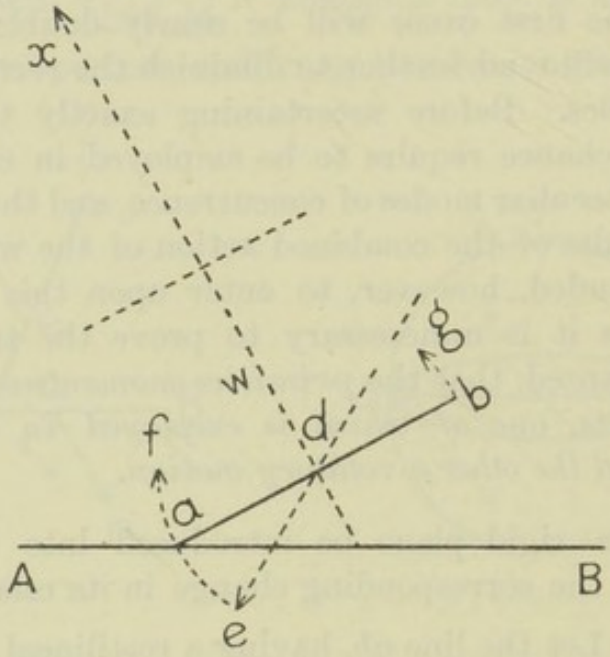



Fig. 3.

same interchange will take place between the second and third films, whilst at every collision an absolute loss of rectilineal momentum will be incurred, which will continually replace the vibratory reflections of the first film on the rigid plane. The radiating influence will be thus conveyed through boundless space, its intensity diminishing in a ratio of the distance. The balance of forces which before the introduction of the plane AB had preserved the homogeneity of the medium will be now destroyed, and the density of its relative parts will be proportional to the intensity of the rectilineal velocity of the component particles. Thus the difference between the density of the fluid at any point surrounding the plane, and

of the same before the introduction of the plane, will decrease in a ratio expressed by some function of the distance of that point from the centre of impulsion. (Fig. 4.) This ratio if *linearly* developed will be represented by a curve *kl* originating at a finite distance *ak* from the axis *ab* expressing the density of the medium of the surface of the plane at *a*, and gradually approaching the asymptote *mn*, which is parallel to *ab*; their mutual distance *am* being proportional to the density of the fluid at an infinite distance from *a*, or to every part of it before the introduction of the rigid plane.

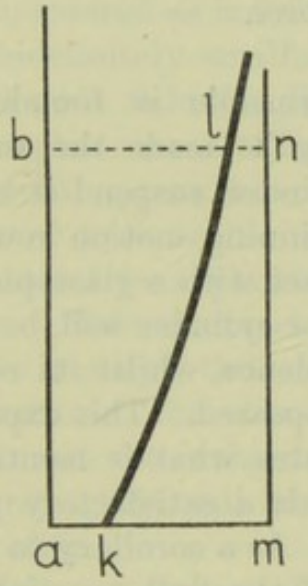



Fig. 4.

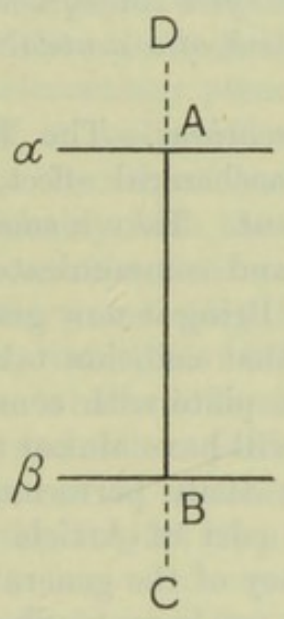



Fig. 5.

(3). Let a second rigid plane be introduced and placed at a distance from the first, indefinitely greater than the extent of its longitudinal dimensions. What are the effects consequent on this arrangement?

(Fig. 5.) Let AB be the relative position of the two planes, which let first be considered parallel, and intersected by a common perpendicular AB. Let $\alpha$A, $\beta$B, represent their indefinite extension. The rareness of the medium occasioned by the impulsive force radiating from the plane being proportional to the quantity of rectilineal force exerted amongst the particles in the same space, and the sums of the intensities of the rectilineal motion proceeding from both planes being

greater in the interior space AB than in BC, AD, on the exterior sides, it follows that the medium will be denser in the latter than in the former, *and more particles will thus impinge in the same time on the exterior than on the interior face of each.* The equilibrium which kept the first plane at rest before the introduction of the second will be therefore destroyed, and a motion communicated to each, which will cause them mutually to approach with an accelerating velocity; and this by the decomposition of forces will likewise take place at whatever angle the particles are inclined to each other. *Thus the effect consequent on this new arrangement will be the development of a mutual attractive force.*

*Observations.*—The foregoing principle is founded on a simple mechanical effect, which may be made the subject of experiment. Take a small glass cylinder, suspend it by a fine thread, and communicate to it a spinning motion round that centre. Bring it now gently in contact with a glass plate; the instant that collision takes place the cylinder will be thrown from the plate with considerable violence, whilst its revolving motion will have almost totally disappeared. This experiment, which is easily performed, corroborates what is mentioned in the first part of Article 2, and affords a satisfactory proof of the efficacy of the general principle. As a corollary to Articles 2 and 3 we have to observe, that if the finite particles were rigid planes of uniform thickness, the attractive power at the same distance would be proportional to the extent of surface; in other words, to the quantity of matter contained in the particle. Again, since the intensity of all radiating influences are inversely as the squares of the distance, it is extremely probable, although not yet demonstrated, that the attractive power generated by the above principle will likewise follow the same law. When the distance, however, is so small that the peculiar shape of the particles will modify the effect, polar forces will be developed, and the planes will gradually arrange themselves in a parallel direction, while other peculiar changes relating to chemical phenomena will simultaneously take place. These modifications of the influence of the medium when the mutual distance of the rigid planes is small, will

be more complex and interesting according to their number and mode of clustering together. Thus if we were to suppose every particle to be composed of an indefinite number of rigid planes arranged in a fixed order, and kept in their relative places by the balancing influence of an attractive and repulsive force, an accordance may be discovered with the theory now generally adopted, that the atomic weights of all the chemical elements are simple multiples of that of hydrogen; and that the particular properties of the particles of every different substance result from their individual organisation.

The introduction of another medium, the particles of which may be considered as indefinitely larger than those of the first, and yet indefinitely smaller than the elementary plane, opens a new and wide field for the display of an unlimited number of curious phenomena. These, as far as the subject has as yet been explored, appear to coincide remarkably with the known properties of heat, whilst other simple combinations are likewise successful in explaining many other interesting facts in chemistry. Before entering, however, into this boundless region of inquiry, the circumstances which influence the variable ratio of the attractive force, and other preliminary theories, must be subjected to a course of mathematical examinations: this will perhaps form the subject of a future communication.

In conclusion it may be observed, that were the size of the particles indefinitely reduced, whilst their velocity was indefinitely augmented, the density of the medium would be simultaneously diminished, whilst the quantity of force existing in a finite portion by remaining constant may be conceived sufficient to impel bodies with powers of equal intensity to those which are exhibited in nature; and this more especially since we may likewise suppose the rigid planes which constitute these bodies to be indefinitely reduced in thickness, whilst they still present the same superficies: thus by lessening the quantity of matter acted upon, the intensity of the action will be proportionally more vivid and efficacious. Thus were the velocity of the particles such as would carry them in void space from one extremity of the planetary regions to another in a second of time, the ratio of their magnitude and mean

distance will approach that of the stars which form a nebula; whilst the mean space described in the medium by a particle before the direction of its motion is altered by impinging against others, may be indefinitely smaller than any conceivable magnitude. However extreme the rapidity of this action may be considered, it is yet finite, and cannot therefore be reckoned irrational or contrary to the institutions of nature. It is the true primitive standard to which all velocities (upon the hypothesis) may be compared, as from it every other motion in the universe is derived.

Professor Œrsted, as far back as 1813, in his work on the *Identity of Electricity and Chemical Affinity*, deduced, from an extended series of experiments, a general view of science, "that all effects are produced by a fundamental power operating in different forms of action." It is remarkable that the principle we have been explaining is in perfect unison with the sentiments of this eminent philosopher. A medium is supposed to envelop the universe, every particle of which is a minute reservoir of power, which is conserved in the rectilineal and rotatory motion with which it is endowed. These motions, by the introduction of the gross particles of different substances, are alternately transformed into each other; and thus the primordial power of Œrsted's doctrine by the varied structure of the particles of matter "operates in different forms of action," and is everywhere developed in the diversified series of natural changes.

St John's Hill,
Edinburgh, 14*th April* 1831.

# ON THE INTEGRAL OF GRAVITATION, AND ITS CONSEQUENTS WITH REFERENCE TO THE MEASURE AND TRANSFER, OR COMMUNICATION OF FORCE

(From the *Philosophical Magazine* for May 1858.)

Modern ideas with relation to heat and the active condition of the molecular element naturally incline us to estimate every force with regard to its work-producing capacity. In the following paper I have considered gravitation under this aspect, and in doing so, have been led to discuss some points relating to dynamical sequence in the abstract.

The principle of the conservation of force may now be termed the guiding star of physical inquiry, inasmuch as it appears to be generally admitted as the effective antecedent of the transmutations and correlations of natural forces. Although received and upheld as an abstract principle in philosophy before the time of Newton, it was distinctly rejected both by him and by Laplace as inconsistent with natural phenomena. Even at the present day, mathematicians have been so long accustomed to and brought up in the statical method of treating molecular physics, initiated by those great leaders, that the inefficiency and inconsistency of this mode of inquiry with the mechanical theory of heat seems as yet not to be fully appreciated by some even of the most zealous upholders of that theory. With this impression, it seems desirable that every effort should be made to arrive at a clear understanding of fundamental points, and of the principle of physical causation which the mechanical theory supplies.

§ 1. The Integral of Gravitation is a function of Space.

Suppose a central homogeneous globe to augment in bulk by the descent of similar matter from an infinite distance in radial directions all round; each descending element, on arriving at the surface of the globe, presents itself charged with a certain amount of mechanical force equivalent to the square velocity with which it impinges. If we confine our attention to *the centripetal influence of the original central globe only*, the square velocity of the descending element diminishes in the inverse ratio of the radius of the augmenting globular mass; for it is upon the surface of this that impact takes place, and the matter that has been added to the original globe is assumed not to augment the centripetal force acting upon the descending matter.

Taking for standard unit the square velocity generated by falling through the radius of the globe with the force of gravity uniform as at surface, and computing the integral mechanical effect between the original surface of the globe and any other spherical surface external to and concentric with that surface, it is found to be equivalent to the product of the standard square velocity by a mass of matter that would cover a surface equal to a great circle of the outer sphere, *minus* a great circle of the central globe, to a depth equal to the radius of the globe. This ratio is equal to the ratio of the square of the cube root of the space between the concentric spherical surfaces, and continually approaches the ratio of the surface of the outer sphere.

If the matter composing a planetary globe is assumed to have originally descended from space, and to have become centrally collocated in successive layers, it may be viewed with reference to its gravitation integral (1) as having accomplished work, (2) as having the faculty of accomplishing work, of generating force. The following are a few theorems developing the quantitative relations.

1. *The work accomplished, or the mean square velocity of the molecules of a planetary globe acquired by the centripetal precipitation of the matter of that globe from an infinite distance, is equal to* $\frac{3}{5}$ *the square velocity acquired by a body*

*falling through radius of the globe with the uniform force of gravity at its surface.*

The following may serve to illustrate this.

If as much matter (iron) descended to the earth from planetary space as would cover the whole surface to a uniform depth of $\frac{1}{200}$ of an inch, it would, on entering the atmosphere with the mean velocity of 20 miles per second, generate as much heat as the whole atmosphere contains. The steps of the computation of this are as follows:—the motion of water moving with velocity of 20 miles per second, converted into heat, would raise its temperature about one million degrees; iron about six millions. In about 10 feet depth of water there is as much heat as in the superjacent atmosphere. The quantity of heat in a cubic foot of iron is nearly the same as in a cubic foot of water. Iron $\frac{1}{100}$ inch in thickness, heated six million degrees, is equivalent to 10 feet heated 500°, and 500° absolute is 40° on Fahr. scale. The mean absolute temperature of the atmosphere is 250° when temperature at base is 40° F.

2. *Comparing the work accomplished, or molecular square velocity in globes of different radii and composed of the same kind of homogeneous matter, it is found to follow the ratio of the square of radius.*

As an example, the earth may be compared with the sun in this respect, supposing them of equal density and homogeneous. If the temperature in descending the earth's radius increases uniformly 1° in 50 feet, the mean temperature of its whole mass would be about 100,000°. This is also nearly the mean temperature that would result from the *work accomplished*, if the matter composing the earth had the specific heat of iron, the mean square velocity being nearly the same as the square velocity of a projectile moving in a circular orbit round the earth grazing its surface. The sun's radius being about 100 times that of the earth, its mean temperature would be 100,000 × 10,000, or one thousand million degrees. The force of gravitation may thus be viewed as the effective antecedent of the sun's heat.

3. *The capacity of a globe to generate force, increases in a ratio that continually approaches the square of the radius of the sphere within which its force of gravity is assumed to be limited, and the capacity of different globes follows a ratio compounded of the mass of the globe and the square of the cube root of the space throughout which the action is supposed to be limited.*

4. *The limiting sphere of the action of gravity being supposed constant, and the size of the globe to vary, the capacity increases as the surface of the globe.*

5. *The capacity of different globes is the same if their force of gravity is assumed to be limited within spheres that are in the inverse ratio of the globes respectively.*

## § 2. The Force-generating faculty exists in space and is directed centripetally.

This is proved by the following considerations.

The integral force-producing power of any body, however small, subject to the law of universal gravitation, is illimitable as space. It is impossible to imagine an infinite attribute belonging to a finite entity. It is therefore in space that the energy that contributes the power of gravitation exists, and the element of matter merely gives to it a centripetal direction.

This, as a consequent of the law of gravitation, seems noteworthy from it probably being applicable to molecular forces generally. It favours the idea that the function of the material element is to give *direction* to a living force that pervades space.

This is further discussed in § 14.

## § 3. The law of Gravitation with respect to the element of radial space.

This law is usually defined with reference to a constant *element of time*; the increment of velocity generated being proportional to the increment of time—whatever the direction or velocity of the motion—and inversely as the square of the central distance. If we view it with reference to a

constant *element of radial space*, we find that the increment of square velocity generated by the force of gravitation is proportional to decrement of radial distance, and inversely as square of central distance. This holds whatever the velocity or direction of the motion, whatever the orbit of the projectile.

Thus each element of radial distance has associated with it a fixed element of mechanical force, to be given to or taken from all bodies traversing it; whatever may be the direction of their motion or the time taken to pass through it. A weight attached to clockwork may take a day to descend through one foot, and the same weight falling freely from a height may take only $\frac{1}{1000}$ of a second to pass through the same foot, yet the mechanical force communicated in each case is the same.

§ 4. The mutual gravitation of two bodies develops mechanical force in each of them inversely proportional to its mass.

Suppose two bodies to descend towards each other by their mutual gravitation, their common centre of gravity being at rest. At any time before they meet, their acquired velocities being examined will be found inversely as their masses, which assume as 1 to 10. Suppose them removed to the earth's surface and each projected up a vertical with their acquired velocities respectively: the smaller body rises 100 times the height ascended by the larger, and thus in again descending would be able to perform ten times the work.

Although in ordinary parlance the action and reaction of these two attracting bodies are equal according to Newton's third law, their common centre of gravity being at rest, so that if we "suppose an obstacle interposed to hinder their congress," * that obstacle must be as much pressed on one side as the other; nevertheless, when after being set free to obtain motion from the impulse of the mutual gravitation, and after a certain time their condition is examined, we find that the lesser has gained the power of performing the work 10, while the greater has only obtained the power of performing the work 1.

* *Principia.*

§ 5. The development of mechanical force, during the same interval of time, in bodies set in motion by the same pressure is inversely proportional to their mass, if the pressure acts with the same intensity when a body is in motion as when it is at rest.

Suppose a long horizontal cylinder with its transverse section equal to one square inch. Let one end be stopped with a piston weighing 15 lbs., and the other end with another piston weighing 150 lbs. Let the cylinder be now exhausted of air, and suppose the pistons to be movable air-tight without friction. If they are supposed to press on each end of a rod passing through the axis of the cylinder, they would be *in equilibrio,* they would press equally in opposite directions. Take away the rod, and allow them to move freely by the atmospheric pressure. At the end of one second the 150 lb. piston acquires a velocity of 3·2 feet per second, and the 15 lb. piston a velocity of 32 feet per second. With these velocities, suppose them to be projected upwards in a vertical, the lighter would ascend 16 feet and the heavier $\frac{16}{100}$ of a foot. In descending, the lighter would thus be enabled to exert ten times the mechanical power of the heavier.

Strictly, the pressure on the smaller piston in this example is not quite equal to that on the larger after they have begun to move, in consequence (if the expression may be allowed) of the *vis à tergo* not being infinitely prompt.

## § 6. Equal pressure does not imply equal capacity of generating motion.

This position, inferred from the examples given in §§ 4, 5, shows the necessity of guarding against being led away by the words action (which implies states of acting or moving) and reaction, to conceive that an equal impressed tendency to move implies an equal capacity of generating motion.

A motive cause may act *equally* on two bodies at *rest,* but *unequally* on bodies moving with *unequal velocities,* and the effect may be in the proportion of these velocities respectively.

The following dynamic arrangement will serve perhaps to convey a distinct idea of this:—

Let there be two perfectly elastic plates presenting the same surface, but unequal in mass, say in the ratio of 1 to 10. On one side of each let a current of perfectly elastic particles be supposed to impinge continually,—the minuteness as well as velocity of these particles being excessive. Whether these plates are at rest or moving in a direction perpendicular to the plane of their surface, the number of particles that impinge in an element of time must be constant so long as the velocity of the particles is incomparably greater than the velocity of the plates. If the plates are kept at rest by an interposed obstacle, the impinging particles are reflected without change of velocity, and no motion is transferred from them to the plate, or from the plate to them. If the plates move in the same direction as the particles with velocity 1, the particles at impact lose one decrement of square velocity, and if with velocity 10, then the particles lose ten decrements of square velocity. These decrements being transferred to the plates, become collectively increments of motion having the character of an accelerating force.

While the plates are prevented from moving by resting on an interposed obstacle, the current of impinging particles forms a *pressure or tendency to put the plates in motion*, which is equal in both plates, but *the faculty of taking motion from the current depends on the plates being in a state of motion and increases with their velocity*. The heavier plate, by its greater mass being confined in its velocity to one-tenth, has its ability to take force from the current reduced in this proportion.

## § 7. Gravitation as an impulse.

If the fact of the integral of gravitation as a force generator, being a function of space, is admitted as an argument for the existence of the force in space before it is communicated to the falling body, it realises the idea or impression which Newton has expressed in the introduction to Section 11 of the First Book of the *Principia*, viz.: "I shall therefore at present go on to treat of the motion of bodies mutually attracting each other, considering the centripetal forces as

attractions, though perhaps *in a physical strictness they may more truly be called impulses.* But these propositions are to be considered as *purely mathematical,* and therefore *laying aside all physical considerations,* I make use of a familiar way of speaking to make myself more easily understood."

Considered as an impulse, gravitation has several characteristics,—statical, dynamical, and directive.

1. It is an impulse the *statical* magnitude of which upon an element of matter is determined by the inverse square distance of that element from another: precisely as if an emanative influence had issued from each as a centre and become diluted as it became diffused through an augmenting spherical surface.

2. An impulse that accumulates square velocity on an element in motion in a ratio combined of the rate at which its distance from the central element diminishes, and of the amount of statical impulse due to that distance, and to the number of elements to which the impulse is directed. This defines its *dynamical* or force-producing magnitude.

3. An impulse operating exactly in the direction of and towards the distant material element.

4. An impulse cumulative to an apparently illimitable extent, according to the number of the distant collocated elements of matter: *e.g.,* at the earth's surface an element of matter in passing—in any direction and with any velocity—through a foot vertical, has given to it 64 feet of square velocity. At the surface of the sun, a foot vertical contributes 1800 feet square velocity.

5. An impulse, that, so far as observation extends, although dependent on matter at a distance, is uninfluenced by intervening matter.

6. An impulse that is not diminished by the velocity with which a body moves in the direction in which it operates.

Two of these characteristics, the fifth and sixth, are physical paradoxes of the same degree as the marvel in the wave theory of light, that the medium or media that convey the impressions of heat and light, although acting and acted upon by the elements of bodies, presents no resistance to the planetary motions.

The sixth has been put to the test by Laplace while seeking to account for the secular inequality of the moon. Mr Adams having lately found the treatment of this subject defective, it might be worth while to examine analytically the influence that an infinitesimal divergence from the law of gravitation in the case of bodies approaching or receding from each other would have on the planetary motions, so as to be prepared to recognise its accumulated effects through geological periods of time. The tendency of first differentials of an elliptic orbit seems to be motion of apses in the direction of the motion of the planet; diminution of the major axis, and the minor axis unchanged; ellipses thus tending towards circles with fixed diameters. The chief question is as to the moon's motion being so delicate a test of the divergence as Laplace has deduced from his analysis.

## § 8. The quantity of motion in a body is proportional to the square of its velocity.

Motion as a physical entity is a combination of two quantitative elements, *matter*, *velocity*, separately measurable. When united the standard of measurement is not obvious. Is the velocity or square of the velocity to be taken *as that which takes equal part with matter* in the phenomenon which results from their union? This question, debated since the time of Newton,* seems now to have settled down into a tacit admission of both. This was the opinion of Dr Thomas Young, who considered the fact that two bodies while freely acting on each other obtained from their mutual action velocities inversely proportional to their respective masses, as conclusive of the simple ratio of the velocity being the natural standard.

* This is discussed in the last part of the article on virtual velocities, *Penny Cyclopædia*. It concludes as follows:—"But if pressure be merely considered as the cause of motion, and called force, in that sense it is very difficult to see why the cause, which is only known by the effect, is to be measured by anything but the simple effect. Probably this discussion gave rise to the chapter of the *Mécanique Céleste*, in which Laplace speculates upon what the laws of motion would have been if force had been as a function of the velocity instead of as the simple velocity. We have never met with anyone who could give us an intelligible account of the meaning of this investigation.

If this proposition had been stated in other terms as above, § 4, viz. that bodies while freely acting on each other obtained from their mutual action powers of producing work inversely proportional to their masses, no such necessity of adopting the simple ratio of the velocity would have been felt; only a noteworthy characteristic of centripetal action. The position of the centre of gyration in a revolving body is clearly on the side of the square velocity being the natural standard. The motion of impinging elastic bodies also requires the square velocity.

The following considerations as to impacts, it may be useful to keep in view as accessory to the principle of the conservation of force.

### § 9. Force of impact depends on the relative motion of the impinging bodies.

Whatever be the molecular texture of bodies, we may bring all impacts to the condition of perfect elasticity by interposing a spring. There are three stages in such phenomena:—1. The motions before impact before the spring is acted upon. 2. The motion at impact, or at the instant when the maximum compression of the spring has been effected by the force of impact. 3. The motions after recoil, after the spring has given out again the force it had received. During all three the common centre of gravity is unaffected in its state of motion or rest, and *the force of impact, as measured by the spring's tension, is dependent, not on the absolute, but on the relative* motion of the impinging bodies.

### § 10. The condition of impinging bodies demonstrates the absurdity of assuming the motion of a body to be proportional to its velocity.

Let us take a simple numerical example. A ball of 1 lb. weight with velocity 9 strikes a spring interposed upon another ball of 2 lbs. weight which is at rest. The velocity of the centre of gravity is 3; the motion of the 1 lb. ball *relative to centre of gravity* is 6, and of the 2 lb. ball 3. These relative motions are exactly reversed by the recoil of the spring after impact. Thus the *absolute* motion of the 1 lb.

ball after impact becomes 3, and the absolute motion of the 2 lb. ball 6. At the instant when the spring has obtained its maximum compression by the force of the collision, the velocity of both balls is 3, the same as the centre of gravity.

Let us now compute the quantity of motion in the system, assuming the motion of a body to be proportional to its velocity.

Before impact the amount is . . . $1^{\text{lb.}} \times 9 + (2^{\text{lbs.}} \times 0) = 9.$
After the spring has received the force of compression, the amount is . . $(1^{\text{lb.}} + 2^{\text{lbs.}}) \times 3 = 9.$
After recoil, when the spring has given out its force, the amount is $1^{\text{lb.}} \times 3 + (2^{\text{lbs.}} \times 6) = 15.$

Thus the spring has been bent, compressed, or as it were, wound up without taking any motion from the system, without any cost of work; and the system has actually gained motion by the collision. The concurrence of the two bodies has nearly doubled their collective momentum.

If the motion of a body is assumed to be proportional to the square of its velocity, the quantity of motion in the system before impact is . . . . . $1^{\text{lb.}} \times 81 + 2^{\text{lbs.}} \times 0 = 81.$

After the spring has received the force of compression, the amount is . . . . . $(1^{\text{lb.}} + 2^{\text{lbs.}}) \times 9 = 27.$
After recoil, when the spring has given out its force, the amount is $1^{\text{lb.}} \times 9 + 2^{\text{lbs.}} \times 36 = 81.$

The collective motion before and after impact is thus the same, and $81 - 27 = 54$ is the force that is taken to compress, bend, or wind up the interposed spring: it is the work rendered latent at the instant of impact but immediately restored. It is a dynamic, not a static force, and with the same impinging bodies it is proportional to the *square* of their *relative* velocity. Thus if the 2 lb. ball with velocity 1 meets the 1 lb. ball with velocity 8, the relative velocity is still 9, and the force of impact still 54. This is also apparent from the general expression for the force of impact, viz.

$$\frac{}{v_1 - v_2}^2 \left(\frac{w_1 w_2}{w_1 + w_2}\right).$$

§ 11. The condition of a system having both a rotatory and rectilineal motion demonstrates the absurdity of assuming the motion of a body to be proportional to its velocity.

In the 31st Query of Newton's *Optics* (vol. iv. p. 258, Horsley's edition), the following occurs:—"From the various composition of two motions, it is very certain that there is not always the same quantity of motion in the world. For if two globes joined by a slender rod revolve about their common centre of gravity with a uniform motion, while that centre moves on uniformly in a right line drawn in the plane of their circular motion, the sum of the motions of the two globes, as often as the globes are in the right line described by their common centre of gravity, will be bigger than the sum of their motions when they are in a line perpendicular to that line. By this instance, it appears that motion may be got or lost. But by reason of the tenacity of fluids and attrition of their parts, and the weakness of elasticity in solids, motion is much more apt to be lost than got, and is always upon the decay."

This extract is remarkable as showing the obstinate influence of a preconceived notion upon a powerful intellect. The alternate appearance and disappearance of motion in connected globes did not seem to Newton inconsistent with any of the laws of motion previously laid down in his *Principia*. Let us view a numerical example. The globes suppose to be equal and to move with a circular velocity of 3, and the common centre of gravity with a rectilineal velocity of 4. As often as the globes are in a line perpendicular to the line described by the centre of gravity, the absolute velocity of one is 7, and of the other 1, making the sum of their motions 8. As often as the globes are in the right line described by the common centre of gravity, the absolute velocity of each is 5, making the sum of their motions 10. From the first to the second position, the motion in one has diminished from 7 to 5, losing 2, and the motion of the other has increased from 1 to 5, gaining 4. Hence not only has the 2 been transferred from the one globe to the other, but another 2 has been supplied. There is an intermittent augmentation and diminution of motion; and as by the second law of motion "the alteration

of motion is ever proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed," it comes to pass, that, by giving motion to the centre of gravity of two revolving balls in the plane of revolution, we introduce the action of a tangential force; but if the same motion is given in a direction perpendicular to the plane of revolution, no force is introduced, the sum of the motions being always 10.

If we inquire what is the function of the absolute velocity which, being multiplied by the mass of each globe respectively, shall give a sum that is constant, whatever may be the direction of the motion of the common centre of gravity, we shall find that the square velocity is the only function that answers, and that although the absolute velocities are continually changing through a cycle, the sum of the products of the mass of each body by the square of its respective absolute velocity, is a constant quantity, and equal to the sum of the rectilineal and rotatory *vis viva* taken separate.

## § 12. Distinction between momentum and motion.

In our educational treatises, up to the present time the idea continues to be inoculated into the student of physical science, that the natural standard measure of motion in the abstract is the product of the mass of a body or system by the velocity of its centre of gravity. The ambiguity and contradictions that this involves when he extends his studies, might be avoided if such definition was confined to the word momentum, and the word motion taken to mean the product of the mass by its square velocity. The following instance will serve to make this appreciable.

In the use of the Ballistic pendulum, the same velocity is communicated to it by the stroke of a 2 lb. ball moving 100 feet per second as by a 1 lb. ball moving 200 feet per second; the *momentum* or velocity of the centre of gravity of the system composed of the bullet and pendulum being in the two cases the same, or very nearly so. Suppose the 1 lb. ball to be hollow, so as to have the same diameter as the 2 lb. ball, its penetration into the timber of the pendulum would be found to be double

that of the 2 lb. ball, because its *quantity of motion* is double. Its working power or efficacy is double. The definition of *momentum* would thus conform to the condition of inelastic impact, where the force of impact disappears in molecular disruption, or production of heat, or both combined. The definition of *motion* confined to the natural standard of living force would conform to the causal relation of dependent action.

§ 13. Newton and Laplace's arguments in favour of the simple ratio of the velocity being the natural standard of motion.

Newton employed the word motion to denote the velocity of a body conjointly with its quantity of matter, and it is evident that, confining the word to this meaning, the correctness of the *Principia* is no argument in favour of such definition applying to the natural standard. There is no doubt, however, that both Newton and Laplace believed it to be the natural standard. It may assist in setting the subject in a clear light, if we take a deliberate view of the arguments they have advanced.

In Dr Brewster's *Life of Newton*, vol. ii. p. 379, there is given what is stated to be Newton's argument as opposed to Leibnitz, who had asserted the force of falling bodies to be as the square of their velocities. It is based on the assumption that fine threads of silk may be broken with less and less force as the velocity of the body that breaks through them increases. These threads individually may be taken to represent the resistance that gravity opposes to upward motion through a constant element of the vertical, as they are supposed to be arranged at equal distances, and each requires the expenditure of an element of force, or square velocity, to rend it. The retardation of the motion of a globe through such threads horizontally, is precisely similar to the retardation of the motion of a body projected up a vertical. In an element of time there is a loss of an element of velocity, or in an element of space there is the loss of an element of square velocity. Thus the argument derived from such an instance is not conclusive; it is not, any more than the laws of falling bodies, decisive of the question.

Laplace's argument is stated in the *Système du Monde*, p. 300 (Pond's translation), as follows:—

"It is observed upon the earth that a body solicited by any force moves in the same manner, whatever be the angle which the direction of this force makes with the direction of the motion which is common to the body and to the part of the terrestrial surface to which it corresponds: the same thing takes place in a vessel whose motion is uniform: a *movable body submitted to the action of a spring or of gravity*, or any other force, moves *relatively* to the parts of the ship in the same manner, whatever be the velocity and direction of the vessel. It may then be established as a general law of terrestrial motions, that if in a system of bodies carried on by a common motion, *any force be impressed on one of them, its apparent or relative motion* will be the same, whatever be the general motion of the system and the angle which its direction makes with the impelling force. *The proportionality of force to velocity results from this law, supposed rigorously exact; for if we suppose two bodies moving upon one straight line with equal velocities, and that by impressing on one of them a force, which increases the primitive force, its relative velocity to the other body remains the same as if both of them had been primitively in a state of repose:* it is evident that the space described by the body in consequence of its primitive force, and of that which is added to it, becomes equal to the sum of the spaces which each of them would have caused it to describe in the same time; which supposes the force proportional to the velocity."

We may understand, from the instance of collision given in § 10, that the fact of relative motion being the same whatever the general motion of the system, is quite consistent with the proportionality of force to square velocity. It may seem at first somewhat of a paradox, that on striking a billiard ball in a westerly direction at noon so as to give it a velocity of 10 feet per second, we should actually communicate to that ball such a force or motion as would give it a velocity of 1500 feet per second if the earth were at rest; but the difficulty disappears when we consider that *the force of impact or shock is made up of the difference between the sum of the absolute*

*motions before impact, and at impact or just before recoil begins, and that this difference is proportional to the square of the relative velocity.* The transference of force from one body to another depends on the *absolute velocities*, so that it is physically possible to transfer any amount of force, however large, by an impinging velocity, however small. The shock of impact, and the amount of force transferred, do not stand in the relation of cause and effect. The one is not a function of the other, and the force of impact indicates nothing in regard to the "*impressing of force*" or "*increasing of force*" which depend on the absolute velocities, of which we cannot be said to know anything certain.

Take the instance of a ball swinging as a pendulum within a railway carriage in the direction of its motion. Whether the carriage is at rest or in uniform motion at any speed, the motion of the pendulum relative to the carriage is the same. This supposes the mass of the carriage to be incomparably greater than the ball. When they draw near to each other, as with a gunner's eprouvette, the carriage sensibly reciprocates the oscillation of the suspended cannon. Suppose between the pendulum ball and point of suspension, a spring is interposed and the force on this spring resolved in a horizontal direction. This horizontal part accumulates to a maximum in one direction at the lowest part of the arc of oscillation, then changes the direction of strain, and accumulates to a maximum in the opposite direction; in one-half of the arc pulling forwards and in the other pulling backwards. The amount of strain is like the force of impact, a function of the *relative* velocity; the force transferred backwards and forwards from the carriage to the ball, and *vice versâ*, is a function of the *absolute* velocities.

### § 14. On the Transfusion of *vis viva* across a system that simultaneously revolves and progresses.

Perhaps the most difficult point to realise to the mind consistently with the causal relation of dependent action, is the periodical ebbing and flowing of *vis viva* from and to each part of a system endowed with a rectilineal as well as a rotatory movement. In the simple example of two equal

globes, given in § 11, as often as they are in a line perpendicular to the line described by the common centre of gravity, the *vis viva* of one (A) is 49, and the other (B) 1. Setting out from this, A loses and B gains *vis viva* until they come to the line in which the common centre of gravity moves, when A is found reduced to 25 and B has risen to the same amount, having in the quarter revolution gained 24 parts of square velocity which has been transferred to it from A. During the next quarter another 24 parts have passed over. The ebb of force from A here ceases, and is succeeded in the next quarter revolution by a flow of *vis viva* from B to A, which continues until the revolution is completed, when the same circle of changes is repeated. This transference of force from one part of a system to another being unaccompanied with any appearance of action, may be termed *transfusion*, as in the heading of this paragraph; it is nevertheless as real a transference of force as what takes place on a billiard table.

How are we to present such phenomena to our minds in conformity with Newton's second law of motion, viz., "*The alteration of motion is ever proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed*"?

Let us take, for example, the system formed by the earth and the moon. At full moon the absolute motion of our satellite considerably exceeds the amount it contains when at the opposite point of its orbit, and in the earth it is *vice versa*. How may we conceive this flux and reflux to take place?

In § 6 a dynamic arrangement was suggested, which represented the peculiarity in the development of mechanical force, specified in §§ 4 and 5, as a physical characteristic deduced from the law of elastic impact: is it possible to deduce transfusion from the same primary mode of action?

Let there be two perfectly elastic plates of equal surface and mass placed opposite and parallel to each other; on the exterior surface of each, let a current of perfectly elastic particles be supposed to impinge continually; the minuteness as well as velocity of these particles being excessive. The effect of the current would be to drive the plates towards each other with an accelerative force (§ 6). Let the velocity

acquired at the end of nine seconds be 9, and in the next second 10; the square velocity increases in one second from 81 to 100, the augmentation in each being 19, or 38 in the sum of both. Let us now suppose the plates, before the respective currents set upon them, to have had a uniform velocity of 20 feet in the direction AB. In nine seconds after the currents commenced to act, the velocity of A has become 29, and that of B 11. In the next second, A is up to 30 and B down to 10, and in one second the square velocity in A has increased from 841 to 900, and that of B decreased from 121 to 100. The sum of both has during this second increased from 962 to 1000: the augmentation being 38, as before, when the common centre of gravity was at rest. It will be remarked that the relative motion of A and B, as well as increase of their united *vis viva*, is the same in this last case as in the former; nevertheless there has been a transfusion of force from B to A, the amount of which is 40, being equal to twice the velocity of the common centre of gravity. If A had received no greater augmentation of square velocity than in the former case, the amount at the end of the tenth second would be $841 + 19 = 860$, but it is actually 900. Again, if B had received an augmentation the same as in the previous case, the amount at the end of the tenth second would be $121 + 19 = 140$, but it is 100. Hence 40 has been transfused from B to A.

If the motion of the common centre of gravity had been in a direction perpendicular to AB, the relative motion and augmentation of united *vis viva* would be the same, but there would be no transfusion; it is only when the current acts in the direction of or contrary to the absolute motion.

Suppose the plates A, B to revolve around their common centre of gravity with such velocity that the centrifugal tendency just equilibrates the effect of the currents. So long as the centre of gravity is at rest, no force is given to or taken from the impinging currents or particles; but when rectilineal motion is given to the system, transfusion takes place. The greatest rate at which this takes place is when A and B are in the line of their absolute motion. At other parts of the revolution, when A and B have a position oblique

to the direction of absolute motion, the rate of transfusion is diminished in the ratio of the cosine of the contained angle. During one half of a revolution, the absolute motion resolved in the direction of the current goes with the current upon one of the plates and against it on the other, so that the flux and reflux of the force upon each plate alternates positive and negative to exactly the same amount.

It is thus that we may obtain a clear conception of transfusion as a necessary sequent to the mode of action of the forces of gravity, cohesion, etc., viewed in their dynamic aspect as conforming to impulses derived from elastic impact.

### § 15. Destruction and Reproduction of Force in Nature is apparent, not real.

In the mutual action of bodies there is an apparent destruction and reproduction of force, but the more such phenomena are investigated experimentally with respect to their quantitative elements, the more reason there is to believe that the disappearance and reappearance are merely transference from one agent to another, and are as exactly regulated as the communication of force by impact in the preceding examples.

Even if we had not those proofs of the existence of a universal medium that the undulatory theory of light supplies, it would be well to admit the existence of media to which ordinary matter may be assumed to be entirely subordinate in all their potential relations, so as to give order to our ideas in tracing out the dynamical sequence of nature. It would be taking too narrow a view if we limited the function of the luminiferous ether to the conveying of physical pulses only. The atmosphere also conveys physical pulses, but that is the least important of its functions in the economy of nature. There is nothing that should hinder us attributing to the media concerned in the radiation of heat and light, the higher functions of electric polarity and gravitation. The special dynamic arrangements by which this is effected may ever elude research, but as there is no limit to the *vis viva* which such media may conserve in their minutest parts, so there is no physical impossibility in that *vis viva* being suddenly transferred to the molecules of ordinary matter in the proportions

and sequence required to carry out the order and system of nature.

The fundamental principle of action in such media must be in accordance with *elastic impact*, for upon that the dynamic theory of heat and conservation of force rests as a foundation. The statical and dynamical characteristics of gravitation and transfusion of force have also been shown to conform to it, so that all the forces that hold the molecules of bodies together must also be in subjection to it.

Force issues from such media and becomes apparent in ordinary matter as *vis viva*. Force disappears from ordinary matter, and being transferred to the media, becomes non-apparent or latent. When we raise a weight from the ground, we give force (A) to the agent of gravitation; when we let fall the same weight, we take force (B) from the agent. Directing attention to the *mode* of raising the weight, we inquire where was A before it was given to the agent; what other agent held it? Also what becomes of B; what new agent holds it? Suppose the weight was raised by the action of steam, we trace it back to the force engendered by the chemical action of the fuel combining with oxygen. The force of the chemical action is transferred to the molecules of water and makes its appearance as molecular *vis viva*. Before the chemical action, the relation of A to the molecules about to combine is similar to the relation of force to a body about to fall from a height. The force about to be given is concealed in an agent, a medium, the instrument of chemical or electric force; is evolved from it, and returns to it again when the products of combustion are decomposed at a future time.

Thus may every appearance in nature be viewed as the exhibition of a part of a cycle of dependent action; and we have reason to be impressed with the necessity of keeping the dynamical aspect of a phenomenon in view, if we desire to explore its relations with others, and its place in the general scheme.

EDINBURGH, 18*th March* 1858.

# ON CHEMICAL NOTATION IN CONFORMITY WITH THE DYNAMICAL THEORY OF HEAT AND GASES.

(From the *Philosophical Magazine* for October 1863.)

In a lecture "On the Molecule of Water," lately delivered at the Royal Institution, Albemarle Street, by Mr Odling, Secretary to the Chemical Society,* the question as to the molecular weight of water was discussed with much detail, and arguments adduced to show that the correct number is 18, represented by the formula $H^2O$, and that it cannot be 9, as might be inferred from the specific gravity of the vapour, following the idea that equal volumes of bodies in the gaseous state contain an equal number of atoms.

The dynamical theory of heat and gases imperatively requires us to believe that in equal volumes of gases and vapours in equilibrio of pressure and temperature (and conforming to the primary laws of Mariotte, and of Dalton and Gay-Lussac), there exists the same number of separate parts. On the other hand, these parts cannot be in all cases the atoms that enter into chemical union, or what is understood by the term "combining element." The researches of Dumas and Mitscherlich have brought out this fact very obviously in the case of sulphur, phosphorus, arsenic, and selenium. In sulphur the ultimate element of the vapour or gaseous molecule must consist of six chemical atoms up to the temperature of 860° C., at which according to MM. Deville and Troost's researches, the vapour-density is reduced to one-third, showing that the gaseous molecule had then split into three, each of which was made up of two chemical atoms. In selenium, the vapour molecule

* A complete report of the lecture is given in "The Engineer" for 26th June.

must consist of four chemical atoms; and at 1040° C. there was found such alteration of vapour-density as led to the anticipation that from about 1200° to 1400° the ultimate vapour molecule would contain only two chemical atoms (MM. Deville and Troost, 1860).

If we were to view the ultimate moving part of all the primary gases as consisting of *at least* two chemical atoms, the same symbols would express not only the combining proportions of all the bodies entering into union, but the actual composition of a volume of the compound, and we should get rid at once of all the ambiguity and difficulties in the arithmetic of combining proportions. The absolute necessity of doing so may, I think, be easily demonstrated from the examples chosen by Mr Odling in his lecture.

The argument of the lecture is to show from the phenomena of substitution that the hydrogen of the water molecule is experimentally divisible into two equal parts; for it is from such phenomena that the chief, if not only chemical proof is to be found. To quote Mr Odling's words, "It is evident, for instance, that the molecule of marsh-gas must contain four proportions of hydrogen, because we are able to replace one-fourth, two-fourths, three-fourths, and four-fourths of its hydrogen by four successive substitutions to produce a series of bodies differing from one another by a regular gradation of properties."

In marsh-gas the ratio of H$y$, the quantity of hydrogen by weight, to the carbon C$x$, is $\frac{1}{3}$. If the vapour-density of carbon were 3 to hydrogen unity, the numerator added to the denominator of the fraction $\frac{1}{3}$, making 4, would represent the vapour-density of marsh-gas, and be represented by HC($x = y = 1$), in which the letters represent the vapour-densities of each of the constituents respectively. But the vapour-density of marsh-gas is 8; and assuming the vapour-density of carbon to be 12, we have $\mathrm{H}y + \mathrm{C}x = 8$ and $\frac{\mathrm{H}y}{\mathrm{C}x} = \frac{1}{3}$; substituting 1 = H and 12 = C, we have $y + 12x = 8$ and $\frac{y}{12x} = \frac{1}{3}$. From these two simple equations we find $x = \frac{1}{2}$ and $y = 2$. Hence marsh-gas is expressed by $\mathrm{C}\frac{1}{2} + \mathrm{H}2 = \mathrm{C}^{\frac{1}{2}}\mathrm{H}^2$, which

means that a gaseous molecule of marsh-gas is composed of half a molecule of carbon united to two gaseous molecules of hydrogen.

In monochloromethene, or muriate of methyle, the ratio of the hydrogen to the carbon is $\frac{1}{4} = \frac{\mathrm{H}y}{\mathrm{C}x} = \frac{y}{12x}$. The ratio of the hydrogen to the chlorine is $\frac{1}{12} = \frac{\mathrm{H}y}{\mathrm{Cl}z} = \frac{y}{36z}$, and the vapour-density of the compound is

$$25\tfrac{1}{2} = \mathrm{H}y + \mathrm{C}x + \mathrm{Cl}z = y + 12x + 36z.$$

From these three equations we find $x = z = \frac{1}{2}$ and $y = 1\frac{1}{2}$. Hence this compound is expressed by $\mathrm{H}1\frac{1}{2} + \mathrm{C}\frac{1}{2} + \mathrm{Cl}\frac{1}{2} = \mathrm{H}^{1\frac{1}{2}}\mathrm{C}^{\frac{1}{2}}\mathrm{Cl}^{\frac{1}{2}}$; which means that its vapour-molecule is composed of one and a half gaseous molecules of hydrogen, half a molecule of carbon, and half a gaseous molecule of chlorine united together.

In the same way dichloromethene, or chloride of methyle, is expressed by $\mathrm{HC}^{\frac{1}{2}}\mathrm{Cl}$, the vapour density being

$$43 = \begin{cases} \mathrm{H} + \mathrm{C}^{\frac{1}{2}} + \mathrm{Cl} \\ 1 + 6 + 36 \end{cases};$$

and chloroform by $\mathrm{H}^{\frac{1}{2}}\mathrm{C}^{\frac{1}{2}}\mathrm{Cl}^{1\frac{1}{2}}$, its vapour-density being

$$60\tfrac{1}{2} = \begin{cases} \mathrm{H}^{\frac{1}{2}} + \mathrm{C}^{\frac{1}{2}} + \mathrm{Cl}^{1\frac{1}{2}} \\ \frac{1}{2} + 6 + 54 \end{cases};$$

and the tetrachloromethene, or perchloride of muriate of methyle, by $\mathrm{C}^{\frac{1}{2}}\mathrm{Cl}^2$, its vapour-density being 78.

Following the same system, water is expressed by

$$\mathrm{HO}^{\frac{1}{2}} = 9 = \begin{cases} \mathrm{H} + \mathrm{O}^{\frac{1}{2}} \\ 1 + 8 \end{cases},$$

its vapour-molecule being composed of one gas-molecule of hydrogen united to half a gas-molecule of oxygen.

Phosphoretted hydrogen consists of one part by weight of hydrogen united to 10·5 phosphorus, or $\frac{\mathrm{H}y}{\mathrm{P}x} = \frac{1}{10{\cdot}5}$, and its vapour-density is $17{\cdot}5 = \mathrm{H}y + \mathrm{P}x$. The vapour-density of phosphorus being $64 = \mathrm{P}$, we have

$$\frac{y}{64x} = \frac{1}{10{\cdot}5} \text{ and } y + 64x = 17{\cdot}5.$$

From these two equations we have $y = 1\frac{1}{2}$ and $x = \frac{1}{4}$; and phosphoretted hydrogen is represented by $H^{1\frac{1}{2}} P^{\frac{1}{4}}$, which signifies that a gaseous molecule of phosphoretted hydrogen is composed of $1\frac{1}{2}$ gas-molecule of hydrogen united to one-quarter of a gaseous molecule of phosphorus. In the sesquichloride of phosphorus the weight of phosphorus is to the weight of chlorine as 1 to 3·375, hence $\frac{Px}{Clz} = \frac{1}{3{\cdot}375} = \frac{64x}{36z}$. The vapour-density of the compound is $70 = Px + Clz = 64x + 36z$. From these two equations $x$ comes out $= \frac{1}{4}$, and $z = 1\frac{1}{2}$. Hence $P^{\frac{1}{4}} Cl^{1\frac{1}{2}}$ expresses the constitution and vapour-density or weight of a gaseous molecule of the sesquichloride. Thus the combining element or chemical atom of phosphorus is still one-quarter the gaseous molecule.

In the preceding examples the smallest combining element of chlorine is half the gaseous molecule. In combines also in the proportion 1, $1\frac{1}{2}$, and 2 gaseous molecules.

The smallest combining element of hydrogen is half the gaseous molecule. It combines also in the proportion 1, $1\frac{1}{2}$, 2 gaseous molecules; and further, in the organic compounds, its combining proportions extend up to 16 gaseous molecules, as in petrolin or colophon, $H^{16} C^{10}$.

The smallest combining element of oxygen is one-half the gaseous molecule. It combines also in the proportion 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3 gaseous molecules: *e.g.*, an arsenious acid vapour-molecule is composed of one arsenic vapour-molecule united with three oxygen gas-molecules.

Muriatic acid gas, $Cl^{\frac{1}{2}} H^{\frac{1}{2}}$, is composed of single half molecules of its constituents united together.

Cyanogen, NC, is composed of whole single molecules of its constituents united together; and this compound molecule assumes the deportment of a chemical element by splitting into two in some of its combinations with hydrogen, etc., as $N^{\frac{1}{2}} C^{\frac{1}{2}} H^{\frac{1}{2}}$, prussic acid; $N^{\frac{1}{2}} C^{\frac{1}{2}} Cl^{\frac{1}{2}}$, chloride of cyanogen; and $N^{\frac{1}{2}} C^{\frac{1}{2}} Br^{\frac{1}{2}}$, bromide of cyanogen.

In silicated hydrogen the ratio of hydrogen to silicon is $\frac{1}{7} = \frac{Hy}{Si \,.\, x}$, $H = 1$, $Si = ?$, $Si \,.\, H = ?$. Hence there are no data to compute $x$ and $y$. By analogy we might infer $Si = 28$;

and if $\mathrm{Si}\,x\,\mathrm{H}y = 16$, we should have $x = \frac{1}{2}$ and $y = 2$; and silicated hydrogen, $\mathrm{Si}^{\frac{1}{2}}\,\mathrm{H}^2$, homologous with marsh-gas, $\mathrm{C}^{\frac{1}{2}}\,\mathrm{H}^2$.

In ammonia the ratio of hydrogen to nitrogen is 1 to 4·67, or $\frac{\mathrm{H}y}{\mathrm{N}x} = \frac{1}{4{\cdot}67}$. The specific gravity of the gas being $8{\cdot}5 = \mathrm{N}x + \mathrm{H}y$, and $\mathrm{N} = 14$, we have $\frac{y}{14x} = \frac{1}{4{\cdot}67}$, and $14x + y = 8{\cdot}5$; hence $x = \frac{1}{2}$, and $y = 1\frac{1}{2}$. Ammonia is thus expressed by $\mathrm{N}^{\frac{1}{2}}\,\mathrm{H}^{1\frac{1}{2}}$, which means that a gas-molecule of ammonia is composed of half a gas-molecule of nitrogen united to one and a half gas-molecule of hydrogen.

In some cases of composite bodies, the components separate on boiling or sublimation. When this happens, the specific gravity of the vapour reveals the fact, as in the following instance.

In sal-ammoniac the weight of ammonia is to the weight of muriatic acid as 1 to 2·235. Its vapour-density is 13·5 $(\mathrm{N}^{\frac{1}{2}}\,\mathrm{H}^{1\frac{1}{2}}) = 8{\cdot}5$, $\mathrm{H}^{\frac{1}{2}}\,\mathrm{Cl}^{\frac{1}{2}} = 18{\cdot}5$. Hence

$$\frac{8{\cdot}5x}{18{\cdot}5y} = \frac{1}{2{\cdot}235},\quad 8{\cdot}5x + 18{\cdot}5y = 13{\cdot}5,$$

from which we find $x = y = \frac{1}{2}$; and following the usual formula, we might say half a gas-molecule of ammonia *unites* with half a gas-molecule of muriatic acid to form one gas-molecule of muriate of ammonia; or if this splitting of $(\mathrm{N}^{\frac{1}{2}}\,\mathrm{H}^{1\frac{1}{2}})$ into two $(\mathrm{N}^{\frac{1}{4}}\,\mathrm{H}^{\frac{3}{4}})$, and $\mathrm{H}^{\frac{1}{2}}\,\mathrm{Cl}^{\frac{1}{2}}$ into two $(\mathrm{H}^{\frac{1}{4}}\,\mathrm{Cl}^{\frac{1}{4}})$, is inadmissible, we have the alternative of the two whole molecules remaining separate, and the ammonia and acid vapour forming a simple mixture with specific gravity 13·5, equal to the mean of the two $\left[\frac{8{\cdot}5 + 18{\cdot}5}{2} = 13{\cdot}5\right]$.

The same is the case with the nitrous, formic, and acetic ethers, but not so with the oxalic and succinic ethers.

The three first are

$$\frac{\mathrm{O}^{1\frac{1}{2}}\,\mathrm{N} + \mathrm{H}^4\,\mathrm{C}^2\,.\,\mathrm{HO}^{\frac{1}{2}}}{2} = \frac{38 + 37}{2} = 37\tfrac{1}{2},$$

$$\frac{\mathrm{O}^{1\frac{1}{2}}\,\mathrm{CH} + \mathrm{H}^4\,\mathrm{C}^2\,.\,\mathrm{HO}^{\frac{1}{2}}}{2} = \frac{37 + 37}{2} = 37,$$

$$\frac{\mathrm{O}^{1\frac{1}{2}}\,\mathrm{C}^2\,\mathrm{H}^3 + \mathrm{H}^4\,\mathrm{C}^2\,.\,\mathrm{HO}^{\frac{1}{2}}}{2} = \frac{51 + 37}{2} = 44.$$

The two last

$$O^{1\frac{1}{2}}C + H^4C^2.HO^{\frac{1}{2}} = 73,$$
$$O^{1\frac{1}{2}}H^2C^2 + H^4C^2.HO^{\frac{1}{2}} = 87.$$

It will be remarked that the symbols and notation, compared with the vapour-density, reveal such facts without any effort of the attention.

Mr Odling advocates Gerhardt's system of notation; but although it is nominally based, like that of Berzelius, on the so-called hypothesis that all simple gases contain equal numbers of atoms in equal volumes, it does not recognise the difference between gaseous molecule and chemical atom so distinctly as to avoid obscurity and misrepresentation: *e.g.* Mr Odling states that "when water is acted upon by metallic sodium, one equivalent of sodium expels one equivalent of hydrogen to form the well-defined body hydrate of sodium, and the action proceeds no further; but when melted hydrate of sodium at a temperature approaching that of redness is acted upon by metallic sodium, a second equivalent of sodium turns out a second equivalent of hydrogen to produce oxide of sodium." The diagram appended represents the half of the hydrogen in *one* molecule of water as expelled by a molecule of sodium to form the solid hydrate, and afterwards the second half to produce the oxide of sodium. Now the fact is, that two molecules of water are engaged in forming the one molecule of the hydrate, and the hydrogen of only one of them is expelled in forming the hydrate; and in the terminal action, the hydrogen of the other molecule of water is expelled by a repetition of the same action between the newly supplied sodium and the oxygen of the second molecule.

Thus also Dr Graham, in his second volume, p. 514, states with reference to the analogous action between potassium and water, "The representation of water by HHΘ, as in Gerhardt's system, possesses the additional advantage of corresponding with the important fact that it is possible to replace either the half or the whole of the hydrogen in * water by a metal. Thus potassium thrown into water displaces half the hydrogen

* Here is an ambiguity. Is one molecule meant, or two?

and forms hydrate of potash, HKΘ; and when this compound, in the solid state, is heated with an additional quantity of potassium, the remaining half of the hydrogen is displaced, and anhydrous potash, KKΘ, is formed. On the contrary, when potassium acts on hydrochloric acid, HCl, it displaces the whole of the hydrogen and forms chloride of potassium, KCl. This is an important difference, which is easily understood on the supposition that water contains two atoms, and hydrochloric acid only one atom of hydrogen; whereas if these two compounds are represented by the analogous formulæ HO and HCl, the cause of the difference of action is by no means apparent." The ambiguity here is caused by Gerhardt's symbols actually representing *two* gaseous molecules; but they are not distinctly recognised as having this meaning either by Mr Odling or Dr Graham, as the following shows. In the above extract HKΘ represents one molecule of the hydrate of potash, but HHΘ represents two molecules of water, and KKΘ two molecules of anhydrous potash. The actions expressed by the fractional system of notation (submitted as the *only* system mathematically conformable to the law of volumes) is as follows:—Potassium, K, in contact with water, $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$, etc., expels the H of one $HO^{\frac{1}{2}}$ and unites with another $HO^{\frac{1}{2}}$, forming the solid $KO^{\frac{1}{2}}HO^{\frac{1}{2}}$. Another K heated with this solid expels the H and takes the $O^{\frac{1}{2}}$ of the $HO^{\frac{1}{2}}$, the result being $KO^{\frac{1}{2}}$, $KO^{\frac{1}{2}}$. The first is a double act of combination, forming one molecule out of three. The second is a case of resolution by preponderating affinity.

With reference to chloride of potassium: when potassa, $KO^{\frac{1}{2}}HO^{\frac{1}{2}}$, or $KO^{\frac{1}{2}}$ is presented to $H^{\frac{1}{2}}Cl^{\frac{1}{2}}$, $H^{\frac{1}{2}}Cl^{\frac{1}{2}}$ in water, the $O^{\frac{1}{2}}$ in one molecule of the oxide unites with the two $H^{\frac{1}{2}}$ of two molecules of the acid to form water (no hydrogen is evolved), and the two $Cl^{\frac{1}{2}}$ of the same two molecules of acid unite with the K, forming KCl, the chloride of potassium.

Potassium in hydrochloric acid gas, K, $\begin{matrix} Cl^{\frac{1}{2}}H^{\frac{1}{2}} \\ Cl^{\frac{1}{2}}H^{\frac{1}{2}} \end{matrix}$, releases two $H^{\frac{1}{2}}$ of two molecules of the acid to form H (which is evolved), and the two $Cl^{\frac{1}{2}}$ unite with K to form KCl. Lastly, potassium, K, in chlorine, Cl, takes fire and burns into KCl.

After discussing several cases of substitution and of chemical parallels as arguments adduced "to show that the molecular weight of water is 18, corresponding to the formula $H^2O''$, and not 9, corresponding to the formula $HO'$, Mr Odling makes the following remark:—"In addition to the above-mentioned chemical parallel, there is one very remarkable physical relation subsisting between marsh-gas, ammonia, water, and hydrochloric acid, namely that all four molecules occupy the same bulk. Two cubic feet, for instance, of hydrochloric acid gas, HCl, yield one cubic foot of hydrogen and one cubic foot of chlorine. Two cubic feet of steam, $H^2O$, yield two cubic feet of hydrogen and one cubic foot of oxygen, etc." The meaning is apparently that the symbols attached to the letters express the number of volumes of the constituent gases in *two* volumes of the compounds. That this should be noticed as "a very remarkable physical relation" at once indicates that the mode of notation, although it is founded on the law of volumes, is nevertheless defective. *All* molecules, simple or compound, in the gaseous state, occupy the same bulk. This the dynamic theory proclaims as a fact. Let the notation be founded thereupon, and we obtain, expressed with perfect conciseness, at once the chemical composition, the physical density of a *unit* volume, and the constitution of a molecule. The analytical chemist may object to the introduction of fractional exponents, from the preconceived notion that a gaseous molecule is with more propriety to be viewed as a combination of two or more chemical indestructible atoms than that such primary atom is a fraction of a gaseous molecule; but if this feeling can be overcome, the advantage obtained would, I believe, fully repay the effort. To the mathematical student of thermo-molecular physics, there is as distinct evidence of the gaseous molecule of oxygen splitting into two equal parts when combining with hydrogen to form water, as there is to the astronomer of any of the results of the law of gravitation. When, besides expressing such facts, the vapour-molecule system of exponents is also so much more comprehensive in the information it affords, surely an impartial and deliberate consideration of the question cannot fail to have its influence.

I have before me a Table of nearly two hundred gases and vapours whose densities have been experimentally ascertained, all expressed in this way. Each symbol or formula expresses the actual constitution of the gas-molecule in terms of the gas-molecules of its constituents, or, in other words, each expresses the constitution of a unit volume of the body in a state of vapour in terms of the unit volume of its constituents; also the equivalent weight by adding up the value of the symbols. Thus sulphurous acid, $OS^{\frac{1}{2}} = 16 + \frac{96}{6} = 32$, which is the vapour-density and also the equivalent. Muriatic acid, $Cl^{\frac{1}{2}} H^{\frac{1}{2}} = 18 + \frac{1}{2} = 18\frac{1}{2}$, which is the vapour-density, but only one-half of the equivalent usually given.

The mixing up of single and double numbers in tables of equivalents, which still continues in works of authority, seems prejudicial to advancement in theoretical chemistry. For example, if the individuality of the molecule were properly kept in view, alcohol could never have been viewed as the hydrate of oxide of ethyle in a system of notation like that of Gerhardt's, which professes to recognise equal number of atoms in equal volumes, the fact being that two vapour-molecules of alcohol contain the same elements as one vapour-molecule of ether *plus* one vapour-molecule of water. If the system recognises only the simple gases as having the same number of atoms in equal volumes, and the compounds as having varying multiples of these according to what is termed their "*rational formula*," then in a cubic foot which contains $n$ number of atoms of oxygen there is $\frac{1}{2}n$ number of ether-atoms as the greatest number possible; because if the number were $n$, each atom of ether would contain half an atom of oxygen (4), which is inadmissible, as an atom applies to the least combining element. In a cubic foot of alcohol vapour, considered as the hydrate of ether, there cannot be more than half the number of atoms that ether has, or $\frac{1}{4}n$, for the same reason in respect to the atom of oxygen. On the other hand, if a cubic foot of the vapours of ether, of alcohol, and of *all* other compounds of oxygen and other bodies, simple or composite, contains $\frac{1}{2}n$ number of atoms or smallest parts of the respective compounds, then

must a cubic foot of oxygen contain $n$ atoms: or giving to the ultimate moving part of a gas or vapour the name *molecule*, we find that in all compounds containing oxygen the atom of each corresponds to the molecule, and has the same constant volume in all, if we regard only the molecule of oxygen to be dual in its nature, or composed of two atoms. Reverting to the usual definition of gaseous volume being the quotient of the equivalent weight by the specific gravity, we have to make an exception to this in the case of the primaries, and define their gaseous volume as being the quotient of twice the equivalent weight by the specific gravity; but it would be better to substitute the word *molecule* for *volume*, as the theory of heat and gases which attributes to the ultimate separate part, whatever its weight or constitution, a constant volume, because of the *vis viva* with which it is charged, seems to be favourably received by professional chemists (Graham, vol. ii.).

With respect to ethyle, etc., and the "rational formulæ" in which they enter, some further remarks seem necessary, as the existence of such radicals is incompatible with the dynamical theory. In the *English Cyclopædia*, article "Chemical Formulæ," the following occurs: "All formulæ are partial because they cannot express the whole of the molecular arrangements of the bodies to which they refer, and arbitrary because they cannot but give undue prominence to particular theoretical views and tendencies." In Graham's second volume, p. 521, it is stated that "the rational formula of a compound is inferred from its modes of formation and decomposition." Such mode applied to alcohol would define it as a hydrate of olefiant gas. At p. 522 the following occurs: "It appears, then, that the same compound may have several rational formulæ. . . . The greater the number of elementary atoms entering into the constitution of a compound, the more numerous will be the possible arrangements of those atoms, and the greater therefore the number of rational formulæ which may be assigned to the compound." It thus appears that rational formulæ are as yet little better than possible arrangements, and the insufficiency of the chemical evidence to determine one in preference to another is admitted. To those who consider the truth in nature as of paramount

importance, here is a motive for seeking evidence in departments not strictly chemical, more especially since the discovery of the alcohol-mixed metals, and of hydrocarbons having the constitution of ethyle and methyle, seem to be regarded as settling the point in favour of the modern theory.

A hydrocarbon, $C^2H^5 = 29$, has been discovered in addition to etherine, $C^2H^4 = 28$, and olefiant gas, $CH^2 = 14$. Ether, $C^2H^5O^{\frac{1}{2}} = 37$, is considered better represented by $C^2H^5 . O^{\frac{1}{2}}$ than by $C^2H^4 . HO^{\frac{1}{2}}$; alcohol better represented by $C^2H^5 . O^{\frac{1}{2}} . HO^{\frac{1}{2}}$, with some mark or expression appended to signify that such a molecule occupies two volumes, than by $CH^2 . HO^{\frac{1}{2}} = 23$ without any mark.

As the chemical evidence with respect to the formation of ether and decomposition of alcohol is not deemed of importance, I may revert to a fact which appears from the researches of MM. Fabre and Silberman, namely that the quantity of heat evolved by the combustion of alcohol and ether is exactly equal to what it would be if they were simple hydrates of $CH^2$ and $C^2H^4$. These (and all other?) hydrocarburets, when burnt, evolve the same amount of heat as if the carbon and hydrogen were burnt separately; so that the force of union between the C and H of these bodies appears to have no sensible dynamical equivalent as between the C and O of carbonic acid, and H and O of water, which unite with an immense evolution of force. The heat developed in burning a unit weight of ether, $C^2H^4 . HO^{\frac{1}{2}} = 37$, is 16249; in burning the $C^2$, or $\frac{24}{37}$ of a unit weight of carbon separate, the heat is 9430, and the $H^4$, or $\frac{4}{37}$ of a unit weight of hydrogen separate, is 6700: these two added make 16130, being almost exactly the same as the unit of ether. If the other element of hydrogen belonged to a hydrocarbon and not to a water-molecule, there would be 1675 to add. The same mode applied to alcohol and pyroxylic spirit gives results even more striking, and precisely such as might be expected if they were hydrates of $C^2H^4$, $CH^2$, $C^{\frac{1}{2}}H$ respectively.

What is the nature of the evidence afforded by the organo-metallic bodies? "Ethyle is a colourless gas burning with a brilliant white flame, and decomposes at less than a red heat,"—so far being similar to olefiant gas. Its specific gravity is 29,

the carbon being 24, and hydrogen 5. The sulphide and bisulphide of ethyle have vapour-densities such that they may be regarded either as primary compounds of this gas and sulphur, or secondary compounds of etherine and the sulphuretted hydrogens. The chloride of ethyle has vapour-density $32\frac{1}{2}$, being 18 chlorine, 12 carbon, and $2\frac{1}{2}$ hydrogen. It may thus be viewed as *half* a gas-molecule of chlorine combined with *half* a gas-molecule of ethyle, or *one* gas-molecule of olefiant gas in union with *one* molecule of muriatic acid. The same remark applies to the iodide and bromide, also to the oxide (ether).

The hydride of ethyle has specific gravity 15, the carbon being 12 and hydrogen 3, being *half* a molecule of ethyle combined with *half* a gas-molecule of hydrogen; or it has nothing to do with ethyle, but is a primary hydrocarbon isomeric if not identical with methyle. Marsh-gas, called hydride of methyle, has its molecule composed of *half* a gas-molecule of methyle united with *half* a gas-molecule of hydrogen; or it has nothing to do with methyle, but is a primary hydrocarbon.

Zincethyle has vapour-density 61·5, showing that a vapour-molecule is composed of 32·5 zinc, 24 carbon, and 5 hydrogen. If it is a primary compound of one molecule zinc with one of ethyle, the combining affinity of the elements for oxygen must in consequence be much exalted, because it takes fire instantly on coming in contact with the air. Or it may be viewed as a secondary compound of a metallic hydride with etherine. There is at least one metallic hydride, the hydride of copper, which presents the same exaltation of affinity in respect to chlorine. Its composition is 1 hydrogen to 64 copper, or $\frac{1}{2}$ to 32, which gives the formula $H^{\frac{1}{2}}Cu$. When treated with muriatic acid, $Cl^{\frac{1}{2}}H^{\frac{1}{2}}$, the $H^{\frac{1}{2}}$ of the cupride unites with the $H^{\frac{1}{2}}$ of the acid to form H, which is the hydrogen evolved, and the Cu unites with the $Cl^{\frac{1}{2}}$ to form the dichloride of copper, $Cl^{\frac{1}{2}}Cu$. This action is stated to be very remarkable (Graham, vol. ii. p. 96), inasmuch as metallic copper is scarcely acted on by muriatic acid; *i.e.* Cu and $H^{\frac{1}{2}}Cl^{\frac{1}{2}}$ are passive, while $CuH^{\frac{1}{2}}$ and $Cl^{\frac{1}{2}}H^{\frac{1}{2}}$ are intensely active. At p. 517 there is the following remark:—"To reduce these bodies (viz. the elementary) to the same standard, it is

necessary to assume that each molecule of an elementary body in the free state consists of two elementary atoms." And at p. 517, "It appears indeed as if [such] atoms could not exist in a state of isolation. *An atom* [*i.e.* $O^{\frac{1}{2}}$, $H^{\frac{1}{2}}$, $Cl^{\frac{1}{2}}$, etc.] *of an elementary body must unite either with an atom of another element or with one of its own kind.*" We thus may see the propriety in every way of viewing them and expressing them as fractions of a molecule: with such symbols the peculiar action of the hydride of copper on muriatic acid no longer appears remarkable.

In the case of zincethyl, considered as composed of a hydrocarbon $C^2H^4$ (etherine) and a metallic hydride, the latter must be Zn combined with H, a whole gas-molecule of hydrogen. The action on being presented to the atmosphere is Zn H presented to O of the atmosphere, *i.e.* to whole gas-molecules of oxygen—causing, with their exalted affinity, the division $O^{\frac{1}{2}}O^{\frac{1}{2}}$, one of which burns with Zn, forming the protoxide of zinc, $ZnO^{\frac{1}{2}}$; the other burning with H to form water, $HO^{\frac{1}{2}}$.

The apparent exaltation of affinity in these and other similar cases may be caused simply by the atoms of metal existing unfettered by cohesion, the integral of which bears comparison with the more intense of the chemical forces. The H in its connection with the metal may be compared to the H in the hydrocarburet $C^{\frac{1}{2}}H$, which, being one-half $CH^2$ the olefiant, and one-quarter $C^2H^4$ etherine, we may by analogy hold as possible, yet, like a metallic hydride, having the affinity of its constituents for oxygen and the other electro-negative elements too exalted to exist in a state to be recognised. In pyroxylic spirit it appears combined with water, $C^{\frac{1}{2}}H.HO^{\frac{1}{2}}$; in chloride of methyle, $C^{\frac{1}{2}}HCl$, combined with chlorine, etc.

However this may be, it cannot be disputed that chemical elements unite in modes that, estimated dynamically, are specifically distinct. Besides those above noted, there seems to be another, as in the case of chloride of nitrogen, where not only force, but heat and light accompany the disunion of elements—is evolved instead of being absorbed. In the usual case, force or heat causes the separation; in the fulminate, the separation causes (apparently) force and heat. Such considerations must enter into any system of classification of chemical

compounds that is not quite artificial and arbitrary. To include under the same type a liquid of which the elements are in a burnt condition, with another the major part of which is in an unburnt condition, is simply to ignore the dynamical theory of heat, to reject as a guide a principle that has never failed to yield rich returns to those who have resigned themselves entirely to its guidance in the study of nature.

EDINBURGH, 1*st August* 1863.

# ON CHEMICAL NOTATION

(From the *Philosophical Magazine* for December (Supplement) 1863.)

In your last Number, Mr Odling has remarked on my communication advocating the vapour-density system of chemical notation. I have been in the habit of using it in arranging the thermo-molecular facts of chemistry in a convenient form for easy reference; and finding that it enabled the mind to grasp easily rather complicated molecular relations which could not be done with any other without tedious verbal accompaniment, I endeavoured some ten or fifteen years ago to bring it to the notice either of the Royal Society or British Association; I forget which. The attempt was unavailing, as indeed it is likely to be in the present case, if some professional chemist of eminence does not take it up in a magnanimous spirit. I did not trouble myself further on the subject, because I felt that, as chemical science progressed, it would be certain to be adopted in regular course as the natural symbolic language—indeed that it would be impossible to get on without it—and that it would be better that the suggestion should come from within the schools. But years have passed on, and the chance of this seemed becoming less by degrees, until I was incited to make another effort by Mr Odling's lecture, in which it appeared as if the school of chemists to which he belongs had permanently adopted an improper system, and were actually making mistakes in consequence.

The vapour-density system (or, for shortness, let us call it the V.D. system) is founded on the bulk of all molecules in the gaseous state being equal. This, according to Mr Odling, is an *extremely* recognised fact among chemists.* "Let chemical

* At p. 513 of Dr Graham's second volume the following occurs:—"3. The system (of notation) of Gerhardt, based, like that of Berzelius, on the *hypothesis* that all simple gases contain equal number of atoms in equal

notation be founded thereupon [upon the fact (1) only, not upon the hypothesis (2) (see note), or upon any hypothesis], and we obtain with perfect conciseness at once the chemical composition, the physical density of a unit volume, and the constitution of a molecule." On this Mr Odling remarks, "*he certainly never before heard that the vapour-density of any given body pointed to some particular number as the expression of its molecular weight.*" This is an extraordinary admission. Is it possible that the first and most obvious consequence of the equality in bulk of gaseous molecules is a sealed book to Mr Odling's school? How powerful then must be the influence of preconceived notions when they shut out from intellectual vision the almost self-evident fact that vapour-densities must be proportional to molecular weights if molecules are of equal bulk, and therefore that they (the vapour-densities) point directly to, or in fact actually express, the molecular weights. If it is not self-evident, here is the proof:—

Let $w$ = weight of a molecule of the gas A, and W = weight of the molecule of the gas B. If there are $n$ molecules of the gas A in the space of a cubic foot, there are $n$ molecules of the gas B in the space of a cubic foot. The weight of a cubic foot of gas A is therefore $nw$, and the weight of a cubic foot of gas B is $n$W. The vapour-density of gas A is to vapour-density of gas B as $nw : n\mathrm{W} :: w : \mathrm{W}$. Q.E.D.

Molecular weights are ratios, vapour-densities are ratios; and these ratios are identical if all molecules in the gaseous state have the same bulk.

Mr Odling may say that by vapour-density he means the specific gravities of the vapours expressed in terms of water or air as unity, and that these *numbers* do not point to "some particular numbers as the molecular weights": but they certainly do, inasmuch as molecular weights are ratios which are indicated or "pointed to" by the ratio of the numbers that express the

volumes." See also p. 512, vol. ii. This, I presume, is concurred in by Mr Odling and the school; so that we are to understand that they are agreed that (1) it is a *fact* that all simple or compound gases and vapours contain equal number of *molecules* in equal volumes; and (2) that the system of notation they adopt is based on the *hypothesis* that the simple gases contain equal number of *atoms* in equal volumes.

vapour-densities. That the density of the gas of least specific gravity should be selected in preference to any other as the standard unit of comparison is clear from this, that it is easier for the mind to compare magnitudes together when they are expressed in terms of the least; because if they were expressed in any other, we should have to compare multiples with reciprocals, reciprocals with reciprocals, and multiples with multiples; whereas expressed in terms of the least, we have to compare multiples with multiples only—one mental process in place of three.

The specific gravity of gases and vapours in terms of hydrogen unity are identical as *numbers* with the molecular weights expressed in terms of hydrogen unity. This Mr Odling's school must admit is as indisputable as that of the equality in the volume of gaseous molecules, which Mr Odling informs us is as clearly recognised as the most simple truths of the multiplication table. May we not therefore ask, Why, in the name of Cocker and common sense, are these numbers not given to the chemical symbolic letters? which would thus be made to express vapour-density as well as molecular weight, and in compound or composite molecules would carry the same meaning; while if bodies entered into combination by fractions of molecules they would appear in the formulæ as such, and require no verbal discussion.

If the specific gravity of oxygen is 16 times that of hydrogen, and if the molecular weight of oxygen is 16 times that of hydrogen, is not 16 associated with O and 1 with H irrevocably? and does it not disturb this association, and therefore fatally distract the attention, to give any other numerical values to these symbols? I use the word *fatally*, because I believe that serious errors as well as general confusion have arisen from this apparently insignificant cause. A few of these were noticed in my previous communication. It is specially when chemistry is studied in its thermo-molecular relations that they assume a prominent aspect.

Mr Odling states that "the object of his lecture was to show on chemical grounds that the chemical molecule of water is identical with its physical molecule, and consequently that this molecule contains double the amount of hydrogen contained in

the molecule of muriatic acid." If the lecture was delivered to teach that the hydrogen of the water-molecule was double the hydrogen of the muriatic acid molecule, there would have been no occasion for it if the vapour-density system of chemical notation had been in vogue, because such facts of molecular composition are expressed by it so completely that any verbal addition whatever becomes superfluous, *e.g.*, water is $HO^{\frac{1}{2}} = 9$, which means that a gaseous molecule of water is composed of a gaseous molecule of hydrogen (1), united with half a gaseous molecule of oxygen (8). Muriatic acid is $H^{\frac{1}{2}}Cl^{\frac{1}{2}} = 18\frac{1}{2}$, which means that a gaseous molecule of muriatic acid gas is composed of half a molecule of hydrogen united with half a molecule of chlorine.

The verbal explanation here appended to these formulæ would not of course be required if the V.D. system were fully recognised. The difficulty of making known anything that requires a little quiet application and study to master is well illustrated by the formula for nitric acid given by Mr Odling as expressed in the V.D. system (*Phil. Mag.*, p. 381), $H^{\frac{1}{2}}N^{\frac{1}{2}}O^{1\frac{1}{2}}$. Now dry nitric acid is $NO^{2\frac{1}{2}} = 54$, which means that a molecule of its vapour is composed of a molecule of nitrogen gas united with $2\frac{1}{2}$ molecules of oxygen gas; the monohydrate by $HO^{\frac{1}{2}} . NO^{2\frac{1}{2}} = 63$ if the water-molecule continues united with the acid molecule when in the vapour form, or $\frac{HO^{\frac{1}{2}} . NO^{2\frac{1}{2}}}{2} = 31\frac{1}{2}$ if they separate; the quadrihydrate by $\frac{4HO^{\frac{1}{2}} . NO^{2\frac{1}{2}}}{5} = 20{\cdot}4$ if they separate, and so on.

The method of determining these formulæ methodically by simple equation in the most involved cases is given in the previous communication. We ought to be able to read the name of a compound in its V.D. formula; and reversely we may correct the name by means of the same when it has been improperly bestowed.

But without the V.D. system, the ratio of hydrogen in the water and muriatic acid molecules might be easily proved as follows:—

In muriatic acid, one part by weight of hydrogen unites with 35·5 parts by weight of chlorine, and its vapour-density

is 18·25 times that of hydrogen. One cubic foot of muriatic acid gas contains as many molecules as one cubic foot of hydrogen gas (according to Mr Odling's ridiculously clear two and two fact); therefore the weight of each molecule of muriatic acid gas is 18·25 times that of each molecule of hydrogen gas; but the proportion of the weight of a molecule of the acid gas that is hydrogen is only $\frac{1}{36{\cdot}5}$ part; and since the molecule of the acid gas weighs 18·25 times the weight of the molecule of hydrogen, $\frac{1}{36{\cdot}5}$ part of 18·25 is 0·5, which is the weight of hydrogen in a molecule of the acid gas in terms of the hydrogen molecule unity. In the composition of water, 1 part by weight of hydrogen unites with 8 parts by weight of oxygen, and the vapour-density of steam is nine times hydrogen; the weight of its molecule is nine times the hydrogen molecule, and one-ninth part of the weight of the steam-molecule being hydrogen, the weight of hydrogen in a steam-molecule is 1 expressed in terms of the hydrogen molecule unity. Thus the weight of hydrogen in a molecule of muriatic acid gas is to the weight of hydrogen in a molecule of water as 0·5 : 1, or as 1 : 2. Q.E.D.

Since this obvious deduction from the recognised fact of the equality of gaseous molecules has escaped notice (for if it had not escaped notice, how could a lecture to prove so simple a matter have been thought necessary), it is not incorrect to say that the arithmetical part of chemistry is not in a satisfactory condition, and that any attempt to put it in a more satisfactory condition deserves the support of chemists.

When previously remarking on Mr Odling's statement with reference to sodium, I find that I have understood what he calls hydrate of sodium to be hydrate of soda, and applied the same formulæ to it as to the hydrate of potash. Thus Na in contact with $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$ expels the H of one $HO^{\frac{1}{2}}$, and unites with the other $HO^{\frac{1}{2}}$ to form the solid hydrate of soda, $HO^{\frac{1}{2}}.NaO^{\frac{1}{2}}$. When this is heated with sodium, the H of the $HO^{\frac{1}{2}}$ is ejected and gives place to a second Na; so that $HO^{\frac{1}{2}}.NaO^{\frac{1}{2}}$ becomes

$$NaO^{\frac{1}{2}}.NaO^{\frac{1}{2}} = 2NaO^{\frac{1}{2}}.$$

Of hydrate of sodium (if it is not a mistake) I know nothing, nor can I find mention made of it in any work on chemistry that I have access to. If Mr Odling would have the kindness to express what he means in the V.D. notation, it would then perhaps be possible to understand him.

With reference to the statement in my previous communication, "that HKΘ represents one molecule of the hydrate of potash, but HHΘ not one but two molecules of water and KKΘ not one but two molecules of anhydrous potash," Mr Odling informs us in a note that Mr Watts (author of the Supplement of Dr Gregory's *Chemistry*) states to him "that the three formulæ were meant to express a single molecule each, the three quantities represented being exactly comparable." I repeat that this is impossible, and strikingly shows the inefficiency of Gerhardt's system when it can so notoriously mislead eminent chemists. The following is the proof:—Let us assume that they do express a single molecule each; then, since HHΘ represents a single molecule of water, the value of the symbol H in terms of the weight of a hydrogen gas-molecule considered as unity must be $\frac{1}{2}$; and the value of Θ must be 8, because the single molecule of water weighs nine times the single molecule of hydrogen, and is $\left.\begin{matrix} H + H + \Theta \\ \frac{1}{2} + \frac{1}{2} + 8 \end{matrix}\right\} = 9$. Potassium, K, thrown into water displaces half the hydrogen and forms hydrate of potash, HKΘ, or $\left.\begin{matrix} H + K + \Theta \\ \frac{1}{2} + 39 + 8 \end{matrix}\right\} = 47{\cdot}5$; so that, if the symbols H and Θ retain the same value in the single molecule HHΘ as in the single molecule HKΘ, the hydrate of potassa, HKΘ, must consist of half a gaseous molecule of hydrogen ($\frac{1}{2}$), half a gaseous molecule of oxygen (8) and potassium (39). But 39 of potassium in union with 8 of oxygen is potassa, so that all that is left to represent the water of the hydrate is $H = \frac{1}{2}$, whereas the water-molecule is HHΘ.

Reverting to the single molecule HKΘ, or

$$\left.\begin{matrix} H + K + \Theta \\ \frac{1}{2} + 39 + 8 \end{matrix}\right\} = 47{\cdot}5.$$

When it is heated with potassium, "the remaining half of the

hydrogen is displaced and anhydrous potash KKΘ, is formed," which KKΘ Mr Watts holds is a single molecule. The remaining half of the hydrogen is H = ½, and K = 39 substituted for it gives: $\left.\begin{matrix} H + K + \Theta \\ \frac{1}{2} + 39 + 8 \end{matrix}\right\} = 47{\cdot}5$ converted into KKΘ, or $\left.\begin{matrix} K + K + \Theta \\ 39 + 39 + 8 \end{matrix}\right\} = 86$, which is stated by Mr Watts to be a single molecule of anhydrous potash, which is thus represented as being composed of 78 potassa in union with 8 of oxygen, instead of 39.

On the other hand, if HHΘ represents 2 molecules of water, H = 1, and Θ = 16, the line through the O representing Θ = 2 × 8, showing that Gerhardt understood his symbol Θ to represent a dual nature, but unfortunately avoided fractional exponents. Let $O^{\frac{1}{2}}$ represent one of those components of Θ, and $O^{\frac{1}{2}}$ the other; then $HH\Theta = HO^{\frac{1}{2}}.HO^{\frac{1}{2}}$ = two molecules of water; and K brought in contact displaces one H, forming one molecule of the hydrate $KO^{\frac{1}{2}}.HO^{\frac{1}{2}}$; and this, heated with potassium, exchanges the remaining H for another K forming

$$KO^{\frac{1}{2}}, KO^{\frac{1}{2}} = 2KO^{\frac{1}{2}},$$

or twice the molecule of the potassa contained in the hydrate.

Can anything be clearer than this? Will no chemist enter the list and advocate the V.D. system, which after all is but Gerhardt's system slightly modified, so that there should be nothing but arithmetical symbols required. But the advantages attending the alteration are great, as will be acknowledged in due time if it ever be adopted.

A few formulæ are appended as illustrations of the system.

| | | | | | |
|---|---|---|---|---|---|
| The value of C is taken at | | | 12, and $C^{\frac{1}{2}} = 6$, | | unknown as vapours. |
| ,, | Bo | ,, | 11, ,, $Bo^{\frac{1}{2}} = 5\frac{1}{2}$, | | |
| ,, | Fl | ,, | 19, ,, $Fl^{\frac{1}{2}} = 9\frac{1}{2}$, | | |
| ,, | Cl | = | 36 | = the spec. grav. of its vapour. | |
| ,, | As | = | 150 | ,, ,, ,, | |
| ,, | S | = | 96 | ,, ,, ,, | |
| ,, | P | = | 64 | ,, ,, ,, | |
| ,, | Hg | = | 100 | ,, ,, ,, | |

The product of 100 by ·033, the specific heat of mercury, is 3·3. This number, divided by specific heat of iron, gives

Fe = 56; by the specific heat of aluminium, gives Al = 27. And these weights correspond to the vapour-densities of their compounds with chlorine, etc., as follows:—

| | | | |
|---|---|---|---|
| $HO^{\frac{1}{2}}=9$ | Water | $NC=26$ | Cyanogen |
| | | $N^{\frac{1}{2}}C^{\frac{1}{2}}H^{\frac{1}{2}}=13\frac{1}{2}$ | Hydrocyanic acid |
| $NO^{\frac{1}{2}}=22$ | Protoxide of nitrogen | $N^{\frac{1}{2}}C^{\frac{1}{2}}Cl^{\frac{1}{2}}=31$ | Chloride of cyanogen |
| $N^{\frac{1}{2}}O^{\frac{1}{2}}=15$ | Nitric oxide | | |
| $NO^{1\frac{1}{2}}=38$ | Hyponitrous acid | $H^{\frac{1}{2}}Cl^{\frac{1}{2}}=18\frac{1}{2}$ | Hydrochloric acid |
| $N^{\frac{1}{2}}O=23$ | Nitrous acid | $H^{\frac{1}{2}}Br^{\frac{1}{2}}=40^{\frac{1}{2}}$ | Hydrobromic acid |
| $NO^{2\frac{1}{2}}=54$ | Nitric acid (dry) | $H^{\frac{1}{2}}I^{\frac{1}{2}}=63\frac{1}{2}$ | Hydriodic acid |
| | | | |
| $C^{\frac{1}{2}}O^{\frac{1}{2}}=14$ | Carbonic oxide | $As^{\frac{1}{2}}Cl^{1\frac{1}{2}}=92$ | Sesquichloride of arsenic |
| $C^{\frac{1}{2}}O=22$ | Carbonic acid | $As^{\frac{1}{2}}Br^{1\frac{1}{2}}=158$ | Sesquibromide of arsenic |
| | | $As^{\frac{1}{2}}I^{1\frac{1}{2}}=225$ | Periodide of arsenic |
| $P^{\frac{1}{2}}O=32$ | Sulphurous acid | | |
| $S^{\frac{1}{2}}O^{1\frac{1}{2}}=40$ | Sulphuric acid (dry) | $S^{\frac{1}{2}}Cl=52$ | Chloride of sulphur |
| $AsO^3=198$ | Arsenious acid | $S^{\frac{7}{2}}Cl=68$ | Dichloride of sulphur |
| $As^{\frac{1}{2}}H^{1\frac{1}{2}}=39$ | Arseniuretted hydrogen | | |
| $S^{\frac{1}{2}}H=17$ | Sulphuretted hydrogen | $Bo^{\frac{1}{2}}Cl^{1\frac{1}{2}}=59\frac{1}{2}$ | Terchloride of boron |
| $S^{\frac{1}{2}}H^{1\frac{1}{2}}=17\frac{1}{2}$ | Phosphoretted hydrogen | $Bo^{\frac{1}{2}}Fl^{1\frac{1}{2}}=33$ | Fluoride of boron |
| $N^{\frac{1}{2}}H^{1\frac{1}{2}}=8\frac{1}{2}$ | Ammonia | | |

*Note.*—In this last, though the vapour-density of Fl and of Bo are both unknown directly, they become so indirectly by the measured vapour-density of the compound.

| | | | |
|---|---|---|---|
| $N^{\frac{1}{2}}C^{\frac{1}{2}}Br^{\frac{1}{2}}=53$ | Bromide of cyanogen | $AlCl^3=135$ | Chloride of aluminium |
| | | $AlBr^3=267$ | Bromide of aluminium |
| $FeCl^3=164$ | Sesquichloride of iron | $AlI^3=408$ | Iodide of aluminium |

*Note.*—The values of Fe and Al here become known, though the vapours of iron and aluminium cannot be weighed. The specific heat seems a sure guide to molecular weights of metals.

*Organic Compounds.*

| | | | |
|---|---|---|---|
| $C^{\frac{1}{2}}H^2=8$ | Marsh-gas | $CH^2.HO^{\frac{1}{2}}=23$ | Alcohol |
| $CH^2=14$ | Olefiant gas | $2(CH^2).HO^{\frac{1}{2}}=37$ | S. ether |
| $C^2H^4=28$ | Etherine | $C^{\frac{1}{2}}H.HO^{\frac{1}{2}}=16$ | Pyroxylic spirit |
| $C^3H^3=39$ | Benzin | $2(C^{\frac{1}{2}}H).HO^{\frac{1}{2}}=23$ | Pyroxylic ether |
| $C^3H^5=41$ | Naphtha | | |
| $C^5H^8=68$ | Turpentine | | |

*Note.*—Alcohol cannot be expressed by $CH^3.O^{\frac{1}{2}}$, because one H is in a burnt condition, and the only constituent it has to have burnt with is $O^{\frac{1}{2}}$.

Pyroxylic spirit cannot be expressed by $C^{\frac{1}{2}}H^{2}.O^{\frac{1}{2}}$ for the same reason. The ethers also for the same reason.

$CH^{2}O^{\frac{1}{2}} = 22$ . . . Aldehyde.

*Note.*—The formula might require to be modified to $CH.HO^{\frac{1}{2}}$, or otherwise if the heat of combustion were known. The same remark applies to chloric ether, $CH^{2}Cl^{\frac{1}{2}} = 32$, and indeed more or less to all organic compounds.

EDINBURGH, 8*th November* 1863.

# ON THE APPLICATION OF THE VAPOUR-DENSITY SYSTEM OF NOTATION TO EXPRESS CHEMICAL REACTIONS

(From the *Philosophical Magazine* for April 1864.)

In the following communication I give an example of the application of the vapour-density system of notation to express chemical reactions; and to place it in comparison alongside of Gerhardt's system, I have selected the formulæ from M. Wurtz's memoir on the glycols (*Ann. de Chim.*, vol. lv.), where that system is applied with great distinctness, and accompanied with vapour-density measurements. It appears from a passage in M. Wurtz's lecture on the Oxide of Ethylene, delivered before the London Chemical Society, 5th June 1862, that he considers Gerhardt's system as only provisional. * * * "car les barres qui ne sont point superflues dans *ce moment de confusion*, ne sont que provisoires et devront disparaître par la suite" (*Ann. de Chim.*, Nov. 1863, p. 361).

I have appended some remarks on the so-called hydrates of metals of the new school of chemists, as one of M. Wurtz's experiments has the appearance of favouring their views, but is, in my opinion, rather an example of the liability of being led astray by defective symbolism—defective because founded on substitutional rather than on physical characteristics.

P. 402 (*Ann. de Chim.*, vol. lv.). "Supposons qu'on ait fait réagir sur 1 équivalent de bromure d'éthylène $\Theta^2 H^4 Br^2$, par exemple, 2 équivalents d'acétate d'argent, il se formera par double décomposition du bromure d'argent et du glycol diacétique. Dans cette réaction, ces 2 équivalents de brome se sont portés sur les 2 équivalents d'argent, et le radical $\Theta^2 H^4$, se substituant à ces 2 équivalents d'argent dans les 2 molécules

d'acétate d'argent, relie l'un à l'autre les restes de ces molécules de manière à former la nouvelle combinaison. On a, en effet,

$$\underbrace{\left.\begin{matrix}\Theta^2\,H^3\,Ag\,\Theta^2\\ \Theta^2\,H^3\,Ag\,\Theta^2\end{matrix}\right\}}_{\text{2 équivalents d'acét. d'arg.}} + \underbrace{\Theta^2\,H^4\,Br^2}_{\text{Brom. d'éthyl.}} = \underbrace{\left\{\begin{matrix}\Theta^2\,H^3 & & \Theta^2\\ \Theta^2\,H^3\,C^2\,H^4 & & \Theta^2\end{matrix}\right\}}_{\text{Glycol diacétique.}} + 2Ag\,Br;$$

ou

$$2\left\{\begin{matrix}\Theta^2\,H^3\,\Theta\\ Ag\end{matrix}\right\}\Theta + \Theta^2\,H^4\,Br^2 = \left\{\begin{matrix}C^2\,H^4\\ (C^2\,H^3\,\Theta)^2\end{matrix}\right\}\Theta^2 + 2Ag\,Br."$$

The above, expressed by the V.D. notation, is

$$\left.\begin{matrix}\text{1 mol. brom. of olef.}\\ \text{1 mol. acetate of silver.}\end{matrix}\right\}\begin{matrix}CH^2\,Br\\ AgO^{\frac{1}{2}}\,.\,C^2\,H^3\,O^{1\frac{1}{2}}\end{matrix}\Bigg\} = \left\{\begin{matrix}Br\,Ag & \left\{\begin{matrix}\text{1 mol. or possibly}\\ 2Br^{\frac{1}{2}}\,Ag^{\frac{1}{2}}\end{matrix}\right.\\ C^2\,H^3\,O^{1\frac{1}{2}}\,.\,CH^2\,O^{\frac{1}{2}} & \left\{\begin{matrix}\text{1 mol. acetate of}\\ \text{oxide of olef.}\\ \text{(glycol diacét.)}\end{matrix}\right.\end{matrix}\right.$$

It will be remarked that the reaction consists of $CH^2$ and Ag changing places.

P. 407. "*Décomposition du Glycol diacétique par l'hydrate de potasse sec.*

$$\underbrace{\left.\begin{matrix}\Theta^2\,H^4\\ (\Theta^2\,H^3\,\Theta)^2\end{matrix}\right\}}_{\text{Glycol diacét.}}\Theta^2 + 2KH\Theta = \underbrace{\left\{\begin{matrix}\Theta^2\,H^4\\ H^2\end{matrix}\right\}}_{\text{Glycol.}}\Theta^2 + 2\underbrace{\left\{\begin{matrix}\Theta^2\,H^3\,\Theta\\ K\end{matrix}\right\}}_{\text{Acétate de potasse.}}\Theta."$$

Expressed by the V.D. notation, the above is

$$\left.\begin{matrix}\text{1 mol. acetate of oxide of olef.}\\ \text{1 mol. of caustic potash.}\end{matrix}\right\}\begin{matrix}C^2\,H^3\,O^{1\frac{1}{2}}\,.\,CH^2\,O\\ KO^{\frac{1}{2}}\,.\,HO^{\frac{1}{2}}\end{matrix}\Bigg\} = \left\{\begin{matrix}C^2\,H^3\,O^{1\frac{1}{2}}\,.\,KO^{\frac{1}{2}} & \left\{\begin{matrix}\text{1 mol. acetate}\\ \text{of potash.}\end{matrix}\right.\\ CH^2\,O^{\frac{1}{2}}\,.\,HO^{\frac{1}{2}} & \left\{\begin{matrix}\text{1 mol. glycol}\\ \text{vap.-density}\\ \text{meas.} = 31.\end{matrix}\right.\end{matrix}\right.$$

Heat is evolved while $KO^{\frac{1}{2}}$ and $CH^2\,O^{\frac{1}{2}}$ change places.

The symbol $2KH\Theta$ employed by M. Wurtz to denote caustic potash, shows that he does not follow the new school in their views on the constitution of this body.

P. 412. "*Action du Potassium et du Sodium sur le Glycol.*"

11·5 parts by weight of sodium saturate 31 parts of glycol, during which heat is evolved, pure hydrogen is

disengaged, and a white crystalline matter is formed, which is $\Theta^2 H^5 Na\Theta^2$ "glycol monosodé," "dérivé du glycol par la substitution d'un équivalent de sodium à un équivalent d'hydrogen." This, expressed by the V.D., would seemingly be

$$\left.\begin{matrix}\text{Half a mol. of sodium.}\\ \text{1 mol. of glycol.}\end{matrix}\right\}\begin{matrix}Na^{\frac{1}{2}}\\ CH^2O^{\frac{1}{2}}.HO^{\frac{1}{2}}\end{matrix}\Bigg\} = \begin{cases}CH\,O^{\frac{1}{2}}.H^{\frac{1}{2}}Na^{\frac{1}{2}}O^{\frac{1}{2}} \\ H^{\frac{1}{2}}\text{ escapes}\end{cases}$$

{1 mol. glycol monosodé, in which the views of the new school as to the atomic constitution would seem to be realised.}

But there is another arrangement possible, viz. 23 sodium to 62 glycol, or 1 molecule of sodium to 2 molecules of glycol uniting to form *one* molecule of the glycol monosodé. Thus

$$\left.\begin{matrix}Na\\ CH^2O^{\frac{1}{2}}.HO^{\frac{1}{2}}\\ CH^2O^{\frac{1}{2}}.HO^{\frac{1}{2}}\end{matrix}\right\} = \begin{cases}H\text{, 1 molecule of hydrogen evolved.}\\ 2(CH^2O^{\frac{1}{2}}).HO^{\frac{1}{2}}.NaO^{\frac{1}{2}}\{\text{1 mol. of glyc. monosodé (solid),}\end{cases}$$

or dioxethylate of soda, consisting of 1 molecule of dioxethylene united to 1 molecule of the monohydrate of soda. The duplication of the oxide of ethylene molecule in such reaction will be admitted as extremely probable, although it cannot be tested by vapour-density measurement. See *Ann. de Chim.*, vol. lxix. pp. 324, 326.

The glycol monosodé being melted with sodium at 190° C., more hydrogen is evolved, and the white dry deliquescent glycol disodé, $\left\{\begin{matrix}\Theta^2H^4\\ Na^2\end{matrix}\right\}\Theta^2$, is obtained. This, by the V.D., is either $2(CH^2O^{\frac{1}{2}}).2(NaO^{\frac{1}{2}})$ one molecule, or $2(CH^2O^{\frac{1}{2}}.NaO^{\frac{1}{2}})$ two molecules.

P. 417. The analysis of glycolate of silver is C = 24, H = 3, Ag = 108, O = 48; and since Ag = 108 + O = 8 is $AgO^{\frac{1}{2}}$ one molecule oxide of silver, we have $C^2H^3O^{2\frac{1}{2}}$ as the representative of one molecule of glycolic acid, and the same represents the constitution of the acid in the analysis of glycolate of lime, $C^2H^3O^{2\frac{1}{2}}.CaO^{\frac{1}{2}}$. The hydrated acid molecule is $C^2H^3O^{2\frac{1}{2}}.HO^{\frac{1}{2}}$, and should not be represented by $CH^2O^{1\frac{1}{2}}$, which involves the absurdity of half a water-molecule.

P. 419. *Action of hydrate of potash on glycol* is represented "par l'équation suivante, $\Theta^2 H^6 \Theta^2 + 2KH\Theta = \Theta^2 K^2 \Theta^4 + H^8$"; and by V.D.

$$\left.\begin{matrix}\text{1 mol. glycol} & CH^2 O^{\frac{1}{2}}.HO^{\frac{1}{2}} \\ \text{1 mol. hydrate of potash} & KO^{\frac{1}{2}}.HO^{\frac{1}{2}}\end{matrix}\right\} \overset{250°}{=} \begin{cases} CO^{1\frac{1}{2}}.KO^{\frac{1}{2}}, & \{\text{1 mol. oxalate of potash.} \\ H, H, H, H, & \{\text{4 mols. hydrogen free.}\end{cases}$$

A curious feature of this reaction is the decomposition of the water-molecules.

P. 419. *Action of muriatic acid on glycol.*—"Il a donné à l'analyse des résultats qui conduisent à la formule $\Theta^2 H^5 Cl\Theta$. Ce produit resulte evidement de l'action d'un seul équivalent d'acide chlorhydrique sur l'équivalent de glycol, et se forme en vertu de la réaction suivante

$$\underbrace{\Theta^2 H^6 \Theta^2}_{\text{Glycol.}} + HCl = \underbrace{\Theta^2 H^3 Cl\Theta^*}_{\text{Glycol monochlorhydrique.}} + H^2\Theta.$$

"J'ajoute que le glycol monochlorhydrique représente du glycol dans lequel 1 équivalent de chlore est venu prendre la place du groupe $H\Theta$. Les formules suivantes expriment ces relations:

$$\left.\begin{matrix}C^2 H^4 \\ H\ H\end{matrix}\right\}\Theta^2, \qquad \left.\begin{matrix}C^2 H^4 \\ H \\ Cl\end{matrix}\right\}\Theta."$$

Expressed by the V.D. notation, the above reaction is

$$\left.\begin{matrix}\text{1 mol. glycol} & CH^2 O^{\frac{1}{2}}.HO^{\frac{1}{2}} \\ \text{1 mol. mur. acid.} & Cl^{\frac{1}{2}} H^{\frac{1}{2}}\end{matrix}\right\} = \begin{cases} CH^2 O^{\frac{1}{2}}.Cl^{\frac{1}{2}} H^{\frac{1}{2}} & \{\text{1 mol. muriate of oxethylene.} \\ HO^{\frac{1}{2}} & \text{1 mol. water.}\end{cases}$$

The reaction appears to be simply $Cl^{\frac{1}{2}} H^{\frac{1}{2}}$, driving out $HO^{\frac{1}{2}}$ and taking its place: both molecules have their elements in a burnt condition. To say that $Cl^{\frac{1}{2}}$ takes the place of $H^{\frac{1}{2}} O^{\frac{1}{2}}$, is to convey the idea of $Cl^{\frac{1}{2}}$ leaving the acid-molecule $Cl^{\frac{1}{2}} H^{\frac{1}{2}}$ of which there is no evidence. It is a mode of expression that I do not think could be maintained if the potentialities of the phenomena were kept in view; yet it now pervades the writings of chemists.

[* Query $\Theta^2 H^5 Cl\Theta$.]

Pp. 419, 421. *Action of perchloride of Phosphorus on Glycol.* —Muriatic acid vapour is given off, and there remains the chloroxide of phosphorus and the Dutch liquid. "Ce chlorure prend naissance en vertu de la réaction suivante

$$\underbrace{\text{Θ}^2\,\text{H}^6\,\text{Θ}^2}_{\text{Glycol.}} + 2\text{Ph}\,\text{Cl}^5 = \underbrace{\text{Θ}^2\,\text{H}^4\,\text{Cl}^2}_{\substack{\text{Chlorure}\\ \text{d'éthylène.}}} + 2\text{Ph}\text{Θ}\,\text{Cl}^3 + 2\text{HCl}."$$

By the V.D. notation the reaction is

$$\left.\begin{array}{l}\text{1 mol. glycol} \\ \text{2 mols. perchlor. of phosph.}\end{array}\right\} \left.\begin{array}{l}\{\text{CH}^2\,\text{O}^{\frac{1}{2}}.\,\text{HO}^{\frac{1}{2}}\} \\ \{\text{P}^{\frac{1}{2}}\,\text{Cl}^{2\frac{1}{2}}\} \\ \{\text{P}^{\frac{1}{2}}\,\text{Cl}^{2\frac{1}{2}}\}\end{array}\right\} = \left\{\begin{array}{ll}\text{CH}^2\,\text{Cl} & \text{1 mol. chlor. of olef.} \\ \text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}}\,\text{O}^{\frac{1}{2}} & \text{2 mol. of chlorox.} \\ \text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}}\,\text{O}^{\frac{1}{2}} & \text{of phosphorus.} \\ \text{H}^{\frac{1}{2}}\,\text{Cl}^{\frac{1}{2}} & \text{2 mols. muriatic} \\ \text{H}^{\frac{1}{2}}\,\text{Cl}^{\frac{1}{2}} & \text{acid.}\end{array}\right.$$

The vapour-density of the perchloride of phosphorus, as measured by Mitscherlich, shows that although it boils at 82° C., it is decomposed on rising into vapour, the one molecule, $\text{P}^{\frac{1}{2}}\,\text{Cl}^{2\frac{1}{2}}$, separating into two, viz. $\text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}}$ and Cl; so the above reaction may admit of being represented as

$$\left.\begin{array}{r}\text{CH}^2\,\text{O}^{\frac{1}{2}}.\,\text{HO}^{\frac{1}{2}} \\ \text{Cl} \\ \text{Cl} \\ \text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}} \\ \text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}}\end{array}\right\} = \left\{\begin{array}{l}\text{CH}^2\,\text{Cl} \\ \text{H}^{\frac{1}{2}}\,\text{Cl}^{\frac{1}{2}} \\ \text{H}^{\frac{1}{2}}\,\text{Cl}^{\frac{1}{2}} \\ \text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}}\,\text{O}^{\frac{1}{2}} \\ \text{P}^{\frac{1}{2}}\,\text{Cl}^{1\frac{1}{2}}\,\text{O}^{\frac{1}{2}}\end{array}\right.$$

We have here the water-molecule of the glycol decomposed, its H separated into $\text{H}^{\frac{1}{2}}$, $\text{H}^{\frac{1}{2}}$; also one Cl separated into $\text{Cl}^{\frac{1}{2}}$, $\text{Cl}^{\frac{1}{2}}$, these semimolecules ascending from the burnt condition and descending again to it; or they change their partners without changing their potential condition.

Pp. 430, 431. Ethyl-glycol, vapour-density measured 45 = $\text{C}^2\,\text{H}^4\,\text{O}^{\frac{1}{2}}.\,\text{HO}^{\frac{1}{2}}$. $\text{Θ}^2\,\text{H}^5\,(\text{Θ}^2\,\text{H}^5)\,\text{Θ}^2$ is the formula for this given at p. 431.

P. 454. Butyl-glycol is isomeric with ethyl-glycol, and has the same vapour-density; its formula (p. 455) is $\left.\begin{array}{l}\text{Θ}^4\,\text{H}^8 \\ \text{H}^2\end{array}\right\}\text{Θ}^2$.

Diethyl-glycol, vapour-density measured $59 = C^3 H^6 O^{\frac{1}{2}} . HO^{\frac{1}{2}}$. $\Theta^2 H^4 (\Theta^2 H^5)^2 \Theta^2$ is the formula for this given at p. 432.

Glycol diacétique, vapour-density measured

$$73 = CH^2 O^{\frac{1}{2}} . C^2 H^3 O^{1\frac{1}{2}},$$

or one molecule anhydrous acetic acid united to one molecule oxide of ethylene. The formula for this given at p. 433 is

$$\left.\begin{matrix}\Theta^2 H^4 \\ (\Theta^2 H^3 \Theta)^2\end{matrix}\right\}\Theta^2,$$

and for the same at p. 403 is $\left\{\begin{matrix}\Theta^2 H^3 & \Theta^2 \\ \Theta^2 H^3 & \Theta^2\end{matrix}\right\}$ $\Theta^2 H^4$.

*Appendix.*

In some educational books on chemistry lately published, *hydrates* of metals are freely spoken of; not potassium and sodium only, but of other metals as well: and the name is not merely typical, or given for classification purposes, but the constitution of the hydrated oxide is by the new school viewed (and the view generally inoculated into undergraduates?) as a molecule on which a single atom of oxygen, of hydrogen, and of the metal are united. Thus caustic potash is represented to be $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$, being isomeric with $HO^{\frac{1}{2}} . KO^{\frac{1}{2}}$, which represents what is now called the view of the old school.

Mr J. J. Griffin, in his *Chemical Recreations*, 1834, appears to have first suggested this view, and he boldly announced his opinion that *the hydrates of the metallic protoxides contain neither protoxide nor water*. If the molecule of caustic potash is really $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$, it is evident that in it there is neither $HO^{\frac{1}{2}}$, an ultimate molecule or atom of water, nor $KO^{\frac{1}{2}}$, an ultimate molecule and atom of the protoxide. Mr Griffin considered it absurd to suppose that water can exist in a body like caustic potash, which stands red heat without change or giving off one particle of steam; but it is not likely that this argument has now much weight with chemists who agree with him otherwise because the intense heat given out during the union of the water with the oxide shows that a high degree of chemical action has taken place between them, and it cannot be expected under such circumstances that the physical characteristics of the water-

molecule should be retained. The only way I can imagine the idea $H^{\frac{1}{2}}O^{\frac{1}{2}}K^{\frac{1}{2}}$ to have taken root is from incompleteness in the conception of a compound molecule as an absolute entity. There is at least one distinct case where a compound molecule splits up like a simple molecule, $N^{\frac{1}{2}}C^{\frac{1}{2}}$ appearing in compounds, but unknown in a separate form. There is also the organic proteus $CH^2$, which not only divides into two, $C^{\frac{1}{2}}H$, which does not exist in a separate form, but also appears in an indefinite number of multiple forms. There may be others, too; but no one upholds the possibility of $HO^{\frac{1}{2}}$ being divisible into two atoms, $H^{\frac{1}{2}}O^{\frac{1}{2}}$, or $KO^{\frac{1}{2}}$ into two, $K^{\frac{1}{2}}O^{\frac{1}{2}}$; for although $H^{\frac{1}{2}}$, $K^{\frac{1}{2}}$ undoubtedly exist, $O^{\frac{1}{2}}$ is not admitted even by the new school. The idea of substitution-equivalent which suggested and now upholds $H^{\frac{1}{2}}O^{\frac{1}{2}}K^{\frac{1}{2}}$, implies $O^{\frac{1}{2}}$ as the ultimate atom of oxygen, and $K^{\frac{1}{2}}$ as the substitute for $H^{\frac{1}{2}}$ in $HO^{\frac{1}{2}}$. The vapour-density of caustic potash cannot be measured; but in coming to a decision as to whether the molecule of caustic potash is $H^{\frac{1}{2}}O^{\frac{1}{2}}K^{\frac{1}{2}}$ or H O K, it is well to keep in view that the K in $KO^{\frac{1}{2}}$ and the H in $HO^{\frac{1}{2}}$ must be decomposed in the former case, and not in the latter. There is also to be considered the intensity of the force that unites the molecules of the oxide together: their cohesion-integral must be very great, since the heat of a wind-furnace liquefies it, but does not volatilise it. We have an idea of the dynamic value of the force that unites the K with $O^{\frac{1}{2}}$, from the heat of combustion evolved in burning K. In the production of water from the union of H with $O^{\frac{1}{2}}$ to form $HO^{\frac{1}{2}}$ steam, and the union of $HO^{\frac{1}{2}}$ with $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$, $HO^{\frac{1}{2}}$ etc., to form water $HO^{\frac{1}{2}}.HO^{\frac{1}{2}}.HO^{\frac{1}{2}}$. etc., the dynamic value of each of these two forces has been measured, and the ratio of the chemical to the cohesive integral found to be about 7 to 1. There are cohesive affinities of vast potency as well as chemical; and we have to consider whether it is more probable that, when water and the oxide are brought into contact, the heat evolved (which indicates molecular force expended) is due to cohesive affinity between $KO^{\frac{1}{2}}$ and $HO^{\frac{1}{2}}$, or to chemical reactive force, $\left.\begin{matrix} K^{\frac{1}{2}}O^{\frac{1}{2}}K^{\frac{1}{2}} \\ H^{\frac{1}{2}}O^{\frac{1}{2}}H^{\frac{1}{2}} \end{matrix}\right\} = \left\{\begin{matrix} H^{\frac{1}{2}}O^{\frac{1}{2}}K^{\frac{1}{2}} \\ K^{\frac{1}{2}}O^{\frac{1}{2}}H^{\frac{1}{2}} \end{matrix}\right.$ which effects a crossing over of half molecules between the water-molecule and the oxide-molecule. If we decide on cohesive affinity, we may retain the

name hydrate of potassa. If we decide on chemical reaction, we must find a different name; but hydrate of potassium will not do, because the water has been decomposed and no longer exists in the half-molecule compound. Potassic water does not seem an improvement, though it manifests the desire of the new school to hold on to the name while rejecting the thing named.

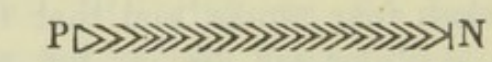
The atomic theory derived its origin from the quantitative relation of the substitution equivalents in chemical reactions. The idea of equivalent had then to give way to that of fixed atomic weight, or smallest combining ratio. Next, these were found to have a simple relation to vapour-density, also to specific heat. The last step has been the dynamic theory of heat and gases, which represents the law of volumes as the strictly physical sequence of an equilibrium of pressure and *vis viva*, or temperature, between perfectly elastic particles or molecules of different weights, and the law of specific heat in simple solid bodies as the probable sequence of the independent motion of the chemical atoms while maintaining an equilibrium of *vis viva*. By measuring the specific heat of elementary solids, we are enabled to obtain approximately the smallest combining proportion or atom of these solids in terms of a constant unit. By measuring the specific gravity of a gas or vapour, we obtain the weight of the molecule or smallest part that exists in a free or separate form in terms of a constant unit. We thus come to know that the molecule in a certain class of bodies contains only one atom, in another class two atoms, in a third class four atoms, and that the molecule of one body, sulphur, contains six atoms; but at a very high temperature this large molecule is separated into three smaller molecules, each of which contains two atoms. Mercury and cadmium are known, from measurement of vapour-density, to have one-atomed molecules; and it is inferred that most of the metals, such as iron, zinc, copper, lead, etc., have also one-atomed molecules, because they also combine with oxygen in the ratio of one specific-heat atom to an atom of oxygen. But there are four, viz. silver, lithium, sodium, and potassium, which combine with oxygen in the ratio of two specific-heat atoms of metal to one of oxygen, so that their vapour-densities

might perhaps show them to have two atomed molecules. The dual or two-atomed nature of the molecules of the elements oxygen, hydrogen, etc., is of high interest in regard to the dynamic integral of the force that unites the $O^{\frac{1}{2}}$ with $O^{\frac{1}{2}}$, the $H^{\frac{1}{2}}$ with $H^{\frac{1}{2}}$: *e.g.* we may thereby account for the extreme violence of the fulminates,—the decomposition of the oxide and alkali in such as fulminating silver being accompanied with the intense union of half molecules, as represented by the formula

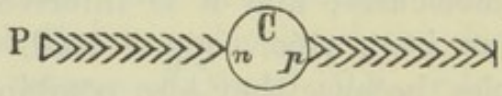
$$\left.\begin{array}{l} \text{3 mols. of oxide of silver} \\ \text{2 mols. of ammonia} \end{array}\right\} \begin{cases} Ag\,O^{\frac{1}{2}} \\ Ag\,O^{\frac{1}{2}} \\ Ag\,O^{\frac{1}{2}} \\ N^{\frac{1}{2}}H^{1\frac{1}{2}} \\ N^{\frac{1}{2}}H^{1\frac{1}{2}} \end{cases} = \begin{cases} Ag, Ag, Ag, \text{3 mols. silver.} \\ HO^{\frac{1}{2}}, HO^{\frac{1}{2}}, HO^{\frac{1}{2}}, \text{3 mols. water.} \\ N, \text{1 mol. of nitrogen.} \end{cases}$$

These half-molecules must be identical; for when water, $HO^{\frac{1}{2}}.HO^{\frac{1}{2}}$, is decomposed by the voltaic circle, the $O^{\frac{1}{2}}O^{\frac{1}{2}}$ of two molecules unite to form O; and as the $O^{\frac{1}{2}}$ in each water-molecule must be identical in properties, the two, $O^{\frac{1}{2}}$ in O, must likewise be so. Instead therefore of denoting one as positive and the other negative, it is better to view them as having poles or potential faces of complementary action which may be represented by symbol thus: $>$, $>$ are identical; but place them together, $> >$, and one is *entrant* while the other is *recipient.*

A line of similar symbols represents the character of the electric force, as between the excited root P and excited root N




of the line of force P N. Standing at P and looking towards N, the aspect is *entrant*; standing at N and looking towards P, the aspect is *recipient.* In each point of the line of excite-




ment the P and N aspects exist, as becomes manifest when a conductor cuts the line; one element, $> n$, appearing at one side of the conductor, and the next element, $p >$, appearing at the opposite.

Considering the vast cohesive power of the metals and their one-atomed molecule, their solid state may be viewed as the combination of their atoms with each other; so that if chemical notation could distinguish what for shortness may be called the burnt and unburnt conditions, the clasped and raised conditions of a chemical body, we should have the H in $CH^2$ different in character from H in $HO^{\frac{1}{2}}$, also Hg of mercury in a state of vapour different in character from Hg in the liquid or solid form.

In electro-plating, consider the condition of a metallic molecule before being deposited and after. In the first it is in the burnt or clasped condition (oxide in solution), in the second it is in the solid form; the dynamic clasp has also been effected. Now the molecule is in both cases in the clasped condition, and the transference is made from the embrace of the oxygen to the embrace of the metal without any appearance of the full equivalent of force that would be generated if the metallic molecule had descended from the vaporous or separate-molecule form.

An atom of metal seems almost as indisposed to exist in a state of isolation as an atom or half-molecule of gaseous element.

To attempt to express such conditions by modifying the notation would probably only lead to confusion. But I submit that the burnt or unburnt, the clasped or raised condition of atoms, should be kept well in view while studying chemical reactions; otherwise we shall be apt to lose sight of their real potential nature and wander into a maze of false analogy, barren of any result but an unmeaning jargon of words. For the same reason I submit that chemical notation should be founded on physical relations only, now that the classical labours of modern physicists have supplied nearly all that is necessary to do so.

EDINBURGH, 21*st February* 1864.

# AN ACCOUNT OF SOME ELECTRICAL EXPERIMENTS AND INDUCTIONS

(From the *Philosophical Magazine* for February 1865.)

In the various forms of electric discharge and their wondrous diversity of phenomena, can we detect a dynamical condition common to all—a single essential principle upon which the cessation of the excitement depends? Light is a common phenomenon attending discharge, having its origin therein; but throughout nature generally light originates in molecules of ordinary matter: a beginning of the phenomena of light is never unassociated with the material element, unless electric discharge be an exception: yet it may not be an exception, since Fusinieri has proved that metallic molecules are transported from one conductor to the other when an electric spark is made to pass between them. This transport of molecules has always appeared to me to be a phenomenon of the highest interest as respects the essential nature of discharge; and some years ago, while engaged experimenting, I endeavoured to find out if it existed in other forms of discharge besides the spark. On looking over the notes of these experiments lately, it has occurred to me that some of them may perhaps interest cultivators of this branch of science. In the following paper I submit these, along with others made more recently, with the view of investigating certain phenomena of induction on non-conducting matter; also some deductions from Harris's original researches of 1834 and 1839 with respect to the integral of the electric force, and the laws of its distribution in space. The remarks relating to each experiment, appended to the paper, are kept separate in order that the facts may be presented quite unalloyed with matter in any degree open to question or difference of opinion.

*Experiments on certain forms of discharge.*

1. A disk of sealing-wax, 4 inches in diameter and $\frac{1}{6}$ of an inch thick, held close to the negative excited end of a shell lac cylinder, then removed and applied to the electroscope, showed no charge; but if before removing it we touch every part of the outer surface of the disk, we find on applying it to the electroscope, that it has acquired a strong positive charge. If we touch only one point, the charge is confined to that point. If we touch with the blunt end of a needle there is no charge; if with the sharp end it is sensible; if with a brass wire it is sensible; with a lead point it is still more sensible, even when that point is held an inch or two from the outer surface of the disk; also with gold-leaf. These phenomena vary with the strength of the excitement of the shell lac, also of the *material* of which the conductor is composed. This last fact is distinctly impressed on the mind after frequent repetition of the experiments.

2. To examine this further, I worked with a jar having about two square feet of coated surface. Its uncoated part was covered with shell lac applied when the glass was hot, and the edges of the tinfoil were covered with sealing-wax, as also the lower part of the jar outside, where the curve was sharp. On the top of the jar was a cake of lac, with a sealing-wax handle to take it off and on; and through its centre passed the charging-rod, which was coated with soft wax, as was also the knob, which was a brass ball 2 inches in diameter. The jar rested on a cake of sulphur which rested on an insulating stool, and was charged by means of another rod and ball uncoated, which was removed after the charge. The soft wax that covered the ball was composed of a mixture of beeswax, resin, and red lead. It was heated and rendered semiliquid before placing on the jar; then, before it hardened, the point of an uninsulated needle was presented about an inch from the knob (first having touched the outside of the jar). Instantly the coated semiliquid surface of the knob opposite the needle was pitted with innumerable small holes, occupying an exactly circular space of three-quarters of an inch diameter. By taking away the

needle, touching the outside of the jar, then bringing the needle to its former position, the action could be sustained (until the coating became hard), having the appearance of a shower of particles as from a watering-pan.

The pitted space being then examined by a lens, the appearance was exactly as if a shower of minute projectiles had been forcibly driven through the wax coating to the metallic surface of the ball. The pitted space was bounded by a perfectly circular ridge, as if there had been a current of air or augmented atmospheric pressure on the part.

In the dark the usual appearance of brush was visible issuing from the needle when it was positive; when negative the star appeared, but the wax coating of the knob was pitted *exactly the same as when positive.*

3. Other experiments of the same kind were made by presenting the point of a penknife to the outer surface of a disk of shell lac upon which there was a thin coating of soft wax in a semiliquid state. The inner surface of the disk touched the wax-coated knob of the charged jar. The effect was very peculiar. When the point was within one-eighth of an inch from the coating of the disk, a sharply-defined perfectly *straight line* of indentation suddenly appeared, forming a small fosse about one-fourth of an inch long and one-fiftieth of an inch broad. This was quickly followed by another of the same kind, radiating from the extremity of the first at an angle of about 60°, and sometimes at right angles; others followed, and sometimes two together, forming the letter T.

The general effect, after the discharging point had been held opposite different parts of the semiliquid wax surface, resembled the marks left by crows' feet on a soft clay surface.

When the rounded extremity of a brass wire was substituted in place of the penknife, a similar effect took place, but with much greater ease, lines being thrown off in rapid succession forming stars. A small brass ball throws off stars.

4. It is only after such lines appear that the disk acquires a charge. When the wax surface has cooled and become solid, no indentation or mark is observed when the wire or knife is presented under otherwise the same circumstances;

but charge being still given as before, it seems fair to infer that the particles or molecules of metals are still projected upon the wax surface in lines.

*The indented line is not simultaneously impressed.*—The end nearest the point of the conductor appears first. This was most distinctly observed when the conductor presented to the molten surface was a needle bent at the point; also a small brass ball half an inch in diameter.

5. With highly-charged jar and knob the following experiments were made with *an insulated circular plate* of tinned iron 4 inches in diameter.

*a.* Held with its centre facing the *coated* knob at a distance of a quarter of an inch; after a few seconds, removed it, and found no charge.

*b.* The same process repeated after a very small *hole* had been made in the coating of the knob (the outside of jar being previously touched); the plate, held as before, obtained a charge of the same electricity as the knob.

*c.* The plate held with its *edge* to the *completely* coated knob obtained a charge of the same electricity as the knob.

*d.* The plate held as in *c*, but with *edge covered* with wax, received *no charge.*

In these experiments the insulation of the plate must be very perfect. The only way found to answer was suspending the plate by a flat ribbon of the finest shell lac, heated, and drawn out strong enough, when cool, to withstand the disposition of such plates to turn end on to the acting surface.

6. *e.* A cake of lac was substituted for the metallic plate in 5 *b*, and, like the plate, the cake obtained a charge of same electricity as the knob.

*f.* Experiment 5 *c* was repeated with the cake, and the effect was a slow gradual charge of electricity different from that of the knob; *i.e.* the knob being positive, the cake becomes slowly and gradually negative.

*g.* The knob of the jar being touched and the lac cake held with its face close to the coated outside of the jar (which teemed with electricity), it received no immediate charge of same electricity as outside coating, but slowly and gradually it received a charge of the opposite electricity.

*h.* The same cake held close to the bottom angle outside of jar opposite a part uncovered with wax, receives an immediate charge of same electricity as outer coating.

*Note.* To remove electrical excitement from shell lac and cakes of non-conducting matter, two small flames were employed, one on each side, simultaneously.

7. The prime conductor of an electrical machine, when hot (cylinder 10 inches in diameter), was coated with shell lac. When cold and the coating hard, the machine was put in action and a blunt wire held near the coated conductor (brass cylinder 15 inches by 3 inches). Brushes were projected

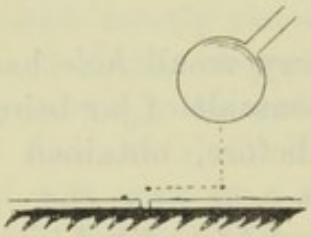


Fig. 1.

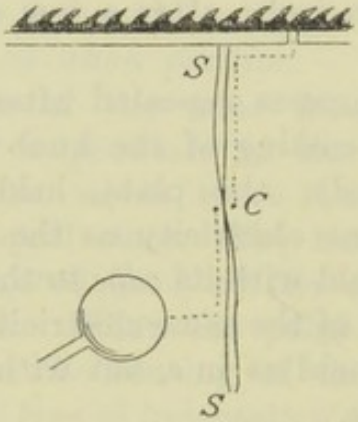



Fig. 2.

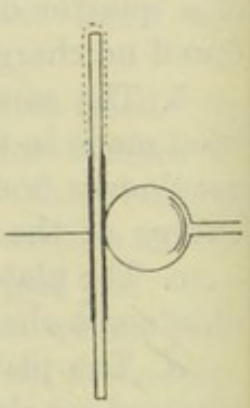


Fig. 3.

from the wire to the coated conductor, and luminous points appeared left by the brush and, as it were, imbedded in the shell lac. To continue to give brushes, it was necessary to remove the wire opposite to parts where these luminous points had not yet appeared.

8. *a.* On taking sparks from this shell-lac-coated prime conductor, each spark makes a hole in the coating; and if we do not hold the ball exactly opposite one of these holes, the electric fire takes an angular course, describing the perpendicular to the surface of the prime conductor, and then along the surface of the lac to the nearest hole (fig 1).

*b.* A thin piece of orange lac in the shell *s s* (fig. 2), having a crack in the centre *c*, but the edges of the crack quite close together, is stuck on to the coated conductor, and a

spark taken from a brass ball held as shown in fig. 2. The spark traversed the surface of the shell lac and passed through the crack, as shown by the dotted line. The distance is much greater than if the spark has to pass through the air alone without the shell of lac to assist it.

*c.* A glass plate, when hot, was coated with lac, and equal circles of tin-leaf stuck on the centre of both sides. It was held touching a brass ball projecting from the prime conductor. On working the machine, the plate discharged itself by occasional sparks round the edge of the plate; and when the action of the machine was reduced, a corona of light appeared at the edges of the tinfoil, which fluctuated in breadth and intensity in accordance with the action of the electric machine, which, if kept moderate, did not cause discharge by spark round the edges (fig. 3).

9. In the still atmosphere of a small room, the cap, wire, and gold-leaves of an electroscope were mounted on an insulating stand and charged. Various non-conducting bodies, such as clear amber, lac, heated crystal, etc., were then successively brought close to the leaves in various positions. Attraction was manifested between the excited gold-leaves and the non-conducting body, which of itself was entirely free from excitement, *i.e.* it did not attract uncharged gold-leaf.

*a.* A stick of clear lac was mounted horizontally on an insulating stand so that its conical extremity was level with the gold-leaves. By gently moving the stand, the pointed surface of lac was brought close to the nearest leaf without touching it When the distance was about $\frac{1}{20}$ of an inch between extremity of leaf and lac-surface, the force kept the leaf suspended and steady, as shown in fig. 4.

*b.* The charge of electroscope was then changed from positive to negative, and the same point of lac held as before with the same result of attraction—showing that it was not due to excitement pertaining to the lac.

*c.* The surface of lac brought to touch the leaf; the attraction continued. Removing it gently out of contact, the attraction remained as before.

*d.* Sometimes a small speck of gold-leaf separates and adheres to the lac; repulsion is then manifested at the point

where the speck lies, but attraction continues at the other parts. (See 50½.)

10. *a.* The mechanical effects of the electric explosion were noted between two balls coated with the soft wax. On discharging a Leyden jar between balls thus coated (about $\frac{1}{50}$ of an inch in thickness) and not in contact, *the appearance on each was the same*, viz. a small circular portion of the coating was thrown off and the surface of the ball exposed, forming a perfect circle $\frac{1}{30}$ of an inch in diameter, and in the exact centre was an excavated cup $\frac{1}{150}$ of an inch in diameter, as if a hole had been drilled to a very minute depth (fig. 5). Examined

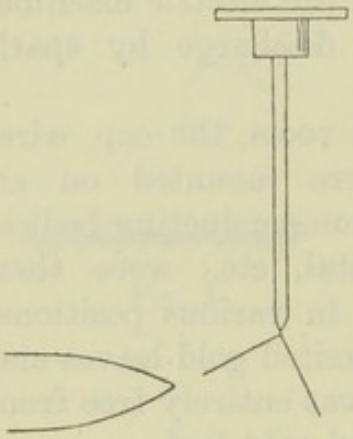


Fig. 4.

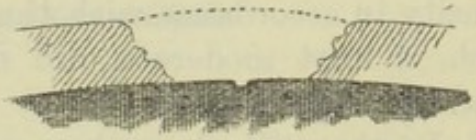


Fig. 5.

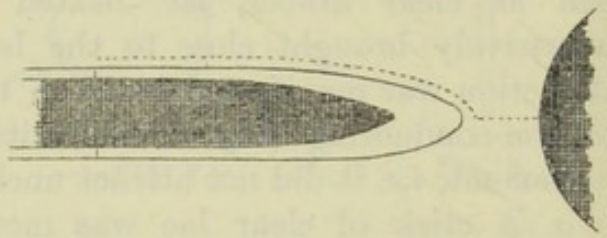


Fig. 6.

with a watchmaker's lens and reflected light in several directions, it was a simple concavity with bright surface, as if metallic particles had been torn off. Examined with a microscope magnifying 40 diameters, the cavity was obviously irregular; there was a slightly-marked ridge round the excavated part, and a few minute speckles of brass outside the rim as if thrown off in a molten state.

*b.* Some experiments were made with the view of discovering what became of the small portion of metal torn off at each discharging surface. When the discharging surfaces consisted of brass balls coated with wax, the appearance was the same in both, even when one ball was much larger than the other. The electric force in the axis of discharge effectually

tore apparently the same number of molecules from each surface; but in only one instance was there any appearance of those particles after the explosion. In this case one of the discharging surfaces was pointed lead, the other a wax-coated brass ball. A hole was made in the wax coating as before, and about half an inch from it a small patch of brass about $\frac{1}{60}$ of an inch in diameter was found, and in an opposite direction a similar patch of lead. On the other side, at about the same distance from the hole, a small irregular fragment of wax was found adhering, with that peculiarity of surface which showed that only the superficial particles had been in a molten state.

*c.* When the discharge was repeated by bringing the leaden terminus opposite the hole of the previous explosion, the bright point of the brass ball was found coated with lead; and a grey powder appeared round the hole in the wax coating, spreading round it to a radius of about half an inch. In this case a brush preceded the spark.

11. The mechanical effects of the electric explosion between a pointed wire covered with a thick coating of sealing-wax and a naked brass ball were as follows. The coating gave way about three-quarters of an inch from point, with sound of a smart crack of a whip, and the electric fire passed along surface of wax from the fissure to the ball. The fissure extended completely round the coating, but there was no projectile force given to it (fig. 6). The front part of the glove of wax had completely separated from the hinder, but the distance it had moved in consequence of the explosion was imperceptible.

### *Description of Electroscope.*

12. The electroscope employed in the following experiments is shown in fig. 7, which is drawn to a scale of one-quarter.

The cage consists of a brass cylinder, on the top of which is stuck a disk of lac with oval hole in centre, in which was fitted, when soft, an oval plug of shell lac that had been melted upon the wire when heated. The lower end of wire was filed to a knife-edge about $\frac{1}{8}$ of an inch long. This edge, when heated,

was brought down on a surface of soft sealing-wax (resin and beeswax), the sides then cleaned, and the knife-edge rubbed on paper so as to leave only the slightest coating of wax. It was then brought down transversely upon the centre of a strip of gold-leaf $1\frac{1}{2}$ inch long and $\frac{1}{10}$ of an inch broad (being less in breadth than knife-edge of wire), lying loosely on gold-beaters' paper. After being trimmed at edges, the angle of divergence is very clearly brought out. It is most convenient to use thick

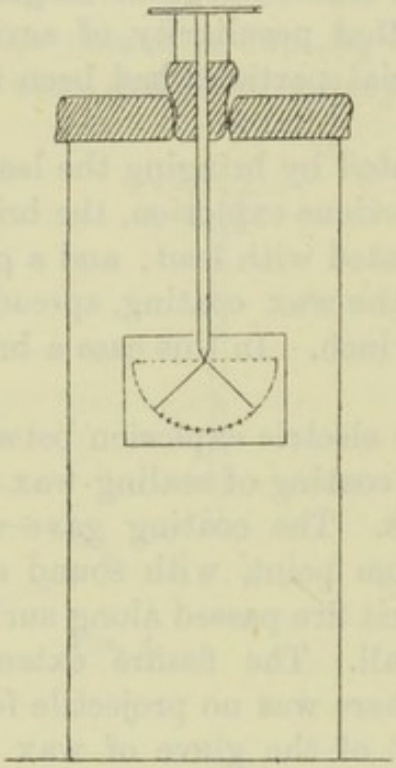


Fig. 7.

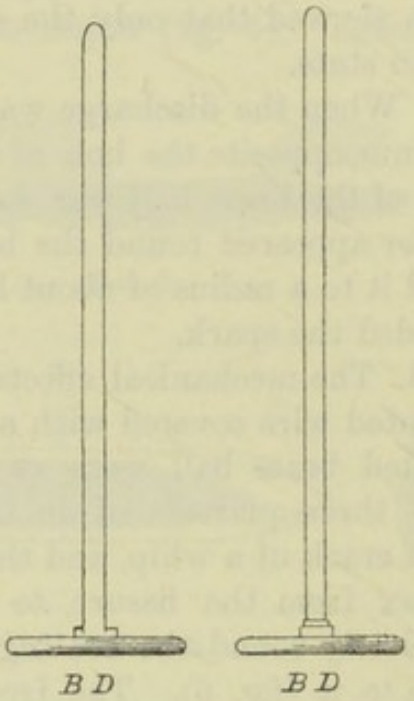



Fig. 8.

gold-leaf which can be handled. I used the thinnest (what is sold in books and used by gilders) in most of the experiments; but latterly I mounted the electroscope with a leaf ten times as thick, obtained from the gold-beater, and found that there was no sensible difference in the divergence for the same intensity of charge.

Two square openings were cut on the opposite sides of the brass cylinder and covered with thin plates of talc, and on the off side an angular scale was applied, which enabled the divergence of the leaves to be measured roughly, the maximum being about 110°. The insulation was almost perfect; even in

rainy weather the leaves would stand at their extreme angle for 10 minutes without sensible diminution.

To obtain some idea of the relation between the intensity of the charge and the angle of divergence, twin brass disks (termed B. D.) were provided (fig. 8), and an insulating handle provided to each, composed of a slender stick of fine hard sealing-wax. If we charge one B. D. inductively from a constant source, such as centre of an excited cake of sulphur (see 35), then touch it with the second B. D., we divide the charge into two equal parts; then discharge B. D. 2, and touch B. D. 1 again with it, we divide the charge into four parts, and so on; three touchings giving $\frac{1}{8}$, and four $\frac{1}{16}$. The cake of sulphur which served as electrophorus was stuck on the top of a cylinder of sealing-wax, as shown in fig. 20. Another cake of the same with shell-lac handle rested upon it, the two surfaces being in close contact.

As an example of graduation, suppose, after exciting the cake and putting on the cover, we leave it in contact for half an hour; then taking off the cover we apply a B. D. to the centre of excited cake, and touching it with a wire take off an induced charge; and reducing this to $\frac{1}{16}$ by four contacts with the twin B. D., we touch the cap of the electroscope with its edge, and after removing it observe the divergence of the leaves to be 65°. This is termed *angle of first contact.* We then repeat the process, and again touching the cap, find the angle rises to 75°, a third time to 80°, and so on, converging to a maximum of say 85°, which is termed *angle of last contact,* and is the angle that truly represents the intensity of the electricity of the induced charge, and therefore of the inducing power existing at the centre of the sulphur-excited surface. But from the angle of first contact we may infer the angle of last contact.

Now suppose the limiting angle of last contact due to three contacts of the twin B. D.s, or $\frac{1}{8}$ of the constant charge, is 100°; and of four contacts of the twins, or $\frac{1}{16}$ of the same constant charge, is 85°; and of five contacts, or $\frac{1}{32}$ of the same, is 60°. We can thus determine the relation between the angle of divergence and the quantity of electricity on the electroscope.

The maximum divergence depends on the proximity of the leaves to the sides of the cage, so it varies with the diameter of the cage and length of the leaves. With cage 6 inches in diameter and upwards the maximum is 70°; and 3 inches in diameter, 105°, the leaves being $\frac{5}{8}$ of an inch long; but if $\frac{6}{8}$, the angle extends to about 115°; and if 1 inch, to 130°: the weight of the gold-leaves seems too small to influence the angle, which seems to be fixed by the repulsion between the leaves themselves and the vertical wire that supports them, and by the attraction of the extremity of the leaves to the sides of the cage. This suggests an arrangement with the wire placed horizontal, as perhaps affording a more equable scale.

Having thus determined the scale of the particular electroscope, it is easy to find its capacity with reference to the B. D. unit.

It must be noted that the electroscope cannot be used as an electrometer when the angle of divergence is near the maximum, because the increment of divergence for the same constant increment of charge becomes very minute towards the maximum. By applying the twin B. D.s to divide and subdivide the charge, we can always keep the angle at a safe distance from the maximum. Beyond a certain intensity, which seems to be a fixed quantity, the gold-leaves discharge towards the air, as it is usually termed (see 1 and 3). If we hold a small disk of sealing-wax close to the leaves while we bring an excited electric towards the cap of the electroscope, it will not obtain a charge until the maximum is attained.

13. The cap of electroscope in contact with a B. D. took as much off its charge as two of the same B. D.s in contact at the same time with each other and with the first; so its capacity may be valued at 2, that of a B. D. being 1. The following is an approximate Table of density of charge and angle of divergence:—

| 20°. | 30°. | 40°. | 55°. | 70°. | 87°. | 100°. | Gold-leaf discharges. |
|---|---|---|---|---|---|---|---|
| 0·10 | 0·12 | 0·14 | 0·16 | 0·21 | 0·27 | 0·40 | 1·00 |

*Limit of discharging effect of some pointed bodies.*

14. On centre of excited sulphur cake placed insulated B. D. with small piece of gold-leaf stuck upon it: touched B. D., then removed it by handle, and while it passed edge of cake a small disk of sealing-wax was held opposite the gold-leaf to receive its discharge (fig. 9). After repeating this three times, the disk was laid upon cap of electroscope, and the leaves diverged 30°.

15. The insulated B. D. with small piece of gold-leaf stuck on rim, was placed on centre of strongly excited sulphur plate;

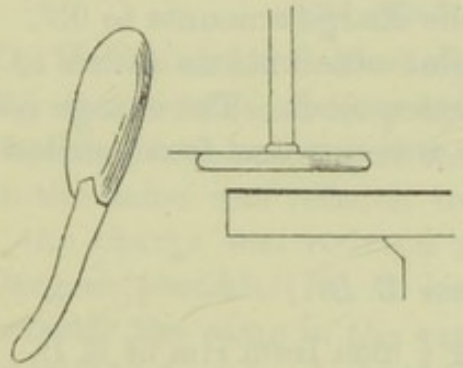


FIG. 9.

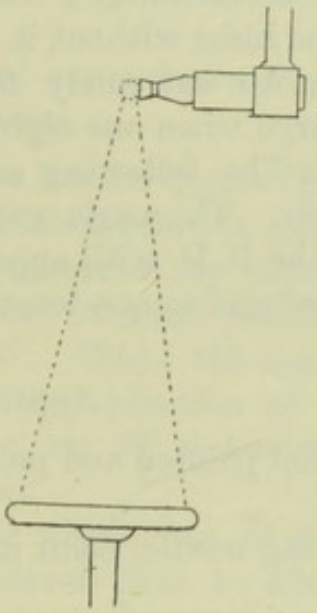


FIG. 10.

then touched it with a wire and removed it from plate. The charge thus obtained was subdivided to $\frac{1}{8}$, and then applied to cap of electroscope. The same operation was repeated without the gold-leaf. In the first case the angle was 58°, in the second it was 95°, thus showing that the gold-leaf appendage disabled the B. D. from retaining electricity beyond a certain tension. There was discharge from the gold-leaf in the act of leaving the excited plate (14), as was made apparent by placing a small disk of wax close to it at that instant, which, acting as a target for the gold molecules then emitted, showed, when applied to the electroscope, a strong charge of the same electricity as B. D.

16. The same experiment was repeated with silver-leaf, which gave angle 62°; platinum-foil, 50°; tinfoil, 56°; lead-foil, 57°; iron wire $\frac{1}{120}$ of an inch in diameter, 62°. In each the charge was reduced to $\frac{1}{8}$; so they compare with each other and with gold-leaf 58°.

17. The same experiment was repeated with floss-silk fibre; the charge was less than one-half that carried when gold-leaf was attached, and it charged the wax disk on withdrawal of B. D. same as the metal leaves.

18. The same floss silk, when highly dried on sand-bath, makes no discharge; the angle was 93° with the fibre attached, and the same without it.

19. An extremely fine fibre of shell lac also makes no discharge when one-eighth of the charge amounts to 93°.

20. The following are similar observations taken in rainy weather. The angle was of *first* contact. The charge retained after the B. D. with appendage was removed from excited plate was reduced to one-fourth.

| *Appendage on B. D.* | Angle. |
|---|---|
| Tinfoil pointed and projecting $\frac{1}{2}$ inch from rim of B. D. | 60° |
| ,, ,, 1 inch ,, ,, | 15 |
| Sewing-needle, point projecting $\frac{1}{20}$ inch . . . | 70 |
| ,, ,, $\frac{3}{4}$ inch . . . | 42 |
| ,, head projecting $\frac{3}{4}$ inch . . . | 70 |
| Silver-leaf, projecting $\frac{1}{2}$ inch . . . . . | 44 |
| Silk thread highly dried in sand-bath, the end projecting $\frac{1}{2}$ inch as a bush of extremely fine fibres | 100 |
| Without appendage . . . . . | 100 |

*Discharging effect of stroking.*

21. When the electroscope was charged to near its maximum a small disk of fine sealing-wax was placed upon the cap; then the elbow of a bent iron wire with a newly filed surface was gently drawn along the top surface of disk, stroking it as it were. This caused the leaves to descend to 50°, and on taking off the disk from the cap the leaves rose again to their original angle, and the disk was found electrified on the surface stroked.

There was no effect by merely touching the disk with the wire at many points.

Gold-leaf, silver-leaf, lead, brass, steel file, etc., employed in a similar way, also reduce the angle to 50°, and not lower.

The stroking of itself does not excite the wax surface when the leaves are at zero.

Note that all metals when *rubbed* on wax disk excite it positively, while silk and woollen excite it negatively. The charge given to the electroscope may be positive in the above experiment, so as to show that the effect is not due to excitement by rubbing.

*Discharging influence of flame and incandescent matter, and effect of screens.*

22. The insulated B. D. charged to a known degree ($\frac{1}{4}$ charge $= 90°$) was maintained in position 5 inches below a gas-flame, the jet being horizontal, and the time measured by metronome. When the flame was reduced to the very smallest possible blue bead, the charge was reduced to $= 40°$. When the flame was the largest possible (No. 2 burner), the reduction of charge was exactly the same in the same time, viz. $90° \times 4$ to $40° \times 1$, or 1·2 to 0·14 density of charge (fig. 10).

23. A bead of gas-flame 2 inches above cap of charged electroscope discharges it until the leaves close to about 1°; certainly not entirely shut, because touching the plate with the point of a penknife brings the leaves sensibly closer.

At the distance of 4 inches the discharge proceeds so far as to close the leaves to about 5°. At the distance of 6 inches the leaves do not descend below 30°.

24. The insulated B. D. charged $4 \times 90°$ is fixed in position at a distance of 6 inches from cap of electroscope, on a level with it. A drop of burning sulphur on the cap causes the leaves to rise first to a maximum of 60° and then to subside.

25. A wax thread stuck upon electroscope and lighted, discharged the B. D. as in 24, but did not charge the electroscope until the flame had subsided and given place to a red spark.

26. A match, consisting of a short length of thread steeped in solution of nitre and dried, was stuck on cap of electroscope.

The charged B. D., placed same as in 24, opened leaves to 30° before lighting the match; after lighting it they diverged to 60°, not further; and on removing the B. D. the leaves gradually subsided to 20°. Setting B. D. in position again, they gradually rose to 60°.

27. An excited cake of sealing-wax or sulphur held near electroscope so that leaves might diverge to 90°. A red-hot poker was then held about 5 inches above the excited surface of the cake, and the leaves subsided in about three seconds. A low red heat is sufficient to discharge such a surface if the iron is held close to it; but to discharge the excited surface of a cake by a hot iron held close to the opposite side of the cake, the heat must be bright orange-red.

28. If a poker, heated to the extreme possible in a bright fire, is held close to an insulated brass plate charged, no effect of discharge takes place; but if after blowing out the flame of a taper we bring the red incandescent point near the charged plate, it discharges it, but less quickly than if the taper were lighted.

29. *Effect of screens on discharge by flame.—a.* The insulated B. D. charged is placed in position 3 inches from gas bead-flame, and half-way between them an insulated tinned plate, 4 inches in diameter, is held as screen. The charge from 92° is in 15 seconds reduced to 60°. Without the screen the same reduction takes place in a fraction of a second. With insulated screen of wire gauze (sixty divisions to one inch), the same reduction takes place in 15 seconds.

*b.* The screen becomes charged with electricity different from that on the B. D., but the charge is less than if the screen were acted upon by the free induction of B. D.

*c.* In such experiments the charge of B. D. is reduced more quickly when it is held opposite the circumference of the screen than when opposite the centre, although in both cases equally hid from the flame.

*d.* A cake of wax held between gas bead-flame and B. D., charged positively, becomes excited negatively *on surface next the flame.*

*e.* Discharge occurs to the same extent with small screen as without it, but much slower.

*f.* If the screen is comparatively large, there is little or no discharge if the flame and excited body are on opposite sides and near the centre of the screen; but if near the edge, the discharge goes on the same as if the screen were small.

*Analysis of the Magic Pane.*

30. The Leyden jar described in 2 was highly charged, *positively inside, negatively outside.* The charging knob, rod, and chain being withdrawn, the jar was placed on a cake of sulphur that rested on an insulating stool. Having touched the outside of the jar, the electroscope was held close to it, and the leaves diverged with negative electricity; the divergence increased as the electroscope was removed outwards, keeping the same level until a maximum was obtained, after which it diminished. It also diminished to zero when held quite close to the outside of the jar. The divergence of leaves was greatest when electroscope was held near the mouth of the jar, and least when held near the bottom.

31. The electroscope being fixed in position within a few inches of the outside of the highly charged jar, the outside coating of the jar is touched, which causes the leaves to diverge with positive electricity. Then watching them, they are seen gradually to close and then gradually to open again with negative. Touching the outside of jar again, the leaves close and open with positive, then slowly close and open with negative, as before.

32. The same experiment as 31 repeated, except that the mouth of the jar was closed with a cake of sealing-wax. This sealing and closing up of the jar did not prevent the phenomena detailed in 31 from taking place, but whether at a slower rate was not remarked. The jar gradually lost the whole of its charge, although the mouth was completely sealed up.

33. A circular plate of fine red sealing-wax, 6 inches in diameter and $\frac{1}{4}$ inch thick, was coated with circles of tinfoil 3 inches in diameter. It was then placed upon a 6-inch diameter cake of sulphur strongly excited negative (fig. 13). The top coating of wax cake was then touched (fig. 14) and a spark obtained. The wax cake was then removed (fig. 16), and its lower side presented to gas bead-flame (fig. 17). It

was then placed with its lower coating on cap of electroscope. The leaves do not diverge until the upper coating is touched, when they spring out with negative (fig. 18). Reversing the cake and bringing the top coating *near*, but not touching, the cap of electroscope, the leaves diverge with faint negative; bringing it down to touch there is no divergence until the other side is touched, when the leaves spring out with positive. Reversing again and bringing lower coating near but not touching cap, faint positive is shown; bringing it down to touch, there is no divergence until upper coating is touched, when the leaves spring out with negative.

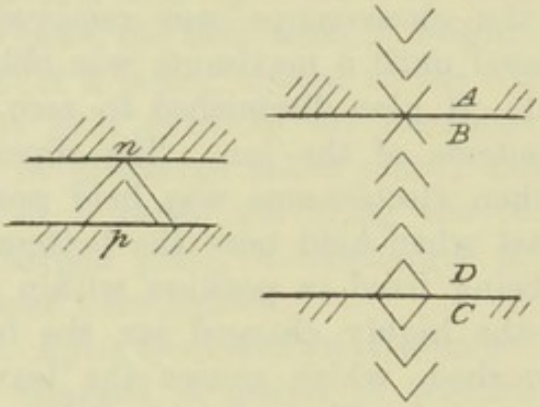



Fig. 11.




Fig. 12.

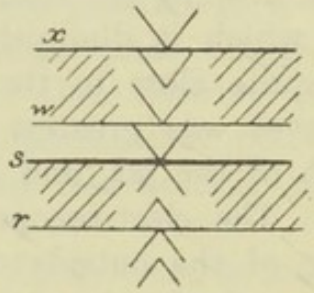



Fig. 13.

This may be repeated many times before the plate is discharged.

34. As these phenomena of the magic pane are fundamentally characteristic of the electric force, and make their appearance in uncoated non-conducting matter after excitement, it is well to have a clear idea of their sequence. To assist in this I submit the following method by diagram.

Let $n$ (fig. 11) be an element of a non-conducting surface negatively excited, and $p$ an element of another non-conducting surface positively excited. These surfaces may be in contact without discharge or depolarisation taking place. The polarised condition is represented by the V lines, their aspect from $p$ towards $n$ being male or entrant, and their aspect from $n$ towards $p$ being female or recipient. When the surfaces are in contact, the electricities are masked; their intensity is inappreciably small. On separating these surfaces, mechanical

force has to be expended, and electric intensity is generated, or polarised lines are spun having one root or extremity on the positive surface and another on the negative surface. When the surfaces are entirely separated, the electric excitement appears on both sides of each excited surface, as at A and B of *n* (fig. 12), and D and C of *p*. It is proper, therefore, to view an excited surface as having two sides.

In the following diagrams, surfaces that are excited are represented by thicker lines than those that are unexcited.

Fig. 13 represents an element of the coated wax plate resting upon the negative excited surface *s* of sulphur plate. The V lines show the female aspect issuing on both sides of *s*.

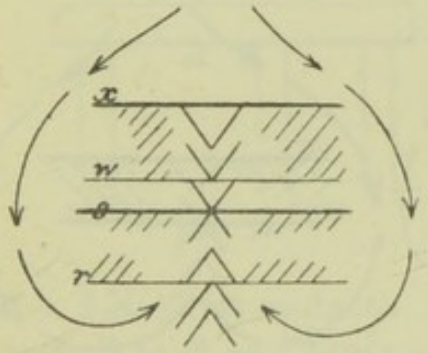



Fig. 14.

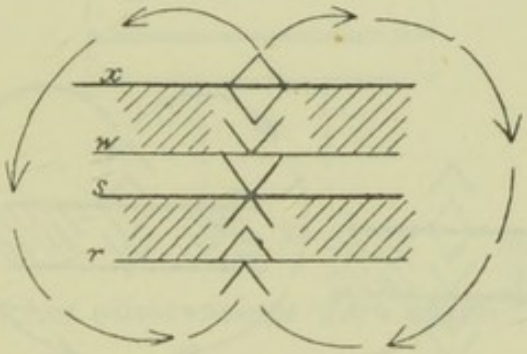



Fig. 15.

Fig. 14 represents the wax plate still resting on the sulphur, but after having its top coating touched. This removes all the V lines above *x*, which is now represented by a thick line, to show that the top surface is now excited or charged; and the aspect of the V in contact with *x* shows that the charge is positive or male; but the charge is masked or engaged by part of the opposite female force at *s*. Part of the force that issues from *s* is free, and is directed in lines that proceed backwards from *s*, and that turn round and, enclosing the engaged lines, appear in front of *x*. This is proved from experiments 30 and 33. In 23 the force that acts inductively on the electroscope is positive, which resides on the interior coating of the jar, so that its influence must radiate in a *curved* line to get at the electroscope.

The characteristic of the magic pane is, having the engaged lines of force enclosed and compressed laterally by the dis-

engaged or free lines, which may issue either from one side or both. Fig. 14 represents them as issuing from one side; but as the free lines, by various causes, are gradually discharged at $r$ surface, the lateral enclosing force of compression diminishes, and the engaged lines expand outwards until on the outer side of $x$ appears the male force in a free or inductively active state: these free lines extend from outside $x$ to outside $r$, and enclose the engaged lines that extend from inside $x$ to inside $r$ (fig. 15).

Fig. 16. The wax plate in the condition represented in fig. 14 is now shown separate from the excited sulphur cake. The engaged lines become free, and are directed with male

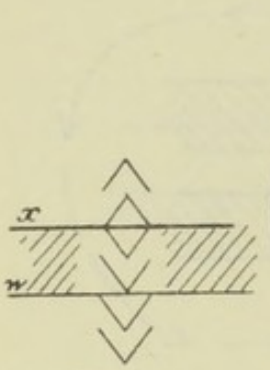



Fig. 16.

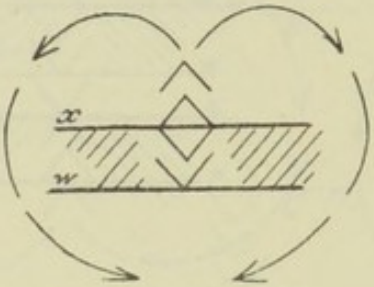



Fig. 17.

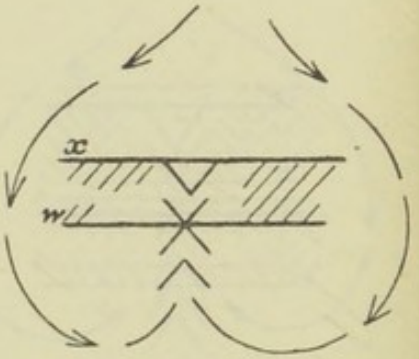



Fig. 18.

aspect from both sides of surface $x$, and the wax plate seems positively excited on every side.

Fig. 17. The coating on $w$ side has been touched and the lines between $w$ and $x$ are engaged, the charge on $w$ being negative but directed inwards, and masked and enclosed by the free lines issuing from the positive roots on the outer side of surface $x$.

Fig. 18. The coating on $x$ has now been touched, by which not only the free positive escapes, but also a certain portion that was engaged by the negative that is now rendered free on the outer side of $w$.

### *The behaviour of non-conducting matter under conditions of excitement and induction.*

35. *The behaviour of an excited electric while isolated.*— A cake of sulphur 4 inches in diameter and $\frac{1}{2}$ an inch thick,

after being excited negatively by slapping one surface, $\alpha$, with a warm silk handkerchief, was placed with this surface resting upon three sticks of sealing-wax set upright (fig. 19). The insulated B. D. was placed on centre of unexcited surface $\beta$, then touched with free wire and removed. The induced charge thus obtained was halved by touching with the twin B. D. Then applying it to the cap of the electroscope, the gold-leaves opened to 90° P., showing the inducing force at $\beta$ to be

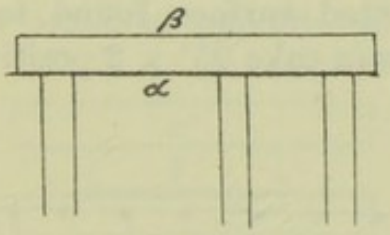



Fig. 19.

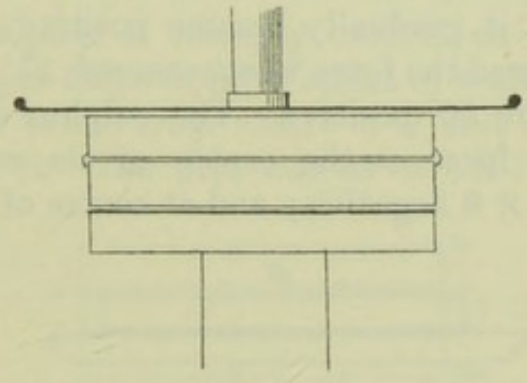


Fig. 20.

90° × 2 N. (first contact). Other observations were taken from $\alpha$ as well as $\beta$, as follows:—

| | h. m. | ° | | h. m. | ° |
|---|---|---|---|---|---|
| From $\beta$ at | 1 24 P.M. | 90 × 2 N. | From $\beta$ at | 1 40 P.M. | 80 × 2 N. |
| From $\beta$ at | 2 15 P.M. | 80 × 1 N. | From $\beta$ at | 3 2 P.M. | 30 × 1 N. |
| From $\beta$ at | 11 20 P.M. | 85 × 1 P. | From $\beta$ at | 10 20 A.M. | 85 × 1 P. |
| From $\alpha$ at | 11 20 P.M. | 85 × 1 N. | From $\alpha$ at | 10 20 A.M. | 85 × 1 N. |

The gradual transition from negative to positive of the electric forces at the surface opposite to the one excited is a phenomenon that invariably takes place in a few hours after excitement.

36. Two cakes of sulphur, 4 inches in diameter and ½ an inch thick, were luted together at the edges with soft beeswax, then placed upon a strongly excited plate of sulphur of same size that was stuck upon the top of a cylinder of sealing-wax. A brass plate 6 inches in diameter was placed on the top of all and touched (fig. 20). These were allowed to remain in position twenty-four hours. The double plate was then removed and applied to electroscope; the under surface showed negative, the upper positive. These surfaces were then completely dis-

charged by gas bead-flame; and the luting being cut, the interior surfaces of the plates showed no excitement when each was separately applied to electroscope. Again tried with elapsed time twenty minutes, still no excitement.

37. An excited cake of sulphur is placed with excited surface downwards resting on a cake of fine sealing-wax that is stuck on top of a lac cylinder (fig. 21). At first the force at $a$ centre of top surface was $85° \times 8$ negative. It then gradually diminished, until in an hour it became zero. After this it gradually became positive; and when eight hours had elapsed the force was measured, 57° positive, and after seventeen hours 50° positive. The sulphur cake was then taken off, and the force at the centre of its excited surface found to be $95° \times 8$ negative; and at centre of wax cake $95° \times 2$ positive.

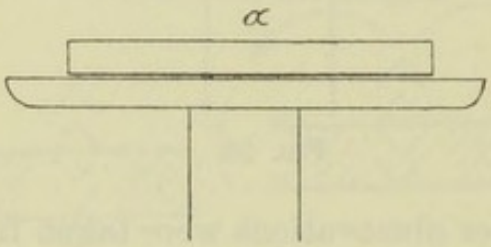



Fig. 21.

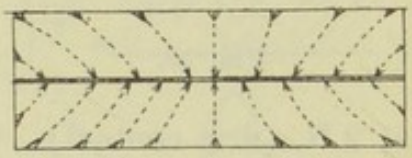


Fig. 22.

38. Two cakes of sulphur were each negatively excited on both sides as equally as possible (fig. 22). They were then laid close together so as to form a double cake, and the outside of this double cake completely discharged by holding a small flame on each side simultaneously. The plates were then separated and reversed, the inside surfaces being now the outside of double plate, which was discharged by two small flames as before. The plates were again separated and examined singly at the electroscope. The sides first discharged were found positive, and the others negative.

39. A negatively excited cake of sulphur 4 inches in diameter, resting on three sticks of wax and with excited side uppermost; on this is placed another cake, $b$, of 2 inches diameter, and on it another, $c$, of 4 inches diameter (fig. 23); $b$ takes on slowly but continuously an inductive charge positive. In ten minutes there is no sensible effect; but in two hours it is quite distinct, showing about 50° positive, when adjacent

excited surface is 90° × 4 negative. The cake *c* is also induced positive. With *b* composed of sealing-wax or beeswax, the effect takes place more quickly than with sulphur.

40. Two 4-inch cakes of sulphur are excited negative on both their sides, and between them is placed a cake of beeswax. It quickly takes on positive on both sides, which increases in intensity with the time of exposure. A cake of sealing-wax is similarly influenced. A cake of sulphur also, but much slower.

41. A 4-inch cake of sulphur is strongly excited on one surface, *a*, and then placed upon three upright sticks of sealing-wax. Induced charges were taken from centre of *a*, and also from centre of opposite side, *β*. The first was 95° × 8 N., the

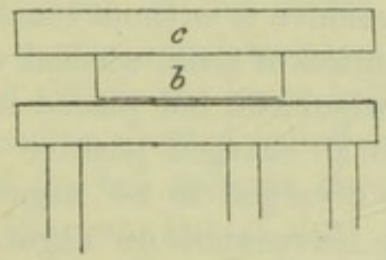



Fig. 23.

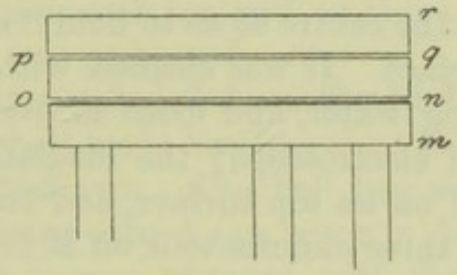



Fig. 24.

second 80° × 8 N. Twenty minutes afterwards the first was 85° × 8 N., the second 55° × 8 N. A lighted taper was then held opposite *a*, which reduced its force to zero, and the force at *β* was then 30° × 8 P. Such results could not be obtained from sealing-wax or beeswax-cakes.

42. The *a* surface of sulphur cake excited as in 41, and lighted taper immediately afterwards held opposite *a*. The result was zero both at *a* and *β*. If the taper, instead of being held opposite *a*, be held opposite *β*, the plate becomes a charged magic pane without coatings.

43. A cake of very strongly excited sulphur resting on wax cylinder; upon it are placed three similar unexcited cakes, *b*, *c*, *d*. After ten seconds they were removed and examined separately at the electroscope; *b* was found 60° P., *c* neutral, *d* 20° P.

44. Three cakes of sulphur resting on three sticks of sealing-wax; the lowest excited N. on its upper surface *n* so as to

give induced charge from its centre 90° × 4. After remaining undisturbed for about forty-eight hours, the centre of *r*, the top surface of upper cake, was found 10° N., and centre of *m*, the lower surface of first or excited cake, 30° P. The three cakes were then separated, and induced charges taken from both sides of each while it rested by itself on the three sticks of sealing-wax (fig. 24). The following was the result:—

Force at centre of *r* greater than at centre of *q* and equal 90° × 2 P.
Force at centre of *o* greater than at centre of *p* and equal 60° N.
Force at centre of *n* 90° × 4 N.
Force at centre of *m* 50° × 4 N.

45. A cylinder of fine black sealing-wax 2 inches in diameter and 4 inches high, with two small handles of stick, was projecting at the centre so as to hold it and remove it without touching its surface. It was cleaned with solution of soda, washed with running water, and dried in oven; when still hot placed it on cap of electroscope; the insulated B. D. charged positive was placed on its top surface, and leaves diverged to 80° negative. After three minutes took off B. D., the leaves show 60° negative; then off with cylinder, and leaves fall to zero. Put on cylinder again but reversed, the leaves diverge to 80° positive. Shellac cylinder showed the same effect.

46. A piece of a roll of brimstone 2 inches long, set on end upon the extremity of a cylinder of fine black sealing-wax 2 inches in diameter and 15 inches high, and upon top of the roll a B. D. strongly charged positive. In a few minutes the brimstone was *positive* all over, and the wax *negative*.

The same piece of sulphur was cleaned with soda, rinsed with water, dried in oven, and then set on the wax as before, with charged B. D. on top. In a few minutes it was found to be *negative* all over, and so was the wax.

47. The electroscope charged 98° positive. On its top plate was put a small plate of sealing-wax unexcited. The leaves fell to 90°, which continued steady for some minutes. Then took off the cake of wax, and the leaves stood out again to 98°; and on examining the cake, found that it had acquired a slight negative excitement.

*Suspended Dielectrics.*

48. White silk thread was for the most part used in suspending them. In its ordinary condition it conducts slowly, as the following experiment shows. (See also 17 and 18.)

*a.* A brass ball was suspended from a free conductor by a white silk thread 4 feet long. At a distance of about 12 inches below the point of suspension, a brass wire fastened to plate of electroscope pressed against the thread. The ball was now charged, and a few minutes afterwards the gold-leaves began to diverge, and continued to do so slowly. The thread was then heated in a close vessel on sand-bath to about 300° F., then taken out and the ball suspended with it as before. No divergence of the leaves ensued, and for two or three hours this perfect insulating quality was preserved.

*b.* While the brass ball was suspended from the high-dried thread an excited cylinder of shell lac is brought close below it, held for ten seconds, then withdrawn. The ball has received no charge; but if dust is flying about (as when a hassock is beaten close to the ball), it is soon charged with the same electricity as the shell lac.

*c.* A brass ball is suspended by the *undried* silk thread, and a negative-excited cylinder of shell lac is held close below it. The ball becomes charged positive, and the thread negative.*

*d.* If the silk is undried and there is dust flying about, the same experiment may be made without any apparent charge to the ball, because the convective charge may neutralise the inductive; but the escape of the negative up the silk line to the electroscope reveals the fact that convection and induction are both at work.

49. Cylinders and plates of sulphur, shell lac, and heated

* This appears to have been the case in Dr Faraday's experiments under the heading "Convection or Carrying Discharge" (Experimental Researches, §§ 1562-67). Being unable to understand the reasoning by which phenomena of apparent induction are represented as due to the conveyance by floating particles, I am inclined to believe that the silk lines employed conducted slowly, as no mention is made of their having been specially dried before making the experiment; and unless they were so they could not prevent an inductive charge.

glass coated with melted shell lac were suspended by dried silk lines, and subjected to the inductive influence of an excited rod of shell lac 18 inches long and $1\frac{1}{2}$ inch in diameter. They turned and were attracted like insulated conducting bodies of the same shape, but with less force.

The non-conducting quality of these substances was tested previously to experiment by placing them on the top plate of the electroscope while charged to its full extent, then touching them with free conductor; the gold-leaves, watched closely for some minutes, showed not the slightest change.

50. A ribbon of the finest sort of shell lac was drawn out about 3 feet long, $\frac{1}{10}$ of an inch broad, and $\frac{1}{200}$ of an inch thick. While hanging freely by one end, an excited cylinder of shell lac was brought within a few inches, and attraction was strongly manifested: a thread of dried silk is similarly attracted. A piece of the ribbon was then laid upon plate of electroscope when it was fully charged. A needle or free conductor was then brought down to touch the top surface of the ribbon within about $\frac{1}{100}$ of an inch of the plate. The leaves, watched closely for a minute or two, showed no loss.

$50\frac{1}{2}$. A. B. D. charged to $90° \times 4$ was placed with its rounded edge close to the end of a small stick of lac that had just been melted by flame of taper. A small cone immediately rose from the melted surface. (See fig. $24\frac{1}{2}$.)

*On the Integral of Electric Force and its mode of distribution in space, as deduced from the elementary laws established by* Cavendish *and* Harris.

51. To prove that *the attraction between parallel electrified conducting surfaces is constant at all distances if the quantity of electricity engaged between them is constant,* suppose two Leyden jars or magic panes exactly the same in every respect, except that the glass of one (L) is twice as thick as that of the other (M). If it takes 100 unit-jars to charge L so that the balance electrometer of Harris connected with its charging side requires 10 grains, there will be required 200 unit-jars to charge M to the same intensity as measured by the electrometer. The attractive force between a square

inch of the two coatings of M is four times the attractive force between a square inch of the two coatings of L. Let us suppose the charge of L to be increased by another 100 unit-jars, making the whole charge 200. The force between a square inch of the two coatings of L is now four times what it was with the charge 100. The force is therefore equal to that of a square inch of M, and the quantity of electricity on a square inch is the same in both, while the distance between the coatings is in L twice that in M. The electrometer connected with the charging side of L now requires 40 grains. Suppose the sides of a magic pane to be separated

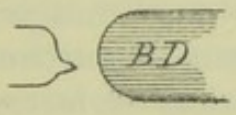



Fig. 24½.

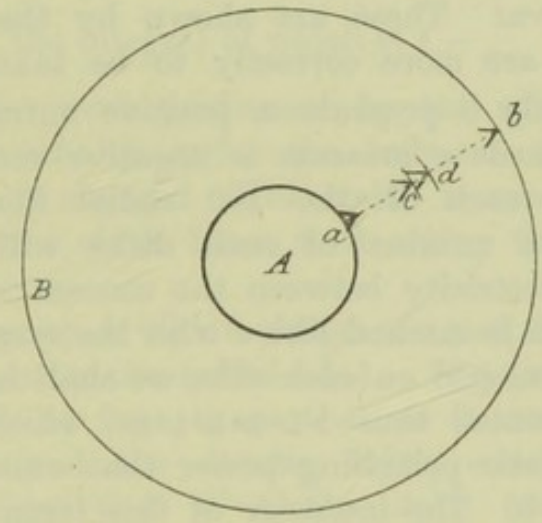



Fig. 25.

by a dielectric plate capable of compression, and which yields stiffly to the attractive force exerted through it from the opposite sides. *The quantity remaining constant, the static force is constant,* and the intensity of the free part of the charge diminishes as the square of the distance between the opposite surfaces.

52. The coated surfaces of the magic pane may be supposed to be perfect concentric spheres, in which case no free electricity is required to enclose and confine the charge, which is wholly engaged and symmetrically placed. This form given to the magic pane simplifies the subject, and enables us without embarrassment to arrive at an accurate conception of the integral of the force.

Let the sphere A (fig. 25) with conducting surface be situated concentrically within the spherical shell B, also composed of conducting matter. We suppose A to be charged with 100 unit-jars of positive electricity. On the inner surface of B we have negative electricity induced; and the quantity is the same as on A, because they mutually engage; but the density is less, because the surface is greater, the ratio being the inverse square of the radii.

Suppose 100 radial lines like $a$ $b$ to be drawn equidistant and let $c$ represent an extremely thin disk of metal perpendicular to one of the radii. We know that on the inner side of $c$ negative electricity appears, and on the outer positive. These are shown by the arrow-heads, or rather what are more correctly to be taken as V marks. Thus $c$ towards $b$ presents a positive entrant or male aspect, and $c$ towards $a$ presents a negative recipient or female aspect. Upon each of the 100 radial lines we may imagine an infinite number of such disks without any disturbance of the electricity between the concentric spherical surfaces; and if each is marked like $c$ with the symbol of the electricity that is developed on each side, we shall have each of the 100 lines represented thus $>\triangleright\triangleright\triangleright\triangleright\triangleright$, which exhibits the nature of the static polarising power that exists in the space between $a$ and $b$. The intensity of this force depends on the distance between the lines, diminishing as the distance increases. To obtain a clear idea of this, we may suppose another disk $d$ similar to $c$ and close to it. We may also imagine these disks to be extended all round so as to become spherical surfaces C, D concentric with A. The electric equilibrium will not be disturbed. We have next to suppose the electricity or electric lines between A and C to be discharged, also those between D and B. We shall then have remaining the electricity between C and D. Let the radius of A be considered as unity, and that of C be denoted by $r$; we may then represent $cd$ by $dr$. Let D be supposed immoveable, and C moveable and expansible. The force that attracts C to D is uniform through the element of space $dr$, and is proportional directly to the spherical surface C, that is to $r^2$, the spherical surface of A being unity, and inversely as

the square of the density of electricity upon C, that is to $\frac{1}{r^4}$, the density upon A being considered unity. Hence the static force of C towards D is thus $\frac{r^2}{r^4} = \frac{1}{r}$ [supposing the static force of A, when as originally charged towards an exterior concentric spherical surface at distance $dr$ (the electricity being supposed engaged between the two as in the case of C and D), to be unity], and the space through which this static force $\frac{1}{r^2}$ acts being $dr$, the differential of the work performed by the surface C in expanding to D by the influence of the electric force is $\frac{dr}{r^2}$, the integral of which is $1 - \frac{1}{r}$. This gives us the value of the work performed by the spherical surface A expanding to infinity under the influence of the electricity with which it is charged. This value is expressed by unity, which means the static force at A acting through the radius of A.

53. Compare the charge on A with a charge of equal density on another sphere Z with twice the radius of A. The quantity of electricity on Z must be four times that on A to have equal density with it; and the density being equal, but the surface of Z four times that of A, the static force on Z is four times the static force on A; and as the integral is that force acting through the radius of the respective spheres, it comes to pass that the integral of the charge on Z is eight times the integral of the charge on A; and the quantity of electricity on a square inch of the surface of Z, although equal to the quantity on a square inch of A, has twice its mechanical equivalent.

This may seem somewhat of a paradox; but a little consideration as to the rate of divergence of the electric lines will make it clear. Thus let $cd$ (fig. 26) represent the space occupied by an electric line or root on the surface of the sphere A, and $ef$ the same on the surface of Z, $b$ being their common centre. Now, the density being the same, $cd = ef = a$, and $\phi$ is the static force common to both. These lines on proceeding outwards diverge, so that if $cd$ or $a$ becomes $a\rho$ in $\delta$ distance

out from the surface of A, $ef$ or $a$ becomes $a\rho$ in $2\delta$ distance out from surface of Z. The static force of both being $\phi$, we have $\phi\delta$ the first dynamic increment of a line issuing from surface of A, and $2\phi\delta$ the first dynamic increment of a line issuing from the surface of Z. All the successive increments have the same ratio, therefore the integrals have also this ratio.

54. Thus we see by clear induction from the elementary laws established by the experiments of Cavendish and Harris, that an electrically excited surface contains only the roots of lines of force, in which lines the mechanical power of the electricity resides. Work is, as it were, stored up in these lines, and the static intensity at a point in one of them depends on the closeness of the neighbouring lines at that point only.

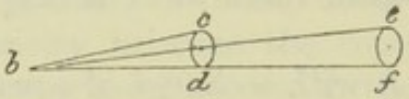



Fig. 26.

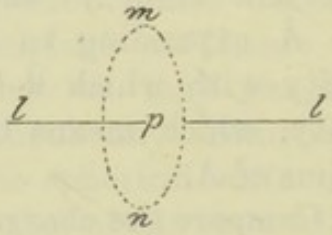



Fig. 27.

The closer they are packed the more intense the longitudinal force, and the more intense the lateral force of repulsion. Indeed these forces, as they increase and diminish together, seem to be identical, or a mode of reaction. The repulsive tension exists in the plane $m\,n$ (fig. 27) transverse to the electric line $l\,p\,l$ in which the contractile tension is manifested.

When two bodies charged with the same electricity are brought closer together, the lines become more closely packed. Mechanical power, or work, requires to be expended in order to force the bodies closer together, *i.e.*, to force the electric lines closer together. The lateral repulsion of the lines has to be overcome, and that increases in a high inverse ratio of their mutual distance, and produces a proportionally intense longitudinal contractile static tension. The mutual distance varies at every point of a line, and responsively so does the transverse repulsive tension and its dependent longitudinal contractile tension.

Let the disk $c$ (fig. 25) have a magnified representation in fig. 28, and let it be composed of a double film of metal $c_1\, c_2$, the two films being perfectly close together, yet not cohering. We may further imagine these double films to extend all round to form concentric spherical surfaces as before (52). It will be remarked that we have negative or $\underline{\vee}$ roots developed on $c_1$, and positive or $\Delta$ roots on $c_2$, the lateral repulsive tension acting in the space between these roots on both sides of the duplex film $c$; but on the side $c_1$ it produces a longitudinal strain in one direction, *i.e.* towards $a$; and on the side $c_2$ it produces an equal longitudinal strain in the reverse direction, *i.e.* towards $d$ or $b$.

Let $a$, $x$, $b$, $z$ (fig. 29) represent four rods jointed at their extremities; press $a$ and $b$ towards $c$, this will press $x$ and $z$ from

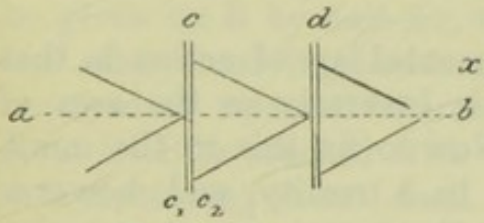


Fig. 28.

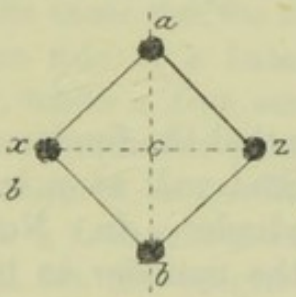


Fig. 29.

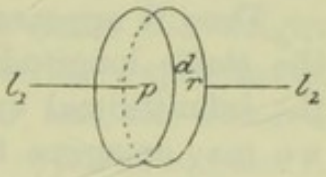


Fig. 30.

$c$ in opposite directions. A rough notion may thus be obtained of the lateral and longitudinal strain that exists on every part of an electric line—the lateral convergent being the $a\, b$ pressure, and the longitudinal divellent being the $x\, z$.

The lateral force affects the position of the lines, and thence of their polarised roots on conducting surfaces; but it is the longitudinal strain at the polarised roots that immediately produces the phenomena of motion and discharge. The motor phenomena resulting from the strain at the roots sometimes assume the appearance of attraction and sometimes of repulsion in adjacent bodies, as, from the lateral action of the lines, the roots happen to be distributed more on their near or their opposite sides.

55. If we examine closer this quantitative relation between the lateral and longitudinal forces, we shall find a certain simplicity that is worth keeping in remembrance.

Suppose two planes at the infinitesimal distance $dr$ to cut the lines at right angles. The lines intersect these planes in points, each point, as $p$, fig. 30, being the centre of a certain extent of surface which may be denoted by $a$, and $dr$ may be viewed as the axis of a cylinder whose area is $a\,dr$. This area is small when the lines are closely packed, and *vice versâ*. What relation does the static intensity of the longitudinal force at $p$ bear to the area of this infinitesimal cylinder?

The force, as it has been proved from Harris (51), is as the square of the density of electricity, or as the square of the number of points in a square inch. Thus $a$ being the area of one point, the number in a square inch is $\propto \frac{1}{a}$, and the force on a square inch is as $\frac{1}{a^2}$; but the number being $\propto \frac{1}{a}$, the force of each one is $\propto \frac{1}{a}$.

Thus it appears that the fundamental law of action is that the static longitudinal pull at $p$ is inversely as the area of the infinitesimal cylinder $a\,dr$. Now to fix this in the mind, we may imagine the cylinder to be a reality, and, however great or however small, to contain the same constant quantity of an elastic fluid, which will thus be more compressed when $a$ is small than when it is large. Further, let us suppose a valve or pressure-gauge at $p$, or rather two small pistons, acting one in the direction $pl_1$ and the other in the direction $pl_2$, under the influence of the elastic fluid contained in the cylinder. This elastic fluid, when of double density, exerts double pressure, according to the law of Mariotte; so that it is not difficult, by keeping this little artificial arrangement in view, to retain a distinct idea of the distribution of force in an electric line.

How Nature accomplishes this curious arrangement of force, and supports it from two excited material terminals, is of course a profound mystery; but that it does exist is, I submit, a fact inductively established. We have no choice, we cannot deviate from it, if we regard accuracy in the conception of the potential nature of electricity. It is a theory, not an hypothesis; a system proved from Harris's observations, not a system coined in the imagination.

56. With regard to practically determining the mechanical equivalent of a given charge (that is, a given quantity of electricity on a given surface), the only difficulty is from the portion that is free—how to ascertain its amount and allow for it.

The following occurs as perhaps practicable with Harris's apparatus. [The unit-jar as a measure of *quantity* seems to be unexceptionable; would it not be well to have an arrangement for an *absolute*-unit standard measure?]

Suppose we wish to compute the integral of a charge of $g$ unit-jars given to a sphere 10 inches in diameter, having a surface of 314 square inches. Let B be a circular conducting plate of 100 square inches mounted horizontally in a perfectly insulated condition, and at the distance of an inch let A, another circular plate of the same size, be suspended by newly high-dried silk lines to one side of a balance. Let a charge be given to B by unit-jar, while a free conductor touches A; and after removing the free conductor from A, let the weight required to raise A be measured as in Harris's experiments. Next let B be touched with a free conductor, and, the distance A B continuing to be 1 inch, let the weight required to raise A be again measured. Let the first weight be denoted by $m$, and the second by $n$, and let the charge given to B at first be $c$ unit-jars. The forces at the constant distance A B being as the square of the quantities of *engaged* electricity between the opposite surfaces, we have $\sqrt{m} : \sqrt{n} :: 1 : \sqrt{\frac{n}{m}}$; and $c\sqrt{\frac{n}{m}}$ is the charge which, engaged between two surfaces of 100 square inches at the distance of 1 inch, engenders the force $m$ of apparent attraction. Now $c\sqrt{\frac{n}{m}}$ upon 100 square inches gives electricity of same density as $3{\cdot}14\ c\sqrt{\frac{n}{m}}$ upon 314 square inches, the surface of the sphere of 10 inches diameter: compare this charge with $g$. The force of apparent attraction follows the ratio of the square of the charge; hence we have

$$c\sqrt{\frac{n}{m}} \times 3{\cdot}14 : g :: 3{\cdot}14 \times m : \frac{mg}{c}\sqrt{\frac{m}{n}} = w,$$

that is, the value of the weight that, acting through 5 inches, the radius of the sphere, expresses the integral of its electric charge—its work-representative.

*On the Arrangement of Electric Lines into Systems.*

57. From the simplest system, viz. that of lines issuing from the surface of an insulated conducting sphere, we may pass to others where exact mathematical treatment seems hardly possible as yet. But one or two salient points arrest the attention.

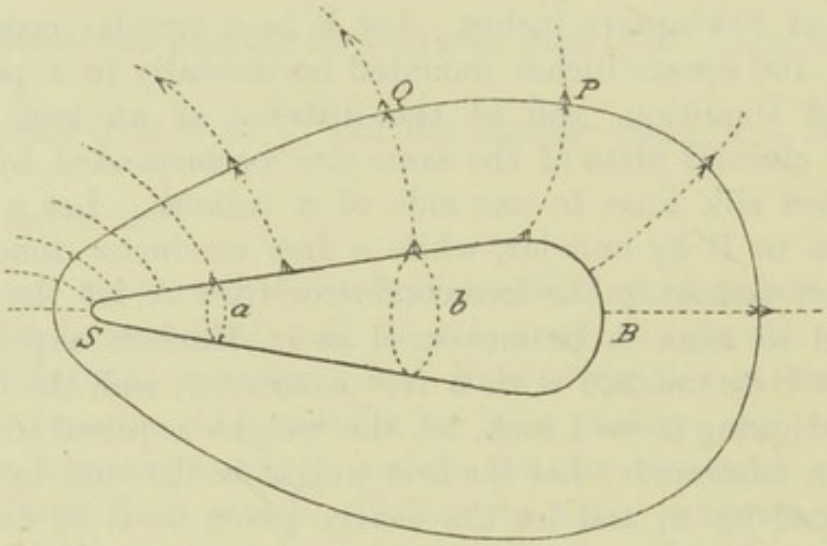



Fig. 31.

58. Free electricity in a conducting surface of unequal curvature, as the solid S B with a sharp and blunt end (fig. 31).

*a*. The mechanical equivalent of a line upon such a surface increases in going from the blunt to the sharp end. The lateral force of the roots lying in the small circle $a$, resolved perpendicular to that circle, must equilibriate the lateral force of those in the larger circle $b$ similarly resolved. This requires closer packing at $a$ than at $b$. The number in $a$ × their lateral force should be equal to the number in $b$ × their lateral force. Let $a$ be the length of one circle, and $b$ that of the other; $m$ the number in $a$, and $n$ the number in $b$. Then $\left(\frac{a}{m}\right)^2$ and $\left(\frac{b}{n}\right)^2$ represents the area of each root respectively; and since

the lateral force at the root is inversely as the area, we have $\left(\frac{m}{a}\right)^2$ and $\left(\frac{n}{b}\right)^2$ representing the lateral force at each root respectively in the circles $a$ and $b$. Multiplying these by the number of roots $m$ and $n$, we have $\frac{m^3}{a^2} = \frac{n^3}{b^2}$. Hence $\frac{m^3}{n^3} = \frac{a^2}{b^2}$; and the density at $a$ being $\frac{m^2}{a^2}$, and at $b$ $\frac{n^2}{b^2}$, we have the ratio of the density at $b$ to that at $a$ as 1 to $\frac{b^2\, m^2}{a^2\, n^2}$, which by substitution is equal to $\left(\frac{b^2}{a^2}\right)^{\frac{1}{2}}$.

*b.* Suppose a continuous metallic envelope to intersect all the lines that radiate from the surface of S B beginning at a certain point P. If P were at an infinite distance from S B, it is evident that the envelope would be spherical: so at intermediate distances such envelopes would graduate between the extremes of such a surface as S B and a perfect sphere, while those close to the primary S B would be less contrasted than it in the curvature of its extremities. The surface would be similar to S B, but less and less pointed as the distance is greater.

*c.* The mechanical action of the electric roots on S B being at each perpendicular to the surface, the integral of all these forces, resolved in one direction, ought to be zero, because all the lines being supposed free, and no discharge taking place at the sharper end, the body is at rest; the static pull at the surface is equal in all directions.

59. Free electricity of the same kind on two adjacent insulated conductors.

*a.* While the excited bodies are forced together, the lines are closer packed and have their integral power augmented, mechanical power being converted into electric tension, and *vice versâ*. When they fly from each other, electric tension is converted into mechanical force or work, *e.g.* the two hemispheres H, K (fig. 32); when separate and equally electrified, their collective surface measures six great circles, when in close contact it measures only four.

*b.* The replusion between H and K is immediately caused by the increased quantity of electricity accumulated on the off sides

of the hemispheres (leading to a preponderance of *root pullings* on these sides, which is effected by the lateral repulsion of the lines that issue from H on those that issue from K bending them round.

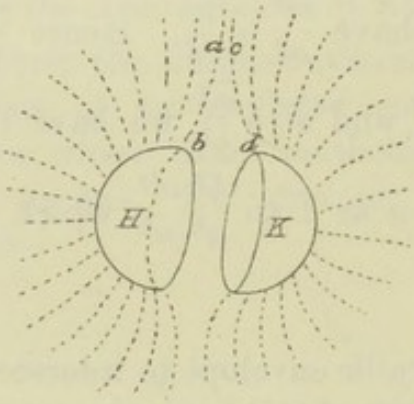



Fig. 32.

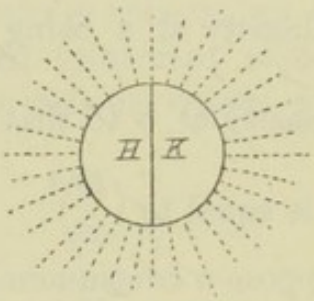



Fig. 33.

*c*. The disposition of the electricity on the adjacent sides of the hemispheres as they approximate must be peculiar. The lines belonging to one that lie next to the lines belonging to the other, as *ab*, *cd* (fig. 32), while approximating at *a* and *c* and

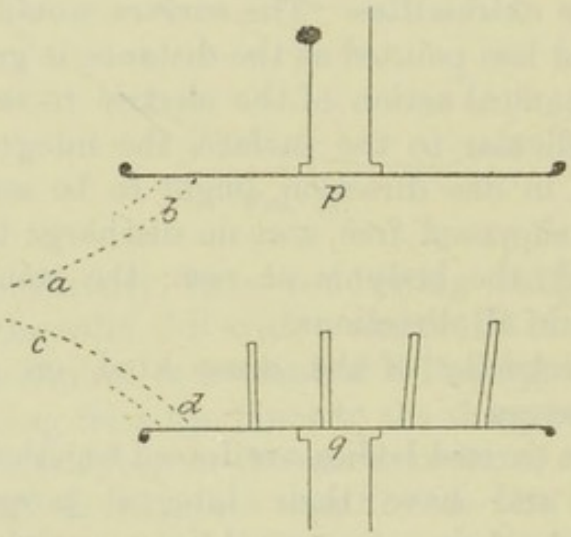



Fig. 34.

external to these points lying adjacent, and forming part as it were of one system, must at the lower part *b* and *d* diverge to these roots on different surfaces, there to form the interior terminals enclosing a space in which no electric lines exist. I have found this confirmed by experiments made with circular plates 5 inches in diameter stuck on cylinders of lac (fig. 34).

Narrow slips of gold-leaf about 1½ inch long were stuck on at three or four points in a line passing through the centre, so that when the plate was charged they rose on end, and would stand thus for an hour (*d*, *q*). When the twin-plate *p*, equally charged, was brought down parallel to *q*, there was a certain distance (about 3 inches) at which they dropped suddenly dead as it were; and looking through a lens at them when the distance was reduced to about 2 inches, there was not the slightest appearance of electric excitement at the edges of the leaves when *p* was tilted a little so as to disturb the general equilibrium of the system of lines.

It appears that in such cases, when bodies similarly electrified are made to approach, we are compelled to admit the

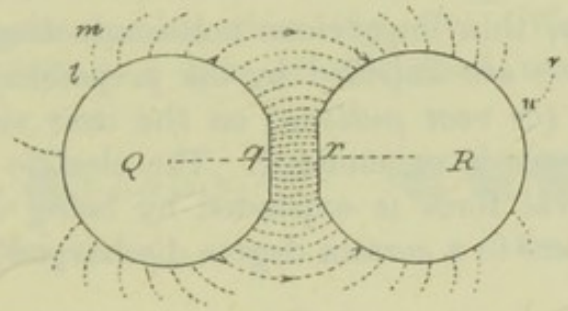



FIG. 35.

existence of a series of *inner terminal lines* acting laterally outwards and sideways, and sustained by their contractile tension. (See 60.)

60. When insulated conductors charged with different electricities approach each other, the charge becomes more engaged and less free. The mechanical integrals of the lines diminish, and electric tension is converted into work. When forced separate it is *vice versâ*.

Let Q and R (fig. 36) represent two equal spheres segmented at *q* and *r* so that there should be equal and parallel circular planes opposing each other at those points. Suppose these spheres to be equally charged with the opposite electricities, and to move backwards and forwards in the line Q R—an impervious dielectric medium, such as a mica plate, intervening between the circles *q*, *r* to prevent discharge when close together.

When Q and R are at a distance, almost all the electricity upon them is free and distributed over their spherical surface (fig. 36); when they are close together, almost all the electricity is engaged (fig. 37), and to be found in the opposite circles $q$, $r$

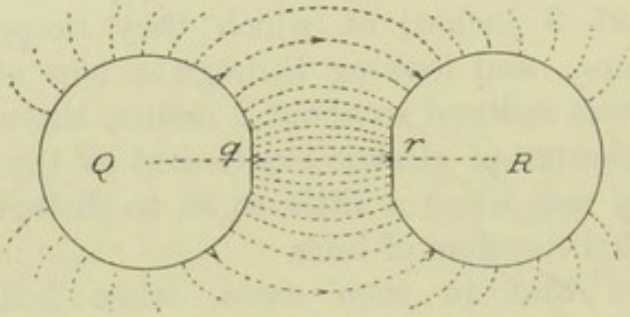



Fig. 36.

separated by the thin impervious non-conducting film. While approaching, they are impelled by the preponderating force of the electricities (or *root pulling*) on the near sides, and work or dynamical force is engendered. The electric lines collapse, and their integral force is expended by being converted into motion. They are to a certain degree discharged; but complete

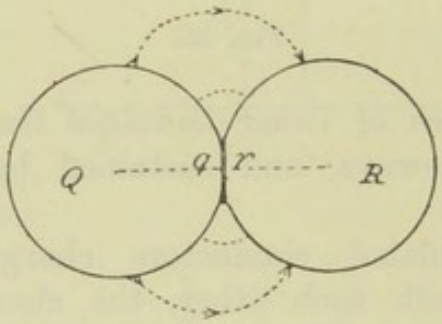



Fig. 37.

discharge has not taken place, for the roots are still polarised, and by applying force the spheres may be withdrawn from each other and work reconverted into electric lines.

Let us consider the equilibrium of the lines in an intermediate position such as fig. 35: what is the force that packs close together the lines on the near surfaces? The condition of equilibrium among the lines requires that they be pressed as much inwards towards $q\,r$ as outwards; but tracing round the spherical surface till we come to the extreme outward points

opposite $q$ and $r$, it is obvious that, although the excited roots of these points may have others adjacent mutually repellent on all sides, yet the lines that issue from these extreme points must in their curvilinear course $l\,m$, $u\,v$ take directions that cannot have juxtaposition except at the issuing points on the surface. What, then, prevents them yielding to the repellent forces of the interior adjacent lines? Let $m$ (fig. 38) represent an ultimate metallic chemical atom in an electric line P N. If such a condition could be realised, there is every reason to believe that the side towards N would be positively electrified, and the side towards P negatively electrified, as represented by the V symbols, and that it is thus being pulled equally from both sides with intensity in accordance with the nearness of adjacent lines. Now let us suppose a similar atom situated at each symbol thus (fig. 39). We have, instead of an electric line, a line of electrified molecules each of

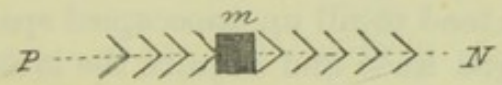



FIG. 38.

FIG. 39.

which is drawn by an equal force in opposite directions, giving to the whole line a *longitudinal contractile tension.* Now supposing such a line to be curved, this contractile force will engender a lateral compressing power towards the concave side; and as all the disengaged electricity in this system consists of lines similarly curved, *i.e.* with similar convexity one over the other, *the lateral packing power is cumulative,* and the central engaged lines will be packed together by means of the curvilinearly derived power of the whole of the system exterior.

The proof of the existence of this contractile power in a line is obtained by employing the method of concentric films as in 52. Suppose B and all the concentric films to contract simultaneously and concentrically upon A by the electric action between the adjacent surfaces in degrees respectively proportionate to their distance from it. Here is a system of lines expending their integral, and in doing so exerting a contractile force throughout, which must therefore have had a contractile tension

as potential antecedent, a force acting at each point of a line towards its opposite extremities. A line composed of india-rubber has a contractile force which might be thus defined; but it differs from an electric line in this, that in the rubber line the force is the same at every point; but in the electric line the force depends on the distance of adjacent lines, and is thus variously distributed according to circumstances. Thus, *e.g.*, in the concentric spherical arrangement of 52, the contractile tension diminishes outwards regularly. When the electric lines are closely engaged, the contractile tension must be nearly uniform. In the systems figs. 34, 35 it may be small at the root, gradually increase for a certain distance, obtain a maximum, and then diminish. Generally it must be in a continual state of change according to the varying condition of the system to which it belongs.

61. In the magic pane the free part of the charge compresses the engaged part by the contractile tension of the free lines. These by their lateral repulsion tend to fill up unoccupied space, and thus bend round the engaged lines and acquire the lateral packing power by their longitudinal tensile force when thus bent.

Fig. 40 is intended to give an idea of the charging of a magic pane. The line B pressed forward by A (intended to represent the lines engendered by the machine) arrives at the balls, $k$, $l$. The ball $l$ cuts the line, and so do the opposite surfaces of the magic pane. The part of it between $k$ and $l$ remains, and so does the part between the coatings of the magic pane. The part $c$ between $l$ and the near surface of the magic pane contracts and discharges itself along the conducting wire $w$; the exterior part D escapes to the ground by the free conductor $x$ after the centre part N has been pressed into the magic pane. The lines between $k$ and $l$ accumulate until discharge takes place, which may be looked upon as a signal that a certain number $a$ of lines *have* crossed over and left segments packed into the magic pane. The spark appears *after* the unit quantity has entered the jar. The propelling force (from the machine) required to bring up $a$ of course increases as the charge M increases; but when brought up, it is probable that discharge between $k$, $l$ destroys

the same constant number of lines, and thus that the unit-jar correctly measures quantity, its coatings $i, j$ being viewed as prolongations of the surfaces of the respective balls.

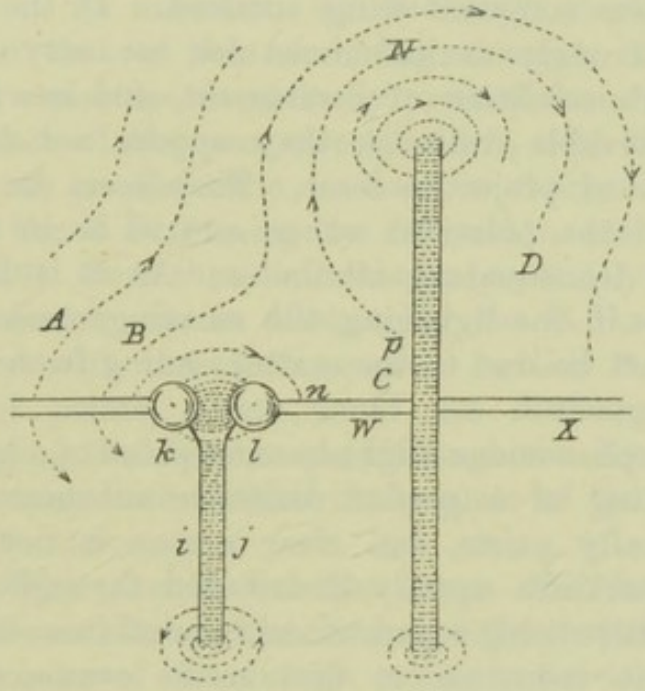



Fig. 40.

*Remarks.*

In experiments such as 1, 3, 5, 6, the phenomena are, strictly speaking, examples of convection or conveying of polarised particles (except 5*b*, 5*c*) to non-conducting matter, upon which they adhere without being depolarised. They all tend to prove that electricity is never separate from matter even in discharge.

(3.) It seems probable (since a small ball throws off stars) that a brush consists of a symmetrical succession of polarised molecules—at least when discharged upon a non-conducting surface, upon which they cling still in an excited condition, and therefore must act repellent upon the next in succession (see 4). The breadth of the fosse is evidently caused by capillarity.

(7.) In this experiment the thin shell-lac coating of the conductor becomes charged as magic pane with one side only coated. Viewed in the dark, the brush was of eccentric forms.

(8 *a*.) Is a remarkable effect that I do not recollect having seen mentioned before, although it can hardly have escaped the notice of experimenters.

(8 *b*, etc.) Are due to a similar quality of shell lac, and probably other non-conducting surfaces. If the electric fire consists of strongly excited molecules, we may remark that when in such condition of excitement, and moving probably with inconceivable velocity, they appear not to obey any ordinary law of projectile force. They seem for the time to form part of the polarised æther, and to be in the grasp of an agent of transcendant attributes. As it is in the spark, so must it be in the lightning, the amazing mechanical effects of which must be due to the matter issuing from the surfaces of discharge.

(9.) Such phenomena might be anticipated of a heterogeneous body consisting of a *perfect* dielectric substance (if such a substance really exists, but clear amber is not) containing conducting particles equally distributed throughout its mass, each one particle being separate and isolated from its neighbour. The apparent induction at first seems evanescent; but in subsequent experiments it will be remarked that, if exposure lasts for some minutes, persistent inductive excitement appears on the surface adjacent to the one excited.

(10.) We may remark in this, that while brush discharge and convective discharge of less intensity (1) takes place upon a non-conducting surface, the spark discharge does not take place without forcing it open: the coating is *lifted up* at both discharging surfaces, *therefore metallic particles issue from both*. Is such the case in lightning between the cloud and the ground? Does the flash consist of luminous water on one side, that of the cloud? Towards the ground there may be many surfaces of discharge, at each of which polarised molecules issue, giving rise to so many different foci of explosion.

(21.) This effect is what would take place by separation of polarised molecules from the metal that strokes the wax surface, because the excitement is located on the surface stroked. Yet it is difficult to see that this is likely, the wax being much softer than the metal, especially when it is steel—as the side of a steel blade.

(22.) The rate of discharge diminishes with the intensity: combustion being chemical action, and chemical action being also electric action, the electrically-excited molecules of the oxygen, carbon, or hydrogen may fly to the excited body under the influence of its induction, which though weak may yet be stronger than that of the combining element. From 29 *d* it appears that molecules do actually proceed from the flame to the near surface of the wax screen, which, with the B. D. and intermediate air as dielectric, becomes a charged magic pane arrangement, the B. D. forming one side and the screen the other.

(24, 25, 26). Here there is evidence, especially in 25, that the flame or match acts in discharging the B. D. partially, as if unconnected with the electroscope. In this it differs from the action of points, which could not discharge the B. D. without charging the electroscope, although the discharge would of course be more limited than if the plate of the electroscope were a free conductor.

(27, 28.) Why should not the brass disk be discharged under the same conditions that discharge the excited sulphur?

(30.) This is proof of induction acting in curved lines, which Faraday was the first to direct attention to (Experimental Researches, 1218, 1231). Not entertaining the possibility of such curved action taking place in a vacuum, he holds that the inductive force is enabled to bend round in consequence of the molecules of air being polarised, and one leading the action to the others in straight lines. In § 63 of paper in *Phil. Trans.* for 1834, Harris states that the operation of electricity on distant bodies by induction is quite independent of atmospheric pressure, and is precisely the same *in vacuo* as in air.

(31.) The free part of the charge which issues from the exterior surface of the inside coating gradually diminishes, and the engaged or masked portion between the coatings consequently expands, and the free part comes to issue from the exterior surface of both coatings. So the electroscope shows latterly the action of that nearest to it, which again is removed by touching the outer coating. (See 34.)

(35.) The diminution of the excitement probably takes place by discharge (the exact nature of which remains to be discovered). This must take place, not only on the excited side,

but all over. Now remark that the discharge of the electric lines that takes place on the excited side is the total of each line discharged; but the discharge that takes place on the side opposite the excited side is the discharge of lines that pass through the cake, so it must leave undischarged that part of each line that lies between the two surfaces, having negative root on $\alpha$ and positive on $\beta$. A charged magic pane without coatings is thus gradually formed and gradually also discharged. It is the engaged lines becoming gradually disengaged that at last brings out the positive electricity on the surface opposite to that originally negatively excited.

(37.) The inductive effect on a wax surface is very marked in this and other experiments. If a B. D. charged negative is placed resting on a cake of sulphur, it induces gradually a persistent positive on that part of the surface of the sulphur upon which it rests. Time is required. (See 39, 40.)

(38.) When first discharged this double cake has positive on the whole of the outside surface, top, bottom, and sides, which is completely engaged by the negative on the two interior surfaces. No free charge is required to hold the engaged lines together; the condition is similar to the concentric spheres of 52.

(41, 42.) The appearance of positive on $\beta$ depends on a partial discharge having spontaneously taken place during the elapsed time, as in 35.

(43.) The lower cake positive by induction; the upper by partial discharge of a few of the negative lines that pass upwards through the three cakes. It is remarkable that the inductive power is limited in its action to the surface in contact with the surface excited.

(44.) In this, as in 36, the surface in contact with the excited surface appears to share, to a small extent, its excitement, as if conduction had actually followed very slowly behind induction.

(45.) The permanent effect on the cylinder was similar to the temporary effect on a conducting cylinder of the same size, the charged B. D. being supposed close to but not touching it.

(47.) This is a very instructive experiment; there was no actual loss of charge, only an apparent loss, so long as the cake was on the electroscope and the contact surface inductively excited.

(53.) The large radius of the hoop appendage that characterises Winter's electric machine gives slow divergence to the electric lines that issue from it. (See 53.) This gives them, as part of the system of electrical lines that includes the lines between the spark balls, great power of lateral compression upon them previous to the spark.

The thunder-cloud as a charged surface is an extreme example of the spark-producing power of slowly converging electric lines.

(58 *a* and 58 *c*.) The distribution computed from 58 *a* subjected to the equation in 58 *c*, ought to stand the test. In an ellipsoid or spheroid it might be practicable to execute the calculation, and thus obtain further confirmation of the law of mutual dependence of the lateral repulsive and root-pulling or contractile force.

In conclusion, I may mention that the theory of electric lines here given was deduced from Harris's experiments about twelve years ago; since which time I have been in the habit of applying it to the published results of experimentalists, and thus continually testing it. It is very suggestive of new experiments. Some of the simpler sort of these I have been able to make, but there are others, chiefly with respect to the production of light and mechanical effect, that require greater means and appliances, not to mention aptitude; for to suggest and to execute are specialities that do not always go together. The main end and purpose of these would be to obtain some idea as to the working arrangements between the æther—that higher potential form of matter in which the might of the Infinite resides—and ordinary molecules, the agents of its development. If we confine our attention to the planetary movements, nothing seems clearer than that its density must be inappreciable. On the other hand, were we to make legitimate inferences from the most obvious phenomena of radiant heat, there is evidence that its density may not differ much from that of water, and at least that it is quite impossible that its non-resistance to the celestial motions can be owing to its extreme rarity.

EDINBURGH, 4*th December* 1864.

# ON SOME ELECTRICAL EXPERIMENTS

(From the *Philosophical Magazine* for May 1865.)

In § 30 of the paper on Electric Experiments, I have inadvertently stated that the leaves of the electroscope diverged with negative electricity, instead of positive. The experiment was meant to direct attention to a simple mode of proving, by means of a Leyden jar and electroscope, that the force of electric induction is capable of acting in *curved lines*, a fact which appears to deserve that distinct announcement due to a fundamental characteristic of the electric force.

Suppose we take a coated plate of glass properly insulated, and charge it highly with positive electricity on the side A, and negative on the opposite side B. After leaving it in this charged state for a few seconds, let an electroscope be brought with its cap near the centre of A, but not touching it: the leaves are found to diverge with positive in consequence of the free positive on the outer side of coating A acting inductively on the cap of electroscope, exciting upon it negative, and on the gold-leaves a corresponding amount of positive. In the same way, if we remove the electroscope from near A and place it on the opposite side close to centre of B, the leaves will diverge with negative from the action of the free negative on the outer side of B.

While the electroscope is thus stationed close to B, let A be touched: the leaves increase their divergence, showing that the free negative on the outer side of B has received an augmentation. The electroscope is then to be immediately removed to the opposite side, and the cap made to touch the centre of coating A. The leaves do not open, there is no longer electric excitement on the *outer* side of A: it has been discharged by touching. If we gradually remove the electroscope further away from the plate, but still keep it opposite

the centre of A, the leaves diverge with *negative* more and more until a certain maximum is attained, after which they gradually sink as the distance is further increased. The inductive action cannot come from any part of the adjacent coating A, which is altogether positive; it must therefore be supplied from B, the free negative on the outer side of which thus acts not only on the B side of the plate, but on the A side also, *to attain which it must pass round the edges of the coating* A.

Faraday supposes this to take place by means of the action of contiguous particles of air; and hence it is to be presumed that it is his belief that a charged pane could not exist in an illimitable vacuum, or at least that the electricity on B could not affect an electroscope on the side of A, because there would be no contiguous particles to transmit the force.

This hypothesis has been supposed to be further supported by the apparent augmentation in the force of induction that takes place when a plate of lac or sulphur is interposed between the charged body and a neighbouring conductor; and the term *specific inductive capacity*, applied to denote the degree of the special faculty of transmitting the inductive power belonging to the particles of a given non-conducting body, seems to be now established, Harris having published experiments as supporting Faraday's views, and teaching them in his elementary treatise.

As this term appears to me inapplicable to matter in relation to electric phenomena, I beg to submit the following experiment as demonstrating the non-existence of such a quality.

An insulated brass disk (the B. D. of previous experiments) is suspended over the cap of the electroscope at the distance of 2 inches above it. A cake of sulphur, 4 inches in diameter and $\frac{1}{2}$ an inch thick, is provided with handles of sealing-wax projected about 10 inches from each side. It is examined to see if there is any excitement upon A; and if any, discharge is affected by presenting it to a flame. The B. D. is charged from an electrophorus so that the leaves of the electroscope diverge to about 60°. The cake of sulphur, supported by its handles (which enable the hands of the operator to be kept at such a distance as not to influence the electroscope), is

brought horizontally into the space between the B. D. and cap of the electroscope. It is then moved parallel up and down; now close to the cap, then close to B. D. When close to B. D., the leaves are not at a higher angle than before its introduction; but on bringing it down towards the cap the angle increases a little, and when quite close to it the angle increases considerably, and this notable augmentation takes place within a distance of about $\frac{1}{4}$ of an inch from the cap. The experiment requires to be promptly performed, because the sulphur soon becomes inductively excited in the manner peculiar to non-conductors and as, to a certain degree, developed in the previous experiments. On first bringing the cake between the B. D. and the electroscope, the leaves descend a little just as the edge of the cake passes in between the outer edges of the B. D. and cap. Next let us take a metallic disk of the same diameter as the cake, and having also applied similar insulating handles, introduce it and move it up and down between the B. D. and cap without touching either. The leaves of the electroscope will be found to be acted upon in the same way as before, but in a greater degree.

I submit that if the sulphur had a greater capacity than air of transmitting induction, the leaves of the electroscope ought to rise higher when the cake is close to the charged B. D. than when it is close to the cap of the electroscope. In the latter case the increase of the angle of the gold-leaves was fully as great as when the electroscope was brought half an inch nearer to the B. D. In the former case no increase of induction was apparent.

The same experiment, repeated with sealing-wax and gutta-percha plates, showed similar results, but more striking. When close to B. D. the inductive force was less than through air; when close to the cap it was greater. [Gutta-percha shows some curious abnormal effects, especially when heated and soft. Although it is then still a perfect non-conductor, it seems to absorb the inductive force like a sponge.] A change in the shape or size of the non-conducting plates modifies the result, exactly in the same manner as the conducting plates, though always in a less degree. The proper way of studying the subject would therefore seem to be to experiment with conduct-

ing and non-conducting plates of the same size, and guard against the effect of induction upon the non-conducting surfaces, which seems to be a phenomenon of a special kind, that stands in need of further elucidation. I have not been able to arrive at any distinct law of action.

The transition from one kind of induction to the other may be conveniently studied in a warm room during dry weather, by placing a shade of thin glass over the electroscope, of such size that the round top may be about an inch above the cap, the agent being an insulated brass disk (the B. D.) charged inductively from an excited plate of sulphur. If we moisten the outside of the shade with a sponge and bring the excited B. D. close to the top, but not touching, there is of course no effect on the electroscope, the surface is opaque to the electric force; the induction is confined to the aqueous surface opposite the B. D. As the shade dries we observe a slight effect on the gold-leaves, which becomes more and more apparent as the glass resumes the dielectric condition. Let the shade be now heated before a clear fire, and replaced, when cool, over the electroscope. We may now study the effect of moving the excited B. D. to and from the top of the shade. If the action is performed without pause, the leaves open and shut in the regular normal way; but if the B. D., when close to the top of the shade, is kept in position for a few seconds, the leaves sink a little (showing a decrement $\Delta$ of the inductive force acting on the electroscope, although there is no decrement of the charge on B. D.); and on removing it they shut and then open with the opposite electricity, and the extent of the opening corresponds to the decrement $\Delta$, thus showing that the top of the glass shade has been inductively excited by B. D. But this excitement is evanescent, the leaves subside in a few seconds. The explanation of such phenomena that first presents itself is, that the outer surface of the shade is a partial conductor; but if this were the case, how is it that on bringing the B. D. into contact with the glass shade it does not lose charge, although its intensity is much greater than the induced excitement which so quickly passes away? The lesser intensity cannot be conducted more easily than the greater. Insulating the glass shade, by resting it upon sulphur, makes no difference.

It appears to be a constrained local polarisation, in which the work-equivalent of the charge is partially expended in effecting a change of molecular condition; but there is no separation of the electricities accompanied with conduction, as takes place on a truly conducting metallic surface subject to the inductive influence of a charged body.

Faraday's view of static induction has led him to give a definition of disruptive discharge, which I think is also unfortunate as tending to close further inquiry in the very direction most likely to lead to important discovery in the dynamical conditions affecting the relations of molecules to the higher potential medium. In recent educational works it is mentioned, not as a conjecture, but as an established fact, that "the particles of the intervening dielectric *are* brought up to a highly polarised state, until at length the tension on one particle rising higher than the rest, and exceeding that which it can sustain, it breaks down; the balance of induction is thus destroyed, and the discharge is completed in the line of least resistance." In my tenth and eleventh experiments it may be remarked that the disruption that accompanies discharge is like what is caused by mechanical impulse of matter pressing with sudden and short-lived force. If this idea is kept in view and arrangements made in accordance with it, experiments on the discharge of high-tension electricity would, I believe, assume a new and exciting interest, and lead, if well planned, to a deeper insight into the arrangement of force in nature than we could otherwise hope for.

EDINBURGH, 20*th March* 1865.

# ON THE THERMAL ACTION IN THE WIRE OF A VOLTAIC CIRCUIT AS INDICATING THAT THE CURRENT-FORCE OPERATES ON THE SURFACE OF A METALLIC CONDUCTOR ONLY, AND NOT THROUGH ITS SUBSTANCE

(From the *Philosophical Magazine and Journal of Science*, July to December 1865.)

While engaged on the subject of electric conduction, I have endeavoured in vain to discover if there is any experimental evidence on record of what seems generally taken for granted —viz., that in the voltaic circle electricity circulates in the mass of the wire through every part of its transverse section, and is not confined, as with common electricity, to the surface only. Professor Ohm's formula, however excellent as a guide in practice, is only empirical; and although one of the terms is the transverse section of the wire, the accuracy with which it applies to the various conditions of a circuit is no proof of the electric current acting directly upon the interior of the wire. Heat simply is produced in the wire, which may or may not be first wholly engendered upon its surface. If on the surface alone, conduction would immediately convey it into the interior; so that there is absolutely nothing to indicate the exact *locale* of the primary thermal action, whether it takes place on the surface alone, or whether in the interior alone, or in both.

Barlow and Harris have proved that the power of magnets resides on their surface, and, of course, inductively in the space exterior to that surface. What if this should also be the case with the current-force in a submarine cable? would it not lead to the adoption of flat conductors? would not a

strip of silver-leaf or copper-foil $\frac{1}{16}$th of an inch in breadth conduct as well as the wires of the Atlantic cable? Theoretical considerations lead me to believe that it would, and that with a strip half an inch in breadth we should have a conductor equal to a wire of nearly half an inch diameter. Ample surface of conduction with corresponding low electromotive force (or, as it may otherwise be expressed, low *electric pressure*) seem to be essential to the permanent success of long submarine cables; and as strength of core is not required, it seems to me that the riband form of conductor might well deserve a trial by those practically interested in the question.

The calorific effects of discharges of static electricity and of dynamic or voltaic electricity through wires of various gauges have both been fully examined experimentally. It will not be disputed that in the former the primary thermal action takes place only on the surface of the metal. Now the law of this with reference to wires of different diameters has been deduced from Professor Riess's experiments, and is thus stated at page 224 of the second volume of De la Rive's Treatise: — "We may therefore consider the following law as being well established: *When the same quantity of electricity completely discharged in the same interval of time traverses wires of the same nature but of different diameters, each wire experiences an elevation of temperature independent of its length, and inversely proportional to the fourth power of its radius.*"

The quantity of material in an inch of the thicker wire is greater than that of an inch in length of the thinner in the ratio of the square of radius; so that if the rise of temperature is inversely proportional to the fourth power of diameter or radius, the heat force engendered in an inch length of the wire is inversely proportional to the square of the diameter; and if the elimination takes place on the surface of the wire, *the force engendered on a square inch of that surface is inversely proportional to the cube of the diameter.*

Assuming that the heat evolved in the wire of a voltaic circuit is also produced on its surface only, we may compute

from Ohm's formula the ratio between the diameter of the wires and the heat evolved on a square inch of their surfaces respectively. I have made this calculation (see the appended note), and find that the ratio is *precisely the same as the above.*

It seems difficult after this to believe that there can be any difference in the *modus operandi:* if it is the surface alone that is primarily affected in one case, it must be the same in the other.

INVERNESS, 22*nd July* 1865.

*Note as to the Heat-generating Faculty in the Voltaic Circuit.*

W = heat-work performed in constant element of time.
E = electromotive power of battery.

Assume that the resistance in the metallic part of the circuit is incomparably greater than in the liquid portion.

C = conducting-power of circuit $= \frac{d^2}{l}$, in which $d$ is diameter and $l$ length of wire.

$$W = EC = E\frac{d^2}{l}.$$

Suppose W and E to be invariable, and the circuit to be composed first of a wire whose surface is $s_1$, diameter $d_1$, and length $l_1$, and afterwards of a wire whose surface is $s_2$, diameter $d_2$, and length $l_2$.

$$C = \frac{{d_1}^2}{l_1} = \frac{{d_2}^2}{l_2}\text{; hence } l_2 = l_1\frac{{d_2}^2}{{d_1}^2}.$$

To find the ratio of $s_1$ to $s_2$, we have $s_1 = \pi d_1 l_1$ and $s_2 = \pi d_2 l_2 = \pi l_1 \frac{{d_2}^3}{{d_1}^2}$; hence

$$s_1 : s_2 :: \pi l_1 d_1 : \pi l_1 \frac{{d_2}^3}{{d_1}^2} :: d_1 : \frac{{d_2}^3}{{d_1}^2} :: {d_1}^3 : {d_2}^3.$$

If the invariable work W of the circuit or heat force is engendered on the surface of the wire in each case, it is obvious that the amount engendered on a square inch of $s_1$ is to the amount engendered on a square inch of $s_2$ as ${d_2}^3 : {d_1}^3$, that is, in *the inverse ratio of the cube of the diameter,*

$$\frac{W}{s_1} : \frac{W}{s_2} :: \frac{1}{{d_1}^3} : \frac{1}{{d_2}^3}.$$

# ON ELECTRIC CONDUCTION, AND THE POSSIBILITY OF CURING THE RETARDATION OF ELECTRIC WAVES IN SUBMARINE TELEGRAPH LINES.

(From the *Philosophical Magazine* for December 1865.)

In the September Number of the *Philosophical Magazine* I endeavoured to show that the thermal action in the metallic part of the voltaic circuit took place on the surface only, and not through the substance of the conductor. I took for granted that there was no question as to this being the case in the conduction of common electricity, since all are agreed that, as charge, it exists only on the external surface of conducting-matter.

It appears I have been too hasty in assuming this to be an established point. In the evidence before the committee on the Atlantic cable, an experienced electrician holds that common electricity is conducted through the substance of the wire, the same as galvanic (Report, Appendix II., p. 327, § 208); and this is more distinctly affirmed in a class-book on chemical physics, which there is little doubt represents what is generally held and taught by professors.

At p. 459, § 276, of Dr Miller's *Chemical Physics*, the process of conduction is described in the following words:— "In all cases where electricity is in motion, whether it be exerted by chemical action as in the voltaic pile, or by friction as in the common electrical machine, the force *is conveyed by the entire thickness of the conductor;* the charge is not confined to the surface, as occurs when the power is stationary, and produces effects by induction only." This is a distinct statement; but the evidence upon which so important a point may be supposed to rest is not given, nor is any reference made

to where it may be found. A little further on (p. 461) a figure is given of the apparatus used by M. E. Becquerel in measuring the conducting-power of wires of different metals for voltaic electricity. If by means of such an apparatus we find that *an iron wire plated with copper conducts the same as a copper wire of the same diameter, and that a copper wire plated with tin conducts no better than a tin wire of the same diameter*, we may at once conclude by such *crucial experiment* that voltaic electric conduction is a surface phenomenon; and *e.g.* in designing a submarine telegraph-cable with its strength in the core, we may be convinced of the propriety of rather having a steel wire coated with copper than, as in Allan's system, a copper central wire with steel wires enclosing it.

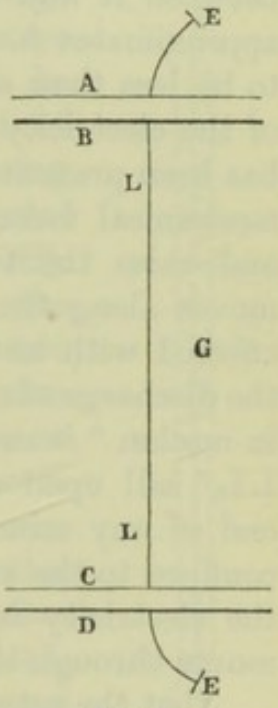


Fig. 1.

That common electric conduction is a surface phenomenon may, I think, be held to be a direct consequence of its existing only on the outer surface of a conductor when in its normal static condition of charge. Thus let A, B, C, D, fig. 1, be four metallic plates parallel and equal to each other. Let A and D be connected to earth or by return wire, and let B C be joined by a long metallic cylinder B L L C. Let there be interposed between A and B a thin dielectric plate, and another between C and D.

Let B and C be equidistant from A and D respectively, and the whole B L L C charged with positive electricity. The greater part of the charge will be located on the sides of B and C that are adjacent to A and D, being inductively retained there as in the magic pane. Let this charged system B L L C be supposed to move closer to A, the surface B draws nearer to A, and simultaneously the surface C withdraws from D (the parallelism being strictly maintained). The attractive force between B and A now becomes greater than between C and D. Also the quantity of electricity engaged between B and A becomes greater than between C and D. Electricity leaves C and travels to B; and this takes place more and more

as B gets closer to A. Thus we have a current of positive electricity travelling from C to B and conducted by means of L L. If the motion of the system is towards D, a positive current travels back from B to C. Thus a current may be made to flow in either direction by mechanical force applied to the system, and it may be made to flow with greater or less speed according as the movement is made more or less promptly.

Let an electroscope be placed at G: the leaves will show maximum divergence when the system is exactly in the middle between A and D, and will close as C approximates D or B approximates A. We may assume the distance between B and A to be less than any given magnitude, in which case the tension of the electricity becomes indefinitely small (although discharge has been prevented by the interposed dielectric). By applying mechanical force and expending work, we separate the plates and raise the tension of the electricity, which consequently moves along the conductor; and this may be supposed to be effected with an indefinitely great quickness, so as to represent the discharge of a Leyden jar. Those who affirm that electricity in motion "*is conveyed by the entire thickness of the conductor* L L," call upon us to believe that, while at the beginning and end of any minute interval in the above motion the charge is confined to the surface, yet during the minute interval in which the electricity is in motion it burrows below the surface and moves through the body of the conductor.

That the actual *potentiality* of the electric force resides, not upon the charged surface, but in the space adjacent and exterior to it, has, I think, been experimentally established (see § 54, "Electrical Experiments," *Phil. Mag.*, February 1865). That such is also the case with the electricity of the galvanic current we have some proof in the phenomena of induced currents. It is true that a current of this kind (M), while the circuit is closed, does not induce a current in an adjacent wire circuit (N) at rest, but the inductive influence is manifested when N approaches or recedes from M; therefore that very potential influence emanating from M must exist in the space occupied by N before N begins to move, and before the inductive current consequent to that motion makes its appearance. That change of distance between N and M should be accompanied by change

of electric tension in N is a direct consequence of electricity being an emanative force. That change of tension accompanied with transference of dynamic or work force is the primary condition of a current we may clearly recognise in the example, fig. 1. The difference of tension in a current giving rise to a gradient of tension has been experimentally established in a happy manner by Mr Latimer Clark (*Atlantic Cable Report*, Appendix II., §§ 23, 25).

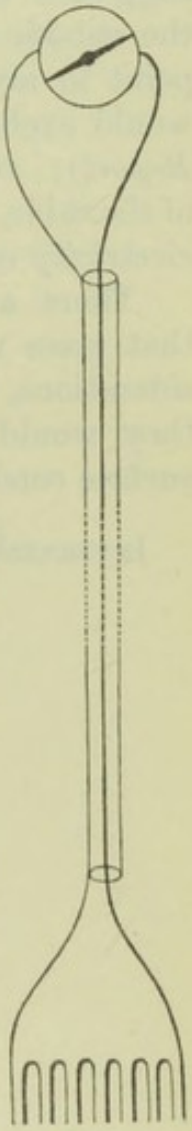


Fig. 2.

Should the crucial experiment above suggested prove that conduction in metal conductors is a surface phenomenon, I think it will be found possible to avoid the retarding effect of induction in submarine cables. The mere covering of gutta-percha upon a wire does not cause retardation (*Atlantic Cable Report*, Appendix II., § 69). The presence of a conducting medium on the outside of covered wire is necessary. Hitherto experiments seem to have been confined to iron and water as the outside conductors. So far as I know, the effect of copper as outside conductor has not yet been tried. Iron has one-sixth or one-seventh the conducting-power of copper; water only about 1000 millionth. Electric conduction as a whole depends on the freedom of motion allowed to the force as a whole. *The induced current should thus have the same freedom to move as the primary.*

Practical considerations would probably require that the outside copper conductor should be insulated; so that, to make the trial effectively, it would be necessary to connect the outside coating (of woven thin copper wire?) with one pole of the battery, while the other pole led to the central wire. At the signal end, of course, a similar arrangement (fig. 2). This would give to the induced current the same propelling force as the primary, and utilise the half of the power of the battery that is now lost by putting to earth. That putting to earth is discharge of half the force of the current is an

obvious deduction from theory (see *Phil. Mag.* for March 1865), but it has also been experimentally proved by Wheatstone (*Atlantic Cable Report*, p. 292, Exp. 5). At p. 103, § 30, *Atlantic Cable Report*, a similar arrangement to fig. 2 is suggested by Mr C. V. Walker; but the necessity of having the outside and inside conductor of the same metal—the vital point in my opinion—is not insisted upon. This arrangement would exclude earth-currents (§§ 21, 25, p. 102, *Atlantic Cable Report*); and if the signal-house, as well as the shore end of the cable, be iron-covered, it is difficult to see how atmospheric electricity could intrude upon the signals.

There are other considerations with respect to conduction that press upon the attenion as suggested by theoretical considerations, but it would be presuming too much to expect they would receive attention before the fundamental point of surface conduction has been experimentally established.

INVERNESS, 20*th November* 1865.

# V

# ASTRONOMICAL PAPERS

# ON FINDING THE LATITUDE, TIME, AND AZIMUTH

(From the *Nautical Magazine*, July 1842.)

[We have received the following solution of the Double Altitude problem from Mr Waterston, naval instructor at Bombay, which, although not likely to supersede the method in use at sea, has peculiar recommendations to the attention of the scientific navigator. For simply obtaining the latitude, it has far too many logarithms to come into general use, while there are so many other means by day and night of getting it. But the author might in some degree have reduced this redundance of figures, by taking out the sine of the elapsed time (in time) in Raper's tables, instead of resorting to the old method of converting it into space for that purpose. Again, he might have avoided the clumsy alternative of halving his log. of arc V, for the sine, and then doubling the quantity for that arc, by simply taking out the arc itself from the first log. in Raper's table of log. sine square, which as neatness and brevity are so much required in these matters, we may recommend to Mr Waterston's especial attention. The rule alluded to in White's Ephemeris, we may also observe is not new, having been given long ago.]

The latitude, time, and azimuth by one computation from two altitudes of the sun, or of a star, taken at an interval of from $15^{m.}$ to $10^{h.}$ at any time of the day or night, may be found by the following method.

The method is a rigorous solution of the problem without the error involved by taking the sun's declination for the middle time. Although apparently tedious, it will be found, I think, fully as easy to compute as the rules in common use. It appears also to be better adapted to short intervals of time.

Under favourable circumstances a good result may be obtained with an interval of from 10 to 15 min. if the time, from apparent noon, does not exceed the meridian zenith distance converted into time.

In this as in all other methods of finding the latitude by a double altitude, an error in the difference of altitudes when the elapsed time is small, causes a much greater error than if it affected both equally. An error of this kind affects the latitude least, and the hour angle most when the two altitudes are taken near noon, and *vice versâ*.

By this method the *time* as well as the latitude may be obtained at or near noon, *with fully as much precision as in the morning*,* and quite independent of the dead-reckoning.

If the greater altitude is in the meridian, the latitude may be found by this method, and by the meridian altitude, which will effectually check the error of the hour angle.

Thus, or even without a meridian altitude, the latitude and longitude for the day may be determined at or near noon, if the sun is obscured in the morning or afternoon; and on the other hand, both latitude and longitude may be determined early in the morning, if it is an object to near the land before noon.

As a general rule, it may be remarked that the time from noon of the lesser altitude ought not to exceed four or five times the meridian zenith distance converted into time; and when the interval does not exceed 15 min., the time from noon ought not to exceed one and a half times the meridian zenith distance converted into time.†

The mathematical elements of the rule (which is rigorous except the small correction applied to arc A for the change of declination, and which will not be found to differ sensibly from a strict trigonometric solution,) depend on the projection of the several points of the sphere on the tangent plane at the

* Having taken several sets of altitudes with sea horizon within half an hour of noon, and each set with an interval of about twenty minutes, I find on computing the error of the watch at the greater altitude of each set, that all the results are within 2 s. of the truth, the meridian zenith distance being 20°.

† The time required to perform the whole computation for latitude, time, and azimuth, is not so great as that for three chronometers.

sun's place, when the greater altitude was observed. The parallel of declination is projected into an ellipse, and the line that joins the two positions of the sun (being the chord of an elliptic arch, of which the major axis is twice the *cos.* of declination, and the minor axis to twice the *cos.* × *sin.* declination) is deflected from the perpendicular to the meridian that passes through the tangent point. The amount of this deflection is arc A. The length of this line is equal to the *sin.* of the arc of a great circle that measures the distance between the two positions of the sun; this is arc B. The angle formed by this arc, and the zenith distance of greater altitude is arc V, which is found by the common rule, and has the advantage of indicating the position of the meridian exactly without ambiguity. By applying corrected arc A to V, and taking the complement, we find arc C which is the angle of variation included by the polar and zenith arcs of the sun at a greater altitude. The last part computes the latitude, hour angle, and azimuth at one operation, and will be found an expeditious form of solving a spherical triangle, of which two sides and their included angle are given, if the three unknown parts are not within 7° or 8° of 90°.*

*Rule.*—Set down distinctly the times of observation, the true altitudes, the latitude by account, and the declination reduced to the time of the greater altitude. Note the hourly difference of declination from the *Nautical Almanac*, which call miles, and divide by 16, if the elapsed time does not exceed 1½ hours; the quotient call the correction of arc A. (When the elapsed time is $2^{h.}$ divide by 15—$3^{h.}$, 14—$4^{h.}$, 13—$5^{h.}$, 11—$6^{h.}$, and 10—$8^{h.}$).

To the *tan.* of half the elapsed time converted into degrees add the *sin.* of the sun's declination: the sum is *tan.* of arc A, to which *add* the correction, if the *declination at the lesser altitude is the greater*, if *otherwise subtract.* Add the *secant*

* To find the correction of arc A exactly:—Take the arithmetical complement of the sum of the *sect.* of A and *sin.* B, and add it to *sin.* of the increment of the declination, during the elapsed time the sum is *sin.* of correction to be applied as previously directed. It will be found that this can be done without taking out any additional logs., and by inspection, as only two figures of the log. require to be computed.

of arc A to the *sin.* of elapsed time in degrees and *cos.* of declination; their sum is the *sin.* of arc B.

Arrange the lesser true altitude arc B, and greater true altitude, as altitude, polar distance, and latitude respectively in the usual form for computing the time from an altitude, and find the hour angle in degrees which call V.

EXAMPLE I.

Feb. 21st, 1842. Lat. 18° 55′ N. Long. 4h. 52m. E.

| Times P.M. | | True Altitudes. | | |
|---|---|---|---|---|
| 3h. | 42m. 16s. | 32° | 35·3′ | |
| 4 | 16 25½ | 25 | 14·3 | |
| | 34 9½ | | | |

Decl. 10° 36′ S. Hourly diff. 55″
55′ ÷ 16 = 3·4′ = correction; to be *subtracted* from arc A, because the declination is the *lesser* at the lesser altitude.

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| El. Time 2)8° | 32·3 | ..... | sin 9·17164 | | | | | |
| Half do. | 4 | 16·1 | tan 8·87294 | | | | | |
| Declin. | 10 | 36 S. | sin 9·26470 | cs.*9·99252 | | | | |
| | | | | ·00004 | Lesser alt. | 25° | 14·3′ | |
| Arc A | 0 | 47·2 | tan 8·13764 | | sin arc B | 8 | 23·8 | cst. ·83580 |
| Correc. | | − 3·4 | | 9·16420 | Grtr. alt. | 32 | 35·3 | sct. ·07439 |
| Cor. arc A | 0 | 43·8 | | | 2)66 | | 13·4 | |
| | | | | | 33 | | 6·7 | cos 9·92304 |
| Arc C | 59° | 16·2′ | sin*9·93428 | cos 9·70841 | 7 | | 52·4 | sin 9·13666 |
| Grtr. alt. | 32 | 35·3 | cos 9·92561 | cos 9·92561 | | | | |
| | | | | | 75 | | 0 | 2)19·96989 |
| Arc D | ..... | | sin 9·85989 | sec ·16144 | | | 2 | |
| | | | | | | | | sin 9·98494 |
| ,, E | 38 | 38‡N. | ..... | sin 9·79546(—) | Arc V | 150 | 0 | |
| | | | | | | | − 43·8 | Arc A corrected to be *subtracted* from V because the sun is *not* betweenthe zenith and equator. |
| ,, G | 28 | 2‡N. | ..... | sin 9·67214 | | | | |
| Lat. | 18 | 55 N. | sec* ·02411 | sin 9·51070 | Arc W | 149 | 16·2 | |
| | | | | | ,, C | 59 | 16·2 | |
| Hour angle | ..... | | sin 9·88400 | 3h 19m 50s | | | | |

Az. * * * sin 9·95091 ..... 63° 16′ or sun's true bearing S. 63° 16′ W.

Add the *corrected* arc A to V (if the sun is between the zenith and equator; if otherwise *subtract,*) and call the *complement* of the sum or difference, arc C. Take out the *sin.* and *cos.* of arc C; to its *sin.* add *cos.* of greater altitude, the sum is *sin.* of arc D, the *sect.* of which is to be added to *cos.* of greater altitude, and *cos.* of arc C, the sum is the *sin.* of arc E, which is of the same name as the declination, if the sun is between the zenith and equator, if not so, it is of a different name, and their

sum or difference is taken for arc G, which has the name of the greater. From the *sin.* of arc G subtract *sect.* of arc D, the remainder is the *sin.* of true latitude at the time of taking the greater altitude.

Add the *sect.* of latitude to *sin.* of arc E, the sum is *sin.* of hour angle at the time of taking the greater altitude. If the *sec.* of latitude, *sin.* of arc C, and *cos.* of declination be added together, the sum is *sin.* of the azimuth at the time of taking the greater altitude.

EXAMPLE II.

March 6th, 1842.

Lat. 18° 55′ N. | 10h. 37m. 19s. } A.M. 55° 49·5′ } True 5° 48′ S. decl.
Long. 4h. 52m. E. | 10 46 20 } A.M. 57 19 } Alt. Hrly diff. 58 ÷ 16 = 3·6′

| | | | | | |
|---|---|---|---|---|---|
| Elapsed Time | 9m. 1s. | | 1 29·5 | | |
| Do. in degrees | 2° 15‡ | | 89·5 | log | 1·95182 |
| | | Decl. | 5 48 | sec | 10·00223 |
| | | | | | 11·95405 |
| Do. in miles | 135·25 | | | log | 2·13114 |
| Arc C | | 41° 41·6′ | | sin | 9·82291 |
| Correction | | −3·6 | | | |
| Corrected arc C | | 41 38 | sin | 9·82240 | cos 9·87356 |
| Mean alt. | | 56 34·2 | cos | 9·74109 | cos 9·74109 |
| | | | sin | 9·56349 | sec. ·03123 |
| | | 26 15·6 N. | | ..... | sin 9·64588 (—) |
| | | 5 48 S. | | | |
| | | 20 27·6 | | ..... | sin 9·54350 |
| Latitude | | 18 59 N. | sec. | ·02429 | sin 9·51227 |
| Hour angle | 1h. 31m. 5s. | | sin | 9·58778 | |

*N.B.*—Take the arithmetical complement of *sin.* B for its *cosect.* Take out *sin.* and *cos.* of declination at once; also *cos.* and *sect.* of greater altitude.

If a star is observed the elapsed time must be *increased* at the rate of $10^{s.}$ per hour.

If the ship has made any way during the interval, the lesser altitude must be reduced in the usual way to the place of the greater.

When arc W is less than 90° it indicates that one observation was taken in the forenoon, the other in the afternoon. When both altitudes are A.M. or P.M. arc W is always greater than 90°,

When the elapsed time is between 6 and 7 hours, arc B may be acute or oblique. To determine which subtract the log. *cos.* of declination from log. *sin.* of declination (adding 10 to its index) if the remaider is *greater* than the log. sin. of arc A, arc B is *acute*; if it is *less* arc B is *obtuse.*

For this and other reasons an elapsed time of from $5^{h.}\ 40^{m.}$ to $6^{h.}\ 50^{m.}$ is to be avoided.

When the interval does not exceed 15 min. a very good approximation to log. sin. of arc C may be obtained by adding the log. sect. of declination to the common log. of the difference of altitude in miles and decimals, and from the sum subtracting the common log. of the elapsed time also in miles and decimals. In this case the mean altitude must be taken in the latter part of this computation instead of the greater altitude. When arc C is thus obtained, a correction ought to be applied to it for the change in declination, the rule for which is:—Take out the hourly difference of declination for the day from the *Nautical Almanac*, which consider as minutes of a degree, and divide by 16, the quotient is the correction of arc C. If the declination at the lesser altitude is the *greater*, *add* the correction to arc C, if the sun is between the zenith and equator; otherwise *subtract.* If the declination at the lesser altitude is the *lesser*, *subtract* the correction from arc C; if the sun is between the zenith and equator, otherwise *add.*

This is similar to the rule given by Mr Woolhouse, in the admirable nautical table recently published along with White's Ephemeris. A satisfactory result for latitude must not be expected from it, unless the time from noon is less than three-fourths the meridian zenith distance converted into time.

The above example computed by the first process gives $18°\ 55'$ N. latitude, and $1^{h.}\ 31^{m.}\ 17^{s.}$ hour angle, which was the actual latitude, and time otherwise known.

In another example the elapsed time being $34^{m.}$, hour angle $3^{h.}\ 40^{m.}$, and meridian zenith distance $29°$, the latitude and time was correct by 1st process. By the 2nd without correcting arc C the latitude was $8'$ in excess, and with the correction, $5'$ in excess. As this correction increases the error when it has to be subtracted, it will be perhaps best not to apply it, unless when additive.

*To find the Latitude and Time by the simultaneous altitudes of two stars.*

This is a modification of the preceding rule. Call the star with lesser polar distance $\alpha$, and that with the greater $\beta$. Set down in a distinct order, their altitudes, declinations, and diff. of R.A.

The first part of the computation is similar to the last part of example I. Put diff. R.A. as arc C, and the declination of $\alpha$ as the altitude: find by the formula arc E to which add the declination of $\beta$, when the difference of R.A. is less than $6^{h}$, and subtract when more. (To prevent ambiguity it will be best, when possible, to select the stars so that their diff. of R.A. may be less than $5\frac{1}{2}^{h}$, or more than $6\frac{1}{2}^{h}$. The distance found by the rule ought to be less than the polar distance of $\alpha$ in the first case, and greater in the other.) The arc that answers to the latitude in the result is the complement of the distance of the two stars, and the arc that answers to the hour angle call arc M.

The second part of the computation is the same as the common rule to find the time by an altitude.

The altitude of $\alpha$, the distance of $\alpha$ and $\beta$, and the altitude of $\beta$ are to be put in the place of the altitude, polar distance, and latitude respectively. Find the hour angle in the usual way which call arc N. To ascertain whether the sum or difference of arcs M and N is to be taken for arc P; suppose a thread to be stretched between the two stars and to extend indefinitely both ways; if the pole and zenith are on the *same* side of this line take the *difference* of M and N, if on *different* sides take the *sum* for arc P.

The last part of the computation is similar to the first. Put arc P in the place of C, and for the altitude take the altitude of $\beta$, and for the sun's declination take the declination of $\beta$, adding or subtracting it from arc E, according as it is between the zenith and equator, or otherwise. The resulting latitude and hour angle is the true latitude and distance of the meridian from $\beta$. The latter added or subtracted from the R.A. of $\beta$ according as it is west or east of the meridian will be the R.A. of the latter, from which subtract sun's R.A. to find apparent time of observation P.M.

Although more speculative than useful, it may be remarked that by the last rule the latitude and time may be computed by any two isolated altitudes of the sun, taken at an interval of from five days to five months. The time at each observation being known near enough to take out the R.A. and declination of the sun with ordinary exactness. The conditions of the problem are evidently the same as if simultaneous altitudes were taken of two stars having the respective positions occupied by the sun at the two times of observation.

BOMBAY, 14*th March* 1842.

# ON A GRAPHICAL METHOD OF PREDICTING OCCULTATIONS

[Communicated by Captain Beaufort, R.N.]

From the *Philosophical Magazine*, Oct. 1844. (*Proceedings of the Royal Astronomical Society.*)

The following is the author's account of his method:—

"The following is a description of a graphical method of predicting occultations, which I have found useful, in drawing the attention of some of my pupils to this valuable method of determining meridian distance.

The prediction of the time is almost an essential preliminary to the observation, and the computation required for this, in the usual way, is such as to put it altogether beyond the reach of many who would otherwise be perfectly able and willing to make the observation, and who have many valuable opportunities in the course of their profession of improving geography by such means. The method is simply delineating the essential points in the orthographical projection of the moon's motion, and that of the observer, in a plane perpendicular to the direction of the star. This is simplified to the utmost by scales, and I find that, with a little practice, my pupils have no difficulty in predicting the time of observation to 1 or $1\frac{1}{2}$ minute, and this without much care being taken in the drawing. From several occultations, which I predicted and observed here, and from numerous examples taken from the *Astronomical Society's Transactions*, I find that, with ordinary care, the predicted time may be depended upon within one minute, and that the time occupied in doing so, varies from 10 to 15 minutes. As this is sufficiently near for all the wants of the observer, it may, perhaps, serve to supersede the method of computation, which, in its simplest form, is an irksome task, and probably

tends to make occultations less frequently observed than they otherwise might be.

"With officers duly initiated, and observatories at work, observing all that are visible as a regular part of their duty, the most valuable results to geography might be anticipated.

"I have appended a rule for computing the moon's right ascension and the consequent error of assumed longitude, which is derived from the same method of orthographical projection. It is rigidly correct in principle (with the exception of taking small arcs instead of their sines, which in no case affects the result in any sensible degree), and the results of computation may be depended upon within a hundredth part of a second of right ascension, if the data are correct, and a considerable error in the estimated longitude, will, I think, be found to have as little injurious effect as in any other method of computation.

"BOMBAY, 1*st May* 1844."

# REMARKS ON THE COMET

*Accompanied by a Chart, marking its course among the Stars. Including also, Observations of the Comet, made at the Madras Observatory, by* T. G. Taylor, Esq.

[Communicated by Captain Beaufort, R.N.]

(From *Monthly Notices, Royal Astronomical Society*, Vol. vi. p. 207, 1845.)

"The new year has brought us a celestial visitor which, by its splendid appearance in our southern sky, reminds us of the great comet of March 1843. There is the same disproportion between the tail and nucleus, but it is altogether on a reduced scale, the tail being only 10° long when first seen, and the nucleus shining with the brightness of a star of the fifth magnitude. The great comet of 1843, when first seen here, was visible in the midst of bright twilight, half an hour after sunset. It shone with the dull, red light of an ignited coal, and the immense tail seemed like a dense white cloud, springing up from the horizon, sharply defined for about 20°, and in a few days it extended to 43°. After a few days, the head of the present comet could not be recognised by the naked eye, but the large tail continued to be a conspicuous object until the increasing moonlight overpowered it. After full-moon, it could still be recognised as a faint streak of light, until this evening when it was only visible through a telescope.

"In the accompanying chart, I have traced its motion with the assistance of the *Madras Catalogue of Southern Stars*, and made two attempts to compute the parabolic elements, but the positions seem too loosely fixed to obtain them with any accuracy. The second trial ought, nevertheless, to be an approximation, as it includes a sweep of twenty-one days, and the utmost error of the observations cannot exceed 5′.

"I have since been favoured by Mr Taylor with a copy

of his observations up to the 17th January, which I annex, but have not had time to compute an orbit from them.

*Observations of the Comet made at the Madras Observatory.**

| Day. | Madras Mean Time. | | | Right Ascension. | | | North Polar Distance. | | |
|---|---|---|---|---|---|---|---|---|---|
| 1845. | h. | m. | s. | h. | m. | s. | ° | ′ | ″ |
| Jan. 5 | 6 | 47 | 31 | 22 | 5 | 7 | 134 | 49 | 10 |
| 6 | 6 | 48 | 33 | 22 | 18 | 6 | 134 | 49 | 20 |
| 7 | 6 | 53 | 13 | 22 | 30 | 35 | 134 | 42 | 45 |
| 8 | 6 | 52 | 15 | 22 | 42 | 38 | 134 | 27 | 39 |
| 9 | 6 | 46 | 23 | 22 | 54 | 31 | 134 | 12 | 16 |
| 10 | 6 | 46 | 4 | 23 | 6 | 25 | 133 | 50 | 29 |
| 11 | 6 | 51 | 23 | 23 | 17 | 45 | 133 | 24 | 15 |
| 12 | 6 | 42 | 27 | 23 | 28 | 46 | 132 | 53 | 54 |
| 13 | 7 | 0 | 17 | 23 | 39 | 38 | 132 | 21 | 49 |
| 14 | 6 | 50 | 23 | 23 | 49 | 52 | 131 | 43 | 40 |
| 15 | 6 | 47 | 12 | 23 | 59 | 42 | 131 | 6 | 39 |
| 16 | 6 | 35 | 14 | 0 | 9 | 4 | 130 | 24 | 30 |
| 17 | 6 | 50 | 54 | 0 | 18 | 29 | 129 | 40 | 57 |

(Signed) T. G. Taylor.

* The irregularity of the differences of the North Polar Distance seems to denote that some of the results are erroneous, probably through errors of transcribing.—Sec.

"These supersede the chart; but it may still serve as a picture to show a singular luminous appendage, which, on the evening of the 16th, I observed for the first time to proceed from the head of the comet *towards* the sun almost diametrically opposite to the proper tail. It consisted of a narrow band of faint light of about the same breadth as the head. The edges were well defined and parallel. It could be traced for 3°, and probably extended much further, as the increasing moonlight was very unfavourable to so faint an object. Its direction and appearance, when first seen on the 16th, is shown on the chart. Another representation of it is given, on a large scale, as it appeared on the evening of the 25th. The two tails now made an evident angle, and the space was filled with a diffused, irregular light, giving a triangular shape to the comet when seen by the naked eye. The direction of the tails, in reference to the adjacent stars, has been carefully set down on the chart. This evening the same appearance

continues, but very faint, the angle at the head of the comet being about 140°. Mr Milne in his essay on comets, states, that in the comet of 1824, the same kind of double tail was observed. It would be interesting to know if there is any chance of their being the same. A newspaper correspondent here remarks, that there is a considerable resemblance in the elements (except the perihelion distance) to the comet of 1737 (Delambre's *Astronomy*). The comet of 1557, which is expected to return in 1848, is also not widely different in its elements."

BOMBAY, 31*st January* 1845.

# ON A DIRECT METHOD OF DETERMINING THE DISTANCE OF A COMET BY THREE OBSERVATIONS

(From *Monthly Notices, Royal Astronomical Society*, Vol. vii. 12th December 1845. No. 2, p. 17.)

The following is the author's explanation of his method:—

"It is well known that three observations of a comet afford sufficient data for computing its distance from the earth independently of any assumption as to the orbit in which it moves. The formula is, I believe, originally due to Lambert, and appears in the works of the principal mathematicians who have given analytical solutions of the problem by the differential method. It is unfortunate that the nature of the equation does not admit of much precision in the results of the calculation, which are consequently apt to be greatly affected by small errors of observation. The disturbing power of these unavoidable inaccuracies varies much according to the conditions of the problem, and it is, perhaps, impossible to recognise it in the analytical expression without a much greater effort of the attention than can be given when merely computing an orbit. With good observations the method has the advantage of revealing any obvious tendency to an ellipse or hyperbola; and, besides, it will in most cases afford a useful approximation to begin with in computing the parabolic formulæ. As to the expediency of putting the observations to this preliminary test in all cases, there would, perhaps, be little difference of opinion, if the labour of computation in doing so were available in the last part of the process, and if the conditions upon which the degree of accuracy depends could be easily distinguished.

"The object of this paper is to submit to the Astronomical Society an account of a method which has occurred to me of solving the equation by means of a constant curve, and to show how the preliminary calculation may be made available in

Olbers' parabolic method, and likewise in a differential method, without requiring the original equatorial position in either case to be transferred to the ecliptic. The conditions of accuracy also become so apparent in using this curve, that the effect of an error of right ascension or declination may be estimated by inspection.

"The method of solution is derived from the projection of the three observations on the plane perpendicular to the direction of the motion of the comet at the middle epoch. The earth's orbit being projected, its deflection, caused by the sun's attraction, is brought into view, and since its apparent direction is the same as that of the sun, and the projected direction of the sun from the comet is the same as at the earth, the radii vectores of both being identical on the projection, it is clear if the differentials at the middle time are alone considered that the deflection of the orbit of the comet, as it appears on the plane of projection, coincides in direction with the projected deflection of the earth's orbit and that its magnitude depends on a function of the angle at the comet. We thus obtain the means of forming an equation for the angle at the comet in terms of the deflection of the earth's orbit; and this equation, although derived from a simple geometrical construction, appears to be similar to that which is given in the analytical discussion of the problem by Laplace, Lagrange, Legendre, and Airy. It depends wholly on the effect of the sun's centripetal force during the elapsed time as it appears on the plane of projection; and, as this, in the short differential period of a few days, bears but a small proportion to the projection of its chord, or velocity, the results are much more liable to be affected by the unavoidable errors of observation than if the equation expressed the same unknown quantity in terms of the velocity. But in the last case we have to suppose the nature of the conic section known; in the first no assumption of the kind is required, the deflecting effect of the sun's force being necessarily the same in all orbits at the same central distance.

"The equation for the angle at the comet is solved by drawing one line on the constant curve, and the preliminary computation required to do so affords an expression for the ratio of the distances at the first and third observations on

the usual assumption that the chord is divided in the ratio of the times.

"This expression may be converted into the elegant form given by Olbers, so that it is identical with the value of M in his formulæ, and is expressed in terms that are likewise required in drawing the line on the constant curve.

"An example is given from the Trevandrum observations of the great comet of 1843. The formulæ are also applied to Göttinger's observations of the second comet of 1813. A copy of the constant curve is given upon a separate sheet, and the lines of these examples drawn. The co-ordinates of the curve consist of the cotangent and cube of the sine. It is easily constructed by the common tables. If drawn with ordinary care, it will give the reading of the angle at the comet to greater nicety than even the best observations can afford.

"I have appended a modification of Olbers' formulæ for the radii vectores and chord adapted to equatorial positions, and involving the use of the angular quantities already computed for the use of the constant curve. The additional work of computation does not appear to be so great as that which is required to convert the right ascension and declination into latitude and longitude; and, besides, it is easier to compare observations with the computed elements when the latter are referred to the equator. The inclination of the orbit and position of the nodes are transferred to the ecliptic by the solution of one spherical triangle.

"In the recent improvements which Olbers has made in his method, by expanding Euler's formula into a series and reversing, the means are afforded of constructing a small table which shortens considerably the process of finding the distance by trial and error. Another improvement consists in the new expression given for the chord being more favourable to accurate computation. I have included a form of the same kind in terms of the right ascension and declination which is almost wholly made up of angular quantities that have already been prepared and used with the constant curve.

"In the last part of the paper an expression for the angle at the comet is given to be used with the differential method, which, in solving by trial and error, requires only five tabular references."

# ON A GRAPHICAL MODE OF COMPUTING THE EXCENTRIC ANOMALY

(From *Monthly Notices, Royal Astronomical Society*, Vol. x., p. 169, 1850.)

The equation expressing the relation between excentricity ($e$), the mean anomaly $\phi$, and the difference between the mean and excentric anomalies $x$, being $x = e \sin(\phi + x)$, or $\sin(\phi + x) = \frac{\mathrm{I}}{e} x$, Mr Waterston graphically solves this equation by means of the curve of sines.

Having drawn this curve, the unknown ordinate is evidently a known multiple $\left(\frac{\mathrm{I}}{e}\right)$ of the difference between the known abscissa ($\phi$) and the unknown abscissa ($\phi + x$). Hence it is found by drawing a line from the extremity of $\phi$ inclined to the axis at an angle whose tangent is $\frac{\mathrm{I}}{e}$.

The accuracy with which the value of $\phi + x$ is determined will depend on the accuracy with which the curve is drawn, and also upon the scale to which it is drawn. If the radius be 8 inches it can with ease be read off to within 5 feet.

When a value of $x$ has been obtained from the scale, it can be corrected by computation. Thus, since

$$e = \frac{x}{\sin(\phi + x)},$$

if the approximate value of $x$, determined from the scale, be substituted, an error of $e$ will be obtained, and the corresponding error of $x$ will be found by differentiating the equation with regard to $x$.

The author shows also how this correction may be obtained by a simple construction, and how the method of graphical projection may be applied conveniently in extreme cases, when the mean anomaly is small and the excentricity great.

A diagram is annexed, exhibiting the curve and scales, with the construction for a given case.

# THOUGHTS ON THE FORMATION OF THE TAIL OF A COMET

(From *Monthly Notices, Royal Astronomical Society*, Vol. xix., p. 29, 1859.)

Any attempt to account for the formation of the tail of a comet upon exact physical principles would be assisted, if, in the drawings of the telescopic appearances of the nucleus and adjacent coma—which are sometimes provided when circumstances are favourable—we were presented with the projection on the plane of vision of the radius vector, and of the tangent to the orbit. In some cases it might be possible to take such observations as would fix the actual curve of the *exterior line* of the tail, referred to the radius vector, passing through the centre of the nucleus; *i.e.* supposing the central axis of the tail to be in the plane of the orbit. This line is very marked in the front side of Donati's comet, and might probably be easily fixed by the equatorial.

If we view the tail as composed of molecules as free from the force of cohesion as the molecules of an uncondensable gas, and raised from the nucleus by the heat of the sun; and that that heat as it strikes upon each molecule is converted into a force centrifugal, that not only effectually counteracts the force centripetal of the sun's gravity, but that greatly exceeds it; such molecules will be quickly removed from the feeble attraction of the nucleus, and assume the motion of bodies entirely free from its influence. This hypothesis is suggested by the mechanical theory as a possibility.

If we inquire as to the centrifugal power of the sun's rays, we may, with the data afforded by modern research, easily compute the accelerative force it is capable of engendering on a single chemical molecule, if their whole heating power were converted into such a force. This is surpassingly great—no less than 800 miles per second! The data are, value of sun's

radiation in a solar day equal to 1·8 feet thickness of ice melted; the mechanical equivalent of which is about 13 lb. raised 1 foot high per second by the heat impinging on a square foot. If this heat impinges on a superficial foot of gold-leaf, one molecule thick (about one 200-millionth of an inch, as deduced from the relation of *capillarity* to *latent heat*, both being the measure of liquid cohesion, the first that of the superficial stratum of molecules, the second that of a cubic mass of molecules), and is converted into an impulsive force in one direction, it would in one second communicate a velocity of 800 miles a second; and in about four minutes a velocity equal to light itself. It is, of course, an extreme case to suppose the whole heat absorbed and converted; but it seems right to have in view the quantitative elements of the question. While the earth's atmosphere absorbs heat from the sun's rays, we have reason to suppose that heat-vibrations are converted into rectilineal velocity of gaseous molecules; but there is no reason to suppose that the impulse is in the specific direction *from* the sun, but equally *from* and *to*, as in vibratory impulses generally. It may be shown that the mechanical equivalent of the solar heat that impinges on the earth amounts to one-hundredth of its gravity towards the sun; so that, if any part of it acted centrifugally, the orbit would be disturbed. The same remark applies to a comet; but it cannot be denied that the phenomena of the tail, more especially as it turns sharp round in the perihelion passage, are such as require for their explanation a very active exhibition of such a centrifugal force as might be engendered by the conversion of heat acting upon single and free molecules.

# AN ACCOUNT OF OBSERVATIONS ON SOLAR RADIATION

(Reprinted from the *Monthly Notices of the Royal Astronomical Society.*)

§ 1. In March of last year I submitted to the Society some computations with reference to the Sun's heat, and suggesting a mode of deducing the potential temperature of its radiating surface. This last summer I have endeavoured to put this method to the proof by a series of observations on solar radiation, supplemented with experiments on the rate of cooling of thermometers in air and in vacuo with different kinds of radiating surfaces; also by another series applying the method of deducing the potential temperature of a radiating surface to predicate the temperature of one that is maintained at a constant *known* temperature.

The success of these last mentioned, so far as they have as yet been carried, encourages me to lay before the Society the observations on solar radiation, with an account of the method employed to obtain the results, and of the mode of reducing them to a vacuum.

§ 2. It will be remarked, on inspecting the chart in which the observations are projected, that a simple law of atmospheric absorption is indicated, which, if confirmed by other similar observations in different climates, would perhaps lead to more exact ideas of the influence of the atmosphere on the Sun's rays. Unfortunately the best part of the summer had passed before I could begin to observe, and there was almost constant interruption with clouds and unsettled weather. In a tropical station, where the Sun rules in a cloudless sky, the presumed law might soon be put to the test, and the heating power of the Sun's rays before entering the atmosphere ascertained with precision. Having determined this for the Earth's mean distance from the Sun, its value for any other planetary distance may be deduced by the law of the inverse square.

§ 3. When a thermometer is exposed to the Sun with its bulb blackened, it is presumed to absorb all the heat that impinges on a plane surface equal to the transverse section of the bulb; it rises and is maintained at a certain temperature; and when this balanced condition is attained we can with certainty assert that the amount that issues from the bulb is precisely equal to the quantity that enters. The elevation of its temperature above surrounding bodies due to the Sun's radiant power (which is denoted by the symbol $r$) would be an exact measure of that power, if no heat issued from it except by radiation, and if the rate at which heat was emitted from it increased exactly in proportion to $r$. Now I find that if the bulb of a thermometer is enclosed in a vacuum, the walls of which are brass, coated with lamp-black, the rate at which it cools is *exactly* proportional to the value of $r$, and this rate has exactly the same value, whether the glass bulb is uncoated or coated with lamp-black. When enclosed in air the rate of cooling increases faster than $r$. The mode of measuring the rate in both cases, and of reducing the values of $r$ observed in air to what they would be in a vacuum, are described at the end of this paper.

§ 4. The instrument employed was designed so that the thermometer exposed to the Sun's rays should radiate against an enclosing metallic surface coated with lamp-black, and so that the temperature of that surface should always be known.

Fig. 1, with the description that accompanies it, gives the details. The rays of the Sun were admitted to strike upon the bulb of the thermometer, X, through a hole but little larger than its diameter, and were entirely screened off the brass tube against the blackened inner surface of which the radiation of the bulb took place. The thickness of the sides of the brass tube was $\frac{1}{4}$ inch, and the thermometer, Y, that indicated its temperature, was lodged in a hole cut in the upper side. The circumference of the bulb touched the brass, and its upper side was enclosed with cork, while the lower was exposed to the air within the tube, but was untouched by the rays of the Sun that passed through. The internal diameter of the tube was 0·9 inch and length 6 inches. The bulb of the solar thermometer was 0·42 inch in diameter, spherical in shape, and fixed in

the centre, as shown in the figure. Its shadow was an easy guide in moving the tube in altitude and azimuth to keep pace with the Sun. It is difficult, if not impossible, to demonstrate that the thermometer, Y, shows the exact temperature of the inner surface of the tube. It was subjected to three tests.

1. The instrument being out of the Sun's rays, and X and Y showing the same temperature, it was removed to a place where the atmospheric temperature was 10° lower. Both thermometers descended and showed a difference equal to about one-tenth the amount they had to fall to arrive at the atmospheric temperature, Y being so much in advance of X.

2. A bat's-wing flame of gas was brought within 3 inches fronting the middle of the tube; both X and Y rose together, keeping pace exactly.

3. While taking observations, the heat absorbed by X from the Sun, and again emitted from it and transferred to the tube, gradually raised its temperature until a maximum was obtained. Now, comparing X and Y while both are rising, and after having obtained their maximum, a difference of 0·3° was remarked, and this difference, no doubt, affected isolated observations when this maximum was not attained, in consequence of interruption by clouds passing, when it was usual to heat the solar thermometer artificially to near the stationary point, in order to save time; the great inconvenience of the apparatus in this climate being the slowness with which $r$ obtained its final value. An arrangement with a differential air-thermometer would, no doubt, be preferable in this respect, but the absolute value of the degrees indicated does not seem capable of being exactly determined.

The thermometers were carefully graduated and compared by myself, and the divisions between two fixed points, 60° and 100° (which included all the observed temperatures), were drawn as nearly equal as a good ivory scale and magnifying lens would admit. The length of a degree on the scale of Y was about ·05 inch, and upon X ·067 inch; with practised eye it was easy to read off the temperature to $\frac{1}{20}$ of a degree with lens; but such accuracy was unattainable for other reasons, and chiefly a sensible difference was caused by the varying

amount of the stem that was under the influence of the Sun's rays as it moved.

The observations taken on the morning of the 21st August continuously, during $2\frac{1}{2}$ hours of uninterrupted sunshine, were graphically equalised, the curve drawn and ordinates measured off at every 20 minutes. This was the only opportunity that occurred of continuous observation between such favourable limits of altitude as to indicate the direction of a line with some precision.

In the table of observations, given in the Appendix, the date and apparent time are given in the first column. The time-piece was regulated daily by the one o'clock signal-gun. The second column contains the values of $r$, the observed difference between thermometers X and Y. The film of talc that was interposed between the Sun and X was found to reflect $\frac{9}{50}$ of the incident rays. This ratio was determined by observations taken with the film off during calm weather. The value of $r$ without the film to $r$ with the film on, was as 1·18 to 1·00; the Sun's power not sensibly varying during the interval. This proportion was maintained at low values of $r$, and even when the source of radiation was a gas-flame. The third column contains the observed values of $r$ increased in this ratio. The fourth column contains the corrections required to reduce the values in the third to a vacuum. The correction is taken from a scale that was constructed by means of an empirical formula derived from observations on the cooling of X, as detailed in the Appendix. The fifth column is the final value of $r$ as it would appear in a naked vacuum, that is, a vacuum without any interposed transparent solid between the Sun and the bulb of the thermometer. The numbers in this column represent the quantity of heat-force supplied from the Sun to the bulb of the thermometer in a constant element of time, or the quantity that emanates from the bulb in a unit of time.

The experiments on the cooling of the thermometer, X, in a vacuum, show that from $r = 30°$ to $r = 15°$, the time of cooling was 294 beats of a time-piece, of which $77\frac{1}{2}$ were equal to 60 seconds; also from $r = 15°$ to $r = 7\frac{1}{2}°$, the time was the same, and generally from $r = 2m$ to $r = m$ the elapsed time is the constant 294, which thus represents the logarithm of 2

in the logarithmic curve, of which the ordinates are $r$ and the abscissæ the time of cooling, $t$. The equation of the curve being $c \log \frac{r_1}{r_0} = t_1 = t_0$, in which $c \log 2 = 294$, or to reduce to seconds, $c \log 2 = 294 \times \frac{60}{77\frac{1}{2}}$, and $c = 756{\cdot}1$. Let $t_1 - t_0 = \delta t$, then $\log \frac{r_1}{r_0} = \frac{1}{\mu} \cdot \frac{\delta r}{r}$, in which $\mu$ = hyp. log of 10; hence

$$\frac{c}{\mu} \cdot \frac{\delta r}{r} = \delta t \qquad [\log \frac{c}{\mu} = 2{\cdot}51636].$$

From this we may compute the quantity of heat supplied to a unit of surface by the Sun in a unit of time corresponding to any value of $r$. As an example, suppose $r = 10°$ and $\delta t = 1$ second, then $\delta r = \frac{10\,\mu}{c} = 0{\cdot}030453°$, or $3{\cdot}0453°$ in 100 seconds, is the rate at which the Sun communicates heat to a thermometer, whose bulb is a sphere 0·42 inch in diameter, when $r = 10°$.

Suppose the glass of the bulb to be $\frac{1}{30}$ of an inch thick, there would be ·0108 cubic inch glass and ·0287 cubic inch mercury heated 3·045° in 100 seconds. If $r = 20°$, the same heating would take place in 50 seconds, and so on.

To reduce this to thickness of ice melted in 1 minute, we have

Specific heat of mercury ·033 and of glass ·177.

Specific gravity of mercury 13·5 and of glass 2·9.

·0108 cubic inch glass equal in weight to ·0313 cubic inch water.

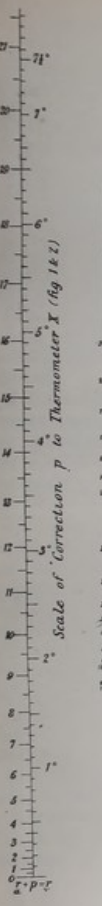
·0287 cubic inch mercury equal in weight to ·387 cubic inch water.

·0108 cubic inch glass raised 3·045° takes as much heat as ·0313 cubic inch water raised 0·54°.

·0287 cubic inch mercury raised 3·045° takes as much heat as ·387 cubic inch water raised 0·101°.

·0313 cubic inch water raised 0·54° takes as much heat as is required to raise 1 cubic inch 0·0169°.

·387 cubic inch water raised 0·101° takes as much heat as is required to raise 1 cubic inch 0·0391°.


Scale of Correction p to Thermometer X (fig 1 & 2)

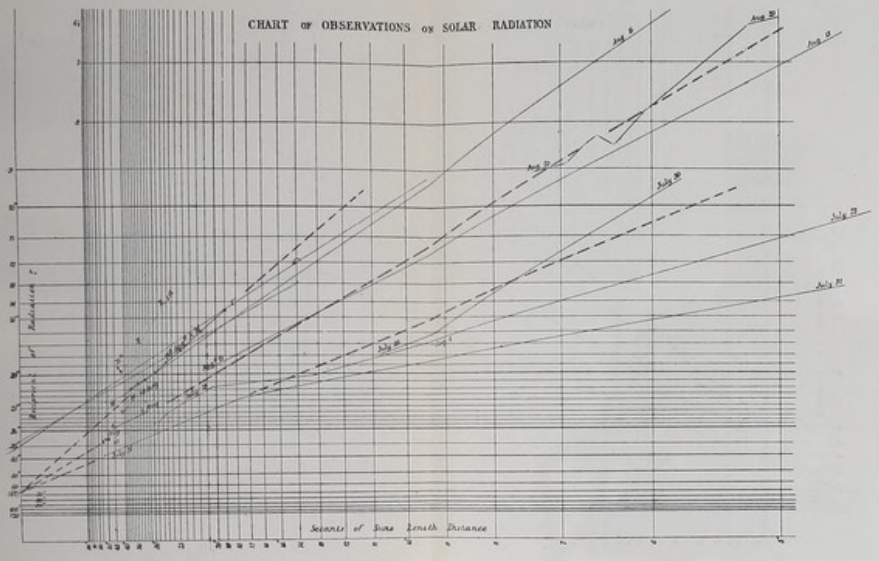


CHART OF OBSERVATIONS ON SOLAR RADIATION
Seconds of Time
Zenith Distance

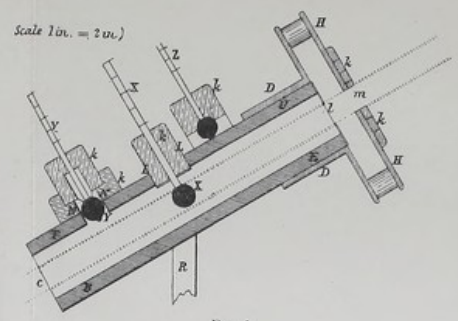

Scale 1 in. = 2 in.)

Fig. 1.

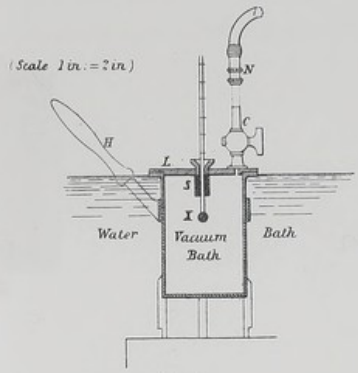

(Scale 1 in. = 2 in.)
Water
Vacuum
Bath
Bath

Fig. 2.

PLEASE
REFOLD THIS ILLUSTRATION CAREFULLY, RETAINING ORIGINAL FOLDS.
REPORT IF IT IS TORN.

The entire bulb of the thermometer thus raised 3·045° is thus equal to 1 cubic inch of water raised ·0169 + ·0391 = 0·056°.

Now, the transverse section of bulb is 0·138 square inch; and since specific gravity of ice is 0·93, and it requires 140° to melt ice, we have $140 \times 0{\cdot}738 \times 0{\cdot}93 \times x = 3{\cdot}045$; hence $x = 0{\cdot}00312$ inch, the thickness of ice melted by the Sun in 100 seconds, when $r = 10°$. This is equivalent to 0·001872 inch in 1 minute. With $r = 20°$ the thickness would be double this amount, and so on. Thus the presumed extra atmospheric value of $r$ being 67° gives 0·0124 inch thickness melted per minute.

From June to December the amount may be expected to vary $\frac{1}{15}$, corresponding to alteration of Sun's distance. In Herschel's *Meteorology* the probable thickness is stated to be ·0109 inch.

If the law indicated by straight lines on the chart is true it would require extremely accurate observations to give the extra atmospheric constant of solar radiation with precision. From a single observation made in Bombay some years ago I am disposed to believe it may exceed 67° considerably.

The mode of approaching the law of absorption is as follows:—Project the values of $r$ as ordinates to the secants of zenith distances as abscissæ. The resulting curve is evidently hyperbolic in character. If it is the conic hyperbola, the reciprocals of the ordinates laid off to the same abscissæ should range in a straight line. The obvious plan is, therefore, to lay off the reciprocals of $r$ in this way, and see how far their range agrees with the straight, and, if it differs, the character of the divergence might lead us to the true function that expresses the natural law, if it was not very complicated, and if the condition of the atmosphere did not vary so rapidly as to obscure it.

The observations, though taken under unfavourable conditions, favour the simple hyperbola.

It will be remarked, on inspecting the chart, that the value of $r$ at the same altitude of the Sun diminishes with the declination as the season advances. If continuous observa-

tions were possible for a few hours each day, when the altitude of the Sun ranged between 15° and 45°, we might expect to see the projection of the equalised observations range each day in a different line; but these lines ought all to converge on nearly the same point in the ordinate at the zero of the secant scale, if the law holds good. The lines A C, A D, A E, will exemplify this.

Let N, fig. 3, be the position of the observer, Z his zenith, and N S the direction of the Sun. Draw parallel lines $a\,b$, $c\,d$, etc.; now $a\,c : b\,d$ : : rad. : sec. Sun's zenith distance, so that only the thickness of each stratum varies as the secant; and if the physical condition of the stratum did not alter between two observations, we may take the secant as the representative of the collective thickness of the absorbing medium traversed by the Sun's rays, except at such low altitudes when the curvature of the earth as well as refraction may be expected to introduce uncertainty. The minimum value of the secant is radius, but we may imagine the Sun's rays to pass through a similarly constituted atmosphere in which the thickness of the same layers proportionally diminishes from unity or radius to zero. The reciprocal of $r$ diminished for values below radius at the same rate as for values above radius, attains at zero the extra atmospheric limit which, in all climates and seasons, ought to be determined by the inverse square of our planet's distance from the Sun in its orbit, and should not vary beyond $\frac{1}{30}$ of its mean value.

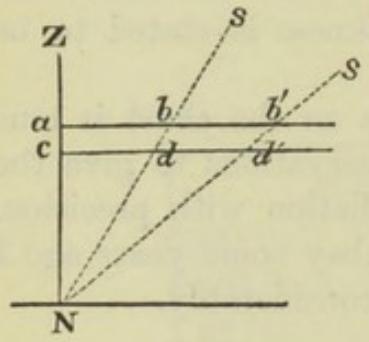


Fig. 3.

Let $m_0$, $m_1$, be the secants at which the radiation is $r_0$, $r_1$, we have, according to the projection $\dfrac{m_0 - m_1}{\dfrac{1}{r_0} - \dfrac{1}{r_1}} = k$, a constant quantity, so long as the physical condition of the atmosphere remains constant; and to find R, the extra atmospheric value of $r$, we have

$$m_0 - m_1 : \frac{1}{r_0} - \frac{1}{r_1} : : m_1 : \frac{1}{r_1} - \frac{1}{\mathrm{R}}.$$

Hence $m = \frac{k}{r} - \frac{k}{\mathrm{R}}$ and $\frac{1}{\mathrm{R}} = \frac{1}{r} - \frac{m}{k}$. Since R is constant, we may put $\frac{k}{\mathrm{R}} = e$, so that $r = \frac{k}{m+e}$. This expresses the presumed law of absorption or interception.

The essential nature of this law is seen by studying the proportionate differential of $r$,

$$-\delta r = \frac{k\,\delta m}{(m+e)^2} \text{ and } \frac{-\delta r}{r} = \frac{\delta m}{m+e} = \delta m \frac{r}{k}; \text{ hence } \frac{-\delta r}{\delta m} \propto r^2.$$

Thus the Sun's rays, in passing through a constant element of the thickness of the atmospheric medium, loses a proportionate amount of its power that is not constant, but that diminishes in the simple ratio of that power.

As an example, suppose with $r = 30°$, the value diminishes $1°$ in passing through 1 mile, it would only lose $\frac{1}{4}°$ in passing through the same mile if $r = 15°$, and $\frac{1}{900}$ of a degree if $r = 1°$. We might thus expect, when the atmosphere is clear, it does not intercept any sensible proportion of the heat radiated from the earth's surface into space.

Compare the value of $r$ with one Sun and with two: the supply from each, supposed equal, doubles the value of $r$, which measured at the extremities of the mile nearest and furthest from them, shows that for the same element of the thickness of the medium the proportionate decrement of $r$ is constant. Let $a$ represent the angular space occupied by the Sun's disk and $t$ the potential temperature of its radiating surface, then $t\,a$ represents the supply of heat by radiation from it upon a unit surface, and is measured by $r$, so that if $a\,t$ becomes $2\,a\,t$, $r$ becomes $2\,r$. Now, the factor 2 may have reference to $a$, the magnitude of the Sun's disk, or it may have reference to $t$, its temperature. The fluctuating value of $r$ from change of altitude or climate represents a fluctuating potential value of $a\,t$, but $a$ being constant, the change is similar to what would take place above the atmosphere by a change in $t$ alone. At different parts of the earth's orbit the value of $a$ changes, so that with $t$ constant and $a$ variable the proportionate absorption represented by $\frac{\delta r}{r\,\delta m}$ is a constant quantity, but so far

as $r$ depends on $t$ the value $\frac{\delta r}{r \delta m}$ increases with $t$, and the causal relation may be expressed as follows:—

The heat-pulse travels, carrying with it an intensity that it borrows from the temperature of its source, and encounters a deflecting or absorbing power in passing through a constant element of the atmospheric medium that is exactly proportional to that intensity.

It would be simpler if the resistance was uniform,—if the proportion of force absorbed was constant; but the observations do not admit of the possibility of this. The curve traced out by the co-ordinates, $r$ and secant zenith distance, would in that case be no longer the conic hyperbola, but the logarithmic curve.

At 6 o'clock in the evening of the 31st July, while making an observation, an extensive shower of thin rain took place overhead and westward towards the Sun, without sensibly obscuring its light or affecting its image when examined through a telescope. The value of $r$ descended immediately from 15° to 13°. The single observation I took in India, compared with those taken at the same altitude in this country, indicates that the value of $r$ is there fully double what it is here, while the quantity of vapour held in suspension estimated from the dew-point is certainly greater. It would seem probable, therefore, that the absorbing power of the atmosphere depends on the watery particles contained in it, not upon the aqueous vapour dissolved in it.

*Explanation of the figures.*

Fig. 1. T, U, B, E, is a square tube of brass, mounted with motion in altitude upon an upright, R, fixed into a round slab of lead. The inner surface of this tube is blackened, and at each end, at $l$ and $c$, a film of transparent talc was stuck on to prevent the wind from moving the air within the tube.

H, D, D, H, a double screen made of cardboard and cork, coated on both sides with tin-foil, and fitted to slip on the extremity of the tube presented towards the sun.

$m$, the hole in centre of screen, about $\frac{1}{10}$ inch greater diameter than the bulb of the solar thermometer, X.

The talc film, *l*, was also coated with tin-foil except the central circle.

X, the thermometer in Sun with spherical bulb fixed in a cork that fitted the hole, L, L, in top of brass tube.

Y, the thermometer in the shade fixed in the hole, N, N, with cork and soft wax as shown.

Z, a thermometer applied to outer surface of tube.

Fig. 2 is a transverse section of vacuum bath, employed to ascertain the rate of cooling of the solar thermometer, X, in air and in vacuo.

It consists of a cylindrical vessel of brass, coated internally with lamp-black; the lid, L, is ground to the upper edge of the cylinder, and in its centre is a stuffing box, S, with Indian-rubber collar, through which the stem of the thermometer is passed, as shown in the figure; C, is a stop-cock, upon which, N, the nozzle of a flexible tube communicating with an air-pump, is ground air-tight. H is a wooden handle for removing the apparatus to and from the bath without touching the metal.

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*Appendix describing the Method employed to discover the Influence of the Air in the Cooling of the Sun Thermometer X, and of Ascertaining the Correction required to be Applied to Observations of* r, *so as to reduce them to a Vacuum.*

Fig. 2 is an upright section of the little apparatus or vacuum-bath. It consists of a cylindrical vessel of brass coated internally with lamp-black. The lid was ground air-tight to the upper edge, and had a stuffing-box in the centre, through which the stem of the thermometer was passed. A stop-cock enabled a communication to be made with an air-pump. With a plentiful supply of lard to the stuffing-box and ground

surfaces, a good vacuum could be maintained for a day unimpaired.

The time was measured by the beats of a clock: to register the number of these at each degree as the mercury of the thermometer descended, a scale of equal parts was prepared extending to 1000, and with distinguishing marks at each 5, 10, 50, and 100. Then with a pencil in the right hand over the scale and a magnifying glass in the left over the scale of the thermometer, I counted the beats, and when the mercury came to the line of a degree, made a mark on the scale of equal parts opposite the number of beats, and at the same time continued to count on; *e.g.* if 57 was the number when the mercury came to a line, a pencil-mark was made at 57 on the scale of equal parts, and the counting went on,—58, 59, etc., until the mercury came to the next line.

Thus, not a beat was lost from beginning to end, and the accuracy was only limited by the accuracy of the divisions on the scale of the thermometer. Indeed, this method is a severe test to the equality of the divisions, because the reciprocal of the differences in the number of the beats for each degree, if laid off as ordinates to the total number of beats, ought to range in a straight line, and any saw-like irregularities indicate inaccuracy in the divisions of the scale of the thermometer. To heat the bulb of the thermometer a funnel was placed over the small flame of a Bunsen; then holding the plate (having the thermometer fixed in its place) by means of the stop-cock, the bulb was brought over the top of the funnel until the mercury had risen to near the top of the scale. The plate was then quickly placed on the cylinder, communication made with the air-pump, and the air exhausted from the cylinder by 20 strokes, the capacity of the pump being about one-third that of the cylindrical vessel or vacuum-bath. The vessel thus exhausted was placed in a water-bath, the temperature of which was ascertained at the beginning and end; the difference seldom amounting to $\frac{1}{10}$ of a degree.

The following table exhibits two series of observations on the cooling of the sun thermometer, x, in the vacuum, and in air taken while the water-bath remained steady at 48°. This basal temperature being at an exact degree enables the rate

of cooling to be studied easily without fractional parts or interpolation :—

| Temp. X. | Beats in Vacuum. | $r$. | Beats in Air. | Temp. X. | Beats in Vacuum. | $r$. | Beats in Air. |
|---|---|---|---|---|---|---|---|
| 90° | | 42° | 0 | 90° | | 42° | |
| 85° | 0 | 37° | 36 | 65° | 323 | 17° | 270 |
| 4 | 12 | 36 | | 4 | 348 | 16 | 291 |
| 3 | 24 | 35 | | 3 | 377 | 15 | 312 |
| 2 | 36 | 34 | | 2 | 406 | 14 | 336 |
| 1 | 48 | 33 | | 1 | 437 | 13 | 360 |
| 80 | 60½ | 32 | 78 | 60 | 470 | 12 | 385 |
| 9 | 73 | 31 | | 9 | 510 | 11 | 414 |
| 8 | 86 | 30 | | 8 | 549 | 10 | 448 |
| 7 | 100½ | 29 | | 7 | 594 | 9 | 484 |
| 6 | 115 | 28 | | 6 | 643 | 8 | 527 |
| 75 | 130½ | 27 | 129 | 55 | 704 | 7 | 576 |
| 4 | 145½ | 26 | | 4 | 765 | 6 | 630 |
| 3 | 161 | 25 | | 3 | 843 | 5 | 695 |
| 2 | 179½ | 24 | | 2 | ... | 4 | 780 |
| 1 | 197 | 23 | | 1 | ... | 3 | |
| 70 | 214 | 22 | 191 | 50 | ... | 2 | |
| 9 | 235 | 21 | | 9 | ... | 1 | |
| 8 | 255 | 20 | | 48 | ... | 0 | |
| 7 | 276 | 19 | | | | | |
| 6 | 298 | 18 | | | | | |

Thus, 53, being the last observation of the vacuum cooling, corresponds with $r = 5°$; then

At $r = 5$ we have 843 beats — Difference 294
$r = 10$ ,, 549 ,, ,, 294 — Mean Difference 291·7
$r = 20$ ,, 255 ,, ,, 287
$r = 40$ ,, −32 ,,

Again, beginning with $r = 7°$,

At $r = 7$ we have 704 beats — Difference 298
$r = 14$ ,, 406 ,, ,, 291 — Mean Difference 294½
$r = 28$ ,, 115 ,,

And beginning with $r = 9$,

$r = 9$ we have 594 beats — Difference 296
$r = 18$ ,, 298 ,, ,, 286 — Mean Difference 291
$r = 36$ ,, 12 ,,

The numbers in the column of differences ought to be the same if the law of cooling in a vacuum is perfectly true, if the

vacuum is complete, and if the graduation of the thermometer is correct. The difference between them is so small that the result must, I think, be deemed satisfactory.

Let us study the same differences with the cooling in air :—

| | | |
|---|---|---|
| At $r = 5°$ we have 695 beats | 1st Difference 247 | 2nd Difference 19 |
| $r = 10$ ,, 448 ,, | ,, 228 | ,, 22 |
| $r = 20$ ,, 220 ,, | ,, 206 | |
| $r = 40$ ,, 14 ,, | | |
| $r = 7°$ we have 576 beats | 1st Difference 240 | 2nd Difference 23 |
| $r = 14$ ,, 336 ,, | ,, 217 | |
| $r = 28$ ,, 119 ,, | | |
| $r = 9°$ we have 484 beats | 1st Difference 230 | 2nd Difference 20 |
| $r = 18$ ,, 254 ,, | ,, 210 | |
| $r = 36$ ,, 44 ,, | | |

Thus, it appears that in air the cooling takes place in a ratio greater than $r$, the 1st difference of the times diminishing and the 2nd difference slightly increasing between 247 and 210. The limiting value of the 1st difference must be 294 when $r = 0$, and 294 minus the 1st difference increases nearly as $\sqrt{r}$. An empirical formula constructed in conformity with this ratio cannot differ much from the observations.

Let $\Delta$ represent 1st difference and $g$ a constant,

$$\Delta = 294 - g\sqrt{r} \text{ and } g = \frac{294 - 247}{\sqrt{7\frac{1}{2}}} = 17{\cdot}162 = \frac{294 - 210}{\sqrt{27}} = \text{ etc. (nearly).}$$

$\Delta$ represents the logarithm of 2, so that $c$ being a constant, we have $\log r = \dfrac{\Delta}{c}$. In the curve that represents the cooling in air we may assume a small arc of it to coincide with a true logarithmic curve or the curve of cooling in a vacuum, and we have to find the value of $r$, the ordinate of the true logarithmic curve at the given point.

The logarithmic curve is defined by the equation

$$c \log \frac{r_1}{r_0} = t_1 - t_0 = \frac{\Delta}{\log 2} \log \frac{r_1}{r_0}.$$

Let

$$r_0 = (r_1 - 0{\cdot}1^0).$$

To find the value of $r_v$ corresponding to a given $r_a$ ($r_v$ vacuum, $r_a$ air), we require to compute the value of $t_1 - t_0$, employing $\Delta_a = 294 - g\sqrt{r}$ in the equation

$$\frac{\Delta_a}{\log 2} \log \frac{r_a}{(r_a - 0{\cdot}1^0)} = t_1 - t_0;$$

then, with this value of $t_1 - t_0$, and with $\Delta_v = 294$, find the value of $r_v$ in the equation

$$t_1 - t_0 = \frac{\Delta_v}{\log 2} \cdot \log \frac{r_v}{(r_v - 0{\cdot}1^0)}.$$

The direct equation is

$$\Delta_a \{\log r_a - \log (r_a - 0{\cdot}1^0)\} = \Delta_v \{\log r_v - \log (r_v - 0{\cdot}1^0)\}$$

and

$$\frac{\Delta_a}{\Delta_v} = 1 - \frac{g}{294}\sqrt{r_a} = 1 - \frac{\sqrt{r_a}}{17{\cdot}131}.$$

Hence $r_v$ may be ascertained by inspecting the differences of a table of logarithms; and it was from these that a scale was constructed for reducing the values of $r$ taken in air to what they would be if taken in a vacuum, where the emission of heat was by radiation alone.

The cooling of the sun thermometer in air when fixed in its place in the tube, as in fig. 1, was found to be exactly the same as when fixed in the cylinder, fig. 2, unexhausted.

A chemical thermometer with cylindrical reservoir was tried in the vacuum-bath, and the cooling was found to take place exactly in the logarithmic curve. It is difficult to adjust the vacuum-bath in time to observe a high value of $r$, but good observations were obtained from $r = 190^\circ$ downwards; so there is little doubt that the law of cooling by radiation is general and independent of the shape of the cooling body. I purpose extending these observations with different surfaces. One result is interesting, as showing the perfect reciprocity of the radiation, viz. a gilt bulb radiating against a blackened metallic surface loses heat at the same rate as a blackened bulb

against a bright metallic surface; the rate being slower than when both are blackened.

Another fact that it is well to keep in view is that, although the uncoated glass bulb radiates as quickly as the same coated with black, it does not absorb the incident rays of heat to the same degree; nearly one-half being reflected without entering the glass.

*Observations on Solar Radiation.*

| | Apparent Time. Latitude, 55° 58′ N. Longitude, 0h 12m 44s W. | Observed Values of $r$. | Ditto corrected for Talc Film. | Correction to Reduce to a Vacuum. | Value of $r$ Reduced to a Vacuum. | Reciprocal of Ditto. | Apparent Altitude of Sun's Centre. | Cosecant of Apparent Zenith Distance. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| | | $r_o$ | $r_a$ | $p$ | $r_v$ | $\frac{1}{r_v}$ | App. Alt. | | |
| 1861. | h m | ° | ° | ° | ° | | ° ′ | | |
| July 28 | 4 25 P.M. | 17·5 | 20·6 | +7·4 | 28·0 | ·0357 | 28 49 | 2·075 | Bulb coated with lamp-black and turpentine; surface moist. |
| | 4 36 | 16·5 | 19·5 | 6·8 | 26·3 | ·0380 | 27 16 | 2·183 | |
| | 4 56 | 15·5 | 18·3 | 6·2 | 24·5 | ·0408 | 24 30 | 2·411 | |
| | 5 26 | 14·0 | 16·5 | 5·2 | 21·7 | ·0461 | 20 19 | 2·880 | |
| | 6 18 | 13·2 | 15·5 | 4·7 | 20·2 | ·0495 | 13 11 | 4·385 | |
| | 7 26 | 7·2 | 8·5 | 1·7 | 10·2 | ·0980 | 4 27 | 12·888 | |
| | 6 36 P.M. | 11·8 | 13·9 | +3·9 | 17·8 | ·0562 | 10 41 | 5·394 | |
| | 6 45 | 10·8 | 12·7 | 3·3 | 16·0 | ·0625 | 9 12 | 6 255 | |
| | 7 13 | 6·6 | 7·8 | 1·4 | 9·2 | ·1087 | 5 44 | 10·010 | |
| 31 | Noon | 23·0 | 27·2 | +11·4 | 38·6 | ·0259 | 52 20 | 1·263 | Extra favourable conditions. |
| | 5 41 P.M. | 15·0 | 17·7 | 5·8 | 23·5 | ·0425 | 17 38 | 3·301 | Ditto. |
| | 6 6 | 14·0 | 16·5 | 5·2 | 21·7 | ·0461 | 14 3 | 4·119 | |
| | 6 29 | 13·0 | 15·3 | 4·6 | 19·9 | ·0502 | 11 6 | 5·194 | |
| | 7 18 | 9·0 | 10·6 | 2·5 | 13·1 | ·0763 | 4 36 | 12·47 | From July 31, noon, to Aug. 5, noon, the bulb coated with China ink and gum solution; surface dry. |
| | 7 34 | 6·0? | 7·1 | 1·3 | 8·4 | ·1190 | 2 52 | | |
| Aug. 1 | 9 51 A.M. | 20·5 | +?24·2 | +9·5 | 33·7 | ·0297 | 44 58 | 1·415 | |
| 4 | 11 1 A.M. | 20·0 | 23·6 | 9·1 | 32·7 | ·0306 | 49 35 | 1·313 | |
| | 6 36 P.M. | 11·3 | 13·4 | 3·6 | 17·0 | ·0588 | 9 15 | 6·221 | |
| 5 | Noon | 20·2 | 23·8 | 9·2 | 33·0 | ·0303 | 51 0 | 1·287 | Extra favourable. |
| B | 3 44 P.M. | 11·3 | 13·3 | 3·6 | 16·9 | ·0592 | 33 14 | 1·825 | Bulb uncoated. |
| B | 4 30 | 9·7 | 11·4 | 2·7 | 14·1 | ·0709 | 27 10 | 2·190 | Ditto. |

All the above, taken with thermometer Y in the sun, and its correction for air, has been assumed to be the same as that experimentally determined for thermometer X in the subsequent observations.

In the following observations the thermometer in sun was x, with bulb coated with China ink :—

| | | $r_o$ | $r_a$ | $p$ | $r_s$ | $\frac{1}{r_s}$ | App. Alt. | cosec. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| 1861. | h m | ° | ° | ° | ° | | ° ′ | | |
| Aug. 9 | 3 34 P.M. | 14·4 | 17·0 | +5·5 | 22·5 | ·0444 | 33 7 | 1·830 | |
| 10 | 8 50 A.M. | 15·2 | 17·9 | +5·9 | 23·8 | ·0420 | 35 56 | 1·704 | Taken from a consecutive series of 18 observations. |
| | 9 8 | 15·5 | 18·3 | 6·1 | 24·4 | ·0410 | 38 5 | 1·621 | |
| | 9 23 | 16·2 | 19·1 | 6·6 | 25·7 | ·0389 | 39 48 | 1·562 | |
| 13 | 4 56 P.M. | 12·8 | 15·1 | +4·5 | 19·6 | ·0510 | 21 1 | 2·787 | Not less, perhaps greater value of $r$. |
| | 6 56 | 4·8 | 5·7 | 0·9 | 6·6 | ·1515 | 4 37 | 12·42 | |
| | 7 27 | 2·2 | 2·6 | 0·3 | 2·9 | ·3448 | 0 53 | | Sunset. |
| 15 | 6 1 P.M. | 9·5 | 11·2 | 7 | 13· | ·0719 | 11 25 | 5·05 | |
| | 7 4 | 3·8 | 4·5 | 0·6 | 5·1 | ·1961 | 3 4 | 18·69 | |
| 16 | 5 16 A.M. | 4·4 | 5·2 | 0·8 | 6·0 | ·1667 | 5 29 | 10·465 | Sun rises perfectly clear of clouds. |
| | 9 39 | 14·6 | 17·2 | 5·6 | 22·8 | ·0439 | 39 51 | 1·561 | |
| | 11 3 | 15·0 | 17·7 | 5·8 | 23·5 | ·0425 | 46 20 | 1·382 | |
| 21 | 7 20 A.M. | 9·8 | 11·6 | 2·9 | 14·5 | ·0690 | 19 0 | 3·071 | Consecutive series graphically projected and equalised. |
| | 7 40 | 10·7 | 12·6 | 3·3 | 15·9 | ·0629 | 21 45 | 2·698 | |
| | 8 0 | 11·6 | 13·7 | 3·8 | 17·5 | ·0571 | 24 29 | 2·413 | |
| | 8 20 | 12·3 | 14·5 | 4·2 | 18·7 | ·0535 | 27 9 | 2·191 | |
| | 8 40 | 13·0 | 15·3 | 4·6 | 19·9 | ·0502 | 29 45 | 2·015 | |
| | 9 0 | 13·5 | 15·9 | 4·9 | 20·8 | ·0481 | 32 14 | 1·874 | |
| | 9 20 | 13·8 | 16·2 | 5·0 | 21·2 | ·0472 | 34 37 | 1·760 | |
| | 9 40 | 14·1 | 16·6 | 5·3 | 21·9 | ·0457 | 36 50 | 1·668 | |
| | h m s | | | | | | | | |
| 20 | 6 21 16 P.M. | 6·6 | 7·7 | +1·4 | 9·1 | ·1099 | 7 21 | 7·817 | Consecutive series not equalised. |
| | 6 23 16 | 6·5 | 7·6 | 1·4 | 9·0 | ·1111 | 7 4 | 8·128 | |
| | 6 24 31 | 6·4 | 7·5 | 1·4 | 8·9 | ·1124 | 6 55 | 8·304 | |
| | 6 26 16 | 6·1 | 7·2 | 1·3 | 8·5 | ·1177 | 6 41 | 8·592 | |
| | 6 27 16 | 6·0 | 7·1 | 1·2 | 8·3 | ·1205 | 6 33 | 8·767 | |
| | 6 28 36 | 6·1 | 7·2 | 1·3 | 8·5 | ·1177 | 6 22 | 9·018 | |
| | 6 31 0 | 5·7 | 6·7 | 1·1 | 7·8 | ·1282 | 6 2 | 9·514 | |
| | 6 37 16 | 4·8 | 5·7 | 0·8 | 6·5 | ·1538 | 5 12 | 11·034 | |
| | 6 39 0 | 4·7 | 5·6 | 0·8 | 6·4 | ·1562 | 4 59 | 11·512 | |
| | Small cloud passes over the sun. | | | | | | | | |
| | 6 43 56 | 3·4 | 4·0 | 0·5 | 4·5 | ·2207 | 4 22 | 13·134 | |
| | 6 45 46 | 3·3 | 3·9 | 0·5 | 4·4 | ·227 | 4 5 | | |
| | 6 47 16 | 3·2 | 3·8 | 0·5 | 4·2 | ·235 | 3 54 | | |
| | 6 49 16 | 3·0 | 3·6 | 0·5 | 4·0 | ·249 | 3 38 | | |
| | 6 51 0 | 2·8 | 3·3 | 0·4 | 3·7 | ·272 | 3 25 | | |
| | 6 52 16 | 2·7 | 3·1 | 0·3 | 3·5 | ·287 | 3 15 | | |
| | 6 54 0 | 2·4 | 2·9 | 0·3 | 3·2 | ·312 | 3 2 | | |
| | 6 55 45 | 2·2 | 2·6 | 0·3 | 2·9 | ·348 | 2 48 | | |
| | 6 58 46 | 1·9 | 2·2 | 0·2 | 2·4 | ·408 | 2 30 | | |
| | 7 1 16 | 1·6 | 1·9 | 0·2 | 2·1 | ·483 | 2 7 | | |
| | 7 4 0 | 1·1 | 1·3 | 0·1 | 1·4 | ·714 | 1 48 | | |
| 28 | Noon | 12·5 | 14·8 | 4·3 | 19·1 | ·0524 | 43 42 | 1·447 | $r$ sensibly augments after culmination. |
| | 0 44 P.M. | 13·2 | ... | ... | ... | ... | ... | ... | |

In the following, thermometer Y was in sun, with its bulb coated with lamp-black in gold size:—

| | | $r_o$ | $r_a$ | $p$ | $r_s$ | $\frac{1}{r_s}$ | App. Alt. | cosec. |
|---|---|---|---|---|---|---|---|---|
| 1861. | h m | ° | ° | ° | ° | | ° ′ | |
| Aug. 6 | 8 7 A.M. | 16·2 | 19·1 | 6·6 | 25·7 | ·0389 | 31 17 | 1·926 |
| | 9 8 | 18·0 | 21·2 | 7·7 | 28·9 | ·0346 | 42 21 | 1·484 |
| | 9 32 | 18·3 | 21·6 | 8·0 | 29·6 | ·0338 | 45 12 | 1·409 |

*Note.*—Referring to the method of computing the Sun's potential temperature, described in the proceedings of the Society for March 1860, and employing the same rule with R, the extra atmospheric value of $r$ equal to 70° at Earth's mean distance, we arrive at 12,880,000° as the potential temperature of its radiating surface.

If we expose the flame of a bat's-wing-jet to one ball of a differential thermometer, the effect is the same, whether the broad side or the narrow side of the flame is presented, as I have found on trial. Now the potential temperature being equal to the product of $r$ by the reciprocal of the angular space occupied by the flame, it is in the one case about five times greater than in the other. In the same way we might compute the potential temperature of an angular space occupied by many thousand flames placed one behind the other, extending in a line from the observer, and probably we should find it cumulative in the ratio of the number of flames.

From observations I have made on gas flames with the radiation meter, fig. 1, it would seem to require about 4000 bat's-wing-flames ranged behind each other to give a potential equal to that of the Sun.

If the upper radiating matter of the Sun is in any degree transparent or permeable to radiation from lower strata, it is obvious that the *actual* temperature may thus be much below the *potential*.

26 Royal Crescent, Edinburgh,
25th *November* 1861.

# ON THE CHANGE THAT WOULD TAKE PLACE IN THE ELEMENTS OF THE EARTH'S ORBIT BY A SUDDEN ACCESSION TO THE SUN'S MASS

(From *Monthly Notices, Royal Astronomical Society*, 1866.)

That the appearance seen in the Sun on the 1st of September 1859, by Mr Carrington and by Mr Hodgson, indicated an accession to the Sun's mass is no doubt very generally admitted, but that the amount of that accession can have sensibly diminished the length of the year is an idea that may perhaps incur ridicule, not from its impossibility, but because the Greenwich observations must ere this have detected so remarkable a circumstance. There seems also to be an impression that the fall of such a planet as the Earth into the Sun "would, from the conversion of its previous mechanical energy, give out an awful blaze of light, such a blaze as . . . would scorch up in a moment all the inferior planets, and probably the Earth also, with everything upon it" (*Monthly Notices*, vol. xx. p. 89).

When this is put to test of figures, as at p. 197 of the same volume, such a catastrophe is clearly out of the question. Even if the blaze were persistent, the general rise of the temperature could not exceed 10° or 15°, but it could not be persistent, inasmuch as the extra power of radiation can only act for the short time taken to traverse the Sun's atmosphere before plunging under its surface, which observation shows to be not only incandescent but highly mobile as a fluid; the blaze would then terminate, and the general temperature of that part of the Sun be very sensibly increased, yet not so as to increase the radiating power in a perceptible degree.

Take the extreme case of the whole potential radiating power of the Sun being thereby raised 1000°, even this is only $\frac{1}{12000}$ part of the potential temperature that sends heat to us sufficient

to maintain a general average temperature over the surface of the Earth of about 500° above the absolute zero. Now this proportion of 500° is only $\frac{1}{24}$ of a degree, and this is the extreme maximum effect that can be reasonably expected from such a planet fall.

The phenomenon observed on the 1st September is thus quite consistent with what might justly be expected from an accession to the Sun's mass approaching in magnitude to our planet. Mr Carrington describes the intensity of the blaze to be fully as great as if there had been a hole in the screen attached to the object-glass of the telescope; fully as great as the direct unscreened sunlight. Mr Hodgson describes it as being most dazzling to the protected eye. Such descriptions

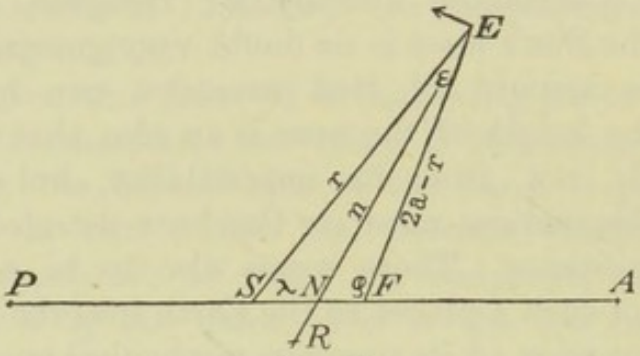



Fig. 1.

warrant an estimate of intensity several hundred times that of the normal surface; enough if persistent to produce a considerable change of climate; but as persistence is physically impossible, nothing of the kind need be looked for. The only indication of the mass thus added to the controlling centre of the system which we can become cognizant of is the possible decrement in the length of the year.

The following is the process of computation I have employed to ascertain this. The resulting formula, it will be remarked, is extremely simple in consequence of the excentricity of the Earth's orbit being small.

If the mass equalled that of our globe the decrement in the length of the year is $130^s$. This would cause a difference of 5·3″ in the longitude of the Sun at the end of the first year (1st September 1860), of 10·6″ on 1st September 1861, and on 1st September 1866, the Sun would be 37″ in advance of the

position given in the *Nautical Almanac*. If the mass was less than this the difference would also be less in the same proportion.

In fig. 1 the major axis of orbit is P S A; the triangle formed by the Sun, Earth, and aphelion focus is S E F; the radius of curvature is R E, and the normal E N.

An addition of $\frac{1}{m}$ to the Sun's mass causes an immediate decrement of $\frac{1}{m}$ in the length of R E and a change in the length of the normal, but no change in the direction of R E or F E.

Suppose a line to be drawn from S, passing through N',

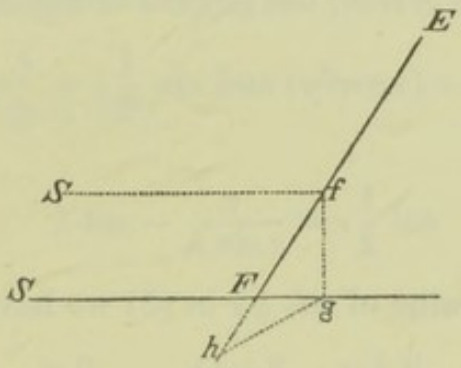



Fig. 2.

the new position of N, and produced to meet F E in $f$ (fig. 2). Draw $fg \perp$ S F and make F $h$ = F $g$.

The incremental change in the direction of the major axis is $\frac{fg}{\mathrm{SF}} = \delta\lambda$. The decrement of major axis $2\,\delta a$ is F $f = -2\,\delta p - 2\,\delta e$ ($p$ being the perihelion distance P S and $2e$ = S F). The increment of $2e$ is F $g$ = F $h$, and the decrement of $2p$ is $fh$.

Let $\phi$ represent $\angle$ E F S, $\lambda = \angle$ E S F, and $\epsilon = \angle$ E, $fg = 2e\delta\lambda$, F $g = \cot\phi\, 2e\delta\lambda = 2\,\delta e$, F $f = \operatorname{cosec}\phi\, 2e\,\delta\lambda = 2\,\delta a$.

With $\delta a$ represented by $x$, we have

$$\delta\lambda = \frac{x\sin\phi}{e} \quad . \quad . \quad . \quad (1)$$

$$\delta e = x\cos\phi \quad . \quad . \quad . \quad (2)$$

$$\delta p = x - x\cos\phi \quad . \quad . \quad (3)$$

The radius of curvature $\xi$ is equal to the cube of the normal $n\left(=\frac{r\sin\lambda}{\sin(\lambda+\frac{\epsilon}{2})}\right)$ divided by the square of the semi-parameter $s\left(=\frac{b^2}{a}\right)$ hence

$$\frac{\delta\xi}{\xi}=\frac{1}{m}=\delta\left(\frac{n^3}{s^2}\right)\cdot\frac{s^2}{n^3}=\frac{3\,\delta n}{n}-\frac{2\,\delta s}{s} \quad . \quad (4)$$

The first term

$$\frac{3\,\delta n}{n}=3\,\delta\lambda\cdot\frac{\sin\frac{1}{2}\epsilon}{\sin\lambda\sin(\lambda+\frac{1}{2}\epsilon)}=\frac{3\,\delta\lambda}{\sin^2\lambda\cot\frac{1}{2}\epsilon+\sin\lambda\cos\lambda} \quad (5)$$

In the triangle S E N, the angle $\epsilon$ being small, we have

$$\frac{n\sin\frac{1}{2}\epsilon}{\sin\lambda}=e\ (nearly)\ \text{and}\ \sin\frac{1}{2}\epsilon=\frac{e}{r}\sin\left(\lambda+\frac{1}{2}\epsilon\right)$$

hence

$$\cot\frac{1}{2}\epsilon=\frac{r}{e\sin\lambda}-\cot\lambda$$

substituting the value of $\cot\frac{1}{2}\epsilon$ in (5) we have

$$\frac{3\,\delta n}{n}=\frac{3\,e\,\delta\lambda}{r\sin\lambda}=\frac{3\,x}{2\,a-r} \quad . \quad . \quad (6)$$

The second term

$$-\frac{2\,\delta s}{s}=-\frac{4\,\delta b}{b}+\frac{2\,x}{a}$$

and since $b=\sqrt{a^2-e^2}$ we have

$$-\frac{4\,\delta b}{b}=-4\cdot\frac{a\,x-e\,\delta e}{a^2-e^2}=-\frac{4\,x}{a}\ (nearly)$$

hence

$$-\frac{2\,\delta s}{s}=-\frac{2\,x}{a} \quad . \quad . \quad . \quad (7)$$

and (5), (6), (7),

$$\frac{1}{m}=-x\left\{\frac{3}{2\,a-r}-\frac{2}{a}\right\}\ \text{or}\ x=\frac{2\,a-r}{2\,r-a}\cdot\frac{a}{m} \quad . \quad (8)$$

Computing this for 1st September 1859, with $m=354936$ we obtain $x-261$ miles, which compared with $a$ and the

length of the year, is found to represent a decrement of this length equal to 130 seconds.

The change in the values of $\lambda$, $e$, and $p$, may be ascertained from (1), (2), (3), $\delta\lambda = -29''$, $\delta e = +137$, $\delta p = -398$.

Supposing many such planet-falls to have happened in the history of the Sun, and to take place at every different part of the orbit, the motion of the apses would be cumulative in the direction of the signs.

If the accession took place in July $x = -255$ and $\delta e = +255$.

If it took place in December $x = -281$ and $\delta e = -281$. The change of $e$ is $(+)$ at the upper half of the orbit and $(-)$ at the lower.

INVERNESS, 25*th April* 1866.

# ON THE CHANGE THAT WOULD BE SUPERINDUCED UPON AN ELLIPTIC ORBIT IF THE INTENSITY OF THE FORCE OF GRAVITY WERE INFLUENCED BY THE CENTRIPETAL VELOCITY OF THE ORBITAL BODY

(From *Philosophical Magazine* for July 1867.)

The law of gravitation, as applied to the planetary motions, assumes that the power acts with a velocity that is practically infinite, that the acceleration of a body descending when its motion is in the same direction as the acting force, is exactly the same as the retardation of the same body ascending when its motion is contrary to the acting force.

To state this in its potential aspect: a body descending towards the sun at the earth's mean distance through a mile of its radial distance acquires an addition to its square velocity that amounts to 7 millionths (of a square mile), and in ascending through the same it loses the same amount. This result is held not to be influenced by the velocity with which the body moves through that mile. It may be an inch per second, or it may be 100 miles per second, the accession to the *vis viva* or square velocity at the end of the mile is the same, viz. 7 millionths.

The maximum velocity with which the earth approaches the sun or the rate at which the radius-vector diminishes, is about one-third of a mile per second; so that in three seconds it diminishes one mile, and in that time acquires the increment of square velocity 7 millionths (of a mile); *i.e.* a mechanical or work-force to this amount is transferred to each ton of its mass from some unknown source.

This is equal to the work performed by the same ton descending through 3·25 feet at earth's surface.

If this gradual appearance of *vis viva* at uniform rate in

respect to radial distance passed through (but increasing as the inverse square of that distance) is a transference and not a genesis of force, the amount transferred must diminish as the velocity of the motion in the direction of the transference increases.

Let U be the velocity with which the agent of gravitation acts, and $w$ the velocity of the descending body in the direction of its action; then, if by supposing U to be doubled the force of gravitation is doubled, it is evident that U diminished to $U - w$ (which it is by the motion of the descending body) diminishes the force by $\frac{w}{U}$ part. Again, if U is supposed by doubling to quadruple the force of gravitation, U diminished to $U - w$ diminishes the force by $\frac{2w}{U} - \frac{w^2}{U^2}$. Thus upon whatever power of U the force of gravity may be supposed to depend, the decrement of force is always in the simple ratio of $w$ to U, so long as $w$ is small in comparison to U.

An orbital body in descending from aphelion to perihelion acquires centripetal velocity $w$, which attains its maximum value when radius-vector is perpendicular to axis, thence diminishes to zero at perihelion. From aphelion to perihelion the force of gravity is thus to be viewed as having a decrement, the amount of which is proportional to $w$, increasing with it to a maximum and diminishing to zero at perihelion. On the ascending side of the orbit the force of gravity is to be viewed as having an increment which passes through the same changes as the decrement, and has the same maximum value. Let us suppose that maximum value to be equal to $100\,\omega$. The decrement of gravitation caused by $w$ increasing to $w + \omega$ is $\frac{\omega}{U}$. This is equal to the decrement that would be caused by the sun s mass losing a proportionate part $\frac{\delta m}{m} = \frac{\omega}{U}$. Hence the proportionate increase of semi-major axis is $\frac{\delta a}{a} = \frac{\omega}{U} \cdot \frac{2a - r}{r}$.

In the same way the increment of gravitation caused by $w$ diminishing from $w$ to $w - \omega$ is $\frac{\omega}{U}$, and the proportionate

decrease of $a$ is $\frac{\delta a}{a} = \frac{\omega}{U} \cdot \frac{2a - r_1}{r_1}$. This is negative and larger than the preceding, because the point in the orbit where $w$ diminishes is nearer the perihelion than the point where the same value of $w$ is found increasing; hence $\frac{\omega}{U}\left(\frac{2a - r_1}{r_1} - \frac{2a - r}{r}\right)$ is the value of the ordinate to $w$ as abscissa. The area resulting from such ordinates is the integral value of the proportionate decrement of semi-major axis made good in the descending semi-revolution.

In the ascending semi-revolution gravitation is augmented. From perihelion to the maximum of $w$ the augmentation increases from zero to $\frac{100\,\omega}{U}$, and from the maximum to aphelion the augmentation diminishes again to zero. Thus the integral for the ascending half is merely a repetiton of the integral for the descending half.

Let $c$ represent the orbital velocity in a circle with radius $a$; and let $\tan p = \frac{\sqrt{e^2 - (a - r)^2}}{r}$; then shall $w = c \tan p$ (as shown in Appendix) $\omega = dw = c \,.\, d \tan p$; and

$$\frac{\omega}{U}\left(\frac{2a - r_1}{r_1} - \frac{2a - r}{r}\right) = -\frac{c \,.\, d \tan p}{U}\left(\frac{2a - r_1}{r_1} - \frac{2a - r}{r}\right).$$

The relation between $r_1$ and $r$ through $p$, which is common to both, enables this expression to be integrated, as shown in the Appendix. Twice the integral gives the proportionate decrement of major axis $\left[\frac{\delta a}{a}\right]$ made good during one revolution. From this we obtain the proportionate decrements of minor axis, excentricity, and period,—also the change in perihelion distance, which is an increment:—

$$\left[\frac{\delta a}{a}\right] = -2\,\frac{c}{U} \cdot \pi \cdot \frac{ae^2}{b^3},$$

$$\left[\frac{\delta b}{b}\right] = \frac{1}{2}\left[\frac{\delta a}{a}\right],$$

$$\left[\frac{\delta e}{e}\right] = \left[\frac{\delta b}{b}\right]\left(\frac{a^2}{e^2} + 1\right), \text{ or } \left[\frac{\delta e}{a}\right] = \left[\frac{\delta b}{b}\right]\left(\frac{a}{e} - \frac{e}{a}\right),$$

$$\left[\frac{\delta p}{p}\right] = -\left[\frac{\delta b}{b}\right] \cdot \frac{p}{e},$$

$$\left[\frac{\delta t}{t}\right] = \frac{3}{2}\left[\frac{\delta a}{a}\right].$$

On examining these expressions, it will be found that when $e$ is small with reference to $a$, as in the planetary orbits, there is little change in axes or period, but the excentricity decrement is $\frac{a}{2e}$ time the decrement of $a$.

The following is computed for the earth's orbit, taking U = one million, which is tantamount to assuming gravitation to be reduced one millionth for each mile of centripetal velocity:

$$\left[\frac{\delta a}{a}\right] = -\cdot 0000000334; \quad \left[\frac{\delta e}{a}\right] = -\cdot 000001016; \quad \delta t = -1\cdot 58^{s}.$$

The following is computed for an ellipse with $a$ same as our planet, but with $\frac{e}{a} = \cdot 99$, and U as above:

$$\left[\frac{\delta a}{a}\right] = -\cdot 042; \quad \left[\frac{\delta p}{p}\right] = +\cdot 00021; \quad \delta t = -23 \text{ days}.$$

This example shows that a periodic comet with small perihelion distance and large excentricity is favourable for discovering the absolute value of U. $\delta t$, computed for one revolution of Halley's comet with U as above, gives 70 days as the decrement of period.

The following is computed for Encke's comet:—

$$\left[\frac{\delta a}{a}\right] = -\cdot 00037; \quad \left[\frac{\delta e}{a}\right] = -\cdot 00038; \quad \left[\frac{\delta p}{p}\right] = +\cdot 00003;$$

$$\delta t = -0\cdot 66^{d}.$$

It is admitted that the period of Encke's comet is shortened half a day by some unknown cause; so if the finite value of 1·32 million miles is assigned to U, we have a sufficient cause for the decrement. On the other hand, the excentricity of the earth's orbit would diminish by this cause ·0000008, which is double the amount of decrement assigned to it by the secular variation: also there would be a shortening of the period of Halley's

comet by 53 days. These, it may be presumed, are both inadmissible.

It would be interesting if physical astronomers could assign a limit; for it cannot now be believed that the living force, which is apparently generated and lost by means of gravitation, is absolutely created and destroyed. That would be to make gravitation an exception to the general rule of conservation. Besides, it is demonstrated that space is filled with a material instrument that is the carrier of living force outwards; so may that or another even more subtle and dominant be the source from which descending matter acquires the *vis viva* that it carries centripetally inwards.

## Appendix.

To prove $w = c \cdot \tan p$.

$g$ = feet fallen through in one second at earth's surface.

$4g$ = *feet* square velocity acquired in falling through one foot.

$\frac{4g}{5280^2}$ = *miles* square velocity acquired in falling through one foot.

$\frac{4g}{5280}$ = *miles* square velocity acquired in falling through one mile.

$$\frac{4g}{5280} = \left\{ \frac{1}{3956} - \frac{1}{3957} \right\} \mathrm{G}.$$

Hence

$$\mathrm{G} = \frac{4g}{5280} \times 3956^2 = \text{dynamical constant of gravitation having reference to the earth's volume and density as unity} \quad . \quad . \quad . \quad (23)$$

S = 355000 the sun's mass (earth unity).

$$\sqrt{\frac{\mathrm{GS}}{2a}} = c = \text{orbital velocity in circle radius } a \text{ (miles)} \quad . \quad (26)$$

$\frac{a}{2}\sqrt{\frac{\mathrm{GS}}{2a}}$ = area described per second in circle.

$\frac{b}{2}\sqrt{\frac{\mathrm{GS}}{2a}}$ = area described in ellipse.

$v$ = velocity, and $r$ = radius vector at a point in elliptic orbit.

$$v = b \cdot \sqrt{\frac{GS}{2a}} \cdot \frac{1}{r \cos \frac{1}{2}\epsilon}, \text{ in which } \epsilon = \angle SEF \text{ (fig. 1).} \quad (27)$$

$$w = v \sin \tfrac{1}{2}\epsilon = b \cdot \sqrt{\frac{GS}{2a}} \cdot \frac{1}{r} \cdot \tan \tfrac{1}{2}\epsilon \quad . \quad . \quad . \quad (28)$$

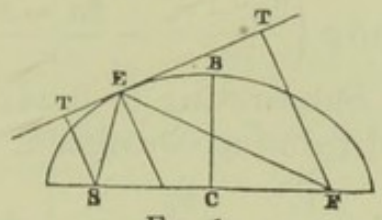


FIG. 1.

Because the product of the perpendiculars from the two foci of an ellipse to a tangent at any point is equal to the square of the semi-minor axis

$$\frac{(2a - r)r}{b^2} = \sec^2 \tfrac{1}{2}\epsilon,$$

and

$$\tan \tfrac{1}{2}\epsilon = \sqrt{\frac{(2a - r)r}{b^2} - 1}, \quad . \quad . \quad . \quad (29)$$

and

$$\frac{b}{r} \cdot \tan \tfrac{1}{2}\epsilon = \frac{\sqrt{e^2 - (a - r)^2}}{r} = \tan p.$$

Hence

$$w = c \tan p. \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

The following construction (fig. 2) shows $p$ and its relation to

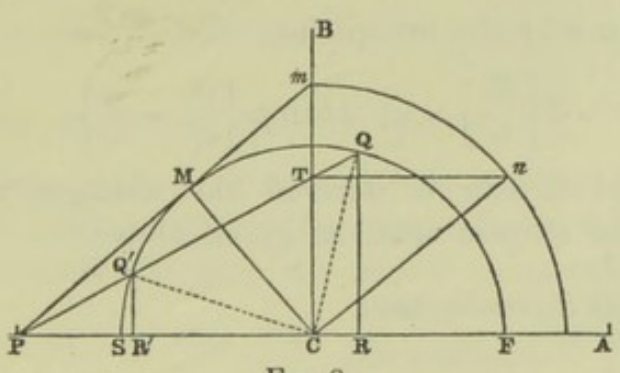


FIG. 2.

the elements of the ellipse. PC = $a$ = CA, SC = $e$ = CM = CF. Draw PT cutting semicircle in Q′ and Q. Draw Q′R′, QR, ⊥ to

PA, $\angle \mathrm{TPC} = p$, $\mathrm{PR}' = r_1$, $\mathrm{PR} = r$, $\mathrm{QR} = \sqrt{e^2 - (a-r)^2}$ and $\frac{\mathrm{QR}}{\mathrm{PR}} = \tan p$; hence $r^2 \sec^2 p = 2ar - b^2$, and

$$r = a\cos^2 p\left\{1 + \sqrt{1 - \frac{b^2}{a^2\cos^2 p}}\right\} = a\cos^2 p\,(1+q), \qquad (31)$$

$$r_1 = a\cos^2 p\,(1-q). \qquad (32)$$

This enables $\frac{1}{\mathrm{U}} \cdot c\,d\tan p\left\{\frac{2a - r_1}{r_1} - \frac{2a-r}{r}\right\} = \left[\frac{\delta a}{a}\right]$ to be integrated as follows. Substituting in it the values of $r$ and $r_1$ given in (31), (32) and reducing, we obtain

$$d\left[\frac{\delta a}{a}\right] = \frac{c}{\mathrm{U}} \cdot d\tan p\,\left\{\frac{4q}{\cos^2 p\,(1-q^2)^2}\right\} = \frac{c}{\mathrm{U}} \cdot d\tan p \cdot \frac{a^2}{b^2} \cdot 4q,$$

because $(1 - q^2 = \frac{b^2}{a^2\cos^2 p}$; and since $q = \sqrt{1 - \frac{b^2}{a^2\cos^2 p}}$,

$$d\left[\frac{\delta a}{a}\right] = \frac{c}{\mathrm{U}} \cdot d\tan p \cdot 4\frac{a}{b}\sqrt{\frac{e^2}{b^2} - \tan^2 p} = \frac{c}{\mathrm{U}}\,4\,\frac{a}{b} \cdot d\cos\phi\sin\phi\frac{e^2}{b^2},$$

because maximum value of $\tan p$ is $\frac{e}{b} = Cm$ (fig. 2), and

$$\tan p = \cos\phi\,\frac{e}{b}.$$

In fig. 2 the angle $\mathrm{TC}n$ is $\phi$. Hence for one complete revolution,

$$\left.\begin{aligned}\left[\frac{\delta a}{a}\right] &= -\frac{c}{\mathrm{U}}\,4\,\frac{a}{b} \cdot \frac{e^2}{b^2} \times \text{area of semicircle radius unity} \\ &= -\frac{c}{\mathrm{U}} 2\pi\,\frac{ae^2}{b^3}.\end{aligned}\right\} \qquad (33)$$

In the same way the integration of

$$d\left[\frac{\delta b}{b}\right] = \frac{c}{\mathrm{U}}\,d\tan p\left\{\frac{a}{r_1} - \frac{a}{r}\right\}$$

is effected, and thence is derived the changes in the other elements of the elliptic orbit as given above.

INVERNESS, 5*th September* 1866.

# ON THE HEAT OF THE STARS

(Submitted to *The Royal Astronomical Society*, 1878.)

In a recently published book on Astronomical observation mention is made of Mr Stone's observations on the radiant force of the Star Arcturus with respect to its heating power, which it is stated was found to be equal to the heating power of a cubic vessel of 3 inches kept at the temperature of boiling water at the distance of 400 yards. As the parallax of this star has been determined by Peters to be about $\frac{1}{8}$ of a second making its distance 800,000 times that of the sun we have the means of comparing its absolute radiant force with that of the sun as determined by the observations on Solar Radiation given in the *Monthly Notices* for December 1861. Assuming the atmospheric temperature at which Mr Stone's observations were made to be 60°, we have $212° - 60° = 152°$ as the excess of temperature of 9 square inches which affects the thermometric apparatus at 400 yards distance. Now if the whole concavity of the sphere of 400 yards radius was heated to the same temperature, the effect would be to raise the apparatus to 212° and no higher. If one-half the concavity were heated to the same the rise in the apparatus would be $\frac{1}{2} \times 152$ and $\frac{1}{m}$ the concavity so heated gives a rise of $\frac{152}{m}$. Thus we have to compare the surface of one side of the tin vessel with the surface of the sphere which is in the ratio of 1 to 290 millions, and we thus ascertain that the rise of temperature due to the radiant force of the star was $\frac{152°}{290 \text{ millions}}$ or about one two-millionth of a degree.

Although I do not know the altitude or time of the year at which Mr Stone's observations were taken, it will be sufficient

for the purpose of rough comparison to take the sun's radiant force at 25° (see Table of Observations, p. 74, *Monthly Notices*, December 1861, in column under the heading $r_a$) which is probably a few degrees too high. This amount has to be divided by the square of 800,000 to give the value of the sun's radiant force if placed at the distance of Arcturus. Thus we obtain $\frac{25°}{640{,}000 \text{ million}}$ or one twenty-five thousand millionth of a degree. Thus the absolute radiant force of Arcturus is represented to be more than twelve thousand times the absolute radiant force of the sun, which the observations on Solar Radiation show to be potentially the same as if it had a surface temperature of not less than twelve million degrees Fahr.

Although it cannot be affirmed that a star may not so far exceed the sun in power, it may be questioned whether one two millionth of a degree can be measured by any thermometric apparatus.

More certain results are likely to be obtained by photometric appliances.

The vast temperature assigned to the Solar surface has been deemed incredible, but we are called upon to believe in a still higher temperature below the surface if heat is "a mode of motion" and that it must go on increasing towards the sun's centre from the action of gravity on that part of the motion of the molecules that is in the vertical. In our atmosphere we observe this increase in descending to be at the rate of 1° in 320 feet on an average, and this corresponds with the results of calculation of the effects of gravity on the freely moving perfectly elastic molecules of which the air is composed. The gravity at the surface of the sun being about thirty times that at the surface of the earth, the rate of increase would be so many times greater, and although in descending towards the centre the force diminishes as the radius, the most moderate calculation gives a probable temperature of the sun's mass so great as may well be deemed incredible. In descending below the surface of the earth, we observe a regular increase of temperature which is also probably due to the effect of gravity on molecular motion in the vertical. In other planets there must also be vertical increase according to their magnitude

and molecular constitution. Thus a central heat of great intensity is the necessary consequence of gravity and deserves to be kept in view while discussing the physical aspects of the heavenly bodies. In the atmosphere we have storms caused by the action of the solar heat on the aqueous envelope of the earth. In the crust of the globe we have volcanic storms caused by the action of the central heat on the same aqueous envelope.

Gravitation is the parent cause of all.

28 Gayfield Square, Edinburgh,
*7th May* 1878.

# ON A SOLAR THERMOMETER COUPLE TO MEASURE THE RADIANT FORCE OF DAYLIGHT

(Submitted to *The Royal Astronomical Society*, 1878.)

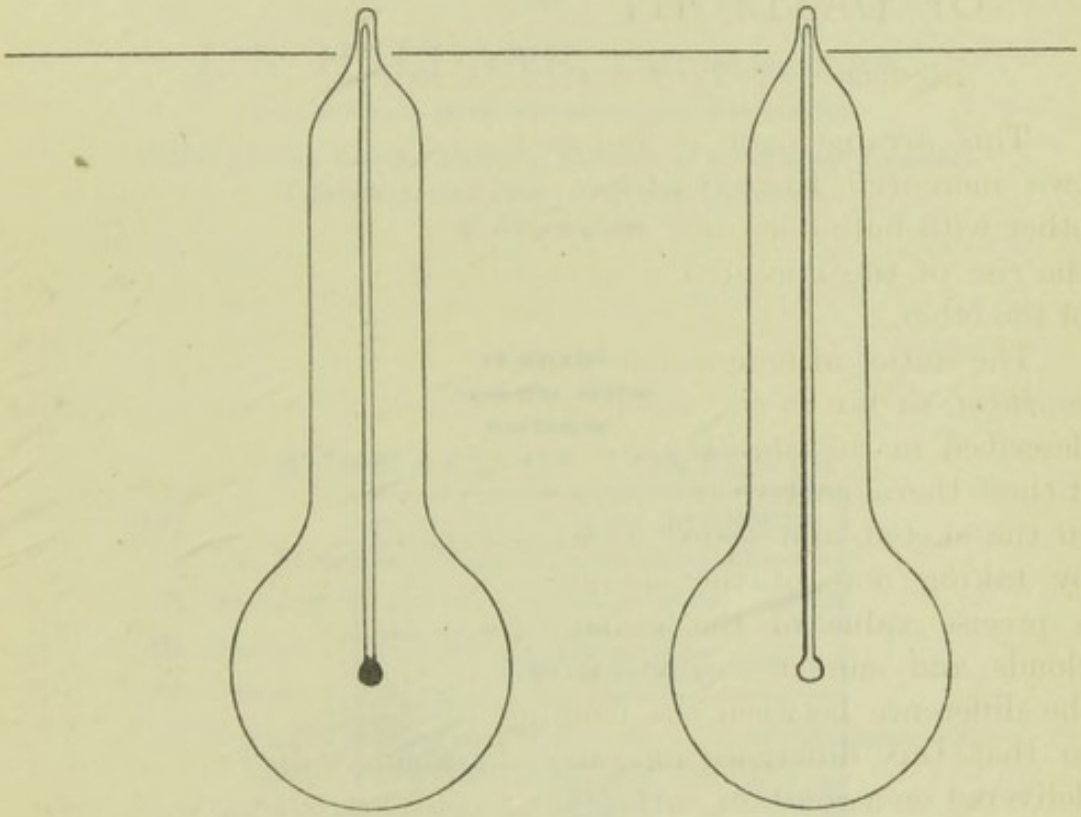
This arrangement is founded on the principle that when two mercurial thermometers, one with black bulb and the other with bulb uncoated, are exposed to the same radiant force, the rise of the uncoated is always nearly one-third of the rise of the other.

The ratio, although not exactly one-third, appears to be *constant*, so far as my observations extend with the apparatus described in the *Monthly Notices* for December 1861. Hence if these thermometers are enclosed in glass receptacles, as shown in the sketch, and sealed up after exhausting the air, we can, by taking note of the simultaneous reading of each, obtain a precise value of the radiant force of the sky—including clouds and sun if visible—which is simply proportional to the difference between the readings of the two thermometers; so that this difference indicates the amount of radiant force delivered on a constant surface in a constant time. If covered so that they receive the rays of the sun only, the difference of the readings will represent what was denoted by $\gamma_v$ (*Monthly Notices*, December 1861) or at least a close approximation if the air has been well exhausted. The *absolute* value of $r_v$ would be nearly $1\frac{1}{2}$ times the difference of readings, the actual ratio being easily determined as a constant; but that is of less consequence than that it should be constant.

In Bombay during the dry season, I found that the sun delivered more than twice the heat in a constant time than it does in Edinburgh with same altitude and clear sky, and in Edinburgh that the amount was greater in July than in June at the same altitude, and generally that the atmosphere screened

off less of the sun's force as the temperature in the shade increased.

The question I tried to solve was, what is the amount in supplies outside the atmosphere altogether, *i.e.*, what would a black bulb thermometer show if we supposed it to circulate by itself round the sun in the earth's orbit. Receiving the heat in one direction and delivering it by radiation in every



other, it was shown that the reading would not be less than 67° above absolute zero and probably much more.

As the sun occupies only $\frac{1}{183960}$ of the spherical concave, if we suppose the whole of that concave to be occupied with suns, the thermometer would rise to 183960 times 67° or 12,325,320°, which thus indicates the potential temperature of the sun's radiant surface corresponding to 67°, the assumed value of $r$.

28 Gayfield Square, Edinburgh,
11*th June* 1878.

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# GASES AND LIQUIDS

## A CONTRIBUTION TO MOLECULAR PHYSICS

BY

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### SUMMARY OF CONTENTS

CHAP.

I. The Dynamical Theory of Gases and Heat.

II. Heat-engines and the Dynamical Theory of Heat.

III. Gaseous Diffusion and Osmosis.

IV. Osmosis in Liquids.

V. Depression of Freezing-points and Vapour Pressures, and Elevation of Boiling-points, of Solutions.

VI. The Liquid and Gaseous States.

VII. The Internal Pressures and Saturated Vapours of Liquids.

VIII. The Steam-engine and some other Heat-engines.

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# PUBLISHERS' NOTE

THIS book contains a re-statement of the Gas Laws in such a form that they can be extended to liquids and solids, and yield equations giving the actual osmotic pressures, depressions of freezing-points and vapour-pressures, and elevations of boiling-points of solutions up to high concentrations. The confusion attending van't Hoff's conception of osmotic pressure is pointed out. A new interpretation is given to van der Waals's equation and to the transition from the gaseous to the liquid state, as well as to critical temperatures. A further interpretation of the properties of liquids is based largely on Waterston's investigations, as now fully published in his Collected Papers.

The rest of the book deals with the theory of heat-engines, and shows that Carnot's conception of a reversible heat-engine is irreconcilable with the modern conception, originating with Waterston, of temperature and heat. A new light is thus thrown on the action of heat-engines, and on what is at present known as the second law of thermodynamics. Discussions of the theory of the internal combustion engine and steam-engine lead to the conclusion that the maximum theoretically obtainable thermal efficiency of the former is considerably under 50 per cent., while that of the steam-engine is not very much lower. Stress is laid on the future possibilities of well-designed small steam-engines as applicable to many purposes for which either internal combustion engines or very wasteful steam-engines are at present used.

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