AN INQUIRY

INTO

THE NATURE AND PROPERTIES

OF THE

BLOOD,

IN HEALTH AND IN DISEASE.

BY THE LATE

CHARLES TURNER THACKRAH.

A NEW AND ENLARGED EDITION:

ARRANGED AND REVISED BY

THOMAS G. WRIGHT, M.D.

TO WHICH IS PREFIXED,

A BIOGRAPHICAL MEMOIR OF MR. THACKRAH.

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TO

SIR ASTLEY P. COOPER, BARONET, F.R.S.,

&c.

UNDER WHOSE AUSPICES

MR. THACKRAH'S TREATISE ON THE BLOOD ORIGINATED,

AND TO

WHOSE ENCOURAGEMENT IT WAS FIRST INDEBTED FOR PUBLICITY,

THIS ESSAY

WAS ORIGINALLY INSCRIBED BY HIS OBLIGED PUPIL,

THE AUTHOR:

AND IN ACCORDANCE WITH THE GRATEFUL

WISHES OF HIS WIDOW,

AND WITH A SENTIMENT OF HIGH PROFESSIONAL ESTEEM FROM

THE EDITOR,

THIS NEW IMPRESSION OF THE WORK IS RESPECTFULLY

DEDICATED.

ERRATA.

Page.	Lines.	
44	note.	*for* Far. *read* Fahr.
49	11 & 15	*for* OZMAZOME *read* OSMAZOME.
57	7	from bottom *for* Dr. Hunter's *read* Mr. Hunter's.
108	22	*for* of *read* in.
141	11	*for* 10 *read* 11.
146	9	*for* 11 *read* 12.
151	23	for *Entoxoa* read *Entozoa*.
154	7	*add* reference *a*
159	5	*dele* air.
159	8	read *a* vascular system.

EDITOR'S PREFACE.

The papers from which this posthumous edition of Mr. Thackrah's "Inquiry into the nature and properties of the blood" has been compiled, were bequeathed by him to a young physician who had been his pupil, with a direct view to his editorial care. That gentleman, however, shrunk from the task of arranging and transcribing the manuscripts, and seeing the work through the press; and at the request of the author's widow, whose veneration for her husband's professional ability induced her to be devotedly bent on carrying his wishes into effect, I undertook to complete and publish the volume.

It is now twelve months since the MSS. were placed in my hands: yet on mentioning the state in which the author had left them, and the precise acquaintance with the subject which it was necessary to acquire before commencing my duty, I feel that no further apology need be offered for the length of time that has elapsed in its execution. Mr. Thackrah's pen was too fertile, and his professional engagements too numerous, especially during the latter years of his life, for him to be particularly systematic in the arrangement of his notes and memoranda; and on attempting to transcribe the manuscript of his intended work, so many detached fragments presented themselves,—in numerous places the MS. appearing a chaos of disjointed sentences,—that my progress in unravelling the thread of the author's discourse was slow and tedious.

It was also a disadvantage to me that I became acquainted with Mr. Thackrah and enjoyed his valuable friendship but a very few months previous to his lamented death. I trust, however, that I have not left much short in editing this work which studious and faithful attention could effect for its credit: and Dr. Whytehead has obligingly supplied a memoir of our mutual friend, which on personal authority, no one else was, perhaps, so competent to furnish.

Mr. Thackrah's MS. fails in stating either the pages from whence he drew his references to the authors quoted, or (with trifling exceptions) even the title of the works in which the information was contained; and not unfrequently omits an exact notation of such passages from other writers as the author intended to extract. I have been anxious not to leave this edition defective in these important particulars; and, by diligent reading and search, I hope I have succeeded to an extent that will be found satisfactory.[a]

Unfortunately the author had not left any accurate memoranda as to the division of the chapters, but on this point, as in every other arrangement of the volume, I have adhered as closely as possible to his intentions, so far as I was able to obtain a clue to them. According to the division I have adopted, no less than three chapters are wanting in the original MS. I have supplied one, (Chapter IV.) from the former edition of this Inquiry, and the others (Chapters VI. and VIII.) have been principally compiled by the editor. In transcribing Mr. Thackrah's remarks I have not scrupled to make occasional alterations in the expression of such sentences, as, had he lived to revise them, he would probably have recomposed.

For distinction between the text of the author and the editor's remarks, the latter are printed in smaller type, and within brackets. The author's

[a] For this purpose I consulted the libraries of the *British Museum*, *London University*, and *Medico-Chirurgical Society*; for admission to the last of which I beg to acknowledge myself indebted to the kind introduction of the President, Professor Elliottson. I have also pleasure in expressing myself much assisted by the excellent notes to Dr. Bostock's *Physiology*.

notes are in like manner distinguishable from those furnished by the editor: the former being referred to by the usual typographic marks of asterisks, &c.; the latter invariably denoted by the letters of the Greek alphabet.

Some detached observations from Mr. Thackrah's manuscripts will be found under the head "Appendix;" in another division of which is also given a selection of experiments from his note-book: a few of the latter are cursorily alluded to in the body of the work, and all bear a relation to the subject of which it treats. A third section of the Appendix contains some passages, which the lamented author had minuted down as parts of a preface to this enlarged edition.—His indefatigable professional labour, his acute observation, his patient researches, are now ended! May their results, in the following pages, gratify the philosopher, and contribute to the advancement of medical science!

T. G. W.

Wakefield, November, 1834.

CONTENTS.

BIOGRAPHICAL MEMOIR.

It has long been a subject of discussion amongst philosophers how far the gift of extraordinary talent conduces to the happiness of man. It would too often seem that superior energies, however well directed, have rather benefited the world at large than their unfortunate possessor. When to an active and vigorous mind is conjoined a delicate frame of body, the consequences are still more obvious. The earthly tabernacle proves a frail habitation for its etherial tenant, and the victim of science sinks exhausted into a premature grave.

It may be asked, whether these fatal consequences do not arise rather from the abuse than the use of natural talents? Men have indeed been found in every age, who, with great mental powers, have possessed also a happy calmness of temperament. But these appear exceptions to the general law of nature, and it can scarcely be doubted that acute sensibility is the usual, if not the natural concomitant of genius. We may, however, admit that the picture has its lights as well as its shadows; but scarcely that the transient delights of sanguine anticipation will outweigh the calmer and more durable pleasures of reality. To those who desire an illustration of this painful yet truly interesting subject, the following brief memoir is presented.

Charles Turner Thackrah was born in Leeds, on the 22d day of May, 1795. His parents appear to have resided chiefly upon a small patrimonial estate at Shadwell, a village in the neighbourhood, and, at an early age, their only child was placed under the tuition of the Rev. Thomas Harrison, of Bardsey. He soon exhibited unusual activity of spirit, combined with that warmth of feeling which distinguished his character in after life. The Rev. Hammond Roberson, of Heald's Hall, was his next preceptor; and we find that he was here distinguished above his schoolfellows, by a spirit of enterprize and undaunted resolution of purpose.

The wishes of his mother having devoted him to the church, he was sent to read divinity with the Rev. James Knight, of Halifax. But to his active disposition the clerical profession presented few attractions; when he ought to have been reading Grotius he was studying fortification among the hills, and, fortunately for the cause of science, he was finally destined for the medical profession. Though a mere youth, he had already evinced a taste for literature, and composed the outline of an ingenious tale of fiction.

In 1811 he entered the surgery of Mr. O. Brooke, of Leeds, and from this period his devotion to study was unremitting. In 1812 he seems to have commenced a diary in Latin; and during this year, whilst he performed the laborious duties of an apprentice in an exemplary manner, he found time to read thoroughly 25 works on medicine, history, and general knowledge, and partially studied 38. In 1813 he remarks, that although during this year he was far more engaged with professional duties, yet he had surpassed the exertions of 1812. He closely studied 43 works, and partially 18, besides committing to memory 1600 lines of Latin and English verse, and practising himself daily in composition.

In 1814 he entered himself as a pupil to the Leeds Infirmary. His course of reading this year was not less extensive than in 1813, but the works he studied were almost exclusively on professional subjects. Amongst his resolutions in 1815 were "to consume in sleep not more than eight hours in twenty-four, unless in case of sickness or disturbed rest, and to make himself acquainted each day with the nature and treatment of some disease." About this period he began to keep records of medical cases, a practice which he constantly adopted in after life.

He spent the winter session of 1815-16 in London, where his regular attendance on the lectures, Guy's Hospital, and practical anatomy, became an object of remark. He was especially distinguished as a student by Sir Astley Cooper. In October, 1815, he entered the Physical Society at Guy's; and about the same time he experienced the first attack of that visceral affection from which he suffered occasionally during the remainder of his existence. Close study, and long confinement in the dissecting rooms, with accidental exposure to cold and wet, induced a serious disease of the mucous coat of the intestines, and his health declined rapidly under the effects of pain and profuse diarrhœa. Still, however, his exertions were unremitting. It is stated in his diary that his usual hours of study were from seven in the morning to half-past eleven at night; and although his bodily sufferings were severe, his chief cause for uneasiness was "lest he should lose opportunities of gaining improvement, and spend his time without adequate advantage." In the spring of 1816 he passed the examinations at Apothecaries' Hall, and the College of Surgeons, and his essay on Diabetes gained the Physical Society's prize of several valuable medical works.

After his return to Leeds he remained a short time with his friends, and at the commencement of 1817 he resolved upon endeavouring to establish a practice of his own. His health was still much impaired, and domestic afflictions combined to give the tone of his feelings a decidedly melancholic tendency, which they had seldom before exhibited. In a letter to a friend he says, " On a temperament like mine, naturally melancholic, with feelings naturally keen—at times, alas! painfully acute—the common trials and disappointments of life produce an effect which is unknown to a man of less sensibility." He would appear, however, to have mistaken his own character. His spirits were from childhood buoyant and cheerful, until close and unwearied study had broken a constitution originally vigorous. And whenever, in after life, the influences of his friends could persuade him to relax his efforts, the effect on his mind was striking in the extreme. His professional career was at first unpromising, but his prospects became more encouraging after his election to the office of town's surgeon.

During the year 1817 he applied himself to the study of physiology, pathology, medicine, surgery, and comparative anatomy, but more especially to the investigation of the nature and properties of blood. He had been making numerous experiments on the subject in the early part of the summer, and arrived at some satisfactory conclusions. At this period he was ignorant that Sir Astley Cooper had determined to give annual prizes to his pupils for the best dissertations on subjects proposed by himself, and that the subject for the ensuing year, was " The Blood." So soon as he became acquainted with Sir Astley's proposal, we may readily imagine that it would stimulate him to prosecute the subject more closely ;

and in August 1818 he received the gratifying intelligence that he had gained the prize.

The flattering reception which this essay met with from Sir Astley determined the author to publish it, with additional experiments on the cause of the blood's coagulation, and in July, 1819, the first edition of the " Inquiry" made its appearance. The reviewers were nearly unanimous in their approval, and the character of the author for acute observation and ingenious deduction was at once established.

At the request of the committee of the Leeds workhouse, he drew up a striking report of the miserable and pernicious state of the inferior class of lodging-houses; and the measures which resulted were most beneficial to the health of the lower orders.

In 1820 he associated himself with other junior members of the faculty in delivering lectures to their pupils on the various branches of medicine. He had long before this commenced the system with his own apprentices. In 1821 we find him actively engaged in practice, and yet devoting a large portion of his time to study and the instruction of his pupils. A few extracts from his private thoughts at this period may not be uninteresting. He justly remarks that "a state of literary leisure is by no means requisite for great attainments, or the culture of genius." " Oh," he exclaims, " that God would grant me that stamina of body which supports the vigour of the mind! How much knowledge should I then acquire, and what discoveries might I not hope to make! In my present state, the periods of study are lamentably short; so often does gastric disorder depress my faculties, and sink me in the dust."—"Oh that I could employ every moment to an useful purpose; that from each flying minute I might obtain a particle of knowledge. How much time, alas! is occupied with the taking of food and rest; and how much in that state of listlessness which a deficiency of bodily energy occasions."

The adoption of Cicero's motto strikingly illustrates the state of his feelings—"Incumbe toto pectore ad laudem." The education of his pupils had ever been a favourite object, and during the present year he drew up a strict code of rules for their instruction. In those days the attention of masters in general was not so universally directed towards this subject as at present, although many bright exceptions might be adduced. The system adopted by him rendered the care of his pupils no sinecure. He delivered lectures on the various branches of medicine, not excepting practical anatomy, to which examinations always succeeded. He practised them in classical exercises and English composition, in taking notes of medical cases, and drawing up reports of post mortem examinations. Classical instruction also formed a most important part of his system.

But the performance of his duties did not interfere with his private studies. In the month of April, on the opening of the Leeds Philosophical and

Literary Society, he delivered an eloquent introductory discourse, which was subsequently printed at the request of the members. The mode of its reception was most gratifying to the author, but the general tenor of his thoughts remained the same. "After all," he remarks, "I fear that I shall be obliged to leave my native place and settle in some distant town; perhaps my remains will ultimately be deposited in a foreign land. I may lie on my deathbed without a friend or relative to close my eyes; but I shall have one satisfaction, at least, that my remembrance will not perish."

In July he commenced his preparations for a popular course of seven lectures on physiology, which were delivered in the Philosophical Hall to a crowded audience. In the autumn of the same year he was elected a member of the "Société de Médicine Pratique de Paris;" and in January and February, 1823, he delivered a second course of ten physiological lectures. The interest which they excited led to the publication of a portion of them in 1824, under the title of "Lectures on Digestion and Diet." The style is decidedly popular, and although the work met with a favourable reception from the public, it subsequently maintained a low station in the opinion of the author. His practice was now rapidly increasing, and for the first time since his establishment, he felt perfectly easy in worldly circumstances. As might naturally be expected, he applied himself with redoubled vigour to the investigation of science, and about this period commenced a series of inquiries into the effects of various manufacturing processes upon the health of artisans.

In the spring of 1824, he was married to Mary Henrietta, daughter of Mr. J. Scott, of Wakefield. She only survived the union four years, and in a few months afterwards he was deprived of his mother and an only daughter. To one possessed of such warm and devoted affections, the trial was severe; but it only served to attach him still more strongly to his studies. "I must strive more, study more, and be more regular in recording cases of disease."

At this period of his life he employed eleven hours of the day in professional avocations, and one third of that time in close medical study and the instruction of his pupils. He did not allow himself more than six hours and a half of rest on the average. In the early part of 1826, he had contemplated the formation of an anatomical school; and in May 1827, he concluded his first course of lectures on practical anatomy. His applications to the College of Surgeons and Apothecaries' Hall, for the admission of his certificates to pupils, were unsuccessful, but he continued his anatomical labours, and delivered a second course in the spring of 1828. He made additional experiments on the nature of the blood, and especially that of the vena portæ, a paper on which subject was read before the Royal Society, in 1831. The investigation of the diseases of artisans also continued to be an object of his inquiries.

On the 8th of March, 1830, he married Grace, daughter of A. Greenwood, Esq. of Dewsbury Moor, and from this period, although his bodily health was still impaired, the morbid irritability of his feelings was sensibly diminished. But nothing could abate his zeal for the profession.

In 1831 he joined some other members of the profession in establishing the Leeds School of Medicine, and in the sessions of 1831-2 and 1832-3, he delivered lectures on anatomy, physiology, pathology, and surgery. In this year also he published the first edition of his work on "the Effects of Arts, Trades, and Professions on Health and Longevity," which experienced a reception equally favourable from the profession and the public in general. The impression was speedily disposed of, and a new and improved edition was issued in the following year. About this time the alarm excited by the prevalence of cholera led him to visit Newcastle and Gateshead. On his return he published an interesting pamphlet on that prolific subject.

From this period, until the day of his death, he was indefatigably employed in preparing a new edition of his work on the Blood. Bodily suffering, and the entreaties of his dearest friends, were insufficient to induce him to relax his ardour in the pursuit of fame. His health was manifestly declining, and the short intervals of repose, in which he indulged when nature could endure no more, only served to protract his sufferings. A pulmonary affection was superadded to the original visceral disease; and the only remaining wish, that he might live to complete his favourite work, was denied him.

Thus died Charles Turner Thackrah, on the 23d of May, 1833, in the prime of life, surrounded by every earthly comfort, and with the most brilliant prospect before him of attaining high professional eminence. The enthusiasm of his character, joined with patience of research, had long promised vigorous efforts in behalf of science. Without pretence to scholarship, he was not unacquainted with the ancient authors. He had read much, but his reading, especially in his youth, had been desultory. He rarely employed his energies on a branch of knowledge which he did not master with comparative ease, or his pen on a subject which he did not improve or elucidate. The strength of his social affections has been before noticed. He was a kind master, an affectionate father, a devoted husband, and a sincere friend. But ambition, his ruling passion, goaded on by a sense of superior talent, was all powerful—and to this his life was sacrificed. His own views and feelings are strikingly depicted in the following extract from one of his unpublished lectures:—"How can you employ your energies? Is the fervour of youth to be wholly expended in the accumulation of wealth? Where will you seek your happiness? In the cold respect which mere property acquires? In the toils of traffic, or the honours of the miser? Are there no noble objects for your ambition? Why should you not be Harveys, Hallers, and Hunters? In the present enlightened day you have better prospects of success than these men, who, without the aid of collateral science, made themselves immortal in the page of physiology. Why not emulate the examples of Hewson, Desault, and Bichat? Soaring above their professional associates, deriding the attacks of envy, unbroken by anxiety and toil, they held on their course of glory. They all died at an early age; but their youthful studies bought an honour which the maturer efforts of their envious competitors could never attain. Their names are engraven in the temple of fame."

H. Y. WHYTEHEAD.

Craike, October, 1834.

PREFACE TO THE FIRST EDITION.

Mr. A. Cooper having offered to the gentlemen, educated at the School of Guy and St. Thomas, a prize for the best Dissertation on the Blood, I instituted some experiments on the subject, and stated the results which my observations afforded. This essay being so fortunate as to obtain the prize, I have been induced to present it to the public. And, since the late period at which I heard of Mr. Cooper's proposal prevented the inquiry's comprehending, in the first instance, some points of importance, I have, during the last year, been endeavouring to supply the deficiency.

In reference to the physiological parts of this essay, it is scarcely necessary to remark, that much labour and considerable difficulty attended the research. Whoever, indeed, has studied experimentally to elucidate the principles of the animal economy, will admit, that, notwithstanding the interest it may have excited, or the gratification which success may have produced, the inquiry has demanded no ordinary degree of attention, perseverance, and ardour. Nor will these qualities succeed in their object, unless conjoined also with patience, caution, and candour. To the want of this combination, may be attributed the exuberant growth of those hypothetical opinions which choke the spring of truth, and impede the progress of knowledge.

With all the efforts, however, and all the circumspection of the best-conducted inquiry, it is impossible to avoid the casual commixture of error, and the occasional assumption of doctrines, which subsequent examination proves erroneous. In physiological pursuits accurate data are not afforded; demonstrative evidence, therefore, cannot be produced. The nature of the subject allows ample room, not only for experimental research, but for unbounded speculation, contradictory notions, and inconclusive statements. But these observations should by no means induce the opinion, that examination is fruitless, or truth undiscoverable. Such an inference is the refuge of ignorance and indolence. In the courts of justice, scarcely a cause is brought forwards, in which discordant evidence is not adduced; yet, who would thence conclude that a right decision is never formed, or truth never elicited? Not only in physiology, but in most inquiries to which the faculties of man are directed, conclusions are drawn, not with mathematical certainty,—but from the balance of probability and improbability; by the contrast and examination of opposite statements; by reflection on the motives, principles, and objects of their authors,—and especially by personal scrutiny, closely and impartially conducted.

Of the principles and laws of the animal œconomy, few are more intricate than those which regard the blood. The common appearances of this fluid may, indeed, be easily stated; but the origin of its changes, and the causes of its phenomena, are peculiarly abstruse; while the erroneous notions, and unfounded theories, which have been vainly adduced to remove the veil of nature, have greatly obstructed the path of inquiry, and added darkness to obscurity.

To clear away these obstacles, has been one object of my attention, and if in this only I have been successful, science will be benefited. The labourer, who removes the rubbish on the site of a projected building, raises not, indeed, the structure, but in preparing the way for the more able workman, he takes an office, though less respectable, yet not, perhaps, less useful.

Whatever opinion may be entertained of the success of my researches, of the mode in which they have been conducted, or of the conclusions to which they have led,—I lay claim to fairness of intention, and honesty of detail. Unbiassed by prejudice, unshackled by preconceived notions, I have impartially stated the individual results of my experiments, and noticed every regular or casual discordancy. It has been my aim rather to ascertain facts than to support opinions; to study the economy of nature, rather than to fetter her with conjectural or inconsistent theories. My inferences may be erroneous, but the facts remain unimpaired and unsophisticated. If they fail to substantiate the doctrines which they seem to support, they will not prove useless to future inquirers, and to science pursued under happier auspices. If the reasoning be rejected as futile or inconclusive, it may serve a minor, though not unimportant office—that of warning other experimentalists, of exhibiting the fallacies into which I have fallen, and by pointing out the ways of error, tend, in an indirect manner, to the advance of truth.

I trust, however, examination and experience will evince these inquiries to have added to the store of positive knowledge; and if one doubtful fact be established, or physiological principle discovered, or practical point enforced,—I shall not have laboured in vain. The noblest works of art have been accomplished by small and reiterated efforts. The stately edifice is composed of single stones, hewn by individuals, and the progress of science has been marked by no sudden bounds, but by the gradual and successive exertions of men devoted to the advance of the respective parts.

In reference to some of the experiments which were made on living animals, it may not be improper to make a remark; not so much, however, on account of the statements in the following pages, as in justification of those who have been thoughtlessly and ignorantly censured. Wanton cruelty, professional men, I believe, universally abhor; the useless repetition of observations on living creatures, or the infliction of unnecessary suffering, they reprobate. And it gives me pleasure to state, that in none of my experiments did the animals seem to suffer more than from a natural death; *in few, so much.* But had my pursuits obliged me to violate my feelings, by the infliction of severe and continued pain, a laudable object would have ensured me the approbation of every friend of science and humanity. From experiments on living creatures, we derive our acquaintance with the functions of the animal economy. Were no such observations made, we should be ignorant of the offices performed by the several parts of the human frame, and consequently we should be unable to comprehend or remove its maladies. Surely then, while hecatombs are slaughtered for the gratification of our palates, a few may be allowed to perish for the benefit of science: and while the luxurious complacently feed on the hare, which, half-dead in the chase, with fear and fatigue, has been worried by dogs,—or on the fish, which are burnt or scalded alive, they should not charge us with cruelty for occasionally sacrificing animals to the acquisition of that knowledge, which so eminently serves the cause of humanity, and prolongs the life of man. If they boast the fineness of their feelings, let them remember that we have feelings also; if they be shocked at the thought of experiments on living creatures, let them consider what we suffer, not only in physiological researches, but in the ordinary offices of our profession, by stifling our natural emotions, and, from a sense of duty and humanity, coercing feelings as acute, perhaps, as theirs,—feelings, which certainly are not mitigated by the constraint under which they are laid.

The practical parts of this essay, require little comment. Extensive opportunities have been afforded me of examining the blood in disease, and of these I have gladly availed myself. To state what observation and experiment have taught me, has been my principal object; not to form a compilation from the works of others, nor to canvass the justice of their notions, or the correctness of their remarks. The subject, indeed, has necessarily led me to the occasional examination of received opinions, but these were chiefly regarded as they influenced the treatment of disease: and if my observations have been frequently opposed to those of respectable writers,—it has ever been my aim, that important facts might be elicited, and safe indications in practice enforced.

The statements of the different sections are not partial and selected, but general conclusions from general results. I trust they will be found to assist us in discovering the nature of disease, in ascertaining its degree of violence, and in deciding on the treatment which it requires. Of this, however, experience must form a judgment. They who apply to particular studies or endeavour to elucidate particular principles, generally overrate the importance of their pursuits: but the modest hope which I cherish, of having contributed, in some degree, to the stock of professional knowledge, will not, I trust, be found to have originated in vanity, rather than in truth.

C. T. T.

Leeds, July 1, 1819.

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C. T. T.

Leeds, Feb. 1, 1819.

AN INQUIRY, &c.

CHAPTER I.

GENERAL PROPERTIES OF THE BLOOD.

Blood is a fluid too well known to require much general description. Distributed through innumerable channels, and penetrating almost every recess of the body, it supports the life and functions of every organ. When propelled from the left ventricle into the aorta and its ramifications, the blood is of a bright scarlet colour; but in its progress through the system, it assumes a darker hue from the absorption of carbon, and when brought back by the venæ cavæ to the right side of the heart, it is of a deep purple. After passing through the pulmonary artery to the lungs, it is exposed to the atmospheric air inhaled in respiration, is unloaded of its carbon, and returned by the pulmonary veins, purified and florid, to the left side of the heart.

The body undergoes a frequent change of its particles: the old constituents being worn down and useless, new ones are continually wanted for the repairs of the structure, and the due exercise of its functions. From the blood is supplied nourishment for the growth of parts, and for the reparation of their daily waste. It is the source, likewise, of the adhesive matter poured out for the union of wounds; and it affords the pabulum of those granulations which fill up the cavity of ulcers. By this fluid, also, are taken up the useless and decayed parts of the animal frame; for in the course of the circulation, carbon is perpetually absorbed, producing that dark colour of venous blood which we have just noticed.

The blood, moreover, by the change it undergoes in the lungs, appears to be the great source of animal heat. Exposed to the action of atmospheric air, it is believed to imbibe either latent caloric or oxygen, and gradually to evolve this in its progress through the frame, for the maintenance of the animal temperature. Blood is requisite to the actions of the nervous system, for if vascular communication be destroyed, the sensibility of a member is lost. And lastly, from this fluid the secretions are derived. Bile, urine, gastric-juice, saliva, serous exhalations, &c., however different in appearance and character, all have the blood for their common source.*

The formation of the blood is a process imperfectly understood. We know that food is digested and chyle formed; that this milky fluid is carried into the blood; that other liquids, thinner, and in larger quantities, are also absorbed, and rapidly transmitted to the circulatory system; and we believe that these are the two principal sources of supply. Wherever sanguification be completed,

* The animated *Bordeu* speaks of the blood as "une masse de chair fondue ou coulante, une sorte de gelée, un amas de suc nourricier semblable, à bien des égards, à la partie d'un œuf qu'on appelle le blanc, mais qui au lieu d'être contenue, ainsi que cette portion de l'œuf, dans les cellules qui se communiques les uns aux autres, c'est dans des vaisseaux, et leur dernières ramifications, et dans le tissu spongieux des parties."—*Analyse Medicale du Sang.*

the materials seem long in the course of preparation. Chyle taken from the lacteals coagulates; and the fluid in the absorbents approaches in character to the serum of the blood.

The temperature of the blood in its living vessels is generally the same as that of the rest of the body. There are, however, slight varieties in the contents of arteries and veins, and more considerable changes produced by disease. Blood flowing from a vein on the bulb of a thermometer raises the mercury to 92°.

The quantity of blood circulating in the human subject has been variously computed by philosophers. *Keill* estimated it at 100lbs.[α] others do not believe it exceeds 8lbs.[β] *Haller* computes it at 28 to 30; [γ] *Young* at 40; [γ] and in *Sir A. Cooper's* Lectures, its proportion to the solids of the body was considered as 1 to 16 or 20.* The wide

α "Rolfinius xxx libris totam massam æstimat, alii xxv, alii xx, alii xv. Cl Harveius vix decem pondo Sanguinis quantitatem esse censet, Moulinus ad plurimum octo." (p. 2)—" Adeoque si decem libræ pro ossium solido, et pro pinguidine totidem plus septem a proposita sanguinis quantitate auferantur, restabunt in corpore ponderis 160 librarum sanguinis ad minimum libræ centum." (p. 22.)—*Tentamen Medico Physica authore* Jacobo Keill, *M.D. Londini,* 1718.

β Blumenbach, *Instit. Physiol. p.* 6.

γ *Elementa Physiol.*—" The mass of the circulating fluids will be at least fifty pounds." Haller's *Physiology,* by Cullen, I. 84. Dumas calculates the quantity of blood passing through the heart at an average of twenty-five pounds. *Principles de Physiologie,* III. 377. *Paris,* 1800. Ed.

* On bleeding a young ass to death, of the weight of 79lbs., *Percival* obtained 5½lbs. of blood. Thus the animal perished from a loss of about one-fifteenth of its weight; and as it must have retained much blood in the capillaries, probably as much as it lost, the estimate of one-fifteenth to one-twentieth, for the total proportion of blood in the animal, must be too low.

[There is a misconception here on the part of the author: I have given the passage precisely as it stands in his M. S., and a similar assertion is attributed to Sir Astley Cooper, in the printed text of the first edition, but I must exercise the privilege of remarking that Mr. Thackrah has fallen into an error which pervades the calculations of many other physiologists as to the quantity of blood in the body. Some of the averages apply to the proportion of blood difference in these estimates seems to depend on the want of such data as are requisite for accurate research. It is true, indeed, that the contents of the principal arteries and veins may be subjected to calculation, but of the blood circulating in the capillaries no accurate estimate can be formed. When we reflect on the minuteness of these capillaries, on the universality of their distribution, and on the large proportion which they constitute of muscle and other solids; when we remark, also, the red colour which the flesh of a slaughtered animal retains, we must refuse to admit any estimate formed from the quantity of blood drawn off in fatal hæmorrhage. The blood, moreover, cannot be obtained in toto, without the admixture of its secretions. If we collect the blood of a slaughtered animal, our estimate is invalidated by the changes which take place during the period, and especially by the water, or a serous fluid, which is largely absorbed into the circulation during hæmorrhage. But if we cannot precisely ascertain the proportion of blood, we may form some opinion of the amount of the circulating fluids, by macerating the slaughtered animal till the red colour is nearly lost, and deducting the loss by hæmorrhage and maceration from the whole weight. This, however, can only be considered as an approach to the truth.

If a drop from crassamentum, or coloured serum, be placed between two pieces of glass, and examined with a powerful microscope, we see an interesting arrangement

only, while others include all the animal fluids in the computation. The statement of Sir A. Cooper refers not to the absolute amount of blood, but to the proportion that may be withdrawn before death ensues. " The proportion of blood, compared with the solids, which can be drawn from an animal before it dies, is about one pound to sixteen."—*Lectures on Surgery, No. IV.* It is rather surprising that an investigator, so generally accurate as Mr. Thackrah, should have been thus incorrect: especially as an experiment of his own is widely at variance with these calculations. See Appendix II. Exp. I. Ed.]

of globules, of figures sometimes round, sometimes ovoid, and white or red, as the colouring matter is separated or adherent. Blood, however, exposed to the microscope as soon as it is drawn, and consequently before coagulation has taken place, does not, so far as I have observed, present the appearance of globules so distinctly as blood which has been kept. The globules frequently arrange themselves into figures resembling those of the kaleidoscope, varying with the slightest changes in the concentration or diffusion of the particle placed under examination. In some specimens of blood we may remark, in addition to the stationary globules, numerous smaller bodies of a roundish figure, constantly in motion, and curiously revolving in limited circles. These appear to be animalculæ; but as we have not found them in recent blood, I suspect them to be the produce of incipient decomposition.* *MM. Prevost* and *Dumas*, on applying galvanism to the white of egg, found the matter at the positive pole to contain globules similar in form and arrangement to those of blood.[α]

The size of the globules of the blood has been variously estimated at $\frac{7}{1500}$ of an inch in diameter, $\frac{1}{3500}$ths, $\frac{1}{4000}$ths, and $\frac{1}{4500}$ths: *Wollaston* computes them at $\frac{1}{4900}$; *Young*, $\frac{1}{6060}$ths; *Hodgkin* at $\frac{1}{3000}$ths; while *Sir E. Home* and *M. Bauer* state the diameter of a globule, devoid of its colouring matter, to be $\frac{1}{2000}$ths.[β] The globules are soluble in alkalies, acids, and alcohol, but not in serum. They

* Some French physiologists are inclined to consider the globules themselves as animalculæ, "*Infusoires du Sang*"!

α *Annales de Chimie*, xxiii. 53, and *Bibliotheque Universelle*: Juillet, 1821.

β BLUMENBACH'S *Physiology*.—WOLLASTON in *Philosoph. Transact*, 1810. —YOUNG'S *Med. Literature*, Lond. 1813, p. 555.—KATER *Phil. Trans.* 1818, 187.—HOME and BAUER, *Ibid*, 1818, 173, and 1820, 1. For a further discussion on the globules of the blood, see chapter viii. ED.

appear to dissolve readily in water, and tinge it with their own peculiar colour; but Dr. Young found that each globule remains entire, though colourless.

The form of these particles has been a subject of dispute with philosophers. *Leeuwenhoek*, *Hewson*, and *Cavallo* have been warmly engaged in this controversy; and though each determines their figure from microscopical observations, each gives a different account of what he beheld.[α] *Leeuwenhock* represents the globules as circular when at rest, and eliptical when in motion, and states that each principal globule is composed of six minor and separable globules, and that every minor globule consists of six others still more minute.* *Haller* and *Senac* compare the red particles to lentils; yet the former, in another part of his works, asserts their spherical form, and doubts their change on motion. *Hunter* considered them as globular and similar in size in the same animal. According to *Hewson* and *Wells* they are flat, with a vesicle in the centre, containing a solid substance: according to *Cavallo* they consist of double spheres: by *Blumenbach* they are considered as globular, when at rest, and when in motion, oval. *Prevost* and *Dumas*, *Home* and *Bauer*, have stated the globules to be solid bodies, with each a peculiar envelope. More recently *M. Raspail* has asserted that they vary in diameter in different vessels of the same individual; and *Dr. Young* and *Dr. Hodgkin*, the latest observers, represent these particles to be like

α See Leeuwenhoek in *Phil. Tr.* 1674, 23,121; 1723, 436; Hewson's *Exp. Inquiry*, London, 1780; Cavallo *on Fictitious Airs*, London, 1798.

* Leeuwenhoek, it appears, was in the habit of examining his own blood with a microscope, to determine his state of health. A darker colour than natural he attributed to the excessive congregation of the globules; and to correct this state he drank four cups of coffee to breakfast, instead of two, and six of tea in the afternoon, instead of three: all, too, as hot as possible.

flattened cakes, thin and depressed in the centre, and thicker towards the circumference.[a]

How far discrepancies in these statements with reference to the globules may depend on different states or morbid conditions of the blood, and what bearings more accurate, extended, and contrasted experiments would have on physiology and pathology, are questions not undeserving further inquiry.*

While circulating in its vessels, the blood is, generally speaking, a homogeneous fluid; but, on its removal from the body, a thin film forms on its surface, it concretes, and becomes a mass like stiff jelly. From this, after a time, spring globules of a transparent liquid; and the whole gradually separates into two parts, one a pale yellowish fluid, denominated *Serum*, the other a thick red cake, termed *Crassamentum*. When the quantity of blood is large, the crassamentum usually floats in the serum.

The period at which concretion takes place depends upon circumstances that will be afterwards considered, but the common time is from three to eight minutes after

a *Philosoph. Mag.* and *Annals of Philos.* II. 131.

* In the *Revue Medicale* for 1825 is an account of some researches and deductions by *M. Schultz*, who particularly adverts to an intestine motion in the circulating blood observed by the microscope. " Au premier aperçu, tout l'interieur, du courant sanguin soumis à un grossissement médiocre, parait être dans un mouvement uniforme de tremblottement. Après avoir grossi davantage, on appercoit la masse du sang divisée en une infinité de corpuscules exerçant les uns sur les autres l'action la plus vive, de telle sorte qu'ils s'attirent réciproquement, se confondent ensemble et se séparent ensuite, en sorte qu'ils se détruisent et se reforment d'eux-mêmes. A peine veut-on suivre un de ces corpuscules, qu'il a déjà disparu, et qu'on en voit paraître un autre à sa place, dont l'existence ne dure pas davantage. On ne peut assigner la forme de ces corps : il n'y a de fixe en eux que le mouvement ; c' est le mouvement seul qui constitue leur existence." * * * " Le but de l'auteur est de faire voir que le sang est doué de vie, de démontrer que cette vie du sang est le résultat d'une opération tout-à-fait différente des opérations galvaniques et chimiques, et de faire connaître comment elle se manifeste d'une manière immédiate."—*Revue Med.* Tom. I. (1825) 136-8.

the blood has been taken from the body. The subsequent effusion of serum is effected generally in from one to three hours: but frequently a much longer time is required, the blood being found but partially coagulated at the end of twenty-four. And even when the separative change appears to take place in the usual time, serum will often exude from the crassamentum for several days, especially if the fluid be repeatedly poured off from the coagulum. After being exposed to the air for some time, the exterior of the floating crassamentum assumes a florid hue. This change appears to arise from the action of the atmospheric oxygen, and to resemble that produced in respiration. It is observed, however, that if the cake be not kept moist in the serum, instead of becoming scarlet the colour is dark brown. After the blood has remained two or three days in a temperature of 60°—70° putrefaction commences, ammonia is evidently evolved, and the crassamentum appears to be dissolved in the serum. By the continuance of this process, the blood is soon reduced to a black fœtid mass.

From the difficulty of accounting for the changes which the blood undergoes on its removal from its vessels, a celebrated physiologist started, or rather revived, the opinion of its possessing life.* The ideas of *Mr. Hunter*, he

* The idea of the blood's vitality was entertained by the ancients. In many passages of the writings of HIPPOCRATES, the connection of the blood with the soul or spirit is clearly intimated.

"Ηγεομαι δε εμπροσθεν μηδεν ειναι μαλλον των εν τω σωματι ξυμβαλλομενωνες φρονησιν αν, η το αιμα. Τουτο δε οταν εν τω καθεστηκοτι σχηματι μενη και η φρονησις, εξαλλασοντος δε του αιματος, μεταπιπτει και το φρονημα."—*De flatibus.*

The illustrious HARVEY also held this doctrine, and expressed it in decided language:—" Habet (scil. sanguis) profecto in se animam primo ac principaliter, non vegetativam modo, sed sensitivam etiam et motivam: permeat quoquoversum, et ubique presens est, eodemque ablato, anima quoque ipsa statim informs us,[a] first arose from observing that the yolk of the egg resisted putrefaction, though exposed to a continued temperature of 103 degrees.

That the egg has "the power of self-preservation, or in other words, the simple principle of life," will be readily admitted, but a question remains, does this principle reside in the yolk, or the albumen, independent of the chick? For the chick is an organized animal, remaining enveloped in these substances till the stimulus of heat brings it to maturity.

The argument drawn from mortification taking place in a member, when the circulation is destroyed, only proves that the blood is necessary for animal life. So is aliment. If we remain for a certain time without food, death ensues; yet no one asserts that the food we eat possesses vitality.

Hunter also advances, in support of his theory, the union of wounds, observing, that extravasated blood and adhesive matter become vascular. Yet, though the extravasated clot and adhesive matter effused for the reparation of injuries be formed from the blood, organization does not originate from their spontaneous action, but from the elongation of the surrounding vessels, which branch out into the deposit, as the roots of a tree extend themselves in the earth. He laid much stress on the idea, that if blood were not alive, "it would be in respect to the body as an extraneous substance." If there exist in the animal structure certain canals, which are destined to convey peculiar fluids, these fluids, I conceive, cannot be considered as foreign to the vessels which contain them. Upon Hunter's principle, more-

tollitur: adeo ut sanguis ab anima nihil discrepare videatur; vel saltem substantia, cujus actus sit anima, estimari debeat."—*De Generatione.*

[a] HUNTER *on the Blood*, p. 78, et seq London, 1794.

over, the lymph is as truly endowed with vitality as the absorbents which convey it.

Mr. H. remarks also, that blood taken from the arm in the most intense cold which the human body can bear, raises the thermometer to the same height as blood taken in the most sultry heat; and he conceives, that since living bodies alone have the power of resisting high and low degrees of heat, the blood, from possessing this property, must likewise possess vitality. It should be remembered, however, that life may maintain its temperature in the system at large, yet particular parts maintain *their* temperature only by contact. The various secretions have the same heat as the glands or muscles; the urine, for instance, on flowing from the urethra, raises the thermometer to 98°. Yet surely this physiologist would not have asserted the vitality of urine!

Another difficulty attends Mr. Hunter's theory. If the blood possesses life, where is this property first acquired? We know that blood is formed from the digestion of various inanimate substances. Is the chyle alive? He thought it was. Admitting the supposition, we must ask, is the chyle endued with vitality, or does our food, by commixture with the salivary and gastric juices, acquire its living principle? If in *vitality* be implied an *independent power of motion*, we should expect that the circulation of the blood was spontaneous, and that its properties were of an active kind. But of such inherent faculty we have no evidence. We observe a simply passive motion of this fluid; the heart impels it, the vessels convey it, the glands act on it.

It may be urged, however, that the support of life must be living. Is then vitality alike in arterial and in venous blood? The life of the body appears to be supported by the former, not the latter. Is arterial blood alive, and venous dead? And is the circulation consequently, as *Dr. John Davy* remarks, "a perpetual miracle, in which material particles are without cessation dying and reviving?" [a] If so, venous blood must receive the spirit of life in the lungs from the air inhaled, and the general atmosphere, on the principle of the argument, must be a living fluid.

Is the sap of vegetables a living fluid? Like blood it maintains its character in its vessels, but loses this character when abstracted.

Perhaps the argument drawn from the coagulation of blood is the most plausible of those adduced in favour of Hunter's opinion.

It has been stated that if blood, before concretion commences, be exposed to a low degree of temperature, it freezes; if it then be placed in a higher temperature, it thaws; and in from four to six minutes, it coagulates. If a piece of muscle, taken from an animal killed by violence, be frozen, and afterwards thawed, it contracts. In both cases, it is evident, that freezing suspends, but does not prevent the natural action. Hence there appears a marked resemblance between the death of a muscle and the coagulation of blood. On examination we are surprised that so curious a statement should have been admitted and repeated with so little inquiry or trial. Experiments in the third chapter of this work, on the effects of temperature on coagulation, will show that the statement of Hunter just alluded to is incorrect or fallacious.

What, after all, is Life? Does it admit of degrees? Can we, in our present state of knowledge, reason safely, and arrive at a satisfactory conclusion on the subject of fluid vitality? If not, we must refuse to admit, though we may not deny, the doctrine of Hunter.

[a] *Edinburgh Med. and Surg. Journal*, xxxi. 22.

CHAPTER II.

CHEMICAL QUALITIES OF HEALTHY BLOOD.

THE SPECIFIC GRAVITY of blood has been stated at 1041,* 1030-55,† 1050,‡ 1054,§ 1053-1126,α 1055-1061,‖ 1059,¶ and at 1527.* The last number is, of course, widely erroneous. In reference to the others, much variety may have arisen from the state of the system at the time when the blood was taken; and in most of the instances the subjects of depletion were probably diseased. My own examination of specimens of venous blood from six persons *in ordinary health* gives the specific gravities 1024, 1030, 1042, 1048, 1053, and 1054. We may therefore rate the mean gravity of healthy blood as nearly 1041.

THE SOLID CONTENTS of the blood have had rather more accordant estimates. *Berzelius* and *Marcet* state the fluid parts to be in the proportion of 900 to 1000;

* *Hist. of the humane blood by the* HON. ROBT. BOYLE, London, 168¾.

† PROUT, *on Diabetes.*

‡ BLUMENBACH, *Institut. Physiol.*; also TURNER'S *Chemistry.*

§ JURIN, *Philosoph. Transactions.*

α HENRY'S *Chemistry.*

‖ WHITING, *Disputatio Med. de sanguine ægrorum.*

¶ DENIS, *Sur le sang humain*, Paris, 1830.

* HALLER, *Elementa Physiologiæ.*

and *Bostock* averages them at 880 to 1000.[a] *Dr. Whiting* ascertained the ratio of *healthy* blood to be in one case 770, and in another 794 to 1000. In six *healthy* specimens which I examined, the numbers were 763, 782, 784, 804, 807, and 845. The solid contents may be averaged, therefore, at very nearly one-fifth of the mass of healthy blood.

In stating the general properties of blood we noticed its spontaneous separation into a yellowish fluid,—serum, and a red clot, crassamentum. The proportion which the former bears to the latter varies according to the state of the system, and the time allowed for exudation, but its medium in health is usually as 10 to 13 or 14,—1 serum to 1.3 or 1.4 crassamentum.[β]

SECTION I.—SERUM.

The specific gravity even of healthy serum appears to vary considerably with the states of the body as affected by diet, and other circumstances. It has, however, a general accordance with the specific gravity of the whole blood. Without adverting to the statements of *Boyle* and other older experimentalists, whose estimates are open to objection, I would mention only that of *Dr. Marcet*, who gives the medium at 1029.5; and that of *Dr. Bostock*, who states

[a] *Medico-Chirurg. Transac.* II. III. M. Le Canu, whose standard analysis of blood is generally received as the most accurate, gives the proportion of water as 780.145 in 1000.—*Journal de Pharmacie. Sep.* 1831, *p.* 502.

[β] Dr. O'Shaughnessy states the usual proportion to be 57 serum to 43 crassamentum.—*Report on the Chemical Pathology of Malignant Cholera*, p. 22.

Dr. Babington, in a highly interesting paper in the *Medico-chirurgical Transactions* xvi. 297, proves that blood as it flows in its vessels essentially consists of two portions, a colourless fluid which he has named "*liquor sanguinis*," and red globules. When taken from the body, the liquor sanguinis separates into serum, and a clot, in which the red particles are suspended, viz. crassamentum. Ed.

it to be 1023. My own examination of six specimens of healthy serum, from which little evaporation could have taken place, produced 1015, 1018, 1021, 1028, 1032, and 1033. The average, therefore, may be stated as 1020 to 1030.

Serum is principally composed of albumen and water; but it contains also carbonate, sulphate, and muriate of soda; muriate and sulphate of potass; phosphates of soda and of lime, and a little impure acetate of soda.[α] Serum largely absorbs carbonic acid. On agitating a small quantity in three inches of carbonic acid gas, one inch quickly disappeared. The proportion of albumen in serum is, on the average of our experiments, 42 in 1000; that of saline matters less than 1 in 1000. Serum is said to contain a free alkali, which, according to *Berzelius*, *Marcet*, and *Bostock*, is soda. Turmeric paper dipped in this fluid quickly becomes of a light brown colour, as we have found in innumerable trials. *Dr. Stevens* has recently asserted that this is only observed when the fluid has exuded for some time.[β] But we have found that when serum has been tested fifteen minutes after the abstraction of the blood, it has produced as marked a tinge of brown as another specimen taken from the same blood at the end of twenty-four hours; and in a second observation, that the first drop of serum which exuded from sheep's blood produced a tinge on turmeric paper not to be distinguished from that which serum subsequently gave after standing twenty-four hours on the crassamentum. A third experiment, on human blood, exhibited a similar result.

α M. Boudet has detected in serum an alkaline soap, chloresterine, and a fatty substance which he calls *seroline*.—*Journal de Pharmacie, June*, 1833. *Edinb. Med. and Surg. Journal, April*, 1834. *p*. 489. Ed.

β Stevens, *On the Blood*, *p*. 23.

The solid contents left by evaporating serum are stated by *Dr. Bostock* to be 12 per cent. My examination presents a different result: 4.4 per cent. is the average of our experiments on the serum of healthy blood. My observations, moreover, shew that the last effusion of serum from the crassamentum contains a much greater proportion of albumen than the first.*

A temperature of 150°—160° quickly reduces serum to a coagulum, which is principally albumen. From coagulated serum a small quantity of fluid may be pressed, which has unfortunately obtained the name of SEROSITY. It is water holding in solution about one-fiftieth part of albuminous matter, with a considerable proportion of alkaline salts. In two cases which we examined, the solid contents of the serosity were ·55 in 1000.

The mineral acids readily coagulate serum, and also coagulate what is not in the first instance affected by heat, the albumen of the serosity.

Sometimes serum presents a milky appearance. The cause of this phenomenon has long been a subject of doubt, and is still not satisfactorily explained.α

SECTION II.—CRASSAMENTUM.

The crassamentum (*cruor* or *clot*) has generally been said to consist of red particles and fibrine; and an important ingredient, one indeed which constitutes the largest proportion, has been overlooked, or but slightly noticed.† Albumen is the principal constituent of crassamentum.

* See experiment 2, in Appendix II.

α See Chapter VII.

† DR. PROUT in his chemical account of the blood, speaking of the crassamentum, says—"By a further examination it is found that this clot consists of a mixture or compound of two principles—the *solid red particles* above mentioned, and a white elastic fibrous matter, termed *fibrin*. (*On Diabetes*, page 2.) No mention is made of albumen. In the last edition of HENRY'S

When serum has apparently ceased to exude from blood, the solid mass on pressure yields, besides the fibrine, a tenacious red fluid. A substance similar, though in a diluted state, is more readily obtained by washing the crassamentum in water. The matter thus pressed, or washed, is promptly coagulated by mineral acids and by prussiate of potass. Under a heat of 150°—160° flakes are formed in considerable quantities, resembling those afforded by serum or diluted yolk of egg. The coagulum thus produced is taken up by a solution of caustic potass, and precipitated again on the addition of acids.

In these principal circumstances and qualities it is apparent that the pressings and washings of crassamentum

Chemistry we find this statement: "*Crassamentum* or Clot is resolvable into two parts, viz. what has been called coagulable lymph, or *fibrin*, and *red globules*." (*Edit.* 1829, p. 455.) In ANDRAL'S *Pathological Anatomy*, in which the subject of the blood is particularly discussed, the following passage occurs:—"The fibrine may be altered either in quantity or in quality. In the first place there are cases in which this principle is more abundant than usual, or at least in greater proportion relatively to the water and albumen. In such cases, the blood, when drawn from a vein, forms in the vessel that receives it a clot, with little or no serum. These are, however, to be divided into two classes. In the first class of cases, the fibrine constituting the clot still contains a pretty large quantity of serum, which may be separated from it by pressure; in these the coagulum has but little density. In the second, on the contrary, the clot is very dense, and a little fluid albumen can with difficulty be squeezed out of it. In the first class the relative increase of quantity of the fibrine is only apparent; in the second it is real."—(*Transl. by Drs. Townsend and West, Vol. I.* 645.) This eminent pathologist evidently considers the crassamentum to be principally composed of fibrine, whereas, in fact, this substance forms but 5 or 7 parts in 1000! The error pervades the reasoning of M. Andral in subsequent pages of his work, and vitiates some interesting speculations.

[I have pleasure in adding another quotation from one of the authors mentioned in the foregoing note. However slightly DR. PROUT may have accounted albumen in his former work, he has amply compensated for the omission in a recent publication, where he remarks that "The chief constituents of the blood are essentially albuminous. Blood contains albumen in three states of modification: namely, *albumen*, properly so called; *fibrin*; and the *red particles*."—*Bridgewater Treatise, No. VIII.* 518.—ED.]

agree with serum. There are, however, some slight points of difference. The former yield to chemical agents a darker colour, and more flocculi, than serum coloured by red particles, and brought to an equal specific gravity. This may be attributed to the greater quantity of colouring matter in the washings of crassamentum, and perhaps also to modifications in the state of its albumen. They do not, it is obvious, prevent our admission of albumen as the largest constituent of crassamentum.

The peculiar colouring matter of blood is so intimately combined with this albumen, that no artificial means of complete separation have been discovered. We have tried the methods of *Vauquelin*, *Berzelius*, *Brande*, and *Engelhart*, but have found them all to be imperfect. The statement of *Engelhart*, that the contents of crassamentum, after the removal of the fibrine, coagulate at 140°, while 150°—160° is required to produce that effect on serum,[a] materially differs from the results of my experiments;* neither are the plans of *Denis* and *Le Canu* satisfactory.†

a *Jamieson's Journal*, Oct. 1826. 314.

* Experiment.—Fluid squeezed from a clot of crassamentum into a gallipot was placed on a water bath, and the bulb of a delicate thermometer introduced into the liquor. On heat being applied, the thermometer rose to 140,° and was kept at that temperature for five minutes, but there was no appearance of coagulation. Heat was again applied to the bath, and the temperature of the liquid in the gallipot was maintained for five minutes at 150°, but still it did not coagulate. When the thermometer rose to 160° coagulation commenced, but was not wholly completed in eight minutes. Washings of crassamentum were experimented on with a similar result.

† The experiments above alluded to have appeared repeatedly in periodicals and systems of chemistry. The work of M. Denis is less known in this country, and I shall therefore make a quotation from it. Referring to the methods of Brande, Vauquelin, and Berzelius, this author says—" J'ai en vain faché de modifier ces procédés, pour les approprier au genre de mes recherches. Rebuté par le peu de succes que j'ai obtenu, j'ai essayé de trouver quelque moyen de suppléer à l'imperfection de l'art. On sait que l'albumine du serum ne se coagule qu' à 74 cent., (165° Far.) et que le serum

Chlorine removes the colour from a mixture of crassamentum and water, and appears to precipitate clear albumen, but the subsequent dissipation of the gas leaves the mixture again of a dark red hue.

Continental chemists have applied to the colouring matter of the blood the name *Hæmatosine.* This term it is convenient and proper to adopt, if we restrict it to the colouring matter, and do not include the albumen with

etendu de 20 parties d' eau ne laisse plus précipiter son albumen à ce degré tandis que l' hématosine est precipitée par la chaleur de 65° à 68° cent., (150°—155° Far.) quand même on l'a dissouté dans une énorme quantité d'eau. J'esperais, suivant ces donnés, qu'en chauffant du sang dépouillé de fibrine et étendu de 40 parties d'eau, j'obtiendrais un précipité d'hématosine à 65° ou 68°; mon attente fut trompée, il ne se fit aucun précipité, même à 75°, 80°, 85°, 90° cent. (167°—195° Far.) Le tout devint seulement troublé et couleur café au lait." Again, a few pages further on M. Denis observes—" Comment utiliser le procédé que je propose, pour apprécier la quantité d' hématosine? Bien séparer au moyen de la pipette, le serum du caillot qui on est recouvert; laisser ce caillot exsuder tout son serum, et enlever ce dernier à mesure qu' il apparait, le caillot étant aussi privé de serum qu' il est possible, on le lave et le malaxe dans un nouet de linge, jusqu' à ce qu' il soit décoloré. Alors on chauffe le liquide de lavage a 68 cent. (155° Far.); mais il est nécessaire d'élever la temperature à 70° ou 71°, (158½° or 160°) pour terminer la precipitation. Avant d'evaluer la quantité d'hèmatosine ainsi obtenue, il faut la faire bouillir dans une certaine dose d'alcool, et l'incinérer. On défalquera alors de son poids ce qu'en a enlevé l'action de l'alcool, et le poids de cendres formées. On aura ainsi au juste la quantité d'hématosine pure que l'on peut raisonnablement croire exister dans le sang."—*Recherches Expérimentales sur le sang humain*, p. 92. 96. Still we have but coloured albumen. My talented friend Dr. O'Shaughnessy gives and improves Le Canu's process, which he details as follows:—" Divide 1000 parts of the clot into two parts; dry one, and estimate the loss; wash the other to ascertain the quantity of fibrine. As the water given off from the first exists therein *in the state of serum*, by subtracting the due proportion of solid matter, (which the previous analysis of the serum has shewn to be contained in 1000 parts thereof) in the interposed serum, from the weight of the dried clot, the difference affords exactly the weight of the fibrine and colouring matter together. Subtracting again from this the weight of fibrine, determined by another experiment, the amount of the colouring matter is accurately obtained. Incineration, finally, of a given weight of the crassamentum, gives the quantity of saline matter. *Journal de Pharmacie, Sept.* 1831. *Appendix No.* iv. *to* Dr. O'Shaughnessy's *Report on the Chemical Pathology of Malignant Cholera.* We obtain, however, only hæmatosine mixed with albumen.

which this matter is combined. The nature of hæmatosine has been disputed. Iron has been considered its base. *Brande*, however, could not detect this metal in the blood in solution; and we have not found, as some have asserted, that the red particles, that is, the dried red mass from crassamentum, is attracted by the magnet. Still *Berzelius* and *Engelhart* appear unequivocally to have demonstrated that iron exists in the ashes of the blood;[a] we must therefore admit the supposition that this mineral is a constituent of blood, though probably in some form or combination as yet unknown.* As the quantity, however, is much too small to produce the red colour of blood, I am inclined to refer this effect to a peculiar animal substance united with albumen. Hæmatosine is the only constituent of blood which is not found in the solids or secretions of the body.

We have thus examined two of the ingredients of crassamentum—Albumen and Hæmatosine: Water is a third; and the fourth and last is a substance peculiar to the clot—Fibrine.

If recent blood be stirred for a few minutes, an adhesion of the coagulating lymph to the rough surface of the stick takes place; and this flaky substance, losing on

[a] Engelhart's *Gottingen Prize Essay* for 1825.—*Jamieson's Journal*, Oct 1826. 314.—Berzelius in *Med. Chir. Transactions III.*

M. Rose is said to have obtained *three grains* of metallic iron from a pound of healthy blood.—*Meyer's Physiologie*, quoted by *Rudolphi*, 137. This statement may remind the reader of a suggestion which the editor recollects to have met with in one of the periodicals. It recommended the adoption of funeral pyres instead of our usual sepulchral interment, in order that the ashes being subjected to chemical process, medals might be cast from the iron contained in the blood of the deceased, and worn by their surviving friends! Ed.

* "Quoi qu' il en soit, je pense qu' il faut admettre dans l'état actuel de la science, que le fer oxidé ou dans un ètat salin encore inconnu, est repandu dans la masse du sang, et qu' il est entraîné par l' hématosine, pendant la coagulation de cette humeur."—Denis *Sur le sang, p.* 100.

repeated washing, its red particles, shews distinctly and beautifully the fibrine which forms its basis.* It has the appearance of white strings laid in striæ, and resembles in form and distribution the planes of muscle which encircle the bladder. Fibrine may be generally obtained by enclosing crassamentum in a bag of linen, and pressing and washing the mass till a stringy substance alone remains. This is fibrine, combined with a small proportion of oil, albumen, and saline matter; all which may be removed by further washing in water and boiling in alcohol. Fibrine consists of azote, carbon, and oxygen; the first of which is found in it in greater proportion than in any other animal substance. There exists also, according to *Hatchett*,† some trace of albumen.

The proportion of fibrine in blood is stated by *Berzelius* at ·75 to 1000, but *Whiting*, from his experiments, concludes it to be from 1 to 2 in 1000. Our experiments give an average of 2·8 in 1000. The sp. gravity of fibrine, according to *Davy*, varies from 1046 to 1060. *Prout* and others have considered it to be less than that of serum, " since fibrine usually swims in the serum." The simple experiment, however, of dropping some *pressed* fibrine into serum shows at once the sinking of the solid, and the consequent error of this opinion. That an atmosphere of oxygen gas produces little effect upon fibrine is illustrated by Experiment 3, Appendix II.

In the circulatory blood, fibrine is either held in solution, or exists, according to the observation of *Bauer*, in the state of very minute white globules. It is well known to form the basis of muscle; and so nearly does it approach

* MALPIGHI first described this interesting appearance:—" Contexturam namque fibrosam, et quasi nerveis fibris compaginatum rete videbis, in cujus exiguis excitatis spatiis et sinibus, veluti cellulis rubicundus stagnat ichor." *Opera*, Lond. 1687. p. 313.

† *Phil. Trans.* 1799. 1800.

to organized matter, that the galvanic aura increases its contraction. It affords the frame work of the body, and that also of the preternatural structures resulting from disease. Coagulable or plastic lymph is a combination of fibrine and albumen.*

Crassamentum is generally observed to be of a bright colour at the surface, and darker below. This has usually been attributed to an action of the atmospheric air, the oxygen of which subtracted the carbon from all that part of the clot which was exposed to its influence. *Dr. John Davy* has controverted this opinion, and maintains that the atmosphere produces this effect by altering the sp. gravity of the hæmatosine, which consequently subsides in the clot, and leaves the surface of a brighter hue.α This author states that if, to exclude the air, venous blood be received as it flows in white of egg, milk, or serum, the surface of the coagulum will be comparatively florid.

Venous blood, when shaken in atmospheric air, becomes scarlet, and retains this colour for several hours. This fact *Dr. Davy* attempted to refute, and asserted that no sensible change took place either in the hue of the blood, or in the quantity or character of the air, unless after the blood had been drawn for many hours, and when he conceives decomposition to be advancing.β

* It is remarkable that Berzelius considered fibrine and albumen as similar substances. (*Medico-Ch. Trans.* III. 232.) Assuredly they consist of the same elementary principles, oxygen, hydrogen, azote, and carbon, though in different proportions: but so do all animal substances. The dissimilarity between albumen and fibrine is marked. The latter is obtained by washing with water; albumen is dissolved: fibrine is not affected by the temperature which coagulates albumen. By mineral acids fibrine is dissolved, albumen rendered concrete. It is true indeed that the coagulum which mineral acids produce in fibrine has properties very similar to those of the coagulum of albumen, but this will not be generally admitted as establishing their identity.

α Experiment to the contrary, Appendix II. 12.

β Dr. J. Davy, *in Edinb. Journal,* xxxiv. 243.

Dr. Stevens has cleared up much of the perplexity arising from these

Venous blood, brought in contact with oxygen, has been considered, ever since the statement of *Priestley*, to become red, and to absorb the gas. This again *Dr. Davy* denies, and attributes the affirmative assertion to the use of blood in which decomposition had commenced. He avers that oxygen agitated with *fresh* blood, and the temperature regarded, does not lose any of its bulk. *Dr. Christison* has maintained the former doctrine, and established it by minute and careful experiments, in which he regarded both the freshness of the blood and the temperature of the air.[a] He found in every experiment that the air was sensibly diminished in volume, and that carbonic acid was formed; the latter at the expense of the oxygen in the former. This point is important in reference to the physiology of respiration. It tends to show, that how great soever the agency of the nervous system in that function, the change which takes place in the colour of the blood is essentially a chemical process.

SECTION III.—REMAINING CONSTITUENTS.

Having examined the more obvious and important chemical principles of blood, we now have to notice the elements, more minute in amount, and some of doubtful or only occasional existence.

An oily substance, named by *M. Denis* "GRAISSE PHOSPHUREE ROUGE," and "GRAISSE PHOSPHUREE BLANCHE," has been noticed by *Berzelius* and other chemists. It appears to be identical with the "Matière

opposite statements, by showing that the florid colour of the clot is attributable rather to the action of the saline ingredients of the serum, than to any of the causes stated above. See his *Essay on the Blood*, p. 10 et seq. Mr. Thackrah's experiments on this point are recorded in Appendix II. Nos. 4 to 9. ED.

[a] *Edinb. Med. and Surg. Journal* xxxv. 94.

cerebrale rouge," and the "Matière cerebrale blanche," which *Vauquelin* discovered in the brain.* In addition to the ordinary elements of animal matter, this oil is found to contain phosphorus and sulphur. It is obtained by exposing the dry contents of blood to the action of æther at a moderately high temperature, and under strong pressure; or by boiling them in alcohol. On gradually cooling the solution the *graisse* subsides.

A substance of rather doubtful character is the MUCO-EXTRACTIVE MATTER of *Marcet;* the IMPURE LACTATE OF SODA of *Berzelius;* the OZMAZOME of *Denis.* It is of a brown colour, and not crystallizable, and when dried it is brittle and attracts moisture. It has, in fact, the properties of the substance which *Thenard* extracted from muscular fibre, and which he named "Ozmazome." It may be procured from blood by digestion in cold alcohol, along with the salts of sodium and potassium; and from these it may be separated by their crystallization.

Cruorine was discovered in the blood by *Mons. Denis.* It is solid, colourless, and transparent; of an agreeable odour, which *Denis* compares to that of *Caramel,* soluble in water, especially in cold water, and insoluble in alcohol and æther. It is most readily procured from fibrine, by boiling this substance dried and pulverized for five to ten minutes, in forty or fifty times its weight of water. The mixture is then to be filtered and slowly evaporated; and when the residue has been washed with hot alcohol, to remove the oil, pure cruorine will remain at the bottom of the vessel.

[The HALITUS[a] which arises on the effusion of blood, when

* M. CHEVREUL also in the 4th Vol of *Majendies Journal de Physiol. et Phathol.* gives his observations on this substance. He estimated the proportion of *Graisse phosphurée* 4· or 4·5 in 100 parts of dried fibrine.

a A lapsus pennæ in Mr. Thackrah's description of the halitus (copied from his former edition) occasions me to substitute the above remarks. ED.

chemically analysed, is found, according to *Fourcroy*,[α] to consist almost entirely of aqueous vapor, evidently holding in solution a minute portion of animal matter, which imparts to it a peculiar odour. *Boudet*[β] supposes this to be a volatile acid substance, analogous to the "graisse phosphurée" of *Chevreul* and *Denis*. *Plenk* fancied that the halitus, or as he called it, *gas animale sanguinis*, produces important effects on the animal economy.[γ] *Haller* considers it identical with the matter of perspiration;[δ] an idea remarkably coinciding with some recent medico-legal investigations, which will be further noticed in Chapter VIII. The halitus is of a more pungent and ranker odour in males than in females; it is less so in children than adults; and in eunuchs and old people it is said to be wanting.[ε] Hence it is supposed to be connected with virility.]

The results of our experiments on healthy blood, so far as regards its three chief constituents, present the following average; in 1000 parts:—

WATER	796·55
HÆMATOSINE AND ALBUMEN ...	200·1
FIBRINE.............................. ...	2·8
DRY CONTENTS OF SERUM (*saline*)	·55
	1000·00

The *ultimate or simple elements* of blood appear to be eleven or twelve in number, viz.: OXYGEN, HYDROGEN, AZOTE, CARBON, SULPHUR, PHOSPHURUS, CALCIUM, SODIUM, POTASSIUM, CHLORINE, IRON, and perhaps MAGNESIUM.

From a review of the constituents of blood can we form a conjecture on their respective uses in the animal economy? Are they elaborated in the blood for their

α *System*, by NICHOLSON ix. 185. β *Journal de Pharmacie, June*, 1833.
γ *Hydrologia*, Vienna 1794, p. 42.
δ *Elementa Physiol.* III. 38. ε RUDOLPHI'S *Physiology*. 134.

several functional offices, or is there a re-combination of their ultimate elements, their oxygen, hydrogen, carbon, and azote in every organ of secretion ? Some observations of *Berzelius* give a probability to the latter idea ; and the small quantity in the blood of some materials which exist largely in the structures of the body, further supports this hypothesis. We may, however, find some points worthy of remark in the former opinion.

Albumen appears to be the great source of nourishment; it is found in large proportions both in the solids and fluids of the system. Is it not also the pabulum whence fibrine is derived ? Salts exist in most of the solids, and abound in all the secretions : they appear to be the stimulants of living action. Fibrine is well known to be the basis of muscle : ozmazome also enters into the composition of the same structure. The phosphuretted oil appears to be the peculiar chemical element of the cerebral mass. Water, the most abundant material of blood, is, of course, necessary both to hold in solution other constituents, and itself to afford the basis of secretions. One of the most important ingredients of blood, hæmatosine, is found in no structure of the body, nor in any of its secretions. Is it a peculiar excitement of the nervous system. Is it essential to the production of animal heat ?

From questions and conjectures, however, let us turn to what is known of the changes in colour which chemical agents produce on blood subjected to experiment.

Blood, deprived of its fibrine, and shaken with atmospheric air, assumes a bright scarlet hue. Oxygen alone has a more striking effect in heightening the colour of the blood, while a portion of the gas is converted into carbonic acid. Azote darkens the blood ; and, according to *Priestley*, loses bulk in the admixture. Nitrous oxyd also darkens the hue of the blood. Hydrogen is said to remove the purple colour; but we have not observed that it has this effect. Carbonic acid gas is stated to deepen the colour of blood, but this is not in a great degree. Chlorine first renders the blood of a light brown, and ultimately destroys its colour altogether. Heat, however, by evaporating the chlorine, restores the colour to a dark brown. A solution of neutral salts, as the chloride of sodium, or the nitrate of potass, renders blood permanently scarlet. Caustic potass, on the contrary, changes its colour to a black.

CHAPTER III.

COAGULATION OF BLOOD.

In stating the general properties of blood, the process of its coagulation was noticed. We will now proceed to examine this interesting subject more at length.

The separation of crassamentum from serum is ascribed to the contraction of the fibrine, for if this constituent be removed immediately on the effusion of the blood, no coagulation takes place. When we consider the small proportion of fibrine in blood, viz. 2-4 parts in 1000, we are surprised at the phenomenon. Is not coagulation rather an effect of the attraction of hæmatosine and albumen? In the living circulation, the action of an affinity may be prevented: deprived of vital influence, the blood, like every other fluid of the animal system, becomes subject to chemical laws. It may be objected to this idea, that the removal of fibrine, as by agitation, prevents coagulation. The mechanical disturbance produced by this removal accounts for the effect. The opinion seems supported by the difficulty, not yet surmounted, of separating albumen from hæmatosine.[α]

[α] MULLER has recently published his conviction, derived from many experiments, that coagulation is, as was thought by Berzelius, a solidification of

I. The arrangement of fibrine, and the proportion which exists in different parts of the crassamentum, have been variously represented. I proceed to detail some experiments we have made on the subject.

Experiment I.

Blood was received from the neck of a stuck sheep into a long wide-mouthed bottle. At the end of twenty-four hours the crassamentum was taken out, and divided transversely into three portions, as nearly equal as possible. These were severally weighed, enclosed in muslin bags, and washed in distilled water. The fibrine in the bags was then dried ; and lastly, the washings of each portion of crassamentum were coagulated by heat, the solid matters dried on a sand bath, and their weight ascertained. The following are the results reduced to proportionate numbers :—

	Upper section.	Middle.	Lowest.
Fibrine	3·8	3·3	6·7
Hæmatosine and albumen	282·6	280·5	295·5
Water	713·6	716·2	697·8
Crassamentum	1000·0	1000·0	1000·0

fibrine which was previously dissolved in the blood. One illustration is quoted (*Med. Ch. Review, April* 1834, *p.* 501), in which the fibrine was seen to coagulate in diluted and filtered serum of frog's blood, upon its standing to rest. Raspail, on the other hand, asserts that coagulation depends on the albumen, of which he says, that the globules, as well as a large portion of the liquor sanguinis, consist, being precipitated from its menstrum in a solid form. According to this hypothesis, the particles observed in the experiments of Muller, might be coagulating albumen; but the inference is contradicted by his statement that liq. potassæ, or sulphuric æther, neither of which affect albumen, precipitate coagula. It is at variance also with Mr. Thackrah's assertion that if the fibrine be immediately abstracted, blood will not coagulate. The theory of M. Raspail is nevertheless highly important, and will be further commented on in Chap. VIII. Ed.

Exp. II.

Blood from a calf, treated as that in the preceding Exp.

	Upper section.	Middle.	Lowest.
Fibrine	9·0	6·9	8·3
Hæmatosine and albumen	185·8	161·5	174·8
Water	805·2	831·6	816·9
	1000·0	1000·0	1000·0

Exp. III.

Blood from a sheep, analysed in a similar manner.

	Upper section.	Middle.	Lowest.
Fibrine	5·0	6·6	7·3
Hæmatosine and albumen	244·6	291·1	209·6
Water	750·4	702·3	783·1
	1000·0	1000·0	1·0000

Exp. IV.

Blood from a calf exhausted by previous bleeding.

	Upper section.	Middle.	Lowest.
Fibrine	29·1	26·3	29·6
Hæmatosine and albumen	214·1	197·2	233·2
Water	756·8	776·5	737·2
	1000.0	1000·0	1000·0

The average of these observations shows *most fibrine in the lowest part of the crassamentum, and least in the middle:* a fact which obviously militates against the opinion that fibrine is the lightest portion of the blood.

From these experiments it also appears, that the hæmatosine and albumen arrange themselves as variously as the fibrine. Their proportion, as far as we have noticed, has no regular reference to any portion of the clot.

II. The crassamentum, as we have before remarked, continues to squeeze out its serum for several hours; sometimes, indeed, at the end of twenty-four the process is scarcely complete. If, however, blood be left undisturbed for a few days, it appears to undergo a change of a different kind; the solid matter increasing, and the fluid diminishing: and this before putrefaction has reduced the whole to a uniform mass; as shewn in the following,

Exp. V.

Blood was taken from a slaughtered animal, and the proportions of serum and crassamentum examined at successive periods. About two hours after death, there appeared,

	℥	gr.
Crassamentum	12 ·	5
Serum..................	20 .	45
	32 ·	50

On the second day, about the same time,

Crassamentum	9 ·	26
Serum..................	22 .	30½
	31 ·	56½

On the third day,

Crassamentum	9 ·	—
Serum..................	22 ·	32
	31 ·	32

On the fourth day,

Crassamentum	8 ·	59
Serum..................	22 ·	4½
	31 ·	3½

On the fifth day,

Crassamentum	9 ·	1
Serum (muddy)	21 ·	19
	30 .	20

On the sixth day,

	℥	gr.
Crassamentum	9 ·	16½
Serum..................	20 ·	33
	29 .	49½

On the seventh day,

Crassamentum	9 ·	23½
Serum......	20 ·	8½
	29 ·	32

On the eighth day,

Crassamentum	9 ·	30½
Serum..................	19 ·	42
	29 .	12½

On the ninth day,

Crassamentum	9 ·	32½
Serum...	19 .	12¼
	28 ·	44¾

On the tenth day,

Crassamentum	9 .	31½
Serum..................	18 ·	51½
	28 ·	23

Putrefaction was scarcely perceptible on the ninth day, but decidedly evident on the tenth and eleventh. We therefore reject the analysis of the last two days. On comparing the results of the others, we find that the serum decidedly increased for three days; was nearly stationary the next day; and on the fifth, sixth, seventh, and eighth, progressively diminished; while, of course, the proportion of crassamentum increased.

May we not infer from this experiment, that the crassamentum presses out its serum so long as the contractibility of the fibrine remains; and that when this quality is lost, or when the action has ceased, a state of relaxation succeeds, and the surrounding fluid, to a certain extent, re-enters, by attraction or affinity, the clot from which it had been expelled?

III. The evolution of heat during coagulation has been asserted and denied. *Hunter*, from his experiments on the blood of the turtle,* and more recently, *Dr. John Davy*, from experiments on the turtle and shark, decided in the negative. *Fourcroy*,† *Dr. Gordon*, and other physiologists, have on the contrary maintained the affirmative,

* "A healthy turtle was kept in a room all night, the floor of which was about 64°, and the atmosphere 65°. In the morning the heat was nearly the same. The thermometer was introduced into the anus, and the heat of that part was 64°. The animal being suspended by the hind legs, the head was cut off at once, and the blood caught in a basin; the blood, while flowing, was 65°, and when collected, was 66°, but fell to 65° while coagulating, which it did very slowly: it remained at 65°, and when coagulated was still 65°."—HUNTER *on the Blood. p.* 28. The rise of the thermometer from 65° to 66° *seems* to oppose Dr. Hunter's inference, that "no heat is found" during coagulation. We have to regret that he did not record his other experiments on the turtle. In Davy's experiments, however, on the same reptile, *no* rise of the thermometer was observed.

† FOURCROY, it appears, found the thermometer rise in one experiment 11 degrees; (*Annales de Chimie* vii. 147.) and GORDON, in one of his, 6 degrees! (*Annals of Philos.* iv. 139.)

and *Sir C. Scudamore* has adduced experiments* to show a rise of temperature at the moment of the blood's concretion. The following are examples of our inquiries on this point :—

Exp. VI.

Blood was drawn from a small bullock, with difficulty stunned :—

In $2\frac{1}{2}$ min. temp.	94°	Coagulation
4	93°	commencing.
6	92°	
8	$90\frac{1}{2}$°	
10	90°	
15	87°	

Serum beginning to exude. Temperature of atmosphere 43°.

Exp. VII.

At the same time as last Exp.; blood drawn from a small ox :—

Temperature while flowing	100°	
In $1\frac{1}{2}$ min. temp.	96°	
$2\frac{1}{2}$	95°	Coagulation commencing.

* I quote the fifth and sixth, as it is to these Sir Charles attaches the most importance.

"Exp. V. A robust young man, in perfect health, was bled purposely for sake of the experiment. One thermometer only was used, and the bottle was not previously warmed. The temperature of the blood was stationary for three minutes at 93°, and then suddenly rose to 94°, after which, the rate of cooling was as follows :—At the expiration of 5 minutes 30 seconds, 93°. 6′ 20″—92°. 8′—92°. 10′—90°. 11′—89°. 12′ 30″—88. The blood was of jelly consistence in eight minutes, and firmly coagulated in ten.

Exp. VI. This was an example of remarkably sizy blood. I immersed the bulb of the thermometer in the mixed serum and fibrin, so as to cover it completely, and held it there steadily. The temperature was raised from 80 to 81° just at the moment that the concretion of the fibrin began. For a short time before, it had been stationary, and afterwards it continued exactly at 80° for the space of five minutes."—Scudamore *on the Blood, p.* 72—73.

The thermometer gradually lowered, till at the expiration of

25 min. it was............ 75°

and at 41, ... 65°

Exp. VIII.

Blood taken nearly at the same time, from a sheep.

In 1 minute, temp.	88°	
$1\frac{1}{2}$	94°	Coagulation
3	94°	commencing.
$5\frac{1}{2}$	$93\frac{1}{2}$°	
7	93°	
10	92°	
14	90°	

As the rise of the thermometer at the commencement of this experiment seemed to be an accidental circumstance, and to depend on the previous coolness of the vessel and the thermometer, the following mode was adopted:—

Exp. IX.

Immediately on the throat of a sheep being cut, some blood was received into a vessel in which a thermometer was placed. It was shortly thrown out, and a second quantity received. As the mercury did not rise to 102°, this, too, was rejected, and a third portion taken when the animal was almost expiring. The thermometer was now at 102°; in one minute coagulation appeared commencing, and the temperature was 101°; in three minutes, $99\frac{1}{2}$°; and in five minutes, 98°. Atmospheric temp. 46°. The loss of heat in water, of a similar bulk, was noted at the same time. From 102° the thermometer sank in one minute to 99°; in three minutes, to 93°; and in five minutes, to 87°.

Exp. X.

Blood was received from the arm of Mr. Thackrah into a gallipot, which had been previously heated by warm water to 100°, and the bulb of a delicate thermometer, also heated, was introduced as the blood flowed. At first it stood at 90°, and sank in successive minutes thus :—

										Coagulation commencing.	complete.
Minutes	—	1¼	2 5″	3	4	5	6	7	8	8½	9½
Thermom.	90	87	85	84	84	83	82	81	80	80	78

Exp. XI.

After ℥vi. of blood had been drawn for other experiments, blood was again taken from Mr. Thackrah's arm into a gallipot, heated to 100°. A delicate thermometer at 100° was introduced as the blood flowed: it sank to 92°, and the fluid cooled in the following gradation :—

Minutes	—	1	2	3	4	5	6	coagulation commencing.	7	coagulation complete.
Thermom.	92	91	90	88	87	85	84		83	

Exp. XII.

Blood was received from the arm of Mr. B. three hours after a meal, into a gallipot heated as in the preceding Exp. When the vessel was filled, the temperature noted was 92°. In successive minutes the thermometer fell as follows :—

Minutes	—	1	2	3	4	5	6	coagulation commencing	7	8	complete.
Thermom.	92	86	84	83	82	82	81½		81	80	

Exp. XIII.

Blood from the arm of Mr. Thackrah's man-servant, treated in a similar manner. Temp. on flowing, 87°.

Minutes	—	1	2	3	4	5	coagulation commencing	6	7	8	complete.
Thermom.	87	86	85	84	83½	83		82½	81½	81	

These experiments manifested no rise of the thermometer at the moment of concretion, and they militate against the opinion that any sensible degree of heat is evolved during that process. That fluids give out caloric in becoming solid is well known, but to this law there are several exceptions. Are we to include blood among hem? or are we, with Dr. J. Davy, to consider the quantity of fibrine which changes from a liquid to a solid state, too small for the *sensible* evolution of heat?

The following experiments were made to ascertain the comparative fall of temperature in the cooling of blood, and a liquid of a similar specific gravity.

Exp. XIV.

The jugular vein of a dog, which had been largely bled two days before, was opened, and blood received into a gallipot containing the bulb of a thermometer. As the blood flowed, the temperature indicated was 94°. The apparatus was placed on the table, and the fall of the thermometer contrasted in successive minutes with that of another thermometer, placed in a gallipot of equal size, containing an equal quantity of mucilage, of the sp. gravity of 1045, and heated to 93°. The result was as follows:—

Minutes	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Blood	94.	92.	92.	91.	90.	89½.	89.	88½.	85.	85.	84.
Mucilage	93.	91.	90.	84.	83.	82½.	82.	81½.	80½.	80½.	80.

Minutes	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.
Blood	83.	82.	81.	80.	79.	78.	77.	77.	76.	75.	75.
Mucilage	79.	78.	77.	76.	74.	73½.	73.	72.	71.	71.	70.

At the twenty-second minute, the blood had begun to separate its serum.

Exp. XV.

Blood from the jugular of a large pointer was treated as in last experiment. The temperature of the blood and the mucilage at the commencement of the Exp. were each 96°. The sp. gravity of the blood 1043; that of the mucilage, 1045.

Minutes	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
Blood	96.	96.	94.	93.	92.	92.	91.	90½.	89.	87.	86.	85.	84.	83.	82.
Mucilage	96.	96.	94.	92.	91.	90.	89.	88.	87.	85.	84.	83.	82.	82.	81.

IV. Carbonic acid gas has been said to be given off during the coagulation of blood. Some curious observations made by *Sir E. Home** and *Mr. Brande*† have favoured this opinion. *Sir C. Scudamore* has urged experiments which he conceives to establish the fact,‡ but he concludes that "the period of time in which the blood coagulates, depends in a great measure on the quick or slow extrication of the carbonic acid gas. Its evolution takes place most freely as the blood begins to concrete, and ceases when coagulation is completed. It is evidently an essential circumstance in the process of coagulation, as the same causes which retain the carbonic acid in the blood, delay coagulation."§ *Dr. Davy*, on the

* *Philosoph. Transactions,* 1818.

† Sir E. Home thus reports the experiment of Professor Brande— "Blood was drawn from a vein in the arm, and while yet warm, was placed under the receiver of an air pump; during the exhaustion of the receiver there was a considerable escape of gas from the blood, so that it had the appearance of effervescing, and soon depressed the quicksilver in the gauge of the pump. He afterwards ascertained that this gas is carbonic acid gas, is met with in the same proportions in arterial and venal blood, and two cubic inches were extricated from every ounce of blood." p. 181. Two cubic inches from each ounce!

‡ "Exp. XI. A cup, containing two ounces of blood drawn from a patient ill with slight pleurisy, was instantly placed in a shallow basin containing lime water, and both were promptly introduced under a glass jar, just of sufficient height, it being desirable to admit as little as possible of atmospherical air. In four minutes the lime water was covered with a very dense pellicle; the blood giving only a thread of fibrine. Exp. XII. Lime water only, placed under the cover, at the end of four minutes, gave a film which was but just discoverable." *Essay on the Blood,* p. 28.

"Exp. XXVII. A portion of blood drawn from a person in health was placed with lime water under the receiver of the air pump, which was immediately exhausted. In three minutes there appeared a thick and universal pellicle, and the blood was considerably coagulated. Exp. XXVIII. I made a comparative trial with lime water only, and could not, in three minutes, discover the slightest pellicle." p. 34.

§ *Essay on the Blood,* p. 103.

contrary, has strongly denied the evolution of carbonic acid gas.[a]

If we acknowledge the existence of a free alkali in blood, we cannot conceive the co-existence of an acid gas. We have nevertheless repeated the experiments of *Sir C. Scudamore* with fairness and care. The following are examples :

Exp. XVI.

Blood was taken from the femoral artery of a dog, and placed on a stand in a pneumatic trough, so that the surfaces of the blood and the water were on the same level; a bell glass, with a stop cock, was inverted over the blood. After remaining thus for two hours, the air in the glass was analysed; no trace of carbonic acid, however, could be detected, and the proportion of azote was found to be the same as in the ordinary atmosphere, 79 per. cent. It was therefore inferred that no carbonic acid was in this instance given off during coagulation.

This experiment was repeated with venous blood, with a similar result.

Exp. XVII.

Blood was received from the arm of a patient into a gallipot, and placed in lime water: the moment the vessel was full, a large gallipot was inverted over it. Two similar gallipots were used in another basin of lime water as a contrast. Both were examined in about an hour after, when a fine crust of carbonate of lime had formed

[a] One of Dr. J. Davy's reasons for this opinion is, that he has added one fourth of a cubic inch of carbonic acid gas to an ounce of blood, and to a similar quantity of serum, "the whole of which has been absorbed, and yet the blood and serum still exhibited free alkali."—*Edinb. Journal,* xxix, 254. Do not these experiments prove that the free alkali exists in some state in which it is not acted on by the presence of carbonic acid ? If so, they invalidate the objection urged by Mr. Thackrah in the next sentence. Ed.

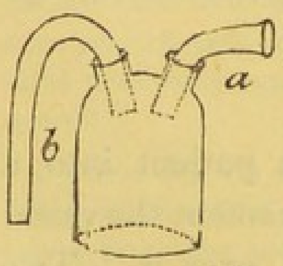
over nearly the whole surface of the water in each basin; but on lifting the inverted gallipots, no trace of carbonate of lime could be observed within the circumference of either.

This experiment was performed on two separate occasions with the same success.

Dr. Stevens contends, that the carbonic acid has been attracted and carried off by the oxygen of the atmosphere before the blood can be subjected to such experiments as these.[a]

[a] *Observations on the Blood, p.* 21.

Notwithstanding the contradictory nature of the evidence quoted on this point, it can hardly be doubted that, under some form or combination, carbonic acid does exist in the blood, and may be extricated by certain processes of experiment. One performed by DR. STEVENS, and detailed to me by my respected friend, PROFESSOR A. T. THOMSON, seems to be an indisputable proof of the fact. Dr. T. showed the apparatus to his class in the London University, and has described the experiment to me as follows:—"To a double necked pint bottle were fitted two tubes, as in the annexed sketch; one, *(a)* ascending, the other, *(b)* descending. The whole was filled with hydrogen gas; the long tube immersed in distilled water, and the orifice of the upper tube *(a)* being placed on the skin near to the bend of the arm, the vein was opened, and the orifice slid carefully along, until it included the incision which had been made by the lancet. As the blood entered this tube, hydrogen gas was expelled through the descending tube, which was in the distilled water. As soon as five or six ounces of blood had entered the bottle, both orifices were carefully closed, and the blood agitated in the hydrogen gas for five minutes. The whole was then permitted to rest for half an hour, so as to allow the hydrogen to attract all the carbonic acid. The orifice of the descending tube was next immersed in Barytic water, and uncorked in that fluid. The whole was then placed in the receiver of an air pump. In proportion as the air was removed by means of the pump, the gas which was over the blood passed through the descending tube into the Barytic water; and so strongly was it impregnated with carbonic acid, that even the first bubbles that arose, instantly caused a dense cloud in the water; demonstrating clearly, that the blood, even in the extremity, is saturated with ready-made carbonic acid." PROFESSOR THOMSON also informs me that the ingenious inventor of this apparatus, MR. SQUIRE, chemist, in Oxford-street, has lately



V. The quantity of blood in relation to the surface over which it is spread has a great effect on coagulation. If it trickle from a wound, or flow over an extensive surface, concretion, as daily observation evinces, almost instantly ensues: but if, on the contrary, it issues in a full stream, and be received in a proper vessel, several minutes elapse before this process commences. Although, however, coagulation *begins* the soonest in proportion to the paucity of the blood, the *complete separation* of the serum and crassamentum is in the inverse ratio; or at least, does not take place when the quantity is very small, or the coagulum thin.

VI. The kind of vessel, as to material and figure, has a considerable effect on the coagulation of the blood it receives.[a] The greater the surface of blood exposed, in proportion to its quantity, the quicker is the concretion.

improved it, by the addition of stop-cocks, but the form just described is that which Dr. Stevens employed. Dr. Clanny's experiments, related in *Edinb. Journ.* xxxii. 40. and *Lancet,* xvii. 450, are to the same purport, and though they do not appear quite so conclusive as the illustration I have related, are also strong evidence that carbonic acid exists in venous blood. See also Brande, in *Phil. Trans.* 1818, 181; and Vogel, in *Annals of Philosophy,* vii. 57. Dr. Prout believes that one copious source of this acid in the blood is the conversion of its albumen into the gelatinous secretions of skin, &c.; gelatin containing three or four per cent. less carbon than albumen.—*Bridgewater Treatise, No.* viii. 524. Ed.

[a] Dr. Babington found the proportions of serum and crassamentum to vary materially in the same blood drawn into differently shaped vessels. The following table exhibits the difference, in four of the cases which he relates, in the proportions of crassamentum to 1000 parts of serum.

Blood drawn into	a pear-shaped bottle.	basin.
Case of Purpura	1495.	2230.
Vertigo during pregnancy	945.	1716.
Phthisis	960.	1090.
Diabetes Mellitus	1292.	1717.

See *Medico-chirurg. Trans.* xvi. pt. 2. 296-7. Ed.

In reference to the shape and material of the vessel, we made two or three experiments.

Exp. XVIII.

Blood was received from the neck of a calf, that had been once previously bled, into a large gallipot, then poured into five vessels, each containing a nearly similar quantity. The periods of concretion were ascertained to be—

Copper vessel of the annexed figure		2 minutes.
Glass vessel		1 10 seconds.
Pewter vessel		1 12
Gallipot.........		1 20
Silver vessel		1 25

After remaining twenty-four hours, the relative proportions of their contents were—

Copper vessel	345·7	Serum.	654·3	Crassamentum,	or as	10 to 18.9
Glass vessel	290·	......	710·	....	or —	10 to 24·4
Pewter	54·1	......	945·9	....	or —	10 to 175.
Gallipot......	212·3	... ..	787·7	....	or —	10 to 37·3
Silver vessel	306.6	......	693·4	....	or —	10 to 22·6

The quantity of serum in each vessel was small, but the proportion of serum in a calf that has been previously bled is always large. The analysis of the blood in the pewter vessel, incredible as it appears, is quite correct.

Exp. XIX.

Immediately after a sheep had been stuck, its blood was received into five vessels, viz. a copper pan, cupping glass, a pewter dish, a gallipot, and a silver punch-ladle, the same as in the preceding experiment. The periods of concretion were—

In copper, 50 seconds; glass, 70 seconds; pewter, 50 seconds; pot, 70 seconds; silver, 85 seconds.

I suspect the curious diversities in the foregoing experiments to have arisen from the electric conditions of the respective metals in which the blood was contained.

VII. Degrees of temperature affect coagulation.[a] A temperature of 120°—130° considerably accelerates the concretion of the blood, and one of 100°—110° generally does so, but in a less marked manner. A temperature of 40°—50° retards coagulation. We have found human blood kept at a heat of 45° fluid for fully sixteen minutes. At a temperature of 20°—30° blood is frozen, and on being afterwards thawed, becomes a grumous mass, and does not separate into serum and crassamentum. As this statement is in opposition to a received opinion, I will state the experiments on which it is founded.

Exp. XX.

Jan. 4, 1833, Blood was drawn from the arm of a patient, into a gallipot placed in a freezing mixture. It froze almost as fast as it fell into the vessel, and was allowed to remain frozen in the open air for twenty-four hours. When thawed, it did not coagulate, or separate into serum and clot.

Exp. XXI.

Blood from the arm of a patient was received into a vessel placed in a freezing mixture, 36° below the freezing point. It became solid before coagulation had taken place. Another specimen was placed at the same time in the open air, the thermometer standing at 26°: it concreted in 21 minutes. A third gallipot was allowed to remain in the room, where the temperature was 45°; it coagulated in 16 minutes. After two or three hours the

[a] The experiments of Hewson and Hey tend to show that a temperature of 98° is most favourable to coagulation.—*Exp. Inquiry*, p. 6. *Obs. on the Blood*, p. 38. Ed.

first and second gallipots were removed to a room at 65°, when the contents became thawed. Eighteen hours subsequently the blood in the first was not concreted; and on being mixed with three times its bulk of water, and passed through a filter, some very small portions of coagulum were retained. In the second gallipot only, a very small quantity of serum was separated from the mass.

When blood, on the point of concretion, is placed in a freezing mixture, the natural action is suspended, and a few drops only of serum exude. How shall we account for the prevention of coagulation by cold? Does cold prevent the play of chemical affinities?

VIII. Agitation of the blood affects its coagulation. When briskly stirred, or shaken, on its effusion, blood seems to retain a homogeneous character, and to lose its coagulating properties, but this appearance is fallacious: for if the blood which has been shaken be afterwards placed on a filter, the fibrine is found in shreds. Strong agitation, therefore, does not prevent coagulation, it merely divides the coagulum. Moderate agitation has a different effect; it appears to promote concretion, as in the following experiments:—

Exp. XXII.

Blood received into a gallipot was moderately stirred with a stick for two minutes. It concreted in 3½ minutes.

Another vessel, from the same stream, was treated in the same manner. The blood was fully concreted in 4 minutes.

A third vessel, filled immediately after the second, remained at rest. In this the blood showed no appearance of concretion for 6 minutes; and at the end of 8, was much less firm than the contents of the other vessels.

Exp. XXIII.

A gallipot full of blood, from a stuck calf, was placed in a vessel of carbonic acid gas, and a fine thermometer introduced into the blood, showed its temperature to be 90°: it was fully coagulated in 3 minutes.

A similar portion in another gallipot was moderately stirred with a small stick for two minutes. In 1 minute a coagulum formed, and in 3 minutes it was a firm mass.

Blood was taken directly after the last into a third gallipot, and allowed to remain at rest. It began to concrete in 3 minutes, but was not a firm mass till 4 minutes.

The calf from which this blood was taken, had been three times bled. Three distinct shades of colour were visible in the three vessels. The blood, which had been stirred, became a bright vermilion hue; that placed in the gas was less bright; and the third a dingy purple.

Exp. XXIV.

Blood was taken from the arm of a patient labouring under acute rheumatism, into two vessels. One was stirred for two minutes, and the blood was concreted in $3\frac{1}{4}$ minutes. The other was left at rest, and concreted in 5 minutes.

In another experiment, blood moderately stirred, concreted 1′ 30″ sooner than blood at rest; in a fifth, 1′ 45″; in a sixth, on sheep's blood, a clot was found at the end of the stick in 40″, though no concretion was apparent in the blood at rest till 1′ 30″.

Lest the stirring of blood with a stick should interfere with the result,

Exp. XXVIII.

Blood was received from the neck of a calf into a six-ounce bottle, which was then corked, and gently shaken for about 15 seconds, and afterwards allowed to remain at rest until it concreted, 50″ from the time of its being drawn. Blood received at the same time, and allowed to remain at rest, was also fully concreted in 50 seconds: of the two, the coagulum in the bottle seemed the firmest.

In another experiment, one ounce of blood was shaken in a four ounce bottle for 10 seconds. It concreted in 65″, while a similar quantity, at rest, coagulated in one minute.

How is it that in the former instances concretion was promoted by moderate agitation? When other liquids become solid, does agitation favour the change?

IX. The effects on coagulation of various substances mixed with the blood have often been stated;* but such

* DAVY gives his statement in the following manner, (*Edinb. Journal*, xx. 255.) and I append the articles examined by SCUDAMORE. *On the Blood*, p. 94, et seq.

1. Those which do not distinctly retard or accelerate coagulation: Chloride of silver, protoxide, and peroxide of antimony, red precipitate, sulphur, iodine, hydrocyanic acid, carbonate of lime, starch, sugar, camphor, sulphuric æther, oil of peppermint, cantharides, oil of turpentine, hydroboracic acid, gum arabic.—Medicinal prussic acid.

2. Those which retard or prevent coagulation: Rhubarb, ipecacuanha, cinchona, columba, myrrh, catechu, jalap, the extract prepared *in vacuo* of belladonna, digitalis, aconite, conium, and sarsaparilla, the common extract of cinchona, magnesia, carbonate of magnesia, tartarized antimony, supertartrate of potash, muriate of barytes, nitrate of barytes, borax, common salt, nitre, sal ammoniac, subcarbonate of ammonia, subcarbonate of soda, distilled vinegar.—Sulphate of soda, tartrate of soda, common salt, muriate of ammonia, nitrate of potash, liquor potassæ, sulphate of zinc, sulphate of copper, sulphate of alum, sulphate of potash and borax.

3. Those which accelerate, or otherwise alter coagulation: Tartaric acid, oxalic acid, chlorate of potash, lime, calomel.

According to SCUDAMORE, by the addition of extract of belladonna, the fibrine formed into a firm clot. Alum, in solution, produces a dense and tenacious coagulum. Tartrate of soda, in solution, (weaker than saturation) produces coagulation of a soft jelly consistence. Common salt, in weak solution, causes loose coagulation. Sulphate of copper produces dense precipitation; nitric acid produces very dense precipitation.

DR. DAVY adds the following paragraph as to "the effect of substances on observations appear of little physiological interest, at least in the present state of our knowledge. The articles which chiefly deserve notice are caustic alkali, common salt, subcarbonate of soda, and the neutral salts in general; mucilage, and water. All these retard or prevent coagulation.; the caustic alkali, by dissolving the fibrine of the blood, and the salts and mucilage on principles not well ascertained. Water, when largely added, seems to prevent coagulation: the filter, however, shews, that by separating the particles of fibrine, it retards, but does not preclude, the process. *Dr. Davy* states, that when a foreign substance has seemed to prevent concretion, dilution with water, to the requisite extent, produces the coagulum. He adds a curious remark, that when the natural change is prevented by a foreign admixture, the blood will remain viscid, without a sign of decomposition for days, or even weeks, and on the addition of water, it will coagulate and putrefy.

An observation was made by the late *Mr. Hey*, that if the flowing blood be received into a cup containing a portion of water, this fluid will be in a great measure

the blood which coagulate serum, or precipitate its albumen :—I have tried the following substances which came under the above denomination, the sulphuric, nitric, and muriatic acids, sulphurous acid, barytes, alum, nitrate of silver, sulphate of copper, alcohol, sulphate of zinc. All of these apparently coagulate both the fibrine and the serum of the blood. The effect is produced almost instantaneously, excepting in the instances of sulphurous acid and barytes, and is generally attended with a change of colour of the red particles. Neither sulphurous acid gas, nor the liquid acid, nor barytes, has any immediate effect, except that of darkening the blood; and in a few hours both the acid and alkaline earth render it of firm consistence, like strong jelly, and they have the same effect in a less degree on serum, which they convert slowly into a jelly-like mass, pretty firm, and semi-transparent. It is curious and worthy of remark, that the compound of albumen, and nitrate of silver, or perhaps it may be more correct to say, the compound formed by the precipitation of albumen by this salt, though insoluble in water, is soluble in acetic acid, and its solution in this acid is not precipitated by common salt." p. 257.

absorbed by the crassamentum during coagulation, the insula, in such circumstances, considerably exceeding that of the undiluted blood.* Though a small quantity of water does not materially affect the coagulating process, yet a proportion of one to two will generally, I believe, produce the effect *Mr. Hey* remarked.

Dropping a piece of crassamentum into blood, as soon as drawn, accelerates concretion. In an experiment, this process commenced in the disturbed blood in 3′ 40″, while in undisturbed blood, taken at the same time, it was 4′ 40″. This is similar to a phenomenon in reference to chemical solutions.

X. The exclusion of air affects coagulation. To ascertain the different periods at which blood concretes in close and open vessels,

Exp. XXXI—III.

Blood was drawn into a bottle, which was immediately corked, and at the same time into a gallipot.

In the 1st instance, it coagulated in the bottle in	2′ 10″	Gallipot	4′ 10″
2nd	1′ 10″	.....	1′ 30″
3rd	1′ 15″	.....	1′ 48″

Hence, it appears, that an imperfect exclusion of the atmosphere retards the coagulation of blood.

Different, however, is the effect on the exudation of serum.

Exp. XXXIV.

Blood from a calf was received in an open earthern vessel, and into a phial of about the same capacity. The

* In one of Mr. Hey's experiments the result was as follows:—

	Serum.		Crass.
1 cup, containing an ounce of cold water	31·6	to	100
2 .. ..tepid water............	45·0	...	100
3 undiluted, but the vessel placed in a cold solution	40·8	...	100
4 ....the temperature of the human body	62·9	...	100

Appendix to Hey's *Observations on the Blood*, p. 75.

latter was closed immediately with a cork, so as to exclude the air as much as possible. At the end of twenty-four hours the proportions of serum and crassamentum in the two vessels were ascertained.

Open vessel.		Closed vessel.	
Serum .... 473·5 Crassamentum 526·5	or as 10· to 11·1	Serum......... 546·8 Crassamentum 453·2	or as 10· to 8·1
1000·		1000.	

Exp. XXXV.

Performed in a similar manner.

Open vessel.		Closed vessel.	
Serum......... 475·3 Crassamentum 524·7	or as 10· to 11	Serum......... 691·8 Crassamentum 308 2	or as 10· to 4·4
1000·		1000·	

Exp. XXXVI.

Blood from a patient.

Open vessel.		Closed vessel.	
Serum......... 505·6 Crassamentum 494·4	or as 10· to 9·7	Serum...... .. 611·8 Crassamentum 388·2	or as 10· to 6·3
1000·		1000·	

Exp. XXXVII.

A similar experiment.

Open vessel.		Closed vessel.	
Serum......... 620· Crassamentum380.	or as 10· to 6·1	Serum......... 682· Crassamentum318.	or as 10· to 4·6
1000.		1000·	

From the marked diminution of the proportion of crassamentum in the closed vessels, it is apparent that the exclusion of air greatly promotes the exudation of serum; that although it retards the commencement of coagulation, it decidedly favours its completion.

[Mr. Thackrah had evidently intended to add a few more remarks to this chapter, after having made some experiments on the effects of vacuum, galvanism, and an atmosphere of certain gases on the coagulation of blood. One or two will be found in Appendix II. Ed.]

CHAPTER IV.

ON THE CAUSE OF THE BLOOD'S COAGULATION.

This is an inquiry, which, although it has given rise to various conjectures, and many ill-founded hypotheses, has been the subject of philosophic research. *Hewson*, especially, has paid particular attention to this interesting study, and merits the credit of having prosecuted the investigation with science and with ardour.

I. Among the opinions held on the subject, some have believed, that the fluidity of the blood is preserved by the heat of the body, and its concretion produced by its removal into a colder temperature.* Experiments on heat and cold will easily confute an idea so devoid of observation. It is well known, moreover, that blood may be frozen and thawed without the occurrence of coagulation. And in animals of low temperature, as the tortoise, if cold produced the concretary process, no circulation could be maintained.

II. The coagulation of the blood has been attributed to the action of the atmospheric air. *Hewson*, having

* HIPPOCRATES, on observing the victims slaughtered for sacrifice, remarks, οκὸταν σφαξη τις ιερειον τεως μεν αν θερμον η, υγρον εστιν το αιμα· επειδαν δε ψυχθη επαγη.—*De Carnibus.*

included a portion of the jugular vein of a living animal between ligatures, admitted air in contact with the confined blood, and finding concretion to occur in a quarter of an hour, he inferred air to be a strong coagulant.

The experiment I attempted to repeat, but found a difficulty in effecting it with success. Besides, were a number of such trials accordant in the result which *Hewson* states, his inference would not still be established, since other circumstances necessarily interfere with the effect: the air is applied to the vessels, and may in a greater or less degree render torpid their vital energy; the current of the blood is no longer maintained; and if his conclusions on the subject of *rest* were admitted, the loss of motion would claim a considerable share in that result, which he here attributes to the action of the atmospheric fluid. As these subjects, however, will be discussed in the succeeding sections, I proceed to state a few observations, which, *prima facie*, support the opinion of the contact of air being the cause of coagulation.

(1.) It has been before remarked, that the blood concretes readily in proportion to the surface over which it is spread; and this effect might naturally be attributed to the air, to which it is so freely exposed.

(2.) In that appearance, which is termed the buff-coat, it has been noticed, that the quantity of size is greatest when the blood has flowed in a copious stream; and since the formation of this tunic depends on the slowness of coagulation allowing the red particles to subside, it seems probable, that the free admission of air which takes place when the blood trickles down the arm, induces its more speedy concretion.*

Exp. XXXVIII.

A vial was filled with the blood of a stuck sheep, and

* See the last chapter in loco.

the cork immediately applied.* During the day it remained a uniform mass, but next morning the serum was completely exuded.

Exp. XXXIX.

(3.) Blood was received from the heart of an ox, dead about 20 minutes. It was fluid; but on exposure to the air, concreted in 2 min. 25 sec.

Exp. XL.

Fluid blood was taken from the heart of an ox dead above half an hour. Coagulation commenced in 2 min. 30 sec.

Such observations may be thought to support the doctrine; but the two last, apparently the strongest in its favour, may be proved, on reflection, to aid an opposite opinion. I refer to the same cause which renders inconclusive *Hewson's* experiment,—the state of the vital energy. If (1) and (2) be considered as circumstantial evidence, other observations more powerfully deny the inference. After death the blood is found coagulated in many of its vessels. In mortified limbs coagulation, strong coagulation, is found to have taken place.† In these and similar cases, there could be no direct communication with the atmosphere. On two occasions, when *Morgagni* witnessed air contained in the blood-vessels after death, there was *no coagulation*, and in a third, but very slight traces.‡

Experiments will place the subject beyond doubt.

Exp. XLI.

Air was forcibly blown from the human lungs into the jugular vein of a horse. The animal almost instantly

* The air in this case could not have been wholly excluded, and hence, if even concretion had taken place immediately, the result would have been indecisive.

† Hunter. ‡ *De sed. et caus. Morb. Ep.* v. *and* xxxi.

expired : but the blood flowing from the body, *though it contained numerous air-bubbles, did not coagulate.*

On reflection, however, it appeared possible that the quantity of carbonic acid gas in expired air, might affect the validity of the conclusion. The following experiment was therefore instituted.

Exp. XLII.

Into the jugular of a bitch, was injected atmospheric air from a half-pint syringe. The blood, after death, *flowed* from the jugular vein, but coagulated on its effusion. Fifteen minutes afterwards, the blood in the vessels, though fully mixed with the injected air, *remained fluid.*

Exp. XLIII.

Blood was received into a vessel, the extremities of which were closed with stop-cocks; and to prevent any connection with the atmosphere, a jet from the vein of an animal was allowed to pass through, before the lower end was secured; on the closure of the upper extremity, the vessel was immersed in warm water; yet in a quarter of an hour, *coagulation took place.*

Since, therefore, the admission of air fails to induce concretion (XLI. and XLII.) and its exclusion to prevent it, (XLIII.) experiment evinces that *this agent is not the cause of coagulation.*

III. *Rest* has been supposed the cause of this process. To ascertain the truth of this opinion *Hewson* instituted experiments on blood contained in the vessels of living animals. He tied the jugular veins of dogs, and leaving the vessels in their situation, he allowed from ten minutes to two hours and a quarter to elapse before their division. The blood he found completely fluid on remaining ten minutes; minute coagula after fifteen minutes; and considerably larger clots at the expiration of two hours.* He admits, however, that even these bore but a very small proportion to the fluid mass.

Exp. XLIV.

Immediately on the birth of a living child, I secured part of the umbilical cord between ligatures, and placed it in water heated to 100°—110°. At the end of fifteen minutes, the vein being punctured, its contents were found fluid, and of a natural consistence.

Exp. XLV.

The experiment repeated. At the expiration of thirty five minutes, the vessel on division, was found to contain considerable coagula, with a small quantity of thick blood.

Exp. XLVI.

A portion of the jugular vein of a large dog was included between two ligatures, and detached from its cellular connection. The integuments were then laid over the wound in the throat, and the animal kept still. On opening the vein, at the expiration of ten minutes, no coagulum could be observed.

Exp. XLVII.

The jugular vein of a cat was secured with two ligatures. The portion between them contained, at the end of five minntes, blood perfectly fluid. The opposite jugular was then tied, and here, also, no marks of coagulation were perceived, though the vessel was divided at the expiration of fifteen minutes. A carotid of the same animal was next secured; and after five minutes, punctured. The blood was fluid.

* Crawford also mentions an experiment performed by Dr. Hamilton, in which a portion of blood was contained between two ligatures, made on the jugular vein of a cat. In an hour, partial coagulation had taken place.—*Exper. and Observ. on Animal Heat.*

Exp. XLVIII.

A similar experiment was made on the jugular vein of a rabbit. A puncture being made three quarters of an hour afterwards, no traces of coagulation were perceived.

Exp. XLIX.

The experiment repeated. After the lapse of three-quarters of an hour, the insulated vein was cut out. Not the slightest appearance of coagulum could be discerned.

Exp. L.

Part of the jugular vein of a dog, with its contents, was included between ligatures, removed from the body, and immersed in water heated to somewhat below 100°. On puncturing it at the expiration of an hour, the minutest coagula could not be seen.

Exp. LI.

A similar experiment; except that the vessel was not immersed in water. On its division, at the end of twenty minutes, the blood was perfectly fluid.

On reviewing these experiments, we find that where the current of blood was stopped in living vessels, concretion did not take place in 5, 10, 15, 20, 45, and 60 minutes,* while the reception of the blood of the same animals in the usual manner, evinced this process to commence in 2—4 minutes. The inference is obvious;—loss of motion is not the cause of coagulation. In reference to the experiments of the able and ingenious *Hewson*, a careful perusal of his remarks on this subject will give reason to believe, that his opinion vacillated from the contradictory appearances which his inquiries produced; nor do I conceive, had they been uniform and constant, that the conclusions would have been satisfac-

* Experiment XLV. on the umbilical cord can scarcely be deemed an exception. It is objectionable from the blood being fœtal, and the vessel probably possessing less vitality than durable veins.

tory. From my experiments, it may be conjectured, that his observation of minute coagula after the insulation of a vein for ten or fifteen minutes, originated in some deception;* and the clots which he observed at later periods, may be accounted for on other principles.† Independent, however, of these considerations, *Hewson's* statements will support the conclusion which I draw. If simple rest were the cause of coagulation, this agent and this only would be requisite; and the process would occur as readily when the blood is at rest in its vessels, as when effused from the body. If on bleeding an animal, concretion take place in five minutes, and on confining blood in its vessels, the change commence not till ten, the *mere loss of motion, it is manifest, cannot be the cause of coagulation.*

Why does not the suspension of the circulation in deliquium, drowning, &c. induce coagulation? Though motion is lost, fluidity is maintained, and these occurrences, therefore, corroborate the negative conclusion which the preceding observations induced.

IV. Since fact and experiment so strongly militate against the supposition of either *air* or *rest*, simply considered, effecting the blood's coagulation, we now examine the opinion that *the nervous influence is the source*

* The solid specks, which he noticed, might possibly have been formed on the outer coat of the vessel, and washed off on its division: or if this supposition be rejected, they might have been produced *after* the exposure of the blood. An observation in his work affords some countenance to the latter conjecture. "If they (i. e. the veins) were opened at the end of fifteen minutes, at first sight it (the blood) also appeared quite fluid: but on a careful examination, I have found sometimes one, and sometimes two or three small particles, about the size of a pin's head, which are coagulated parts of the blood."—*(Exper. Inquiry, Edit.* III. *p.* 23.*)* During a *careful examination,* it is not improbable that a sufficient time might elapse for the commencement of concretion, since after the inclosure of blood in a vein this process occurs in a very short period after its effusion.

† See Sect. 4th.

of the blood's fluidity, and its loss the cause of coagulation. Here, however, the imperfection of our knowledge of the nervous system and its functions, presents a strong obstacle to accurate and satisfactory conclusions.

(1.) In the preceding examination of the effects of *rest*, the veins of different animals were insulated, and it was found, that although the loss of motion failed to produce concretion, yet that the blood acquired an increased disposition to this change.* In such circumstances, however, the vessels, if not partially dead, were at least so situated as not to allow the free admission of the nervous influence. On the supposition, then, of this process depending on the loss of vitality in the blood-vessels, we might conclude, that concretion would have taken place in a few minutes, had the receptacle been wholly devoid of life; but from the vessels possessing a degree of vitality, the blood did not speedily nor freely coagulate. That rest alone cannot give even a disposition to concretion, seems evident from the remarks on that subject.

(2.) In two instances (XXXIX. and XL.) the blood was found fluid in the hearts of oxen, twenty minutes and half an hour after their slaughter, but coagulated quickly on its removal from these vessels. Vitality, in these cases, must have existed, since the period between the apparent death of the animals and the examination of the blood was too short to admit of the extinction of the living† principle; and the circumstance of fluidity being maintained as long as the blood was kept in its vital receptacles, and coagulation shortly occurring on the

* KELLIE also, in his *Observations on the medical effects of compression by the Tourniquet*, remarks that blood drawn from a vein after the circulation has been for some time mechanically suspended, coagulates sooner than blood taken on the restoration of this action.

† See galvanic experiments on the muscles of the ox, page 87.

exposure of this fluid, strongly supports the doctrine under examination.

(3.) In the leech which has died soon after suction, the blood is found concreted, but if it be retained in the living reptile, I have found no trace of coagulation at the end of an hour. It is probable, therefore, that the blood's fluidity is maintained by the life of its receptacle.

(4.) In most of the cases related by *Morgagni*, in which the blood after death remained fluid, or but partially coagulated, the patient had died suddenly from some affection of the brain or nervous system. In such circumstances, it is known, that the contraction of death does not occur, the muscles remain flaccid, and the temperature of the body is long maintained. If these effects be considered as originating from life still lingering in its tenement, we see cause, on the theory adduced, for the blood's tardiness in coagulating.

(5.) On this subject an observation of *Fontana* is deserving of regard. He found, that although the coagulation of the blood out of the body was not affected by commixture with poison of a viper, yet that this substance, when injected into the veins of a living rabbit, produced instant coagulation of the circulating fluid, and the speedy death of the animal. The effect here stated could arise, I conceive, only from the shock given to the living principle resident in the heart and its vessels; and since the preceding remark of Fontana proves the simple mixture of the poison with the blood to have no effect on the coagulating process, we must conclude, that the *sudden destruction of the nervous influence induced instantaneous concretion.*

(6.) I have proved by repeated experiments, that coagulation occurs speedily in proportion to the debility of the system. If debility affect primarily and principally the organs of sensorial power, or (to speak, perhaps, more properly) if these organs first lose their excitability, we remark, as a collateral support to the theory adduced, that in proportion as the nervous influence languishes or dies, the blood assumes a stonger disposition to coagulate.

The faintness which occasionally ensues on venæsection, has a similar and marked effect:* this occurrence, if I mistake not, can arise only from the suspension of that supply which the vessels receive from the nervous system, and the consequently increased disposition of the blood to concrete.

After thus stating the circumstances which afford plausible grounds for the admission of the doctrine, I proceed to more decisive evidence.

Mr. (now *Sir*) *Astley Cooper*, to induce me to the examination of a subject, in which, from its intricacy, I despaired of success, kindly favoured me with an account of three experiments which he had made some years ago: but as the statement was merely verbal, I doubt the complete accuracy of my details. The import, however, I believe to have been as follows.α

* See preceding chapter.

α The above paragraphs, as well as the whole of this chapter, are copied, with very slight alteration, from the corresponding pages in the former edition of Mr. Thackrah's work. Among his MSS., however, I find a letter from Sir Astley Cooper, which it seems necessary for me to insert here; and I take leave to transcribe it in full. Ed.

"My dear Sir,—In the first place let me thank you for your book, and kind dedication of it to me. Your experiments seem to me ingeniously planned, carefully executed, and fair inferences have been made from them, as far as physiology at present admits. My experiments are not quite accurately stated.

Exp. I.

Having carefully excluded the atmosphere from the ureter of the ox, I tied one end and put a cock upon the other. The cock was tied in the jugular vein of a dog, and being then turned, the blood rushed into it. The cock was then shut, and the blood in ten minutes was found coagulated.

Exp. 1.

Mr. C. received blood free from atmospheric admixture into the ureter of an ox, which had been killed some time before; coagulation took place in ten minutes.

Exp. 2.

Lest the ureter might be an objectionable vessel, he next tied the jugular vein of a living animal with two ligatures, the insulated portion of the vessel remaining in its situation. The blood between the ligatures had not coagulated at the end of ten minutes.

Exp. 3.

A portion of the jugular vein of a living animal was included between two ligatures, and then detached at its lower extremity. At the expiration of four hours, blood was allowed to enter; and having been secured in this lifeless vessel, was found to have coagulated in ten minutes.

These experiments of *Mr. Cooper*, therefore, favour the opinion of the loss of the nervous influence being the

Exp. II.

The same experiment was repeated upon the jugular vein of the ox, which was by the same means as the ureter had been introduced into the jugular vein of the dog, and the blood coagulated in ten minutes.

Exp. III.

Two ligatures were placed on the jugular vein of a living dog, and there left for three hours: the blood had not coagulated.

Exp. IV.

Two ligatures were put on the jugular vein of a living dog, leaving a space between them of three inches. Then the lower part of the vein was cut through, and suffered to hang from the wound for four hours. The upper ligature was then removed, the blood admitted into the vein, and the ligature again tightened. The blood thus admitted into the dead vein was coagulated in a quarter of an hour.

* * * * *

You will get great credit by your book, and you deserve it.

I am, yours truly,

ASTLEY COOPER.

C. T. Thackrah, Esq., Surgeon, Leeds."

cause of the blood's coagulation. To ascertain what effect the vital, or lifeless state of the vessel has upon the blood's coagulation, I made the following experiments.

Exp. LII.

A portion of the jugular vein of a living dog was included between two ligatures, removed from the body, and immersed in water heated to 98°. At the expiration of an hour the blood which it contained was found fluid.

Exp. LIII.

To a long portion of the jugular vein of a dog, killed two days before, were affixed brass stop-cocks, and the whole immersed for a short time in water heated to 90°—100°. The jugular of a living dog was then laid bare, and after its puncture, one stop-cock was inserted into the opening. A small current of blood having been allowed to pass through the lifeless vessel, in order to preclude the possibility of its containing any thing but the subject of experiment, the lower stop-cock was turned, and shortly after, the upper. The vessel, with its contents, was then immersed in the heated water, the temperature of which was maintained for a quarter of an hour. At the end of this period, the vessel being taken out and punctured, or rather cut, over a white plate, the blood was found firmly coagulated.

These experiments evidently lead to the same conclusion as those of *Mr. Cooper. Blood confined in a vessel, which, from the time, vitality could not have deserted, did not coagulate in an hour; while that inclosed in a lifeless vein, was firmly concreted in one-fourth of that period.*

Two experiments of different character will now be stated: but it is proper to remark, that they were made on the supposition, that a part of an animal killed by violence retains its vitality for the space of about four hours, a fact of which I was assured on the most respectable authority.

Exp. LIV.*

A considerable portion of the jugular vein of a dog was freed from the surrounding parts; a slip-ligature affixed to the upper extremity, and on emptying the vein, a firm one tied round the lower. The integuments were then brought over the wound, secured with a suture, and supported with a handkerchief bound round the animal's neck. He was then set at liberty, appearing in no respect affected by the operation. Four hours and a half afterwards, the wound was re-opened, the slip-knot loosed, and the blood allowed to enter the vessel. Immediately on its being filled, a firm cord secured the upper extremity, which had been before tied with a slip one. At the end of ten minutes, the insulated vessel was cut out and punctured, but not the least trace of coagulation could be perceived.

Exp. LV.

A portion of the jugular vein of a sheep, killed an hour before, received blood from another sheep; the influence of other agents being excluded by the same means as those used in LIII. After remaining in the temperature of the animal for half an hour the vessel was cut, and a firm coagulum found.

If the doctrine of vitality's continuing for four hours in a part separated from a living animal were correct, the vein in the last experiment was alive, and yet concretion took place: consequently, the loss of vitality in the vessel was not the cause of coagulation. In regard to LIV. likewise, if death necessarily occurred at the expiration

* In this, and the preceding experiment, the mode of operation is minutely detailed; that should there exist any deception or impropriety, it may not pass unnoticed.

of four hours after the insulation of the vein, the blood should have lost its fluidity as readily as if received into any other close vessel; yet no coagulation could be observed.

The obscurity involving the subject caused me much perplexing thought, till I reflected, that the statement in reference to the period of death might be incorrect, and that the ambiguity would be probably resolved, if this process were proved to take place at different times, in different animals.

To ascertain whether or not four hours be requisite for the loss of irritability in slaughtered animals, I procured a small galvanic battery, and having taken a portion of muscle from the neck of a dog, recently killed, I subjected it to the influence of the aura, at intervals, for the space of two hours and a half. The contractions were strong at first, but became gradually weaker till the expiration of that period, when no motion could be produced.

In another dog, three hours were necessary for the extinction of irritability, after apparent death; and in a third, no contraction took place at the end of two. Three portions of muscle were taken from a slaughtered ox; and of these one ceased to contract at the end of two hours, another at about three, but the third was slightly affected by the galvanic stimulus, at the expiration even of six hours and a half.

These experiments fully warrant the conclusion, that irritability is lost at different times after the apparent death of different animals; nay, the last evinces it to cease at different periods in different muscles of the same animal.* If, then, one of the most striking properties of

* The last strong contraction of muscle marks the departure of life, independent of that evidence which the effects of the galvanic aura exhibit.

life be so inconstant in its abode and departure, may we not infer, that to the agency and extinction of vitality in general no definite period can be affixed? What may be deemed the test of life? If the effects of the galvanic stimulus be any criterion, I conclude, from the preceding experiments, that the *death of parts separated from living animals occurs at various and incalculable periods.*

The mode, therefore, of ascertaining the influence of vitality on the coagulating process, is by comparing the effects of vessels undoubtedly alive, or unequivocally dead, on the blood which they contain. With this intention, the following experiments were made.

Exp. LVI.

A portion of the jugular vein of a dog was included between ligatures, removed from the body, and placed on the table. On its division, at the end of twenty minutes, the blood was found fluid.

The character and result of this experiment resemble those of many others, several of which have been already detailed, (XLVI to LI.) Suffice it, therefore, to remark, that except in one instance,* I never knew blood to coagulate in a vessel recently insulated or recently removed from *a living animal;* and from frequent examination, I feel confident, that the blood in such circumstances, will

* Exp.—A portion of vein, included between ligatures, was taken from the neck of a dog, and immersed in water of the temperature of the animal; at the end of half an hour, the blood was found partially coagulated.

From what fortuitous circumstance this exception arose, I am at a loss to conjecture, unless on the supposition of vitality having ceased or declined, sooner than usual. My experiments on irritability (in the preceding page) render the supposition plausible.

Exp.—The next morning the other jugular of the same animal was treated in a similar manner. At the end of half an hour the blood was found *wholly fluid.*

be found to retain its fluidity till the expiration of, at least, half an hour.

Some observations on blood confined in lifeless vessels will now be stated.

Exp. LVII.

The iliac vein of a dog, killed three or four days before, was armed with stop-cocks, and received blood from the brachial vein of a man in the mode of Experiment LIII. On examination, at the expiration of a quarter of an hour, concretion was found to be complete.

Exp. LVIII.

A portion of the jugular vein of a sheep killed four days before, received blood from a similar vessel in a living sheep, in the mode of LIII. At the end of a quarter of an hour, complete concretion was found to have taken place.

These experiments, when compared with LVI. and LII., strongly support the doctrine of the blood's fluidity depending on the life of its vessels. But to make the contrast more marked and decisive, the following experiments were made.

Exp. LIX.

Part of the jugular vein of a small dog was secured with ligatures, and removed from the body. After remaining in the temperature of about 100° for half an hour, it was punctured. The blood was found perfectly fluid. A portion of the vena cava was removed, and reserved for the next day's comparative experiment.

Exp. LX.

Fifteen hours and a half after the preceding experiment, the cava taken from the animal then employed, was filled with blood (as in LIII.) from the jugular of a similar dog. On the division of the vessel at the expiration of a quarter of an hour, complete concretion had occurred.

Thus we remark, that for an half an hour blood remained fluid in a vein recently removed, while in the lifeless vein of the same animal, it was found firmly concreted in fifteen minutes. To what shall we attribute this striking fact, if not to the loss of the vessel's vitality?

On reflecting on the experiments of this section, it occurred to me, that the brass cocks connected with the lifeless veins might afford matter of objection. To ascertain whether they had any share in inducing coagulation,

Exp. LXI.

A length of the aorta of a dog, killed the day before, received the contents of the carotid of a similar, but living animal. The elasticity of the aorta allowing the admis sion of the divided carotid, no extraneous apparatus was required. Preventing therefore, with my finger, the admission of air, and one of my pupils pressing the lower end of the aorta, this vessel was quickly filled. My assistant was then directed to squeeze down the blood so as to empty the vessel; care being taken to exclude the air. The aorta was then re-filled, and ligatures being applied, was immersed in water, of the temperature of 100°. After a quarter of an hour had elapsed, it was divided, and complete concretion found to have taken place.*

It is apparent, therefore, that the same result occurs when stop-cocks are not employed.

* Another experiment, which was made with reference to this subject, produced a similar result, but as the manner of conducting it was not so completely satisfactory as that here recorded, the detail is omitted.

After the preceding illustrations of the opposite effects which the life and death of the vessels produce on the blood, a specimen succeeds of that intermediate state in which it is probable vitality is but partially existent.

Exp. LXII.

A portion of the jugular was taken from a sheep two hours and a half after it had been slaughtered. This vessel was filled with blood (as in LIII.) from a corresponding vein in a living sheep. After having been kept in the temperature of the animal for a quarter of an hour, it was punctured. Though most of the blood was fluid, one considerable coagulum was found.*

The inference drawn from the observations of this section is obvious. Experiments, in which the greatest attention was paid to accuracy in execution, and honesty in detail, have shown that blood retained for the requisite period is found fluid in a living vessel, partially or irregularly coagulated in a semi-living vessel, and firmly concreted in one devoid of vitality. I conclude, therefore, that *the vital or nervous influence is the source of the blood's fluidity,—and its loss, the cause of coagulation.*

How far these experiments and their result bear on the question of the vitality of the blood, I leave for others to discuss. My inferences are stated so far as I dare proceed, in my present knowledge of the subject. Vitality, in its character and relations, is a topic too extensive and obscure for any one to venture a decided assertion upon, without having devoted years to its study.

* In experiments where the vitality of the vessel is dubious, the state of the blood will also be dubious or irregular. In some cases, it may be found perfectly natural, in others grumous, and in some coagulated.

Mr. Grainger, in his able work on General Anatomy, expresses an opinion, that my experiments support the doctrine of the blood's vitality, and though his reasoning appears open to objection, I have no wish to controvert it. My difficulties in admitting the theory are given in the first chapter. I should be glad to have them removed by the experiments of this.

There is a perplexity in this subject.—1. Blood does not concrete in a living vessel. 2. Blood does concrete when that vessel is deprived of life, or when extravasated. 3. The concretion of extravasated blood is retarded by galvanism. Hence we infer, that fluidity is maintained by the living power of the vessel. Further—4. Blood concretes soonest when the animal from which it is taken has been greatly reduced—when its vitality is least. This supports the doctrine: but—5. Though blood taken at a low degree of vitality concretes quickly, it does not separate its serum so rapidly or so perfectly as blood drawn when vitality is unimpaired. Hence, the absence or diminution of vital power gives the disposition to concretion, or, in other words, to the commencement of coagulation, but tends to prevent the completion of the process.

Is coagulation a vital or a chemical action? If the former, perhaps the following conjectures may, in some degree, reconcile the apparent inconsistency of its phenomena:

1. Blood is constantly deriving a certain degree of vitality from the living power of the vessel, so long as this lives.

2. When extravasated, blood retains a proportion of the vital principle.

3. This proportion varies with the state of the vessel. If the vessel be in perfect life, the blood which flows from

it contains much of the living principle: if, on the other hand, the vessel, either from local cause or reduction of the general system, have its vitality reduced, the blood which flows from it possesses comparatively little of its living power.

4. Blood extravasated, whether possessed of a large or small proportion of vitality, must undergo certain changes. It is placed in a new situation, and exposed to foreign agency. Its state and character must, like every substance in nature, change with its situation. It is destined to coagulate. If this be a vital operation, its completion will be most perfect when the vitality of the extravasated blood is greatest. But how shall we account on this principle for the rapid commencement of coagulation in blood with little living power? [α]

[α] The concluding passages of this chapter are written on a fly-leaf in the author's copy of his former edition. They are remarkable, as showing that his arguments against the Hunterian doctrine of life in the blood were by no means convincing to himself. "What is life?" Reason on it as we may, the question still recurs to us. Blood is certainly not a mere hydrostatic liquid, but one possessed of certain functional susceptibilities, and acted on by vital powers. If a distinction can be made between the life of blood and the life of nerves, muscles, or bones, except in relation to their difference of offices, I am not able to perceive where the line of demarcation should be drawn. Ed.

CHAPTER V.

BLOOD FROM DIFFERENT VESSELS OF THE SAME ORDER.

The diversities in blood from vessels of the same order is a subject which has not, so far as I know, been discussed by preceding authors. *Haller* remarked an indisposition to coagulate in the blood of the spleen. *Dr. Schutz*, of Fribourg, on the contrary, has found the blood of that viscus as coagulable as other venous blood, when a quantity of fluid had not just before been injected into the stomach. *Stenon* and *Malphigi* inferred from experiments, that no difference exists between the blood of the vena portæ and the vena cava: and *Saunders* examined the vessels of the spleen and liver with a like negative result. Casual observations, and a few experiments did not, it appears, encourage a more complete investigation.

My first Inquiry on the Blood, published in 1819, led me to suspect a considerable difference in the contents of different vessels, and induced me since to prosecute the subject by numerous experiments.[α]

α M. Le Gallois published a work in 1803, entitled "*Le sang, est il identique dans tous les vaisseaux qu'il parcourt? Dissertation dans laquelle on établit, 1° que le sang artériel est le meme partout: 2° que le sang veineux est différent dans différentes veines.* 8vo. Paris, An xi."

SECTION I.—JUGULAR AND CAVAL BLOOD.

I first advert to the difference between blood from the jugular vein of dogs and that from the vena cava inferior, taken as nearly as possible at the same time, and under similar circumstances.

CONCRETION almost always takes place sooner in blood from the vena cava than in that from the jugular.

In Experiments	I.	II.	III.
Jugular blood concreted in	1′. 16″	40″	45″
Caval .... in	15″	10″	11″

In several other experiments, the subject of comparative concretion was regarded, but none contradicted the preceding. Lest the disturbance of the animal's state in reaching the cava might augment the disposition to concretion, the blood from the jugular, as a contrast, was in most instances taken after opening the abdomen.

THE SOLID CONTENTS we have always found in larger proportion in jugular than in caval blood.

In Experiments	I.	II.	III.	IV.	V.
Jugular } yielded of solid matter	333·3	223·5	231·5	188·4	164·6
Caval... } yielded of solid matter	217·2	217·8	170·2	169·7	94·4

THE PROPORTION OF FIBRINE in jugular, as contrasted with caval blood, is less uniform.

In the several Experiments	I.	II.	III.	IV.	V.	VI.
Jugular yielded of Fibrine	3.4	5·3	4·5	5·0	3·0	2·4
Caval...	4·3	5·2	3·8	3·7	3·4	1·2

Thus, four experiments gave a superiority of fibrine to jugular, and two to caval blood: but the average is in favour of jugular,—3·9 for the blood from this vessel, and 3·6 for that of the cava.

These observations shew such a difference in the contents of the jugular and caval veins, as to urge a more extensive inquiry, and on various vessels. This I have attempted in reference to the vena portæ. The peculiarities of distribution and function which this vein presents, and their especial relation to the digestive operations, gave it a decided preference in research.

SECTION II.—PORTAL BLOOD.[α]

1. One of the more obvious peculiarities in the blood of the vena portæ is the variety of COLOUR. The contents of this vessel are darker than blood from other veins, and more inclined to a ruddy hue than to the modena. Blood from the vena portæ, moreover, has not the homogeneous character of other blood. Its appearance gives the idea of defective elaboration.

2. THE SPECIFIC GRAVITY of portal blood, we contrasted with that of blood from other vessels. The following experiments were made on dogs: blood taken from the contrasted vessels of the same animal, and under similar circumstances; and the calculations made at the usual rate of 1000 for distilled water.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
V. Portæ	1070	1050	1024	1086	1041	1161	1105	1078
V. Jugularis ...	1064	1063	1108	1063	——	1073	1118	1064
V. Cava infer.	——	——	——	——	1047	1107	1103	1036
Arteria femoralis	——	——	——	——	——	——	1069	1085

From these, and similar observations, we infer, that there is *no general and marked difference in the specific gravity of portal blood.* The average presents 1076 for portal blood; 1079 for jugular; and 1073 for caval.

[α] The M. S. from which this section is transcribed, was read to the Royal Society of London, under the title of "An experimental examination of the blood found in the vena portæ," May 19, 1831. It is noticed in the *Lancet*, xxvii. 450, though incorrectly stated to be written by a *Dr. James Thackeray*. ED.

3. THE COMPARATIVE PERIODS OF CONCRETION we have particularly remarked in several experiments.

I. Portal blood was concrete in.................. 1′ 30″
Jugular .... 4′ 30″

II. Portal blood concreted immediately on its effusion from the vein.
Jugular began to concrete in .... 2′ —″
Jugular, (a second portion) in............... 1′ 30″

III. Portal blood .... 0′ 30″
Jugular, in about .... 1′ 30″
Blood from emulgent vein, in .... 1′ —″

IV. Portal blood was concrete in ... . —′ 30″
(a second portion) .... —′ 30″
(a third portion) immediately on effusion.
Jugular in .... —′ 45″

V. Portal blood began to concrete immediately.
Jugular in .... —′ 40″
Caval* in .... —′ 10″

VI. Portal blood immediately.
Jugular concrete in............... .. .. —′ 46″

To avoid unnecessary detail, I may state, that in many other experiments, while blood from the jugular vein of a dog began to concrete in from 1¼ to 3 minutes, the portal concreted immediately on effusion. Indeed, the fact became at length so familiar in the course of our inquiries on other points connected with the vena portæ, that we ceased to record it.† We conclude, therefore, that,

* The fact of the caval blood concreting sooner than the jugular in this experiment, is attributed to the reduced, almost dying state of the animal. Blood from the jugular was first taken ; blood from the cava after pithing; and after also the comparatively slow process of filling a vessel from the v. portæ.

† MR. DOBSON, of Westminster, who was my pupil during part of the time that these experiments were in progress, and who has since published a valuable "*Inquiry into the nature and functions of the Spleen,*" states in his treatise, that the blood of the v. portæ "is dark ; concretion occurs quickly after it is drawn, but coagulation is never perfect."

in ordinary circumstances, portal blood concretes much sooner than blood from other veins.

4. It was necessary to ascertain also THE COMPARATIVE RAPIDITY AND PERFECTION OF THE SEROUS EFFUSION. We took, therefore, portions of blood from the jugular vein and from the vena portæ, as much as possible under the same circumstances. The several specimens were weighed afterwards, generally at the end of two hours; the proportion of serum and coagulum noted; and the serum then returned to the coagulum. At a subsequent period, generally four hours after the first examination, the solid and fluid contents of the several specimens were again weighed, and the results noted.

From these examinations of jugular and portal products in numerous experiments, were formed two tables, shewing the proportions of serum and coagulum in each kind of blood, and at each examination: and these proportions, for the convenience of comparison, were calculated at the ratio of 10 for serum. Finally, a third table was drawn up from the contrast of the two former,—the contrast I mean, between the proportions of solid matter in the first and the second examinations.

Unwilling to encumber my pages with the first and second tables, I detail only the third.

Proportions of Crassamentum to Serum.

Coagulation in Portal Blood.				Coagulation in Jugular Blood.			
	1st Exam.	2nd Exam.			1st Exam.	2nd Exam.	
I.	51	...22	or as 23·1 : 10	I.	22	...16	or as 13·7 : 10
	19	...16	... 11·8 : 10		23	...20	... 11·5 : 10
	38	...17	... 22·3 : 10				
II.	60·8	...28·3	... 21·4 : 10	II.	23	...11·4	... 20·1 : 10
	42·5	...23·8	... 17·8 : 10		21·5	...11·0	.. 19·3 : 10
	44·8	...28.2	... 15·9 : 10		23·6	...11·3	... 20·8 : 10

Proportions of Crassamentum to Serum continued.

Coagulation in Portal Blood. 1st Exam. 2nd Exam.		Coagulation in Jugular Blood. 1st Exam. 2nd Exam.	
III.	17·8...19.0 or 10·6 : 10	III.	19·2...21·6 or 11·2 : 10
	21·7...23·8 ... 10.9 : 10		19.5...19.5 .. 10·0 : 10
IV.	15·6. .13·7 ... 11·3 : 10	IV.	42·6 ..26·2 ... 16·2 : 10
			38·6...26·5 ... 14·5 : 10
V.	36 ...18 ... 20· : 10	V.	19.5...23·0 ... 8·4 : 10
VI.	29· ...12·5 ... 23·2 : 10	VI.	18·0...7·0 ...25·7 : 10
	23·7...16·5 ... 14·3 : 10		
VII	20.1...15·6 ... 12·8 : 10	VII.	47·5...28·7 ... 16·5 : 10
			60·1...44·4 ... 13·5 : 10
VIII.	11·3...12·9 ... 8·0 : 10	VIII.	46·4...31·5 ... 14·7 : 10
	12·4...12·9 ... 9·0 : 10		51·2...48·8 ... 10·4 : 10
IX.	31·0 ..17·3 ... 17·3 : 10	IX.	20·8...10·3 ... 20·1 : 10
	16·3...15·4 ... 10·5 : 10		21·4...19·5 ... 10·9 : 10
	10.8...10·2 ... 10·5 : 10		

This shows the comparative rapidity with which the serum was thrown off from the crassamentum. A larger mass of coagulum at the first examination, compared with that at the second, evinces a proportionate tardiness in the separation of the serum; and on the contrary, a smaller disparity in the quantity of coagulum at the two examinations proves a more rapid separation. In other words, the more nearly the numbers which mark the solids in each line approach each other, the quicker has been the effusion of serum. Thus, the first specimen of blood had 51 parts solid to 10 fluid at the first examination, and 22 to 10 at the second; consequently it had separated a large quantity of serum after the first part of the experiment; and from its thus throwing off much fluid at a comparatively late period, we infer the effusion of serum to have been *slow*. On the contrary, in the next instance given, we find the numbers much more nearly alike, viz. 19 solid to 10 fluid at the first examination, and 16 to 10 at the second. After the first, therefore, there was little effusion. Little serum remained in the coagulum after the first examination, nearly the whole having been thrown off by the crassamentum previously: *ergo*—the effusion had been *rapid.*

The details given in the table show a wide variety of result in the experiments severally. In some, Exp. IV. for instance, we have a rapid separation of serum from portal blood, and a comparatively slow separation of serum from jugular; and in other cases, as in Exp. I., we have a slow separation from portal, and a quick from jugular. The varieties I conceive to depend on different states of the constitution in the animals employed, a subject for more particular discussion in a future chapter.

5. The ultimate proportions of serum to crassamentum in portal, as contrasted with jugular blood, was the next point to be experimented upon. In the following instances, the portions of blood contrasted were taken from the same dog, under circumstances as much as possible the same.

	Portal Blood.		Jugular Blood.	
	Serum.	Coagulum.	Serum.	Coagulum.
Exp. I.	63	141	126	213
II.	204	360	98	204
III.	27	46	142	162
IV.	108	306	163	180
V.	468	1115	210	239
VI.	151	427	71	154
VII.	88	168	89	174
VIII.	478	1139	61	160

	Portal Blood. (Continued.)		Jugular Blood.	
	Serum.	Coagulum.	Serum.	Coagulum.
IX.	27	37	40	106
X.	5	9	6	14
XI.	96	121	110	80
XII.	146	228	98	282
XIII.	47	78	67	298
XIV.	96	124	53	167
XV.	126	163	78	381
XVI.	72	125	119	123
XVII.	93	144	65	127
XVIII.	108	111	—	—
Average	10	20·1	Average 10	19·1

These experiments were made before we were in the habit, in such inquiries, of reducing blood to its ultimate solid and fluid constituents; and in them, as well as in others not detailed, we observed the mass of the portal some days after the blood had been drawn, and when the serum was carefully removed, to be still soft, much softer than the mass of the jugular. It never became solid; and contained a considerable proportion of serum, even when putrefaction commenced.

This observation led me to adopt another mode of examination. Blood as soon as drawn from the jugular and portal veins was weighed, and then put to strain through blotting paper. At the end of twenty-four hours the solid residue from each filter was examined in the scales; and the weight, subtracted from that of the whole mass previously ascertained, indicated the fluid which had exuded.

		Serum.	Crass.	Serum.	Crass.
I.	Portal blood	172	120 or	14·3	10
	Jugular	172	268 ...	6·4	10
II.	Portal	192	144 ...	13·3	10
	Jugular	232	223 ...	10·4	10
III.	Portal	281	128 ...	21·9	10
	Jugular	256	176 ...	14.5	10
IV.	Portal	246	238 ...	10·3	10
	Jugular	225	240 ...	9·6	10
V.	Portal	208	112 ...	18·5	10
	Jugular	284	198 ...	17·7	10
VI.	Portal	194	120 ...	16·1	10
	Jugular	244	248 ...	9·8	10
VII.	Portal	210	106 ...	19·8	10
	Jugular	255	195 ...	13·3	10
	Caval	183	148 ...	12·3	10

These experiments lead to the inference that portal blood contains from about $\frac{1}{10}$ to $\frac{13}{10}$ more serum than blood from other veins. In no experiment since the filtering process was adopted did we find the quantity of portal serum to be less. It is proper to add that these experiments, all but the last three, were made on dogs that had not recently been fed.

The statements in this sub-section also bear on a point which has been the subject of inquiry in a previous one, viz.: the comparative rapidity of serous exudation.*

6. THE PERFECTION OF THE SEPARATIVE CHANGE in portal as contrasted with jugular blood, we repeatedly remarked. One example may be adduced as a general specimen of our observations.

EXP. CXXV.

Blood was taken from the jugular vein of a dog, and a similar quantity from the vena portæ of the same animal,

* See page 98.

at the same time. Forty-eight hours afterwards the effused serum was poured off from the crassamentum of each: and after eleven days the two masses of coagulum were examined. That of the jugular blood was stiff; its surface had the consistence of a pharmaceutical extract, and its inferior part was considerably softer. The reverse of this was observed in the portal. In it the mass appeared more fluid than solid: the surface was the thinnest part, and clots of coagulum were found at the bottom.

From these and many similar observations we draw the inference, that *the separative change is much slower and less perfect in portal than in jugular blood.*

7. THE CHARACTER OF THE SERUM of portal blood we had also to examine.

It differs in COLOUR from the serum of jugular blood. Portal serum is always red; while jugular, if agitation be avoided, is straw coloured. The hue of the portal evidently depends on the detention of red particles of the blood. Does this detention arise from a difference in the sp. gravity of these particles? or from a diminished power in the crassamentum to attract or envelope them? or from a change in the serum, enabling it to hold in solution what other serum precipitates?

With the view of throwing some light on these questions, a definite quantity of the serum of portal blood was poured on the crassamentum of jugular blood, and a like quantity of the serum of jugular blood, taken from the same animal at the same time, poured on portal crassamentum. At the end of twenty-four hours the fluids were separated as carefully as possible from the solids. We found, in the first place, that we could not obtain from the portal crassamentum the quantity of serum that had been poured on it: about one-sixth part was lost in the coagulum. The whole of the serum added to jugular crassamentum, on the other hand, was re-obtained without difficulty. In the next place, we observed that jugular serum which had been poured on portal crassamentum attained the red hue always seen in portal serum. Finally, the weights showed that the jugular serum had gained more from portal crassamentum than portal serum from jugular crassamentum.

These observations, made in three experiments, led to the opinion that the difference uniformly remarked in the colour of portal serum, does not depend on a peculiar power in this fluid of absorbing or holding the red particles in solution, but on the state of the crassamentum, in portal blood.

8. The specific gravity of portal serum was ascertained in several instances. The following numbers resulted, at the rate of 1000 for water.

1090 Portal serum;	
1063	
1032	1016 Jugular serum from
1070	1064 [the same dog.
1028	1019
1016	1008
1032	1024

Thus it appears that the sp. gr. of portal serum when merely poured off from the coagulum, is somewhat greater than that of jugular poured in a similar manner. This, however, most probably results from the detention of red particles in the former.

9. The coagulation of portal serum by heat, we have particularly noticed. It is well known that the serum of blood in general becomes solid at a temperature of 150°—160°. Portal serum is generally slower in

concreting, and the mass is always softer than that of the jugular. Coagulation, in fact, is remarkably imperfect in the serum of the vena portæ. This observation we have found so uniform that a detail of experiments seems unnecessary. It was supposed, however, that this disparity might depend on the commixture of red particles with portal serum, not on the state of the fluid itself. To determine this point;—

Exp. CXXXVI.

Portal serum of its usual red appearance, and jugular serum which had been shaken with crassamentum till it attained the hue of the portal, were exposed to a temperature of 160°. The portal was longer by several minutes before it became concrete; nor did it afterwards form the firm mass which we found in the jugular. At the same time, jugular serum almost free from red particles was exposed to the coagulating heat, and we could find no difference, in the commencement or perfection of concretion, between this serum and the portion that was loaded with red particles, except that the latter formed the firmest mass. We may hence infer that the imperfection in the coagulating process of portal serum does not depend on the detention of red particles, but on a difference in the state of the albumen.

10. The disposition to putridity in portal serum has been examined in a few instances. It was less in portal than in jugular serum which had been kept an equal time. Portal yields a rather peculiar animal odour; while jugular emits the pungent ammoniacal odour of putrefaction. We have made the same remark on the dry contents of jugular and portal serum.

11. To ascertain THE PROPORTION OF SOLID MATTER IN THE SERUM of portal, as contrasted with that in the serum of jugular blood, several experiments were made. In these the degrees of temperature and periods of time were different, but in reference to the contrasted specimens both were the same.

I.	Two fluid drams of portal serum yielded		49	grains
	Two	jugular	34½	...
II.	Two fluid drams of portal serum yielded		16	...
	Two	jugular	14	...
	Heat longer applied than in the preceding experiment.			
III.	Two fluid drams of portal serum yielded		36	...
	Two	jugular	36	...
IV.	One fluid dram of portal serum yielded		23	...
	One	jugular	24	...

We remark that in the first of these experiments the portal serum contained a much larger quantity of solid matter than the jugular; in the second a somewhat larger; in the third an equal quantity; and in the fourth a somewhat less. The average, however, is in favour of portal serum containing more solid matter than jugular. To determine the question more accurate experiments were made, as follows:—

Exp. CXLI.

Two fluid drams of portal serum which weighed 126 grains, and two drams of jugular which weighed 125 grains, were subjected to a temperature of 130° for two hours. The dry glue-like products were then examined.

Portal yielded 12 grains. Jugular yielded 11 grains.

Exp. CXLII.

Two drams of portal serum which weighed 124 grains, and the same quantity of jugular which weighed 123 grains, were subjected to a temperature of 135° for two hours and a half.

Portal yielded 10 grains. Jugular yielded 9 grains.

In both cases the portal product was darker than the jugular.

We may infer from these experiments that portal serum contains more solid matter than jugular: but from the difference in colour of the portal, both in its fluid and solid states, we may attribute the difference rather to the detention of red particles than to a larger proportion of albumen. To establish, however, or contradict this explanation,

Exp. CXLIII.

Two specimens of serum taken from the animal employed in experiment CXLI., and at the same time, were transposed to each other's crassamentum; the portal serum being poured on jugular crassamentum, and the jugular serum on portal crassamentum. A few days after, the fluids were separated from the solids, and one hundred minims of each exposed to a temperature of 130° for two hours, and the solid contents carefully weighed.

Portal serum loaded with red particles from jug. bl. yielded 9 gr.
Jugular...portal bl. .... 9½ gr.

This experiment tended to support the explanation, and render it probable that *portal serum separated from the red particles it actually contains, has no greater proportion of solid matter, and probably even a less proportion than jugular.**

12. The state of the CRASSAMENTUM in portal as contrasted with jugular blood we remarked in the pre-

* These observations are applied, of course, to serum which has spontaneously exuded from the crassamentum.

ceding experiments. It was always much more loose in texture than jugular, and evidently contained serum up to the period of putrefaction.

13. The results of the foregoing sub-sections lead us to institute a fuller analysis to determine THE PROPORTION OF THE CONSTITUENTS OF THE BLOOD which the portal and jugular veins respectively contain.

Exp. CXLIV.

Blood was taken from the jugular of a pregnant bitch, and the vessel then tied. She was pithed; the abdomen immediately opened; the vena portæ punctured, and blood received in a small stream.* The mass of each was then weighed, and left for 36 hours to separate. At the end of this period the serum was carefully removed from each specimen, weighed, then exposed to a temperature of 130° for two hours, and the quantities of coagulum noted. Each specimen of serum after coagulation, and before exsiccation, yielded by compression a few drops of the fluid termed serosity, which were dried by an equal heat.

The crassamentum from each vessel was next enveloped in muslin, and washed of distilled water, till nothing remained but fibrinous matter, of a colour rather lighter than that of common muscle. The water

* It may be objected that blood drawn from two different vessels, the one *before* and the other *after* pithing, cannot be fairly contrasted, as the vital condition of the animal is altered. I would answer that the abdomen was opened so quickly,—instantly, indeed, after pithing—as scarcely to allow time for a change to be produced in the portal blood by the injury of the brain. A more decisive reply results from our observation that in many experiments not detailed in this work, in which the jugular vein was bled before and after the animal was pithed, no difference could be found in the state of the blood as effected by this proceeding. The motive for pithing the dog before we opened the abdomen, was, of course, to prevent suffering to the animal.

in which the crassamentum had been severally washed, was exposed to heat from a spirit lamp till it boiled, and the reddish albuminous coagula thus produced, collected by straining through muslin. The water was subsequently tested with oxymuriate of mercury, but no further albuminous precipitate could be obtained.

A summary of the results of these processes is contained in the following table:—

381 grains of portal blood yielded	Total.	
11 grains solid matter from serum	Solid.	Fluid.
55 .... washings of crass.	69·2 gr. leaving	311·8 gr.
1·5 .... serosity	or	
1·7 .... fibrine	181·6 gr. in 1000 of blood.	
374 grains of jugular blood yielded	Total.	
9 grains solid matter from serum......	Solid.	Fluid.
75 .... washings of crass.	87·5 gr. leaving	286·5 gr.
1·5 .... serosity	or	
2 .... fibrine	233·9 gr. in 1000 of blood.	

Thus 1000 parts of	Fibrine.	Albumen and Red Particles.	Fluid.
Portal blood would have produced ...	4·4	177·1	818·5
Jugular	5·3	228·6	766·1

Exp. CXLV.

A bitch in an early stage of utero-gestation. Examination as in foregoing experiment, except that the blood stood to separate only eighteen hours.

368 grains of portal blood yielded	Total.	
15·5 grains solid matter from serum ...	Solid.	Fluid.
57· .... washings of crass.	74·8 gr. leaving	293·2 gr.
1· .... serosity	or	
1·3 .... fibrine	203·2 gr. in 1000 of blood.	
437 grains of jugular blood yielded	Total.	
22 grains solid matter from serum	Solid.	Fluid.
73 .... washings of crass	98 gr. leaving	408 gr.
1 .... serosity...........	or	
2 .... fibrine	224·2 gr. in 1000 of blood.	

Thus 1000 parts of	Fib.	Alb. & red P.	Fluid.
Portal blood would have produced	3·5	199·7 ...	796·8
Jugular	4·5.....	219·6 ...	775·9

Exp. CXLVI.

A bitch as in last experiment examined as in CXLIV.

The results showed that 1000 parts of	Fib.	Alb. & red P.	Fluid
Portal blood would have yielded	3·4......	189·5 ...	807·1
Jugular	5·*.....	226·5 ..	768·5

Exp. CXLVII.

A bitch not pregnant examined as in CXLIV.

1000 parts of	Fib.	Alb. & red P.	Fluid.
Portal blood would have produced.........	3·	168·6 ..	828·4
Jugular..	3·	185·4 ...	811·6

Exp. CXLVIII.

A large whelp examined as in CXLIV. The separation of albumen in this experiment was effected with difficulty.

1000 parts of	Fib.	Alb. & red P.	Fluid.
Portal blood would have yielded	1·1......	100·4... ..	898·5
Jugular..	2·4 ...	162·2......	835·4

In four of the five experiments of this division, portal blood was found to contain a much smaller quantity of fibrine than jugular blood, and in one an equal proportion. The average, however, exhibits so marked an inferiority that we may safely infer *that portal blood contains in general a much smaller proportion of fibrine than jugular blood.*

* The effect of pregnancy in augmenting the quantity of fibrine in the blood is strongly marked in these three experiments, as contrasted with the two following.

Our inquiries on this part of my subject lead to the following principal inferences.

1. That blood from the vena portæ has the appearance of defective elaboration, and that its colour is darker and more inclined to brown than to the modena.

2. That portal blood concretes much more quickly than blood from other veins.

3. That it contains a considerably larger proportion of serum.

4. That the serum of portal blood, from the detention of colouring matter, is redder than serum carefully separated from the blood of other vessels: and that, from the same cause, it has a higher specific gravity; and yields on exsiccation a greater weight of solid matter.

5. That portal serum, on the application of heat, concretes more quickly, but less completely, than jugular.

6. That the crassamentum of portal blood does not expel its serum as fully as blood from other vessels, but remains a soft mass, unless artificial means be employed.

7. That portal crassamentum contains a considerably smaller quantity of fibrine: and

8. That portal blood, in general, contains a much less proportion of albumen and hæmatosine than jugular.*

* The less elaborated and more liquid state of portal blood tends to show a source of supply not generally supposed. If this blood be less rich, we can refer the difference only to the admixture of a diluent. And whence can this fluid be derived? Only, I conceive, from those surfaces on which we find the ramifying origins of the vein: the alimentary canal, and the stomach in particular, present a source of supply. Much of the liquid we drink is taken up, I conceive, by vessels which open directly into the radicles of the vena portæ, and thus enters the circulation without passing through the route of the lacteals. To physiologists it has long been a matter of observation and surprise that certain drinks show themselves in secretion much too soon to have traversed the intestinal lacteals, mesenteric glands, thoracic duct, and general circulation. Here we have a probable explanation

These inferences, it is obvious, have an important bearing on the theories of digestion and absorption. My present object, however, is not to pursue the subject into its applications, but merely to state the fair results of experimental inquiry.

Blood from the vena cava was examined in many of the experiments which investigated the contents of the vena portæ, as contrasted with those of the jugular. A considerable diversity was found, but the details are not sufficiently numerous and accurate to lead to any positive inferences. I may state, however, my conviction that there is a great difference in the state of the blood in different parts of the circulation, and consequently that the general opinion is incorrect, which considers the blood uniform in every thing but colour. I may hint also my suspicion that if the contents of four or six large vessels in different parts of the body were examined, no two specimens would be found the same, or even nearly the same, in their constituents and proportions. This opinion, if proved to be correct, may not be unimportant in Therapeutics.

Blood is a varying fluid. It varies every moment even in the same vessel; and at no time can we obtain blood with the same proportion of elements from two vessels

of the fact. The fluid received into the stomach is rapidly taken up into the vena portæ, transmitted through the hepatic veins into the cava, and thence thrown into the current of the circulating blood.

From the muddy character of portal blood, the tardiness and imperfection of its coagulation, and of the coagulation of its serum by heat, it seems probable that something more than water is taken up by the vein. Is it albumen in an imperfect state of elaboration?—The comparative rapidity of concretion arises, perhaps, from a lower state of vitality in the vessel.—The quantity of albumen is on the whole less in portal than other blood, but this does not militate against the supposition that a quantity of fluid as taken up by the radicles of the vena portæ contains a portion of this substance in an imperfect state.

even of the same order and size. The current of circulation is, like the stream of human life, incessantly pursuing its round, but as incessantly changing its elements. Sometimes much is thrown out of the blood by the secretions; sometimes much is taken up by the veins or lymphatics: and this contrast and variety are as applicable, I believe, to a single vessel as to the whole system. We at once see both the importance of this property, and the difficulty which it presents to the physiological investigation of this chapter. No two blood-vessels will yield at one time a fluid precisely similar; and I conceive that ignorance of this fact has occasioned many of the errors and the discordant statements of experimenters on the blood.

CHAPTER VI.

DIFFERENCES BETWEEN ARTERIAL AND VENOUS BLOOD.[α]

[It is not intended under this head to enter into the wide field of controversy which this part of Mr. Thackrah's treatise approaches, nor to test by experiment the various theories and hypotheses suggested by those who have endeavoured to reach that *ultima thule* of physiological research—the process by which venous blood is arterialized. The Editor's aim is briefly to notice the differences that may be observed in the physical and chemical constitution of these two branches of the circulation. By examining such data with minuteness and accuracy we lay the surest foundation for right conclusions, and are at least advancing a few steps towards that clear exposition of the phenomena of respiration and their effects on the blood, which medical science has so far in vain endeavoured to arrive at, but, we may hope, will eventually establish.

α Among Mr. Thackrah's MSS. there is a minute of his intention to write "a short chapter" on the important question which the title given to this division involves; the work would be incomplete, indeed, without it: but unfortunately, the lamented Author had not committed any of his opinions or inferences to paper. Some of his experiments, however, are in illustration of this part of his subject, and the Editor has endeavoured to fulfil his design by supplying the brief observations which form this chapter. In compiling them, the Editor has been scrupulously anxious not to intermingle any opinions of his own with those contained in the work he has undertaken to publish. Ed.

In the physical characters of arterial and venous blood the following distinctions have been remarked :—

1. In COLOUR. It is well known that arterial blood is generally of a florid scarlet hue, and venous of a modena purple. This difference is most striking in the vessels near the heart; but it is also observable in the minutest capillaries. It does not, however, uniformly exist. In the *puer ceruleus*, for instance, a portion of venous blood passes to the left side of the heart, without having been transmitted to the lungs, and the blood in the arteries has a tinge of purple: and in those diseases in which respiration is imperfect, there is a similar result. *Mr. Vines* has observed, that if the spinal marrow of a horse or ass be divided as close to the brain as possible, the moment respiration ceases the arterial blood becomes as dark coloured as venous, and of the same temperature.[a] Again, venous blood often approaches in colour to arterial, especially under great excitement. One example of this is stated in Sec. I. of the next chapter, where on bleeding a patient in a warm bath, *Mr. Thackrah* notes that blood from the basilic vein was scarlet. The shade of colour in venous blood is subject to considerable variation, which is either more frequent or more frequently observed, than changes in the hue of arterial blood. Some of the states that influence these changes are alluded to in the succeeding chapter.

The cause of this difference in colour is a question which has not yet been satisfactorily solved. The old opinion, that it arises from oxygenization of the ferruginous envelopes of the globules, has given way before the test of chemical analysis; and various other ingenious hypotheses have shared the same fate. Of those which are now advocated, the most generally received is that which ascribes the alteration to a decarbonization of the venous blood, and to a slight increase in the temperature of the arterial. *Dr. Stevens* has endeavoured to prove that the arterial colour is produced by a change in the saline ingredients of the blood, and has quoted experiments, in which a florid scarlet hue was given

[a] *Lancet*, xi. 423.

to crassamentum by dipping it in an artificial saline serum, and not by oxygen gas.[α] *Professor Turner* states that he is at a loss to draw any other inference from these experiments than "that the florid colour of arterial blood is *not* due to oxygen, but, as *Dr. Stevens* affirms, to the saline matter of the serum."[β] *M. Barrueil* on the other hand, asserts that oxygen gas produces a vermilion colour in dark blood kept for many weeks, even after some of the elements, and particularly the fibrine and albumen have become decomposed.[γ] Whether this is owing to the effect of the gas on the saline matters contained in the blood, and thus indirectly on its colouring particles, or what is the correct solution of the difficulty, must be left to future physiologists to decide.

2. The QUANTITY of blood is much greater in the veins than in the arteries. In addition to their functions as circulatory vessels, they perform the office of reservoirs, through which the current passes more or less languidly, according to the demands made upon them by the heart and the arterial system. Red blood is more abundant during youth than in manhood or old age, and is supplied most abundantly to those organs which are in progress of growth :[δ] in all cases, however, the dark blood pre-

α *On the Blood.* p. 16.

β *Elements of Chemistry.* Edit. iv. 904.

γ "L'action du gaz oxigène sur le sang noir m'a présenté un phénomène digne de remarque, et qui mérite bien, je crois, d'éveiller l'attention des physiologistes ; c'est que, conservé pendant plusieurs semaines, ce liquide jouit encore de la propriété de devenir vermeil, alors même que quelques-uns de ses élémens, et spécialement la fibrine et l'albumine, sont déjà soumis à la loi immuable de l'attraction dont la décomposition n'est que le résultat. Il semblerait que la matière colorante du sang, sur laquelle l'oxigéne se porte de préférence, est douée d'une grande force assimilatrice ou vitale, qui ne s'éteint que long-temps après la mort complète de tous les autres principes immédiates du même liquide."—*Annales d' Hygiène, Avril,* 1829. I. 269. The fact is remarkable, and has an important bearing, not only on the present question, but also on the theories of coagulation, and on the states in which fibrine and albumen exist in the blood.

δ BICHAT'S *General Anatomy, by Calvert,* p. 393, et seq.

ponderates. The relative quantity contained in each series of vessels is subject to such constant variation with almost every movement of the body, that it is impossible to state any definite measure of their average contents. The proportion of venous blood has been estimated at from two-thirds to three-fourths of the whole mass. *Sir C. Bell* supposes the veins to contain about thirty pounds of blood.[α]

3. TEMPERATURE. There is considerable discrepancy in the statements of authors on this point, and from its connexion with the theories of animal heat, it is one to which attention has been much directed.

Crawford asserts, that in the pulmonary vessels arterial blood possesses a larger amount of absolute heat than venous. The average deduced from his experiments is, that the capacity for caloric of the fluid (arterial) in the pulmonary veins, is to that (venous) of the pulmonary arteries, as 97·08 : 112, or nearly, as 10 . $11\frac{1}{2}$;[β] *Majendie* computes it at 852 : 839.[γ] *Dr. J. Davy* has denied that there is any appreciable difference. In the degrees of heat indicated by the thermometer, there is a more positive distinction. In one experiment, *Mr. Thackrah* found the temperature of blood from the jugular vein of a dog 95°, and that of the axillary artery 102°; but this exceeds the variation usually noted. In the experiments of *Sir C. Scudamore,* and those of other investigators, the difference averaged from one to two degrees. *Majendie* states 101·75 to be the mean grade of venous blood, and 104° that of arterial. However we may account for the higher temperature of arterial blood, or whatever theory of animal heat we may adopt, *Dr. Holland* has shown that even this slight difference is sufficient, when the rapidity of the circulation is considered, to produce an ample supply of warmth to every part of the system.[δ]

α *M.S. notes of Lect. on Physiology. London University,* 1829.

β *On Animal Heat.* p. 277.

γ *Compendium of Physiology,* transl. by Dr. Milligan, 310.

δ See his interesting "*Exper. Inquiry into the laws which regulate the phenomena of organic and animal life.* Edinb. 1829."

4. The specific gravity is very nearly the same in the two kinds of blood. *Dr. Davy* stated the average of his experiments at 1049 arterial, and 1051 venous. In one example given by *Scudamore*, the numbers were alike; in another 1055·5 to 1053·5. *Mr. Thackrah* had examined the specimens in three experiments, viz:—

CL. In a small dog, arterial	1045, venous,	1046
CLI. Middle sized dog, ...	1057, ...	1036
CLII. .... , ...	1043, ...	1041

Amid these contradictory results we cannot arrive at a certain conclusion. It appears that no uniformly marked difference is to be observed.

5. Coagulation is said by some writers to take place soonest in arterial blood, and by others in venous. There is, at any rate, so little variation in their periods of concretion, that the differences stated may be ascribed rather to other causes,—as the comparative size of the streams of blood when drawn, the rapidity of its abstraction, or the form, or temperature, or material of the vessels into which it is received, than to a uniform character of the one or other kind of blood. *Richerand* and *Majendie* assert that arterial blood coagulates more readily than venous, and has a smaller proportion of serum. *Sir C. Scudamore* found this to be the case in one instance, but in three others the venous blood coagulated sooner than arterial. *Mr. Thackrah's* experiments are no less contradictory.

Exp. CLIII.	Blood from the femoral artery of a dog concreted in	′ : 45″
	.... jugular vein of the same animal......	2′ : —″
CLIV.	Blood from the femoral artery	—′ : 90″
	.... jugular vein.....	—′ : 35″
CLV.	Blood from the axillary artery	3′ : —″
	.... jugular vein	in a few seconds.

There is so little variety, indeed, in the physical characters alluded to in the last three sections, that physiologists have thought it immaterial whether venous or arterial blood, or, as in the case of animals slaughtered by the butcher, a mixture of both, be used in their experiments on the constitution and changes of this fluid. If the statement of *M. Le Gallois* be correct, that arterial blood is the same in all parts of the system, while the contents of the veins are dissimilar in every vessel,[α]—and reasoning on theory alone, we should be led to anticipate such a conclusion,—it is to be wished that pure arterial blood only should, in future, be made the standard by which all the physical characters might be investigated. If so, we, perhaps, might look for more frequent coincidence in the details of experimenters on the blood, than at present unfortunately is to be met with. The comparative difficulty of obtaining that from arteries, however, has generally led them to employ venous or mixed blood in their investigations.

6. Chemical Analysis has detected only slight differences in the composition of these two fluids. Arterial blood generally appears thinner, and yet it is found to exude less serum than venous. Its odour is also stated to be more pungent. *Dr. Stevens* asserts that "there is not one particle of free carbonic acid in healthy arterial blood," but "that it exists in the venous, even in that of the smallest veins."[β] *Mr. Thackrah* has ascertained the relative proportions in two instances.

Exp. CLVI.

Portions of blood taken from the femoral artery and the jugular vein of a small dog, analysed twenty-four hours after being drawn, were found to contain

Arterial blood.			Venous Blood.		
Serum	439·6	or 10 : 12·7·	Serum	515·2	or 10 : 9·0
Crassamentum	560·4		Crassamentum	484·8	
	1000·0			1000·0	

	Arterial	Venous
Dried fibrine	1·8	2·4
......serum	45·4	41·0
......crassamentum	106·0	130·4
Water	846·8	826·2
	1000·0	1000·0

α See note α, page 98. β *Op. citat., page* 39.

Exp. CLVII.

Blood of a dog from the axillary artery.		Jugular vein.
Dried fibrine	3·0[a]	3·2
.... serum	53·0	40·7
.... crassamentum	209·0	194·5
Water	735·0	761·6
	1000·0	1000·0

Of their ultimate constituents *M M. Macaire* and *Marcet* have detailed the following average.[β]

	Carbon.	Oxygen,	Nitrogen.	Hydrogen.
In arterial blood	50·2	26·3	16·3	6·6
.. venous	55·7	21·7	16·2	6·4

In this table we observe that venous blood contains 5·5 per cent. more carbon than arterial, and is deficient in almost an equal proportion of oxygen. The changes which take place during respiration satisfactorily account for this, as shown by the interesting experiments of *Priestley, Lavoisier, Davy, Allen* and *Pepys, Dr. Edwards,* and many other distinguished philosophers to whose writings it is unnecessary further to allude. *Dr. Dalton* ascertained that eleven ounces and a half of carbon were taken into the stomach daily by a person in ordinary health, in his solid and liquid food, and an equal quantity daily given off from the

α Dr. Alison states that arterial blood contains a larger proportion of fibrine than venous.—*Outlines of Physiology, p.* 62.

β *Mem. de la Soc. de Physique, &c. de Genève,* T. V. 400, quoted by Dr. Roget *in Bridgewater Treatise,* vol. II., 335.

Dr. Stevens conceives that not only oxygen, but atmospheric air in an undecomposed state, enters arterial blood during the changes in respiration, and that "the essential difference betwixt the arterial and the venous blood is that *the former contains pure air and the latter carbonic acid.*" Dr. Paris refuses to admit Dr. Stevens' theory. *Observations on the Theory of Respiration* in *Medical Gazette,* XXI, 49. and Paris' *Pharmacologia,* last edition, p. 135. See also a valuable paper read to the Royal Society of Edinburgh, by Dr. Christison, in *Edinburgh Journal,* XXXV. 94.

body, of which ten ounces and a half are supplied from the venous blood, and expired in the form of carbonic acid gas.[α]

7. In their functions the distinction between arterial and venous blood is well known. The one carries warmth and energy through the system to its remotest capillaries, stimulates the brain and nerves to continued activity, and supplies pabulum to most if not all the secretions: the other returns from the capillaries altered in colour, and no longer possessing its former capabilities; it has lost some portion of its constituents, varying according to the tissues or glands it has permeated; has taken up the waste of different organs in its route; has been diluted by an influx of water, and undergone a process not well understood in the vena portæ, during its passage through the abdomen; and on its approach to the heart, has received a last alteration by the admixture of a minute quantity of chyle, before it is consigned to the laboratory of the lungs, again to be converted into arterial blood.

Both, therefore, have their peculiar offices, and one cannot be substituted for the other without evident derangement of the system. *Bichat* inferred, from repeated experiments, that venous blood acts as a poison, when introduced into the arterial current;[β] but *Dr. Kay* has recently proved that, both in cases of asphyxia, and in those where blood has been artificially transfused from veins into arteries "the venous blood does not possess any noxious quality by which the organic functions of these tissues (lungs, heart, and muscles) can be destroyed, but is simply a less nutritious and stimulating fluid than arterial blood."[γ]

M. Bellingeri has stated that "the electricity of venous blood is generally rather superior, sometimes equal, but never

α In a paper read at the first meeting of the British Assoc. for the Advancement of Science at York, 1831. See *Reports*, vol. I. 73.; also *Transactions of the Manchester Philosophical Society*.

β *Recherches Physiologiques sur la vie et la mort*. Paris, 1805.

γ *The Physiology, Pathology, and Treatment of Asphyxia*, 1834, p. 182.

inferior," to that of arterial.[a] This is the reverse of what might have been expected: it is probable, however, that this condition, as well as most of the other distinctions which have been noticed, are considerably influenced by the states of constitution, and other causes, which form the subject of Mr. Thackrah's next chapter.]

[a] *Annali Univ. di Medicina, April,* 1827, quoted in *Lancet,* xiii. 810.

CHAPTER VII.

EFFECTS OF STATES OF THE ANIMAL SYSTEM ON THE BLOOD.

1. THE TEMPERATURE OF THE BODY is subject to such slight variation, that we can expect from this cause little change in the character of blood. In cold regions, however, the blood is stated to be darker than in temperate ones; and within the tropics, I believe, to be somewhat brighter. In this country, a horse in a straw-fold will have dark blood; and removed to a stable, he presents it considerably lighter. Such effects may be attributed less to the few degrees of variation in the temperature of the body, than to the reduced or excited state of the circulation. Cold is well known to reduce the action of the lungs and heart, heat to increase them: and to this change in the rapidity with which the blood is transmitted through the system, I should ascribe its variations of colour. On bleeding a man immediately after submersion in a warm bath, I found the blood of the basilic vein scarlet.

Concretion and the exudation of serum are affected probably on the same principle.

The buff coat, it appears, may be produced by a certain degree of cold. *Mr. Vines* states, that a temperature of

45°—55° renders the blood of the ass buff-coated; that a change to above 60° removes that character from the blood of the same animal; and that a reduction of temperature below 35° also removes it.[a]

2. The influence which SEASON produces on the constitution, and through it on the blood, I am not prepared to discuss. Its effects, however, on digestion and secretion can scarcely fail to cause some correspondent changes in the character of the blood; but a long range of observations on this subject would be required for the developement of general rules; and these, perhaps, would be so little remarkable as scarcely to repay for the time and labour of collecting them.

3. AGE has considerable effect on the state of the blood. Its quantity is greater in youth than in advanced life. At an early period this fluid is, in general, comparatively bright in colour, coagulates quickly, throws off little serum, and leaves the crassamentum soft and watery. We have observed putrefaction to commence very soon in the blood of the young. Fibrine is generally stated to be scanty, but the observation is not sufficiently established. In reference to the proportion of water, *M. Denis* states that a few days after birth this is low, and adduces an experiment on five puppies a day old, in which it was but 78 per cent.; yet this number is as high as that in many of his experiments on the adult. He found the proportion soon to advance, however, though not very greatly; and the average from 5 months to 10 years is eighty-three per cent.; from 10 years to 20, eighty; 20 to 40, seventy-six; 40 to 50, seventy-six; 50 to 60, seventy-eight; 60 to 70, seventy-nine per cent. The proportions he states in reference to the globules, "la partie

[a] *Lancet*, xi. 294.

en suspension," hæmatosine and its conjunct albumen, with a minute quantity of iron, are from 5 months to 10 years, eight per cent.; 10 years to 20, eleven; 20 to 30, fourteen; 30 to 40, fourteen; 40 to 50, thirteen; 50 to 60, twelve; 60 to 70, eleven per cent.[a] Hence it appears, that the red matter is decidedly in greatest proportion at the middle period of life; less in the aged, and much less in the infant. These statements, it should be remembered, are founded chiefly on experiments with morbid blood, and the character of diseases at different periods of life may affect the result. I believe the inferences, nevertheless, if not fully satisfactory, are at least an approach to the truth. May we not consider the greater quantity of hæmatosine and albumen which appear in middle age, as a provision for the greater exertions, physical and mental, of this period?

M. Denis remarks that when the albumen predominates in the young and the aged, it is only where the colouring substance is in small quantity. The red matter of the crassamentum to which he refers, we have before shown to be chiefly albumen; and the fact he alludes to may therefore depend on a transfer of the albumen between the serum and the crassamentum. Some observations on diseased blood in the next chapter will countenance this supposition.

In advanced life the fibrine is believed to decrease; and at this period we may infer, from the ossific deposits so frequent in the vessels, that the phosphate of soda is in excess.

4. Sex appears to cause some difference in the state of the blood, but chiefly, I conceive, by the different habits of living. We have generally found more water and less fibrine in the blood of females than in that of males.

[a] Denis *sur le sang*, p. 288, 290.

5. The state of the constitution induced by MUSCULAR EXERCISE affects the blood. This is obvious from the brightened colour of the countenance, and the subsequent sweating induced. *Mr. Vines* states,[a] in reference to the horse and ass, that great exertion produces a buff coat on the blood, but moderate exercise prevents it.*

The effect of muscular exertion is more marked in the flesh of the common fowl. Restrained from the full use of its wings, the bird presents these parts white, while the rest of the body is dark red. The domestic state, by diminishing the exercise of animals, reduces the quantity of blood in the system at large.

The changes which take place are evi 'ently the result of increased circulation and respiration on the one hand, and of secretion and absorption on the other; the first heightening the scarlet hue of the blood, the latter, as one or other predominates, diminishing or increasing its watery parts. An *habitual* practice of muscular exertion has, no doubt, a marked effect on the chaiacter of the blood, through the medium of the constitution, and on the nervous system no less than on the vascular. In the well-fed labourer the crassamentum is richest in hæmatosine and albumen.

6. DIGESTION and DIET have, of course, a great effect on the blood. When the digestive organs are

[a] *Lancet*, xi, 294.

* A contradictory experiment, however, is given by MR. PERCIVAL. "A horse, to every appearance in perfect health, was bled to one pound; after which he was galloped (for the space of about twenty minutes), until he sweated profusely; while under extreme agitation, from the exertion, another pint of blood was drawn by unpinning the same orifice. The coagulum of the first parcel of blood was sizy, tough, contracted, and deeply cupped; that of the last exhibited no signs whatever of buff, was extremely loose and flabby in its texture, so that on being handled it readily mingled with the serum; and in a much shorter time than the first went into the putrefactive state."

impaired, the chyle must be different in quantity or quality, and the blood consequently degenerate from a state of perfect health. Habitual vomiting is said to reduce the proportion of red matter, and to increase the element of water. Fasting will be expected to have a similar effect. On this branch of the subject we have made numerous experiments, but as the detail of the whole of them would be tedious, I have made a selection, and thrown it into a tabular form.

Blood was taken from the jugular veins of dogs at different periods after feeding, and its character examined. The effects of *extreme* fasting we have not ascertained.

Exp. CLVIII. to CLXVI.

DOGS WHICH HAD FASTED.

Physical character of the animal.	Hours after feeding.	State of the lacteals.*	Period of the blood's concretion.	Dry contents of serum.	Dry fibrine.	Dry hæmatosine and albumen from crassamentum.
....	......	....	1′ 40″			
....	......	....	2′ 10″			
Large	12	....	0′ 45″			
Large	12	....	....			
Large, fat...	......	....	1′ 16″			
Large male	18	invisible	2′ 18″	33·0	6·2	143·0
Do.	18	slightly distended	2′ 0″	36·0	6·6	198·0
Do.	18	invisible	0′ 45″	27·0	3·6	208·0
Do.	18	invisible	0′ 55″	36·0	3·0	174·0
		Averages	1′ 28″	33·0	4·8	180·3

* In our experiments on dogs, we have found the lacteals distended three or four hours after a meal, but rarely visible at other periods. My excellent friend, Dr. Whytehead, my quondam pupil and assistant, found in a dog which was greatly emaciated, though its appetite had been good, the contents of the jugular vein flow in two distinct streams, of about equal size, one sanguineous, the other milky. Did not the emaciation of this animal depend solely on a defect in the process of assimilation? In most cases of emaciation with craving appetite, the mesenteric glands, or some parts of the chylous apparatus, have been found diseased; but this was not the case with the dog alluded to.

DOGS WHICH HAD RECENTLY BEEN FED.

Physical character of the animal.	Hours after feeding.	State of the lacteals.	Period of the blood's concretion.	Dry contents of serum.	Dry fibrine.	Dry hæmatosine and albumen from crassamentum.
....	......	....	1′ 25″			
....	......	....	1′ 50″			
Large	2 3	....	0′ 10″*			
Large	1½ 2	....	0′ 30″			
Middle size, in tolerable condition ...	4	full	0′ 40″	30·0	3·4	205·0
	5	little distended	0′ 45″	26·0	5·3	200·0
	4	moder. distended	....	52·0	4.5	167·0
Bitch	4	do.	....	30·0	5·0	196·0
Small lean do.	4	do.	....	27.0	3·0	158·0
		AVERAGES......	0′ 53″	33·0	4·2	185·1

The specific gravity of the blood was examined in many cases, but some doubt as to the accuracy of the several observations leads me to omit them. I may state, however, that they exhibit a universal superiority of specific gravity in the blood of dogs which had fasted.

From the preceding table it appears, that blood from the fasted animal does not so quickly concrete; that its serum contains a proportion of albumen about equal to that from one recently fed; that its crassamentum yields rather more hæmatosine, albumen, and fibrine; and, as a consequence of all these, rather less water. The blood, in fact, is rendered somewhat denser by inanition, though on the whole, the contrast is by no means striking.

The principal difference lies in the period of concretion. Is not the greater disposition to cake, in blood from a recently fed animal, dependent on that want of vital energy in the general system, which results from the activity of the digestive organs, and the more than ordinary quantity of this energy which they demand. I scarcely need refer in illustration to the well known languor, chilliness, and shivering which sometimes occur in man after a full meal, and to the more marked sluggish-

* The blood flowed in a small stream.

ness and drowsiness which are constant in the dog. That the disposition in blood to concrete is proportionate to the debility of the system, we shall soon be able to prove and illustrate.

The trifling excess of water in the blood of animals that have been recently fed, may be referred to the absorption from the stomach of the thinner parts of the aliment, before the more solid constituents can be elaborated by the small intestines and lacteals.

Is the milky, or cream-like appearance, which serum sometimes assumes, dependent on digestion? This appearance has been the subject of frequent narration and comment. *Hewson* quotes many examples; and conceives the substance produced by the absorption of fat. Since his time, our periodical publications have adduced numerous instances, but without, so far as I am aware, any satisfactory explanation of the phenomenon. Like other practitioners, I have often witnessed the appearance, but have not found it attended by any accordant peculiarity of constitution or disease. *Dr. Marcet's* observations in the Medico-Chirurgical Transactions render it probable that the substance referred to is derived from the chyle of animal food. He deems it allied to cream; and *Berzelius* states that a portion he examined consisted truly of this fluid and albumen.[a] What chemical observation leaves doubtful, physiological experiment has established. We

[a] M. Raspail considers that this phenomenon is produced by the presence of an acid in the blood which saturates the alcaline menstrum of the albumen, and hence it is precipitated from the serum. He remarks, that excess in the use of spirituous beverages, or inflammatory action, may give rise to this effect. *Chimie organique*, p. 381. Dr. Babington gives the sp. gr. of milky serum at "1019 to 1024, while the average specific gravity of healthy serum may be stated at 1029." In treating the blood with æther, by which he detected the presence of a concrete oil, in the proportion of from two to four parts in 1000, (query—is not this identical with the "graisse phosphurée" of Vauquelin, Chevreul, and Denis? see p. 48.) Dr. Babington did not

find in dogs, that when the lacteals are fully distended, this cream-like appearance is almost always presented in the blood. Indeed, we can generally produce it at will by taking blood a certain time after a full meal. Without adducing observations which run through a course of experiments on other subjects, I give but one example.

Exp. CLXVII.

Four hours after a hearty meal, a dog was destroyed. The lacteals were found distended; and the blood from the jugular, portal, and caval veins presented a cream-like appearance in the serum.

From such as this, contrasted with negative observations, we infer that *the substance mixed with serum is chyle not yet elaborated into blood.*

The quality of the food affects the character of the blood. Vegetable diet appears to dilute the blood, animal to thicken it. This observation applies to the hæmatosine and water; but I am not aware that the proportion of albumen is changed. The fibrine appears to be somewhat, but not greatly, reduced by low diet. These effects of vegetable food, however, do not seem to be permanent. A person suddenly removed from a rich animal to a vegetable diet soon looks paler and feels weaker, but when the constitution has become accustomed to this change, the appearance and strength of the individual improve: and I am not aware that the inhabitants of countries where little animal food is consumed, have thin or deteriorated blood.

find that the opalescence of the serum bore any relation to the quantity of unctuous matter obtained, but that the oil separated more readily from milky than from clear serum. He noticed also, that in every specimen of the former kind, "the albumen was remarkably deficient."—*Medico-ch. Trans.* xvi. 54. 56. Ed.

It appears from observations in disease, that low diet predisposes to dropsy. The medical charities of Dublin, especially, present numerous cases of this disease in the persons of vagrants. *M. Gaspard* describes the effect of a wet harvest in 1816, on some of the departments of France. The wretched inhabitants were obliged to subsist on vegetables, and contend for their food with the herbivorous animals. Cellular dropsy, without disease of the liver or ascites, was the wide-spread result. A similar want of proper nourishment produced dropsy at Guadaloupe, and carried off four thousand negroes. May we not suspect the cause of this serous effusion to be in the altered character of the blood, the reduction of its stronger elements, and the consequent increased ratio of water ?

On the other hand, a large quantity of animal food, without proportionate labour, seems to reduce too much the aqueous part of the blood. It produces an inordinate secretion of uric acid from the kidneys, and in consequence, the gravel: frequently also, concretions on the joints. These states of disease, not confined to one organ or part, obviously point to a morbid condition of the blood as their cause; and in the present state of animal chemistry, we can only refer this condition to an excess of azotized food. It is not, however, my part at present to proceed with references to disease.

7. The state of the general system, as to FATNESS or LEANNESS seems to affect the quantity and character of the blood. Fat animals have, I believe, considerably less blood in proportion to their weight than lean ones; and in the fat human subject, venæsection shows the veins to be comparatively small, and the quantity of blood, even

when two or three vessels are opened, less than flows from one vein of a lean person.

The quality of the blood appears also to be changed. In several cases we have found the albumen and hæmatosine in a relatively small quantity, and the proportion of water consequently large. We have not yet observed any difference in the proportion of fibrine as effected by the same causes.

8. The state of the MUSCULAR SYSTEM is generally supposed to have a relation to the quantity of fibrine,—the labourer to have most, and the debilitated least. Our examination controverts this opinion. Of the six individuals who offered their blood as a standard of ordinary health, two were literary men, who, of course, sit much and use no strong manual exercise; one was a house servant; one was a stout and active medical apprentice; and the other two were recruits for the army, strong and muscular young men. Yet the proportion of fibrine was in the order in which I have referred to the individuals,—great in the literary men, and least in the muscular recruits, viz.—in the ratios of 3·8, 3·6, 3·4, 2·5, 2·, 2·.

9. IMPRESSIONS ON THE NERVOUS SYSTEM, independent of the more permanent and universal strength or weakness of the system at large, which we have last examined, have a remarkable effect on the blood. Syncope immediately disposes the blood to concrete. In venæsection, when this process has commenced in five minutes, faintness has reduced the period to two; and when ninety seconds were before required, deliquium has instantly caused the blood to cake in forty. In this way we have found the warm water bath to accelerate concretion.

Terror, in brutes, appears to have a like effect. My assistant, *Mr. Bell*, in taking blood, has often remarked when the dog shows much alarm, that concretion takes place almost immediately; but when the fright subsides, this change does not occur for one or two minutes.

Mayer, when he tied both pneumo-gastric nerves in animals, found the blood in the whole pulmonary system coagulated, and separated into fibrous and coloured portions.

The influence of electricity in its various forms of manifestation has been already shown greatly to affect concretion.

10. The effects which are produced on the blood by the state of the general system in reference to STRENGTH or DEBILITY is an inquiry important in itself, and interesting from the contradictory statements of authors of eminence on this point.

Let us advert first to the period of concretion. Here we find *Hewson* and the late *Mr. Hey* strongly opposed. On this subject it is astonishing that such a difference of opinion should exist, the experiments are so readily made, the results so distinctly evident, and so generally, if not universally, accordant. To ascertain the point in dispute, I instituted repeated experiments in 1818 on the blood of oxen, sheep, horses, dogs, and swine, carefully noting the periods of concretion, as connected with the state of the vital powers. Aware of what I have before remarked, that coagulation commences speedily in proportion to the paucity of the blood, I took care that no disparity in the size of the stream should invalidate the result of my inquiries—each vessel receiving its contents from a full uninterrupted flow. To avoid unnecessary details, the following experiments only are stated.

Exp. CLXVIII.

A dog was bled to death by dividing the vessels of the neck. The blood received in a glass on the first gush from the wound began to concrete* in 1′ : 10″. A second portion, taken two minutes afterward, began to concrete in 40″. A third quantity, received immediately before the death of the animal, became *instantly* caked.

Exp. CLXIX.

Blood was received in three small cups from the neck of a slaughtered sheep. The first was filled immediately on the knife dividing the vessels; the second about one minute and a half afterwards, and the third a few seconds before death. The time which elapsed between the filling of the cups, and the commencement of coagulation was, in the first, one minute and a half; in the second one minute, and in the third half a minute.

Exp. CLXX.

Under similar circumstances two small cups were filled; the first on the incision being made, and the second when the animal was greatly reduced.

Coagulation commmenced in No. 1 in ... 1′ : 10″
.... 2 ... .. 0′ : 50″

Exp. CLXXI.

The blood of a slaughtered ox was received in three cups; No. 1 being filled on the first flow; No. 2 about

* It should be remarked that in the experiments coagulation was noted as commencing, when a clot could be perceived, and not when the blood had assumed a concrete form. Hence, this process may appear to have taken place sooner in the instances here detailed than in those of other writers. When a small animal is bled, a coagulum soon forms round the wound, and the blood subsequently drawn concretes almost immediately. On this account I have omitted to detail some experiments on whelps and other young brutes.

three minutes afterward; and No. 3 a short time before the death of the animal.

Coagulation commenced in No. 1 in ...... 3′ : 40″
....2 .... 6′ : 45″
....3 .... 0′ : 55″

Exp. CLXXII.

A similar experiment.

No. 1 .... 2′ : 29″
... 2 .... 8′ : 30″
... 3 .... 0′ : 30″

Exp. CLXXIII.

A struggling ox.

No. 1 .... 2′ : 50″
... 2 .... 1′ : 10″
... 3 .... 2′ : 15″*

Exp. CLXXIV.

A large ox.

No. 1 .. .. 2′ : 30″
... 2 .... 1′ : 35″
... 3 .... 1′ : 10″

Exp. CLXXV.

A slaughtered hog.

No. 1 .... 1′ : 30″
... 2 .... 0′ : 50″
.. 3 .... 0′ : 20″

Exp. CLXXVI.

Blood was received from a stuck horse at four periods, about a minute and half intervening between the filling of each cup:—

No. 1, ... 11′ : 10″
... 2, ... 10′ : 5″
.... 3, ... 9′ : 55″
.... 4, ... 3′ : 2 0

Of my remaining experiments on animals several were, from accidental circumstances, rendered inaccurate, but the rest so much resemble those I have detailed as to require no particular notice. From these statements then, it appears, that in the dog, sheep, horse, and hog, the *blood concretes slowly in regular proportion to the tonic state, or that condition of the system in which the vital powers are strongest*: blood received immediately before

* In but one other ox, and this a remarkably unruly animal, did I find coagulation commence late in the last cup.

the death of the animal first assumes this change; next that which is taken at the middle period; and lastly, that which is received on the first effusion from the wound. In oxen, however, it was frequently found (as in Exp. CLXXI. and CLXXII.) that coagulation took place most slowly at the middle period. At first I was at a loss to account for a circumstance so much at variance with almost every other observation. On reflection, however, I concluded that the mode by which oxen are slaughtered was the cause of the disparity; the animal being first stunned with repeated blows on the head, and afterward bled to death by a division of the jugular vessels. The ox on falling, I conceive to be in a state resembling apoplexy, with the vital powers languid; but after the loss of a considerable quantity of blood, the nervous system is relieved of its burthen, and the constitution regains somewhat of its wonted vigour. Here, then, the powers of life are strongest at the middle period; next so is the first or comatose state; and the weakest is after great evacutions, or on the eve of death. I have further noticed, that when a comatose state is not induced, either from the inexpertness of the butcher in striking the ox, or from the animal being more than ordinarily tenacious of life, concretion commonly takes place in the same order as that which obtains in the blood of other quadrupeds. Coagulation was twice observed to commence slowly in the last-received portion of blood, but this exception being found only in the ox, may be attributed to the deranged state of the nervous system.

Soon after the preceding paragraph was written, I had the opportunity of conferring with *Mr. Hey* on this subject. Being informed of the different results of our experiments, he repeatedly expressed a desire for our jointly re-examining the point in dispute, and I was, of course, as willing to accept the offer as he was to make it. *Mr. Hey's* experiments in reference to the comparative periods of coagulation, were made only on the blood of the sheep: it was determined, therefore, that to these animals our observations should be confined. He had also received the blood in half-pint glasses, and though I did not conceive this mode free from objection,* they were used in the first experiment.

The late *Mr. Hey*, the present *Mr. Hey*, three of their pupils, and one of my own, conducted or watched the experiments.

EXP. CLXXVII.

Blood was received in three glasses; the periods of coagulation were not minutely noted, but it was universally admitted that concretion took place soonest in the last-received blood, and latest in the first.

EXP. CLXXVIII.

Three cups were filled with blood issuing from a wounded sheep. The periods of receiving the blood were not remarked precisely, but the intervals between each was fully half a minute. *Mr. Hey* observed that the first concreted at the same moment with the third, and he conceived this in some measure to support his opinion. But it was remembered that a minute had elapsed between the filling of the two vessels, and that consequently the result was in favour of coagulation commencing latest in the first-drawn blood. It was also noticed that the third vessel did not contain the blood of an animal in the greatest stage of reduction, for the sheep lived more than a minute after it was filled. As, how-

* It appears requisite that the size of the vessel bear a relation to the quantity of blood in the subject of the experiment. If the animal be small, or have but a minor portion of vital energy, there will be danger of the blood at the bottom of a large glass assuming the concretory change before the vessel is full.

ever, *Mr. Hey* seemed unwilling to admit the conclusion, a third experiment was instituted, and the greatest care taken to render its result accurate and decisive.

Exp. CLXXIX.

Three cups were filled at intervals of half a minute. One of *Mr. Hey's* pupils took the first, and with a seconds watch in his hand, accurately noticed the time of its being filled, and the precise period of concretion. Another of *Mr. Hey's* pupils took the second, and observed the coagulating process with the same attention; and *Mr. William Hey* took the third.

1st cup began to coagulate in	2′ : 10″.
2nd	1′ : 45″.
3rd	0′ : 55″.

The result was too conclusive to admit of doubt, and *Mr. Hey* declined any repetition of the experiment.*

* Mr. Hey had an idea that the difference of result in the published experiments of himself and Hewson might depend on the seasons in which they were performed, but subsequent observations, at all parts of the year, discredit this supposition.

[To the above notice of Mr. Hey it affords the Editor gratification to add the following eulogium from the appendix to Mr. Thackrah's former edition.]

While this chapter was in the press (*March* 23, 1819) the lamented death of Mr. Hey occurred: and I cannot refrain from paying a tribute to his worth, though my feeble panegyric can neither raise his character, nor prolong its remembrance.

To his zeal for the advance of knowledge, to his eminent attainments, and to his devotedness to the interests of humanity, no ordinary eulogium is due. But to expatiate on his merits, to those who knew him, were superfluous,—for to know was to admire.

While we venerate his memory, may we copy his example; and by a like integrity and steadiness of conduct, by the same undeviating attention to the discharge of our professional duties, and by a similar ardour in the pursuit of useful knowledge, seek that honourable distinction to which he attained!

The general inference from the foregoing experiments, made in 1818 and 1819, has been abundantly confirmed by subsequent observations, a few made with a direct reference to this point, but chiefly when the object of inquiry has been unconnected with concretion of the blood. The mode in which the animal is destroyed does not seem to affect the result. Pithing, poisoning by prussic acid, and the injection of air into the veins, show as strongly as death from hæmorrhage, the last-drawn blood to concrete with greatest rapidity.

Having examined the concretion, we now turn to the second stage of coagulation,—the exudation of serum. Our observations show a retardation or imperfection in this process caused by a state of debility. *Blood taken from an animal in articulo mortis never fully separates its serum, and rarely throws off even a small quantity.* This is an observation made so often, and so uniformly, that it is unnecessary to adduce any experiments in illustration.

To show the effect of less extreme debility;

Exp. CLXXX.

Blood was received from the neck of a stuck calf in two portions, the first on the infliction of the wound, and the second when the hæmorrhage had almost ceased. Examined after twenty-four hours, these yielded,

First-drawn blood.		Last-drawn blood.	
412·3 Serum 587·7 Crassamentum	or 10 to 14·2	361·7 Serum 638·3 Crassamentum	or 10 to 17·8

In the last it is obvious that the crassamentum threw off a considerably smaller proportion of serum.

Exp. CLXXXI.

A similar experiment on a slaughtered sheep.

First-drawn.		Last-drawn.	
484·6 Serum 515·4 Crassamentum	or 10 to 10·6	391·7 Serum 608.2 Crassamentum	or 10 to 15·5

Lest the paucity of the effusion should be supposed to depend on a reduced proportion of water in the blood last drawn,

Exp. CLXXXII.

The materials of the preceding experiment were placed on a sand-bath, and when perfectly dry the solid constituents of the two portions of blood respectively ascertained.

First-drawn.	Last-drawn.
188·1 solid contents.	188·2 solid contents.
811·9 water.	811·8 water.
1000·0	1000.0

In no other experiment have we found so little variety in the solid and watery contents of the two portions. Many experiments to which I shall refer when discussing the effects of hæmorrhage, exhibit a great diversity, but few show a considerable increase of solid in the last-drawn blood, and not one of them such an increase as would account for the slowness and paucity of the serous effusion. I may assert, therefore, that *although debility remarkably promotes concretion, it as decidedly retards and diminishes the exudation of serum.* The crassamentum consequently retains a considerable proportion of this fluid.

How shall we account for these facts? The abstraction of nervous influence from the vessels accounts for the speedy commencement of coagulation, because the elements are more completely abandoned to the action of their chemical affinities. But how shall we explain the subsequent retardation and imperfection of this process? I know not: nor can I offer a conjecture.

The proportions of serum, crassamentum, and fibrine are considerably affected by the strength or weakness of the system at large, but what I have to state on this subject is mixed up with the matter of the next section.

10. The effects which THE ABSTRACTION OF BLOOD has on the state of that which remains, is an important inquiry. I have before sufficiently discussed its effects on concretion in the last section: let us now regard it in reference to the constituents of the blood.

If the blood from a slaughtered animal be received in vessels at the interval of a minute from the introduction of the knife to the convulsion of death, and the contents of these vessels subsequently examined, we find considerable variety in the proportion of serum and crassamentum, and in their ultimate constituents. No two portions of blood exactly agree. In reference to the proportions of serum and crassamentum, and of solid and fluid, some early observations led me to suppose the fluid relatively increased during the slaughter of an animal, and the bearing of this idea on physiology and pathology led me to pursue the investigation by numerous experiments, but I have been disappointed in their result. They show considerable changes in the proportions of elements during bleeding, but they do not show the last-drawn blood to be invariably thinner than the first. They establish no correct and satisfactory conclusions; but tend only to a general observation, that if blood be taken at three periods during the slaughter of an animal, the serum will be greater in the first two than in the last, while in this the comparative softness of the crassamentum will account for the difference: and that the proportion of water ascertained by analysis, though subject to the greatest variety, seems to be less in the second vessel than in either of the others.

Turning from brutes in health to man in disease, we find from experiment that there is here also considerable variety in the portions of blood taken at the same bleeding:[α] that the serum is not uniformly increased nor the proportion of water augmented during the flow of blood. This observation applies to those chronic maladies for which venæ-section is sometimes, but not frequently, performed. In reference to acute and more urgent diseases I adduce two examples.

Exp. CLXXXIII.

About a pound of blood was abstracted from the arm of a muscular man labouring under angina pectoris. It was weighed three days afterwards.

	Serum.	Crass.		Serum.		Crass.
1st cup contained	160	360	or as	10	:	25·2
2nd	420	594	......	10	:	14·1
3rd	418	736	......	10	:	17·6

Great faintness occurred on filling the third cup.

Exp. CLXXXIV.

Blood from a patient labouring under fever with disease of the liver.

α See experiments 10 and 11 in Appendix II.

	Serum.	Crass.		Serum.		Crass.
No. 1	260	335	or as	10	:	12·8
2	500	520	......	10	:	10·4
3	400	526	......	10	:	13·1
4	440	500	......	10	:	11·3
5	450	536	......	10	:	11·9

No disposition to deliquium.

The change in the character of blood found some hours subsequent to bleeding is much more marked. A man who is bled two or three days in succession has, of course, the operation performed on account of unreduced or advancing disease. This necessarily interferes with the result; for the quality of the blood changed by the first venæ-section, may be rechanged, and will at least be modified by the influence of the malady. Nevertheless, the proportion of serum to crassamentum, or better of water to solid, will, in a majority of cases, manifest a consistent modification. The serum and water are increased.

The most satisfactory inference, however, on this subject, is drawn from examination of the blood of animals which have been bled before they were slaughtered.

Exp. CLXXXV.

A calf was killed in the usual way, sixteen hours after a very large bleeding, and subsequent refusal of food. Twenty-four hours after, the crassamentum, which in calves that have not been bled, usually, I believe, exceeds the proportion of 8 crassamentum to 10 serum, was but 3·4 to 10. The serum exuded up to that period constituted 724·9 in 1000 parts of blood.

Exp. CLXXXVI.

Last winter (1832) a calf, bled for the butcher, afforded us specimens three times, at intervals of seven or eight days. Our vessels were filled in each instance at the commencement of the bleeding. They severally yielded,

	First-drawn.	Second.	Third.
Serum	277·2	240·6	600.7*
Crassamentum	722·8	759·4	399·3
Blood	1000·0	1000·0	1000·0
Dried fibrine	5·8	5·3	5·0
.... serum	20·7	18·4	39·9
.... crassamentum	169·0	139·0	79·8
Water	804·5	837·3	875·3
	1000·0	1000·0	1000·0

Without accumulating examples we may safely infer that *hæmorrhage increases the tenuity of the blood.* This opinion is generally, I believe, held by the profession, and my apology for proving what is believed, is simply that I am not aware of its having been proved before.

How shall we explain this fact? Shall we consider that a sensation of inanition is produced by copious

* My friend Dr. Weatherhead (now of Upper Montagu-street, London) suggested the following query: "Does not the abundance of serum in the last drawn cup arise from the immediate effect of the bleeding in rousing the energies of nature to absorb serum from the different cavities, and thus occasion the fact you have remarked? If this be the true cause, it will likewise account for the benefit derived from, and authorise the detraction of blood in both sanguineous and serous extravasations, wherein the strength has not thereby been materially diminished. This absorption ought also to be more obvious in health, where nature acts, unfettered by diseased associations." The circumstance must, I conceive, originate either in the cause which Dr. Weatherhead suggests, or in the greater disposition to concretion which the blood assumes, when the system is reduced. The latter opinion is least probable, for if the increased proportion of fluid in the last-received blood arise merely from the speedy contraction of the crassamentum ejecting a greater quantity of serum than the first-drawn, this substance would be firmer in the last than the first. But since the fact proves the reverse, we may infer that *during hæmorrhage, the ratio of serum is actually augmented.*

bleeding in the blood-vessels and the whole corporeal system, and that, in consequence, the provident principle of nature excites the absorbents to increased action? This hypothesis would be founded too much on opinions, which are not doctrines. Another explanation may be offered which appears less objectionable. Hæmorrhage reduces the action of the heart and arteries; and if it be great and continued, if it surpass the point of removing a morbid state of the secretory system, it decidedly diminishes the activity of the functions of that system by lessening the supply of material. The formation of new products from the blood is also diminished, and this fluid therefore remains comparatively unchanged, so far as regards the secretions it is accustomed to yield. The state of the absorbent system, on the other hand, appears to be different. Insensible to most of those impressions which affect the other systems, and acting vigorously even when they have ceased to act, its vessels continue to take up thin fluids from the numerous and extensive serous cavities, and to throw them into the general current of circulation. In this way, I conceive the tenuity of the blood to be caused.

During hæmorrhage changes also take place in the relative proportions of fibrine, of albumen in the serum, and of solid matter in the crassamentum. I detail one experiment in illustration of this.

Exp. CLXXXVII.

Blood taken from a stuck sheep at three periods of the hæmorrhage.

	First.	Second.	Third.
Dried fibrine	2·6	2·3	3·1
.... serum	45·2	39·0	38·3
.... crassamentum	128·0	138·5	129·6
Water	824·2	820·2	829·0
Blood	1000·0	1000·0	1000·0

These variations are always apparent, and probably arise from the variety in the blood of different vessels, and the succession in which the contents of these vessels are drawn off. Thus if an animal be wounded in the neck, the blood first effused will, of course, be from the vessels of that part and of the head, and the latter portions of the hæmorrhage will proceed from vessels more distant and conveying blood for different functions.

11. UTERO-GESTATION has a marked effect in the proportion of fibrine in the blood.* It will be remembered that our average of this constituent in ordinary healthy blood is 2·8.

Exp. CLXXXVIII.

Blood was taken from a lady the day before her accouchement, on account of a peculiarly oppressive sickness. It was strongly buffed, and yielded by exsiccation,

Fibrine	5·3
Contents of serum	46·7
Hæmatosine and albumen	174·4
Water	773·6
Blood	1000·0

The occurrence of buffed or sizy blood, so generally observed in bleeding pregnant women, has been referred to the disorder for which the operation is performed, rather than to the state of the patient's constitution. Reflection militates against this opinion, but experiments

* In one pregnant female we found the quantity of fibrine so low as 1·9, but she was under the influence of a mercurial ptyalism, and attacked with mild cholera; and it is not improbable that these circumstances might affect the result.

on healthy dogs, if their number be admitted as sufficient, entirely overturn it.

Exp. CLXXXIX to CXCIV.

Blood from the jugular veins of

	Fibrine.
A dog, yielded in 1000 parts	3 · 4
A bitch, not pregnant, ...	3 · 0
Do. small and young, but not pregnant, ...	2 · 4
A bitch about half term of gestation	5 · 3
Do. said to be in 7th week of do.	4 · 5
Do. about the same period	5 · 0

In one experiment on a large dog which had been long without food, the ratio of fibrine was as high as 6·6; but as I never found in any other an approximation to this degree, I suspect the exception to have arisen from some unusual circumstance. Including even this, the average of six experiments gives but 3·5; and, omitting it, that of five gives 3·08 for the fibrine of the dog and unimpregnated bitch, while the average of the three pregnant animals is 4·9.

Does utero-gestation affect the quantity of albumen and hæmatosine? We have stated the ordinary average of these ingredients at 157·0 in 1000 parts of blood. In the experiment on the blood of the lady just quoted the proportion was 221·1: in another similar case it was 172·0.

In the blood of the six dogs referred to for the fibrine, the proportions of albumen and hæmatosine were

In the unimpregnated.		In the pregnant.
232	....	224
185	....	217
162	....	226
3) 579		3) 667
Average 193·0		Average 222·3

From these experiments it appears that *utero-gestation also increases the proportion of albumen and hæmatosine.*

CHAPTER VIII.

PECULIARITIES IN THE BLOOD OF DIFFERENT CLASSES OF ANIMATED BEINGS.[α]

WHEN it is considered that in most animals of the higher orders a marked similarity exists,—that, although some peculiar structures are provided for the particular

[α] Mr. THACKRAH had not re-written this chapter, as appears to have been his intention, nor had he collected much additional information on the subject. I have, therefore, transcribed (with a few alterations) the third chapter in the original edition, interspersing with the author's remarks such further observations as I have been able to compile. I am sensible, however, that no satisfactory conclusions can be drawn from the investigations which have hitherto been directed to this branch of our inquiry. In their principal characters there is a striking resemblance, not only between the blood of different species of the higher orders of animals, but also in the fluid circulating in the vessels of the lower classess, and in the sap of trees and vegetables. "Analysis," to translate the words of a late valuable author, "exhibits a perfect analogy between the juice of the *chara*, the glutinous saps that concrete on exposure to air, milk, chyle, and blood. There is the same albumen in its two states of solution, and globular precipitation; the same salts; the same spontaneous coagulation on its exit from its vessels, and that by the saturation, evaporation, or dilution of the menstruum of the albumen."—(RASPAIL's *Chimie Organique*, p. 374.) But with this perfect analogy, how happens it that there is so little coincidence in the results of experiments on the blood of various creatures? If we reflect on the changes which have been shown in the preceding chapters to take place in the blood in different vessels, and during different states of one animal, and carry them into the infinite diversities of classes and genera, we cannot be surprised at the trifling measure of success that has attended the researches of philosophers on this question. ED.

situation or habits of these creatures, yet all have the systems of circulation, loco-motion, and sensation,—all have organs of digestion and secretion common in their nature and similar in their action,—we cannot expect to find any considerable variety in the character of the blood. This opinion is supported by a remark of *Hunter*. He has stated that no material difference exists in the blood of different animals, except in the proportion of red particles. "Transfusion of the blood of one animal," as he justly observes, "into the vessels of another, proves to a certain degree the uniform nature of the blood."* Nor does it appear requisite, that the animals on which this experiment is made, be of the same tribe.† The blood of a carnivorous creature will circulate in the vessels of the graminivorous, and the blood of the graminivorous will circulate in those of the omnivorous.‡

To form a complete comparative statement of this fluid, as it exists in the different orders of the animal kingdom, if it were either practicable or desirable, would require ample opportunities, much leisure, and the nicest observations. I have examined, however, the blood of those animals which I was able to procure; and although I regret the time spent in this research, yet, since even

* Hunter *on the Blood*, p. 13.

† In an inaugural Thesis published by Dr. Leacock, "*De Hæmorrhagia*," several experiments on transfusion are related. At the conclusion he remarks "——— docet sanguinem ovis, animalis scil. herbivori, posse sustentare canem, animal contra carnivorum." See also the early volumes of the *Philosophical Transactions*.

‡ "For three whole weeks, without the help of any other nourishment, I supported a dog merely by the injection of blood into the jugular vein: every day, or every other day, a few ounces were introduced in this way; and though from want of nicety in my operations, the system became disordered, it was evident enough that the animal was well supported by it."

Dr. Blundell *in Lancet*, xv. 132.

negative knowledge is not always devoid of utility, I proceed to state the results of my inquiries, connecting them, at the same time, with the experiments and remarks of others.

I. Colour. The globules of the blood appear red principally in the more perfect animals;—in the mammalia, and birds, partly in fishes, but not generally in reptiles and insects. In some creatures coloured blood is found in the vessels near the heart, while the rest of the body is supplied only with a serous fluid. We observe some fishes to have red blood afforded to particular parts, while the contents of vessels in the principal structures are pale. In the skate, for instance, the fins have ramifications of scarlet vessels, yet the rest of the body is white. In frogs, the blood brought from the intestines is represented by *Haller* to be of a pale yellow: he also observed, in those animals, two columns, yellow and purple, resisting each other in the same vein.*

[In ascending the scale of animal creation, it is not till we arrive at the class *Malactinia*, that any circulating fluid analogous to blood can be distinctly seen. In the *Medusaria* two vessels surround the mantle, which are supposed to be arteries conveying a clear semi-transparent fluid. In the *Entoxoa* the circulation seems more perfect, but the blood is still colourless. Many species of *Annulosæ* possess red blood, and in this class we meet with the rudiments of a respiratory system. The whole of the circulating fluid, however, does not appear to pass through the respiratory apparatus, yet in several of the genera the blood is of as florid a hue as in vertebrate animals. The vascular system of *Insects* is not well understood, but their principal blood-vessel (dorsal) contains a transparent fluid in which globules can be distinctly seen. The blood of *Crustacea* is of a bluish white colour, and in appearance and qualities

* *Second Dissertation on the Blood.*

resembles the chyle of birds and reptiles.[α] The same may be said of the intermediate classes between them and *Fishes*. Of the latter, as well as of *Amphibia*, the vital organs are generally supplied with red blood, while the rest of the body is colourless. Many diversities, however, are met with among particular species. In *Birds* the blood is generally of a more scarlet hue than in quadrupeds; except in aquatic birds, where the blood is said to be darker, from the frequent interruption to the process of respiration, in diving for their food.]

Haller has remarked, that the blood of young and debilitated animals is of a pale yellowish cast. I have never observed this variety either in whelps and other young animals, or in the feeble and languid. Between the young and the adult, there exists scarcely any difference; and in the blood of the debilitated and the strong, no other disparity than a shade of red.

II. The Quantity of blood in proportion to the bulk of the animal is believed to vary in different tribes. *Dr. Moulin* [β] examined the weight of slaughtered animals before and after death. He found that a sheep, whose weight was 118 lbs. lost 5¼ lbs. of blood, or 1 in 22·4 of the whole weight.

A lamb.. ....1 in 20·3......... ....
A duck............1 in 28·6...................... ...
A rabbit ....1 in 29·2..............................

From these statements it appears that the lamb has a greater proportion of blood than the sheep, the sheep than the duck, and the duck than the rabbit. Conclusions from such premises, however, are by no means satisfactory; since the estimate is formed from the quantity lost by hæmorrhage, and it is well known that in

α *MS. Notes of* Professor Grant's *Lectures*, Lond. Univ.

β *Philos. Transact.* 1687. 433.

such circumstances, the particular state of the nervous system, as retarding or accelerating death, will materially affect the flow of blood, and consequently render the calculation inaccurate. As a general observation, however, I believe that *birds, fishes, and the weaker animals have a less relative quantity of blood than the larger and more muscular;* and that in the highest orders of creation, in man especially, the proportion is by far the greatest.

[The quantity of blood in animals will also be materially affected by the nature and quality of their food, and, in the lower classes, by many circumstances connected with their situation and habits. In *snails,* for instance, *Dr. Carus* informs us that it "varied considerably according to the weather, and the degree of moisture in the soil."α In all the inferior tribes there will be a similar diversity.

"Most cold-blooded animals, as *fishes* and the *amphibia,* have a smaller proportion of blood, and fewer blood-vessels than those with warm blood, though a much greater number of colourless vessels arising from the arteries." β *Blumenbach* found in the water-newt a proportion of blood to the weight of the body as 2½ to 36, while in man, he averages it as 1 to 5. γ

Young animals are said to possess more blood than adults of the same species: and adults more than the aged. Tame animals have a less proportion than wild ones: and when fat, less than those which are leaner. δ]

III. The point on which I expected most diversity was in the RELATIVE QUANTITIES OF SERUM AND CRASSAMENTUM.

I examined the proportion of serum to crassamentum in many experiments on various animals:

α See *Lancet* XVII. 104.

β BLUMENBACH'S *Physiology,* by ELLIOTSON, p. 11.

γ *Comparative Anatomy,* translated by LAWRENCE, p. 245.

δ WILSON'S *Lectures,* p. 12.

In dogs the average appeared to be as 10 to 20 or 25.*

Oxen	10 to 16
Horses	10 to 13
Sheep, the medium of some examples has been	10 to 21
while in others, it was but as	10 to 8!
Swine	10 to 18
Fowls	10 to 16

Although my experiments are far from evincing a disparity uniform in its reference to the classes of animals, yet it appears probable that a more complete examination would prove the *crassamentum to bear a proportion to the strength and ferocity of the animal;* since I never found the serum in such quantity as in the timid sheep, nor the crassamentum so abundant as in the predatory dog.†

IV. The PERIODS OF COAGULATION have also been compared with those of human blood.

In the blood of the

Horse, concretion‡ occurred in from	5 to 13	minutes.
Ox	2 to 10	....
Sheep, hog, and rabbit	½ to 2	....
Lamb	½ to 1	....
Dog	½ to 3	....
Duck	1 to 2	....
Fowls	½ to 1½	....

HALLER observed, that the blood of a mouse coagulates in a moment.

* My estimates of the relative quantities of serum and crassamentum have always been formed from weight.

α In a later experiment the proportion was as 10 : 13·1. See Appendix II. No. 13. ED.

† In one case it was 33 to 10 of serum.

‡ By *concretion* I mean the commencement of coagulation.

From these observations a general inference may be drawn, that *coagulation commences sooner in small and weak animals, than in the large and strong.*

V. Temperature. It has been generally understood, that the heat of the blood does not vary in animals of the higher orders. *Braun*,* however, has stated that the blood of a calf raises the thermometer to 104°, and that of birds to 107°—110°. Doubting the fact, I examined the blood of the ox, horse, sheep, and duck, in reference to this subject. In the horse, the temperature of the flowing blood was 97°; but in the ox, 100°—101°; in the sheep, 102°—103°; and in the duck 107°. It is not, therefore, without reason, that Braun has asserted *the blood of birds, and many animals of the class Mammalia, to maintain a degree of heat higher than that of man.*

[According to the most recent authorities the average temperature of the blood of man is 96·5—98; fishes 52°; dogs and cats 103°; pigs 105°; birds 103°—106°.[α] *Mr. Vines* observed, in his experiments on horses and asses, that in these animals "the arterial blood was from three to five degrees hotter than the venous:" and "that those parts of the body which are supplied with red blood, as the glutei muscles, &c., are several degrees hotter than those which are supplied with white blood, as the interior of the ball of the eye."[β] Does not the latter observation also apply to the amphibia and fish, in which red blood circulates in the vital parts only?]

VI. In the chemical qualities of the blood of animals, no considerable variety has been found to

* Diss. Physica Experimentalis in Acad. Petropolitanæ.

α Raspail *op. citat.* p. 361.

β *Lancet*, XI. 294.

exist. *Berzelius* states the blood of the ox to differ in no respect from that of the human subject, except in containing a smaller quantity of saline matter, and a larger proportion of azote. The latter, however, is a circumstance curious and unexpected, when we consider that man lives in a great measure on animal food, while the bullock's sustenance is wholly vegetable.

The SPECIFIC GRAVITY OF THE BLOOD of animals, and the *solid contents* on evaporation, have been found to bear a general resemblance to those of the human subject, as far as my experiments have been prosecuted.[α]

The SERUM of animals presents no marked diversity, either in its appearance, its taste, its qualities, or its coagulation.[β] In fat animals I have often noticed oily, or rather adipose, matter floating on the surface of the blood: on cooling, this substance has become a white concrete mass.

On the venous blood of whelps, I have more than once remarked a cream-like crust. It was thickest at the edge of the coagulum, though milky streaks pervaded the serum.

The GLOBULES of the blood in amphibia, and those creatures whose circulation is slow, are much larger than in man;* while in some animals they are said to be considerably smaller. In most white-blooded animals,

α See experiments in Appendix II. Nos. 14 to 25.

β Appendix II. Nos. 26 to 41.

* BAKER, in speaking of the *water-sow*, states that the globules "appear about ten times as large as those of the human blood, and their progressive motion is very slow and languid, whereby they become more distinguishable than the globules are in the blood of animals, whose circulation is swifter." *Employment for the Microscope.*

globules are still to be seen; but in the lowest orders, even these cannot be discerned by the microscope.

[*Rudolphi* states the average *size* of the globules of human blood to be $\frac{1}{3000}$ to $\frac{1}{3500}$; that of fishes $\frac{1}{2000}$ to $\frac{1}{2500}$; and in the land salamander to bear a proportion to the globules of man as nearly thirteen to one.[α] The same writer also observes, that in man their *form* is round; in fishes the same; more or less oval in the amphibia; more longitudinally extended in the land salamander and proteus: but that the vesicles of all these amphibia, as well as of the fowl, so long as they are fresh, appear on their convex surface to have a slight degree of elevation.[β] These appearances nearly accord with the remarks of *Dr. Hodgkin*,[γ] *Messrs. Prevost* and *Dumas*[δ] and *M. Raspail;* except that the latter writers represent the globules of fishes to be elliptical. Neither the size nor shape of these particles, as was shown by *Leeuwenhoek* and *Hewson,* bear any relation to the bulk or strength of the animal. Indeed, accurate observation demonstrates, that each globule may vary both in form and dimension, while under the eye of the experimenter, and that there is an infinite diversity in their diameters in each individual. On being mixed with water, the elliptical globules of the batrachia gradually lose their external envelope, and assume the form of those of mammalia:[ε] a similar phenomenon has been noticed in the blood of insects.[ξ] The consideration of this circumstance leads the editor not inappropriately to recur to the *constitution* of the globules.

α *Physiology* translated by Dr. How, p. 132.

β Ibid, p. 130.

γ *Annals of Philosophy,* II. 130.

δ *Biblioth. Univ.* XVII. quoted in Bostock's *Physiology,* III. 402.

ε Raspail *Chimie Organique,* p. 370.

ξ See an interesting paper on the circulation of the blood in insects, by J. Bowerbank, Esq., extracted from the *Entomological Magazine,* in *Lancet,* XVI. 233. Ed.

The theory of *M. Raspail,* though not precisely corroborated by the researches of some other writers,[a] is the most plausible and satisfactory that has yet been proposed. He conceives that the globules are solid particles of albumen, and that this substance also exists in two other states in the blood,—a part being held in solution as an albuminous acetate of soda and potass, while the free alkali (soda) is the menstruum of the remainder. He represents coagulation as the natural consequence of a union of the alkali with the carbonic acid of the air, saturating the menstruum of the albumen, and thus precipitating the clot. *M. Raspail* further considers that the globules are capable of solution and re-precipitation, and in this way present an infinite variety of appearances under different states and combinations, varying with the age, season, and degree of health in each individual, and, in short, with every circumstance which can modify the albumen of which they are composed.[б]

[a] See page 54.

[б] In illustration of the above remarks I quote one or two extracts from the author alluded to.—L'alcalinité constante du sang le plus fraîchement tiré des vaisseaux, et la coagulation produite par un acide étendu d'eau, ne permettent pas de douter que le menstrue de l'albumine ne soit un alcali. Cet alcali c'est de la soude, et surtout de l'ammoniaque dont les auteurs ne tiennent aucun compte, et dont on reconnaît avec évidence les divers sels au microscope. —— 926. Une fois ce principe admis, la coagulation spontanée du sang n'offre plus ancune difficulté inexplicable. Car l'acide carbonique de l'atmosphère, l'acide carbonique qui se forme dans le sang, par son avidité pour l'oxigène, sature le menstrue de l'albumine, qui se précipite comme un caillot." In continuing to explain the process of coagulation Mons. Raspail adds, "Je pourrais ajouter que la fermentation acide est susceptible de se manifester, immédiatement au sortir des vaisseaux, dans un liquide élevé à 37° de température (98° Fahr.) et renfermant simultanément de l'albumine insoluble et du sucre." *Chimie organ.* p. 373. The instantaneous products of this fermentation are carbonic acid and alcohol.

As to the formation and appearance of the globules, we find the following observations:—919. Or, des globules hyalins, solubles dans l'eau, l'ammoniaque, l'acide acétique, l'acide hydrochlorique concentré, coagulables par les autres acides, par la chaleur, par l'alcool, sont évidement de simple globules d'albumine, et non des molécules organisées.—923. Le noyau que l'on remarque dans l'intérieur des globules du sang des batraciens (car sur la

Mr. Brande could not detect any globules in the catamenial fluid.[a] Upon this we might argue, that the colouring matter does not necessarilly appertain to the globules, but may exist in the blood without them: on the other hand we know that globules circulate in many animals without any colour air and we may further conceive it possible for blood to circulate where neither are apparent, as, for instance, in the lowest scale of creatures, in which vascular system has not yet been detected.

When blood has been abstracted, the red particles are the last portion that is re-produced. *Hunter* observed this fact, and further states, that the stronger the animal the greater proportion it has of red globules.[b]]

In the lower classes of animals it is impossible to ascertain the quantities of the other constituents of the blood; reasoning, however, from analogy, we should conclude that the proportion of *fibrine* is small.

[The coagulum of bullock's blood, according to *Berzelius*, is less easily decomposed by water, than that of human blood,

plupart des autres c'est un simple effet d'optique) ce noyau, dis-je, n'est que l'effet de la dissolution successive des diverses couches du globule albumineux. Car la couche externe du globule venant à s'imbiber d'eau la première, s'étend la première dans le liquide, acquiert, par son imbibition et par son aplatissement, un pouvoir réfringent plus faible que les couches centrales, qui dès ce moment, se montrent plus opaques que la couche externe. Lorsque la couche la plus externe s'est entièrement dissoute, la couche plus interne subit la même modification, et ainsi de suite jusqu'á la couche médiane; le globule finit par disparaître entièrement." p. 370–1.

M.M. Prevost and Dumas assume that the globules are composed of fibrine, and in allusion to their calculations as to the number of these in 1000 parts of the blood of different animals. Raspail observes "Tout cela est fort beau, mais c'est encore dans les fables." p. 385. Ed.

a *Philosophical Transactions*, 1812, 114.

b *On the blood*, p. 46.—For a full discussion as to the nature of the colouring matter of the globules, and how far this is owing to the presence of iron, see Bostock's *Physiology*, I. 459, et seq. Ed.

and the fibrine obtained from the former is more transparent.[α] In one of Mr. Thackrah's experiments the ratio of fibrine in sheep's blood is stated to be 2·6 in 1000 parts.[β]

The proportion of fibrine is smaller in the less perfect animals than in the higher classes. The blood of the *annelidæ* has very little fibrine and few globules. In *insects* the globules are more numerous and the ratio of fibrine increased. The colourless circulating fluid in the *crustacea* and the *mollusca* is much more fibrinous, but still contains a less proportion than is found in red-blooded animals.[γ]]

The blood of some creatures is found, while circulating, to contain *air-bubbles.* In the land and sea tortoises, in some fish, in the hedge-hog, and the viper, this appearance has been asserted by respectable writers.[*]

[It has been affirmed that there is a perceptible difference between the HALITUS of the blood of different kinds of animals; and that by this test not only may a spot of blood be distinguished from that of any other liquid, but that human gore may be known from that of any of the lower animals. If established, this would afford a medico-legal evidence of high value; but it has not yet been sufficiently corroborated.

M. Orfila was the first who averred that he could distinguish a stain of blood, were it only the size of a sword's point, from any other. This was denied by *Raspail,* and the statement at length abandoned. *M. Barrueil* has more recently investigated the subject, and arrived at the following positions. First, that the blood of each species of animal contains a principle peculiar to the species. Secondly, that this principle exhales an odour

α *Medico-chirurg. Transactions,* III. 229.

β Appendix II. No. 42.

γ *Lectures,* by PROFESSOR GRANT, in *Lancet,* Sep. 1834, p. 868, et seq.

* MORGAGNI *de sed. et caus. morb. Epist.* V. 22.

similar to the perspiration of the animal;[α] and thirdly, that sulphuric acid sets free this principle.[β] *M.M. Leuret* and *Chevalier* support these assertions,[γ] and a case has been tried in the French courts, in which evidence was given on the strength of their accuracy.[δ] The arguments of *Raspail* and *Soubeyan*,[ε] however, are so strongly opposed to the deductions of Barrueil that considerable doubt hangs over the question, and the test cannot at present be relied on. The halitus from the blood of carnivorous animals is undoubtedly more fœtid than that of graminivorous tribes.[ξ]

As regards their ULTIMATE ELEMENTS, *M.M. Macaire* and *Marcet* have demonstrated, that both the chyle and the blood of herbivorous and of carnivorous animals are identical in composition. These experimenters found the same quantity of nitrogen in the chyle, whatever kind of food the animal consumed. The blood, which contains a larger proportion of nitrogen than chyle, was also found to have a similar quantity in both classes.[η]

α See page 50.

β *Annales d'Hygiène publique et de Médecine légale,* Avril, 1829. I. 274.

γ *Ibid.* Oct. 1829; and *Revue Medicale,* Sep. 1829.

δ *Annales d'Hygiène* Juillet, 1829, I. 550.

ε The following is a summary of the arguments urged against the statements of Mons. Barrueil:—1st. That he operated only on a small number of animals, and that, therefore, it is not certain that others not examined have not a similar odour. 2nd. That having operated only on the pure blood of animals, it is not known what effects accidental or designed impurities may have in altering the odour. Instances are adduced in which human blood gave out the same odour as that of a goat, on being spotted with saliva; others where the blood of a sheep, being sprinkled on linen impregnated with human perspiration, disengaged the odour of human blood; and some in which sheep's blood smelled like that of a cow or horse, when spilled on human excrement. Finally, it is objected, that the characteristic odour of a species of animal varies with each individual, and with the state of health of every separate animal. *Chimie organique,* p. 383. Also, *Annales des Sc d' Observation,* 1829, II. 133 and 465.

ξ WILSON'S *Lectures on the blood,* p. 28.

η *Mem. de la Soc. Physique de Genève,* T. V. 389; quoted by Dr. ROGET in his *Bridgewater Treatise,* I. 58.

VII. The blood of the FŒTUS appears to differ considerably from that of the animal after birth. *Bichat* states that it does not coagulate, and that it contains no phosphoric salts.[a] *Fourcroy* could not detect in it any fibrine. Though a very interesting branch of our subject, fœtal blood has seldom been experimented on; and little more seems known about its constitution, than the peculiarities that have just been stated.]

[a] *General Anatomy*, translated by CALVERT, p. 396.

CHAPTER IX.

BLOOD IN DISEASE.

I have hitherto considered blood in its normal or healthy state. The elaborate work of *M. Denis* is entitled "Recherches experimentales sur le sang humain, *considéré a l'état sain*," yet seventy-six of his eighty-two experiments are on the blood of the sick! The facility of obtaining the blood of patients induces the inquirer to employ this most frequently: but when the blood of a patient is substituted for that of the healthy, or of brutes, great care should be taken, by experiments of comparison and contrast, to prevent the error of false standards, and to guard against those states and circumstances being considered normal which have been produced by disease.

In the inquiries on which the preceding chapters are founded, we have frequently employed the blood of patients, as well as that of brutes: but our inferences in such experiments have, as far as I know, had reference only to those principles or elements which are common to healthy and morbid blood, to the blood of the man and that of the brute.

I propose now to examine the states of blood in disease, a subject of considerable extent, and of the highest importance. It will be convenient, in prosecuting this branch, to advert first to some points which are of inferior consequence, are more rare, and less demonstrable.

SECT I. OBSERVATIONS ON THE BLOOD IN ITS VESSELS.

1. Blood is sometimes found strongly coagulated in its vessels. *Haller* remarked a concreted tremulous jelly in the veins, even of a living person Limited coagulation is often seen in mortification. It is also found, though to a minor extent, above the ligature or division of an artery. But the part in which the appearance is most remarkable is the heart. In this organ considerable masses of white or rose-coloured coagulum (false polypi) are not unfrequently found after death, attached near the large valves. They are sometimes easily separable, and are found to resemble the sizy tunic or buff coat of the blood. On other occasions their base is more strongly attached to the heart; and, on careful examination, they appear to be organized.

Andral seems to consider the structural arrangement found in these masses to result from their own action, the vitality of the blood whence they are formed, independently of the surface with which they are in contact.[a] But to me it appears that the degree of organization is in proportion to the adhesion of their base, and that the vessels of the surface of the old structure with which they are in contact, extend themselves into the new deposit, as those of any other part would into the deposit of adhesive inflammation.

The opinion common in the profession that false polypi are formed after life is extinct, and have consequently no etiological importance, is opposed by the examination of

[a] *Pathological Anat. Trans.* I. 648.

their structure, to which I have adverted. We cannot conceive organization, even in an imperfect degree, to take place after death. The structural arrangement apparent in some of these bodies must have been a work of days, and, in no inconsiderable number of cases, produced the fatal event. The symptoms of death, whether we observe the aged person faintly struggling, or the infant suffocating, indicate that the chief distress is in the heart. The præcordial anxiety so strongly marked, even when words have ceased to express emotions, and the intermittent, or fluttering pulse, all point to obstruction in the source of circulation. Death, therefore, I conceive, in a great number, perhaps in a majority of cases, has false polypi for its *immediate* cause.

How shall we account for the production of these bodies? If we admit the deductions in the chapter 'on the cause of coagulation,' the answer is not difficult. The vitality of the blood, in common with that of the general system, is reduced by continued disease. I say *reduced*, for death is rarely sudden and complete: the cessation of life is partial and gradual; and it is probable that the vitality of the heart and arteries, for some time before apparent death, is so far reduced, that they cannot maintain the blood in complete fluidity, and consequently admit the formation of coagula. But in this reduced state of vitality, how shall we account for the attempt at organization in these coagula? When the vitality is too low for ordinary operations, how can we suppose it equal to the production of new ones? Certain actions of a low grade, and where sensibility is little obvious, may go on during the reduction, and even after the cessation of higher functions. The muscular contraction of the intestines continues, nay, appears to increase, after apparent death. The beard grows on the recent corpse.

2. We next discuss the fluidity of the blood in its vessels. Having, in my tract on Cholera adverted to this subject, I shall quote the passage, and add some observations.

" The works of *Morgagni*, *De Haen*, *Hewson*, &c. present examples of this fluidity. *Wepfer* found the blood dissolved and grumous in many who died from malignant, and in some who died from continued fevers. *Jackson* often remarked this state after death from fever in the West Indies. In the victims also of the plague, the absence of coagulation has been repeatedly noted. *Troillet* observed, in those cases of hydrophobia, in which the blood drawn by venæsection concreted without separating its serum, that found in the veins after death was black liquid, and did not coagulate on exposure to the air. *Valpeau* remarked, in one instance, the blood found in the vessels after death had not a point of coagulum, and its consistence 'un peu plus epaisse que celle du pus bien fié.'" In cholera the blood has been repeatedly found fluid in the vessels. "The late *Dr. Peters*, of Kiel, informed me of an interesting example of the blue disease, in which blood expectorated did not coagulate for three days. Called to a gentleman who fell in syncope as he walked, and died in a few minutes, I found on post-mortem examination, the blood still fluid in the vessels. In a man who poisoned himself with opium, I remarked on dissection, the imperfectly coagulated state of the blood. In another, who was suddenly destroyed by a fall, which fractured the base of the cranium, the blood was fluid in the vessels, though, on exposure to the air, the process of coagulation commenced. In two instances of death from drowning, I repeated the remark of *Hunter* and others, that the blood does not concrete in its vessels. In persons killed by lightning, by mental emotions, by blows on the stomach, by certain poisons, by the venom of serpents, in animals killed in the chase, the blood in its vessels does not perfectly coagulate, and seldom, perhaps, even separates a portion of its serum. To these general statements many more individual cases might be added. In most of those to which I have referred,—in all those which conclude the enumeration,—the nervous system appears to have been principally affected by the morbific agent. A shock had been given to this system, which destroyed the functions of the brain, but left vitality still lingering in the nerves. In such cases the body is generally long warm, and the muscles long retain their irritability. If we consider the fluidity of the blood to depend on the nervous energy in its vessels, and coagulation to depend on the loss of that energy, we have an explanation of this curious fact. In lingering maladies, vitality is gradually, and fully exhausted," a like in the organic and the animal system; "in peculiar and rapid diseases, the great nervous functions are alone abolished, and enough of life remains in the blood-vessels to maintain the fluidity of their contents.

" In the worst forms of cholera, as in the fatal impression from lightning, mental emotions, certain poisons, and accidents, a sudden impression is made on the nervous system, most of the great functions are suspended, or impaired, all these soon cease, and death is said to take place ;—yet, irritability long remains in the muscles, and life, we have reason to believe, also remains in the blood-vessels. Hence, the uncoagulated state of the blood they contain. Remove this fluid out of its natural vessels, and signs of concretion are soon apparent."*

* *Essay on Cholera*, p. 14—16.

This point has been too little regarded. Most of the cases recorded as non-coagulation have, I suspect, been founded only on retardation of the process. In every case but one, I believe, in which I have seen the blood remarkably fluid, and have removed it from its vessels, coagulation has taken place,—not quickly, indeed, nor fully, but till a clot of crassamentum has unequivocally been produced. It may be urged, moreover, that authors who attach great importance to the non-coagulated state of the blood, must have overlooked or forgotten the state in which this fluid is *commonly* found after death. Partially coagulated in the small vessels, it is fluid in the larger ones. In almost every autopsy performed a day or two after the death of the patient, a careless stroke of the knife on either of the cavæ will deluge the body with blood. When at a subsequent period, putrefaction is established no such effusion takes place; and since this is the only undoubted characteristic of death, we are warranted in supposing that life lingers in the vessels and maintains the fluidity of the blood. This view is supported not only by the experiments in chapter VIII. but by the fact of the blood being found in the same subject fluid in its vessels, and concrete when extravasated. In the man referred to a few pages before, who was killed by a fall which fractured the base of the cranium, blood extravasated between the tunica arachnoides and dura mater was coagulated, while that in its natural vessels was fluid. A similar contrast is often exhibited by apoplexy.

Too much importance appears to me to have been attached to the fluidity of the blood in its vessels. It has been considered as an evidence of violent death; and the want of more extensive and accurate knowledge of the subject has endangered the life of an innocent man.

3. Morbid productions and natural substances from other parts of the body are sometimes found in the blood-vessels.α

Purulent matter has often been noticed in the veins, and sometimes also in other parts of the circulatory system. Pus has been known to circulate in the human frame. In an infant which had an abscess in the neck and died of marasmus, *Kerchringius* found after death a communication (and one which seems to have existed for some time,) between the cavity of the abscess and the jugular vein. A notable quantity of pus was found mingled with the blood in the right cavities of the heart. *Meckel* has seen the whole of the aorta studded with ulcers, which from their collective amount of suppurating surface, must have long and considerably impregnated the blood. *Portal* and *Dupuytren* have seen pus in the lymphatics surrounding an abscess.* *Gendrin*, indeed, has adduced experiments

α Two cases are mentioned in the *Philosophical Transactions* for 1665, in which milk is said to have been found in the veins instead of blood. The account seems to refer to some greater change than milky serum only. Vol. I. 100. Ed.

* "Pus has repeatedly been found in the vascular system; its presence has been ascertained particularly in the following places:—1. In the veins which returned the blood from parts where there was a collection of that fluid. In several cases of metritis, for example, terminating in suppuration, the uterine, hypogastric and iliac veins, and the inferior vena cava, have been found full of pus. 2. In veins that had been irritated by a puncture or otherwise, independently of the existence of any abscess or collection of pus. In such cases the pus is evidently formed in the interior of the vein. 3. In the coagula of blood which are formed in the heart and large vessels, whether there had existed at the same time a purulent collection in some other part of the body, or an old discharge of pus had ceased a short time previous to death, or even though no trace of suppuration could be detected during life or after death. 4. In the lymphatic vessels. In some cases these vessels set out from a part in a state of suppuration; but in others there was no pus to be found except in the limphatic vessel itself. I have sometimes found the lymphatics leading from an intestinal ulcer, laden with pus, either fluid, or in a semi-concreted state, and in this latter form bearing a strong resemblance to the matter of tubercle." Andral's *Patholog. Anat. transl.* I. 497.

and observations to prove that pus is a modification of fibrine,—a morbid product from this substance. If we admit the theory, we shall be inclined to attribute that purulent appearance which is sometimes seen in the blood, to the partial conversion of fibrine into pus.

Independent of the direct evidence of pus in the blood-vessels, we have indirect, though not less decisive proof. The practice of surgery not unfrequently shows the removal of an abscess without external evacuation, and the deposit of matter far from its original seat. Whether absorbed by the veins or lymphatics, the fluid must, it is obvious, have been thrown into the circulation, and mixed with the blood.

Medullary or encephaloid matter; a substance resembling the product of scrophulous abscess; various semi-fluids of different hues; calcareous and ossific deposits, have at different times been found in the blood-vessels. Bile, or its elements, abound in the blood of the jaundiced. I have sometimes seen also air mingled with the blood: in a man, for instance, who had poisoned himself by opium, there were bubbles of air in the venæ galeni. In other autopsies we have found air more extensively diffused. Several similar cases are related by *Morgagni*, *Valsalva* and *Ruysch*; and more recently *Professor Odeer* has detailed an instance.*

4. Besides the morbid states of blood which are observable in its vessels, there are others of a peculiar nature, which we know only by their effects. The blood appears to become actually poisonous. On this point, however, we can scarcely admit the inference of *M. Gendrin*, from an experiment in which diseased human blood was injected into the cellular membrane of a cat,

* *Edinburgh Journal*, II. 363.

and produced a fatal result; for we might refer the effect to constitutional irritation rather than absorption of the poison. More conclusive are the experiments of veterinary surgeons who have propagated disease from horse to horse by the transfusion of blood. In this way the fatal catarrh, called glanders, has been unequivocally transmitted by *Professor Coleman*, and the "malignant pustule" by *Dupuy* and *Leuret*. According to *Dr. Hertwich*, of Berlin, the blood of a rabid animal will, by inoculation, communicate the disease.

A curious history is quoted by *Andral*, from *Duhamel*, of the effects which ensued from the slaughter of an over-driven ox. "The butcher put into his mouth, for a few moments, the knife he had employed for the purpose. The consequence was, that in some hours afterward his tongue swelled, his breathing became difficult, and then blackish pustules broke out all over his body: at the end of four days he died. The innkeeper wounded himself with a bone of the same ox in the palm of his hand; his arm mortified, and he died in seven days. Two women having received some drops of blood of the same animal, the one on her hand, the other on her cheek, these parts were seized with a gangrenous inflammation."[a]

How often is a corpse examined without our being able satisfactorily to assign the cause of death! A little blush on the mucous coat of the alimentary canal, some engorgement of the cerebral vessels, a state of the pulmonary apparatus not healthy but much less diseased than that of individuals who never complained of disordered respiration, the heart somewhat dilated,—such may be the only results of a careful examination; perhaps, too, of a patient who has died of so dreadful a malady as

[a] ANDRAL'S *Pathology, transl.* p. 658.

hydrophobia. I need only refer the candid necrotomist to the great work of *Morgagni* for many convincing illustrations. In cases like these what is the cause of death? Though unable to demonstrate, we have cause to suspect, the state of the blood.6

5. The effects on the system of various substances injected into the blood.

Air, when introduced into the circulation in considerable quantity, produces death. *Wepfer*, I believe, was the first to notice this interesting fact; and since his time it has been often referred to. We have employed this mode for destroying animals subjected to experiment, as one of the easiest and quickest. On blowing through a tube, one end of which is inserted into a vein, and in the direction of the circulation, the experimenter sees the animal become convulsed and die in a moment. Smaller quantities of air produce decided, though not mortal effects; such as shivering and slight convulsive actions: these are soon succeeded by perfect health. The operation of bleeding has sometimes allowed a trifling quantity of air, as half a cubic inch or an inch, to enter the vessel, and great distress has resulted. This has been repeatedly observed in the practice of the veterinary surgeon. A horse is bled by a free opening, the ligature removed before this opening is closed, and by a partial vacuum formed, a few inches of air rush into the vein with a noise audible to the by-standers: the animal trembles, respiration becomes laborious, the pulse feeble and irregular, and he suddenly drops.

6 M. Roche has recently attempted to demonstrate that deleterious substances frequently enter into the circulation of the blood, producing illness till they are exuded from it by the perspiratory or other evacuations. He attributes malarious diseases, &c., to this cause. See a paper in *Med. Ch. Review, April*, 1834, 381, translated from the *Journal Hebdomadaire*. Ed.

Majendie relates a case of fatal issue which occurred to a man. During an operation for the removal of a tumour situated below the right clavicle, the patient cried out "mon sang tombe dans mon corps—je suis mort." He immediately became stiff, unconscious, and covered with a cold sweat: a strange noise was heard by the attendants—a rush into the chest, and in a quarter of an hour the unfortunate man was a corpse. On examining the body next day, a small wound was found in the external jugular vein, where this vessel opens into the subclavian. The heart was devoid of blood, and air-bubbles were seen in the vessels of the brain.

Bichat injected into the blood ink, oil, wine, water coloured with indigo, urine, bile, and the mucus of catarrh. Received by the crural artery these severally produced torpor, and sometimes even paralysis, but not death. Received into the carotid, and thus rapidly thrown on the brain, they destroyed life. *Majendie*, however, found oil injected into the jugular vein to destroy life quickly; and on examination he found this fluid lodged in the branches of the pulmonary artery. The same mechanical impediment to circulation resulted from the injection of gum water.

Alcohol injected into the blood produces very marked effects. If pure and in large quantity, the animal instantly dies: and on opening the body, the blood will be found grumous, and nearly similar in colour to turned milk. If the alcohol be diluted, and in small quantity, it produces a greater or less degree of intoxication, which appears to subside as the alcohol is exuded by pulmonary transpiration. In this latter case the blood becomes creamy, and is uniformly thicker than in health.[a]

[a] See an interesting paper read at the Academy of Sciences, by M. Segalas D'Etchepare, reported in *Archives Générales*, Sept. 1826, and in *Lancet*,

Nitrous oxide gas, injected into the veins, gives the blood a chocolate colour, and takes away its power of coagulating. Sulphuretted hydrogen often renders it viscid, greenish or brownish, and incapable of coagulating: hydrocyanic acid sometimes makes it oily, fluid, and bluish in colour.[α]

M. Gaspard has injected acetate of lead into the circulatory system of dogs. On death some days after, the blood was found black and liquid; and in some of the animals, extravasation of this black blood was observed in the large intestines, between the membranes of the gall-bladder, and in the pulmonary tissue.[β]

More remarkable and important are the phenomena produced by the injection of pus or fœtid matter into the circulatory system. On this point *MM. Gaspard* and *Majendie* have made some very interesting statements. The former first injected into the jugular veins of dogs, pus diluted with water. They became immediately agitated, made efforts at deglutition, then sunk faint, moaned, and vomited. The bladder and intestines were emptied. Recumbent on the side, with respiration imperceptible, and pulse very feeble, they at length

Vol. XI. 170. In addition to the experiments with alcohol, the author relates several in which he injected alcoholic extract of nux vomica into the blood; and from them he infers that it is by being introduced into the circulation, and not through the nervous system, that this poison produces its destructive effects. Ed.

α Christison *on Poisons,* p. 586, et alibi.

β In 1712, Courten tried the effects of injecting emetic wine, sal ammoniac, salt of tartar, urine, opium, spirit of wine, camphor, salt, oil of olives, white wine, decoction of tobacco, infusion of senna, &c. into the veins of animals. His experiments are detailed in the *Philosophical Transactions,* XXVII. 485. In a work of still earlier date, Dr. Beale argues that experiments by mixing substances with blood drawn from the body, and by injections into the veins of living animals, both lead to erroneous conclusions. *Essay on vicious blood,* by Barth. Beale, M.B., London, 1706. Ed.

voided fæces liquid and extremely fœtid. This afforded great relief, and either procured a speedy restoration of health, or was succeeded by dysenteric symptoms, exhaustion, and death. When he increased the quantity of pus injected, the nervous symptoms were sooner and more strongly marked, wanderings of the eyes, excessive sensibility, involuntary startings, hiccough, convulsions, and delirium. In one case a sort of emprosthotonos, with stiffness of the limbs, ensued at the end of fifteen minutes, on the injection of three drams of pus. Post-mortem examination exhibited, in less urgent cases, nothing remarkable, with the exception in one of partial hepatization of a portion of lung; and in another, in which dysenteric symptoms had been present, thickening of a portion of the intestine, and some inflammation of its mucous membrane. In the worst case the pericardium contained some extravasated blood; the left ventricle of the heart was very thick, inflamed, and marked by concrete pellicles (false polypi?). The venous blood is represented as very coagulable, and separating its serum on repose.

Without commenting on these experiments severally, I would observe, that the principal impression seems to have been on the nervous system, and through it on the muscular system of organic life.

Although, therefore, the admission of pus into the blood-vessels deranges the constitution, it does not necessarily destroy life, unless the quantity be considerable, and the introduction sudden. This, indeed, is the first of *M. Gaspard's* conclusions. The whole of these, along with some extracts from his experiments, will be found in the appendix; for as his inquiries did not particularly advert to the state of the blood as affected by the admixture of pus, it would be irrelevant here to follow his details.*

M. Andral, reviewing, if I mistake not, the experiments of others, rather than offering deductions from his own, makes a general statement in reference to pus, putrid substances, and poisons, which does not accord with the observations of *M. Gaspard.* He asserts, that after death from such injection, the blood is found remarkably fluid, and preternaturally disposed to putrefaction.

The injection of water into the blood-vessels is now a familiar experiment. α

About 1770, *M. Regnandot*, at Guadaloupe, injected fluids into a brachial vein of a man between 18 and 20, affected with "dartres rouges." He first threw up half a spoonful of infusion of senna, which produced no other effect than a slight head-ache. On succeeding days fluids impregnated with various medicaments, and in greater quantities, were injected. A febrile paroxysm succeeded each experiment, but no dangerous symptoms.

Majendie, in 1820, recorded his trial on a mad mastiff. He bled the animal to the amount of about a pound, and then injected by the jugular vein sixty ounces of tepid

* See Appendix I.

α In order to form an impartial judgment on the value of injections into the veins, as remedial agents in Cholera, The Editor was at one time in the habit of entering, in a tabular form, every case that was reported in the journals, in which this mode had been employed. He collected 105 cases, of which the following statement is a summary:—

No.	Hours ill.	Injected.	Recovered.	Convalescent.	Under Treatment.	Died.
15	4 to 12	℥24 to 305	1	2	—	12
13	12 to 27	℥23 to 292	3	4	—	6
77	unknown.	℥14 to 640	1	1	15	60
105		14 to 640	5	7	15	78

See *Paper on Malignant Cholera*, in *Lancet*, Feb. 9, 1833, p. 629. Ed.

water. During the injection ten or twelve ounces of blood were allowed to escape from the upper end of the punctured vein. After the process the animal became remarkably calm; and although he subsequently died, such an effect was produced that M. Majendie urged its trial in the hydrophobia of man. How far a reduction of the violent symptoms in the mastiff arose from the vast hæmorrhage which accompanied the injection, may be questioned. Most dogs in health would have died from half that loss of blood.

In 1823, *M. Gaspard* attempted Majendie's plan in a case of hydrophobia. He injected four ounces of water, and after waiting a while, four ounces more. A febrile paroxysm induced him to abandon the trial; and, of course, the patient sunk. The experiment was neither fairly commenced nor fully prosecuted. No blood was previously taken from the arm, and the quantity of water was much too small to produce a powerful effect on the nervous system.

6. Here perhaps I should notice the effect on the blood of certain articles taken by the stomach.

Prussic acid thus administered renders the blood florid, and hastens its concretion. After a fatal dose, we have repeatedly observed the blood of a dog to change from its usual modena to bright scarlet, and to become solid immediately on effusion. The latter effect we should refer to an impression on the nervous system, the debility rapidly induced; but the former is not so readily explained. *Krimer* has found the acid in the blood of animals destroyed by it.

Dr. Christison adverts to the detection of many of these in the blood; as salts of copper and lead, camphor, muriate of ammonia, hydrocyanate of potass, iodine, and even mercury. But he adds that poisons taken by the stomach, either from the comparative minuteness of their dose, or their decomposition in the body, cannot generally be discovered in the blood. Their effect on the properties of this fluid has been rarely noticed. α

Mercury is said, by *Drs. Farre* and *Unwins*, to "possess the power of breaking down the crasis of the blood," of preventing, I suppose, or impairing coagulation. We have repeatedly bled patients who were offensively under the influence of this mineral, but have not observed such effect to be produced. β

Alimentary substances of bad quality, or deficient in quantity, must produce a faulty state of the blood, though we have no actual experiments to prove the fact. When a wet harvest has produced sprouted wheat, I have remarked cutaneous disease of a severe character to affect the farmers and peasantry who have eaten unsound flour. When a season of scarcity has obliged a population to subsist on herbs and uncooked roots, dropsy has been a frequent result. When crews were crowded in vessels, and supplied with little else than salt provisions, scurvy was common. In these and other instances, the blood must have been the medium of disease. Bad or deficient chyle produced a degeneration of this fluid, and a consequent depravation of secretions, and reduction of nervous power.

α. *On Poisons*, p. 14.

β M. Gosse, of Geneva, has analysed the blood of persons under the influence of mercury, and found it to contain much less albumen than other blood; that there was less cruor, that it was more liquid, and, in short, less inflammatory. See *Medico-chir. Review*, Sep. 1834, p. 483. Ed.

Sect. II. Blood obtained by venæsection.

Having adverted to the principal changes which have been noticed in blood in its vessels, we now examine those which analysis exhibits in blood obtained by venæsection, in the diseases of general practice. The inquiry is highly important alike to pathology and therapeutics. I need not discuss the various unfounded, incongruous, and often ridiculous notions which, in former periods, were announced without experiment, and repeated without examination. Suffice it to remark, that from the earliest epochs of medical study, the morbid state of the humours has forced itself on the consideration of the physician. All who observed disease were convinced that changes in the fluids produced disorder of the functions, though from the want of physiological knowledge, and the practice of scientific inquiry, their opinions were confused and erroneous. In later times the progress of anatomy has led men to refer to the solids alone the changes of disease, and humoral pathology has been often the subject of reproach and derision. Facts, however, have daily shown to every attentive practitioner, that other parts than solids are greatly in fault in the production of disease; and inquiries have been instituted to ascertain the difference between healthy and morbid blood. The success, however, has been but small. The imperfection of animal chemistry has been one obstacle; the want of perseverance and zeal in prosecuting experiments has been another. The student of disease has been discouraged by statements of eminent inquirers, as *De Haen*; * by the judgment of some distinguished

* "Inversa itidem, mutata, confusa, omnia hæc phœnomena, sæpius vidimus." After a recapitulation of his experiments and observations, (chiefly

practitioners, as *Heberden*; * and by the confused and incorrect opinions of systematic writers, as *Broussais*.† The following observations, the result of an investigation carried on during several years, will, I trust, shew that important pathological information is afforded, and the treatment of disease essentially elucidated, by an examination of the blood we abstract in venæsection.

1. THE COLOUR of the blood in disease is a subject of considerable interest and importance. Paleness and *contra* intensity of colour have been already noticed in the chapter on the effects of physiological states of the system. The deep rich colour of the blood is reduced by hæmorrhage, for hæmatosine appears to be of less easy reproduction than the other constituents of blood.‡ The

however, in reference to the buff-coat) he concludes. " Quæ si ita se habent, quis inflammationem absentem præsentemve; quis humorum aut coagulum aut dissolutionem, ex solo sanguine determinare ausit? utique si in eodem homine alia V.S. ab alia toto cœlo differat; si in eadem V.S. adeo oppositissima sint phœnomena; quis inde de natura humorum aut possit, aut ausit, judicium ferre." *Ratio Medendi*, III. 33—34. See also I. Cap. vi and xxv.

On this subject, however, I am happy in concurring with the judicious remarks of MR. HEY, who, after stating that he believes the appearances of the blood give so much information as to merit our attention, answers the objection which arises from the supposed opposition to other symptoms, which these appearances occasionally assume. " In order to direct successfully our attempts to remove diseases, every symptom must be attended to, and the method of cure must be guided by the indication taken from the assemblage of them all. An attention to all the animal functions is of importance, though considered singly, they may communicate very little knowledge of a disease, or may even mislead us." *Observations on the Blood*. p. 54.

* *Commentaries*, London, 1813.

† *Cours de Pathologie*.

‡ A case of long-continued hæmorrhage is related in the *Edinburgh Medical Essays*, and referred to by MR. HEY, in which the blood had a similar appearance. In a case of purpura hæmorrhagica, DR. WHITING found the red particles to be in the proportion only of 40 in 1000, while the healthy ratio he states at 100—130 in 1000.—*Thesis de Sang. Ægr.* See also DR. MARSHALL HALL's valuable Work *On the morbid effects of loss of blood*.—London, 1830.—ED.]

scarlet hue of arterial, and the purple or modena of venous blood, are well known.

(A) Venous blood is sometimes observed to be scarlet, and the surgeon has been alarmed with the idea that he has punctured an artery instead of a vein. *Baglivi* remarked that the blood of venæsection had a bright scarlet hue in hectic patients. *Simon Pauli* observed a similar appearance in several dangerous cases. This chiefly occurs in a greatly excited state of the circulation, and, I am inclined to think, most frequently when the excitement is recent. In the case of a youth who had small-pox, the blood flowing from the cephalic vein, presented the appearance of that from an artery. A spare and delicate man was bled for peritonitis: during the operation he was sick and vomited; and we immediately remarked a change in the character of the blood: before of the usual dark colour, it became ruddy, like arterial blood, with a small admixture of venous: this change was accompanied with a considerable increase of fulness in the pulse, though without any rise in its frequency. On bleeding a young lady in a paroxysm of palpitation, I observed the stream from the median basilic to be at first rather darker than usual, but after a few minutes to be composed of red blood, intermingled with streaks of purple. The phenomenon is not difficult of solution, if we believe the dark colour of venous blood to depend on the addition of carbonaceous matter. Blood hurried by disease through the circulation, has less time to take up this matter, and hence remains nearly of the colour it had when ejected from the left ventricle of the heart. I am not aware that the phenomenon affords any practical indication.

(B) Far more important is the opposite state, a preternatural darkness of the blood. *Celsus* observed the black and thick appearance which it sometimes presents, and the benefit which resulted from bleeding in such cases:α and since his time many authors have remarked upon the black livid hue of the blood drawn in malignant fevers.

In the early stages of acute diseases, when the circulation is more oppressed by congestion than excited by inflammation, I have generally noticed the blood to be unusually dark,—more black than purple.

Cholera in its purple form, especially shows this darkened and depraved state of the blood. *Dr. Annesley*, in his work on diseases of India, was the first to bring forward in a prominent manner this striking character of the disease;β and so many writers, in various countries since visited by the epidemic, have repeated the observation, that I need not adduce further authorities or examples.* *Wherever the blood is darker than natural, and just in proportion to that change, are the vital functions impaired or abolished.*

The effects of sanguineous congestion depend principally on the deficient supply of scarlet blood. *Dr. Armstrong* published interesting and important observa-

α "Nam si is (sanguis) crassus et niger est, vitiosus est; ideoque utiliter effunditur: si rubet et pellucet, integer est; eaque missio sanguinis adeo non prodest, ut etiam noceat;"—*Lib.* II. x.

β *Op. citat.* II. 405. See also *Sketches of diseases of India*, p. 41.

* One illustration, however, I take from my own practice. Last year (1832) when the cholera spread in Leeds, I bled a gentleman in the purple stage of that disease, whose case was one of the worst I had seen. Blood at first was obtained with such difficulty, that it was necessary to open three veins at the same time, to employ fomentations and other auxiliaries. The fluid more nearly resembled diluted tar than venous blood. When at length we had drawn about sixteen ounces, the colour gradually changed to modena, and when we reached twenty, advanced to a redder hue, and almost approached the colour of arterial blood. This patient was saved.

tions on congestive fevers; but did not particularly advert to the immediate cause of the symptoms he describes. The coldness and shivering, the dull headache, the general languor and debility,—in a word, the oppression rather than the perversion of functions, decidedly indicate a reduction of nervous energy; and this again refers to the blood as the excitant of that energy. A morbid impression on the nervous system first reduces the decarbonizing process, the foul state of the blood re-acts in a powerful degree upon the nerves, and from this mutual action and re-action of the blood and nervous system, proceed all the phenomena of the disease, from the stage of invasion to the collapse of death.

These observations do not apply only to the fevers termed congestive. All fevers begin with symptoms of oppression or reduction of nervous power: and all cases of inflammation, if we could trace their origin, would exhibit similar phenomena. We may even affirm, that all diseases, whether acute or chronic, have their source in a disordered state of the blood and nervous system. Take for instance, the numerous and diversified complaints commonly termed indigestion. In them we cannot, indeed, generally trace the first morbid changes, but we find the existing symptoms clearly dependent on the united causes I have mentioned; and to be removed only by a removal of these causes. The treatment even of dyspepsia, is successful or unsuccessful, as we regard or neglect the circulation and state of the blood.

Take headache with its varieties and degrees, from the slight dull sensation of an hour, to the serious continued advancing disorder which introduces fatal disease. The want of a free circulation of scarlet blood, I conceive, is ever the cause of the oppression of the head. The congestion, however, is sometimes supported by increased impulse, and the hyperæmia of *Andral;* sometimes, on the contrary, it is dependent on defect of impulse and deficiency of blood. We find congestion with increased action of the cerebral blood-vessels, and sometimes with diminution. Thus the same effect seems to be produced by different causes. Reflection, however, soon explains the incongruity. If the cerebral arteries act with more energy than the capillaries of the brain, these will receive blood faster than they can transmit it to the veins and sinuses. A partial stagnation will therefore ensue, and this will prevent the sufficiently free and continual access of scarlet blood. The very fulness of the capillaries produces a defect—an inadequate supply of the pure fluid, which can alone excite the nervous system, and support the functions of life. Congestive headache, in proportion to the extent and degree of its cause, is the result. But how can congestion arise from defect of arterial impulse? The circulation through the capillaries is greatly supported by the *vis a tergo:* and if the cerebral arteries act with less than healthy energy, or contain a diminished quantity of blood, its motion through the minuter vessels must be proportionally diminished. Pure and scarlet blood will not therefore be circulated with sufficient freedom, and congestion must ensue.

Thus we may have congestion depending sometimes on excessive, sometimes on defective, arterial impulse. The principle applies, of course, to other, nay to all diseases, and to their successive stages. It bears also on therapeutic doctrines; and shows why at one time congestion is removed by bleeding, at another by stimulants and tonics

Scarlet blood, I repeat, is essential to the due performance of every function. In a former chapter arterial blood has been examined in contrast with venous, and it has been contrast with venous, and it has been attempted to show in what their essential difference consists. I do not maintain that the disorder of the blood is solely an excess of carbon. Hereafter we shall discuss other changes, very important and remarkable in their effects.

2. THE TEMPERATURE of the blood has been changed in some cases of disease. In fevers and internal inflammations, though the thermometer is not generally raised above 97°, many instances have occurred in which it has been elevated to 104°, 107°, and even 110°, and in such cases, we suppose the blood to have been nearly of the same temperature as that of the body. Other forms of disease, on the contrary, have occasionally presented a considerable reduction of temperature. In the cold fit of an ague, the blood has been as low as 94°. Some curious but less accurate observations have also been noted, which indicate a much greater reduction of temperature. Blood, on flowing from the arm, has produced a marked sensation of cold. *Morgagni* has recorded several instances, in one of which the patient compared the feeling, produced by the stream on the arm, to that of ice. In most of these cases there existed some affection of the nervous system. *Mediavia*, as quoted by *De Haen*, states the circumstance of a woman, whose blood, from the testimony of her surgeon, was quite cold as it flowed from the vein. The catamenia, likewise, were always attended with a similar sensation. The patient laboured under what Mediavia terms " suppressio ac suffocatio pulsuum."[α] A like phenomenon presented itself in one of my patients, in September, 1818. On bleeding a pregnant female, the stream, during the whole period of

[α] DE HAEN, *Ratio Medendi*, III. 36,

its continuance, produced a chilly feel both on my finger and the patient's arm. Having no thermometer at hand, I could not accurately note the degree of cold; but the temperature marked by a stream of water, which produced a similar sensation, was 68°. The case was attended with no remarkable symptoms, the patient suffering only from headache, and the ailments common to her condition.

To explain observations so singular, we require others in addition, and more minutely stated: we need also a fuller acquaintance with the operations of the nervous system.

3. THE SPECIFIC GRAVITY of morbid blood differs little from that of the healthy fluid. *Dr. Whiting* found it less than his standard in seven cases which he examined. These appear to have been all of the acute character, though the patients differed in age and constitution. The lowest specific gravity he states is 1041·8: but I suspect his standard to have been erroneous. That which I have formed from the average of blood from six persons in health is 1041, only a decimal different from the lowest specific gravity of Dr. Whiting's specimens of disease! Consequently, if my average be a near approach to the truth, the instances which he considered below the standard of health, are really above it, and his cases of acute disease show an *augmentation*, not a reduction of specific gravity in such diseases.* This accords with my experiments on the subject.[α]

* I ought to remark that not having now at hand Dr. Whiting's interesting treatise, I am not sure that I correctly represent his statements.

α In Mr. Thackrah's notes the specific gravity of morbid blood is recorded in four instances, viz. :—in one case of pneumonia 1041, in another 1042, in a case of congestion 1042, and in plethora 1045. The specific gravity of the serum was ascertained in twenty-one cases, the average of which I find to be 1031·4. The standard of health, it will be remembered, is 1020—30. ED.

I suspect, however, from the difference found in the specific gravity of healthy individuals, that the period of the day, and the relative time of meals, considerably vary the weight of the blood, and render the average an unsatisfactory standard.

4. Coagulation is much affected by a morbid state of the blood. In chapter VII, the fact that the blood concretes speedily in proportion to the weakness of the system, was particularly stated; and the subject was more regarded, from the practical inferences which may be drawn from it. In this place I introduce two short series of experiments; one to show that the same principle which was in the former case illustrated by animals in health, obtains in man in disease, viz. the last-drawn blood in venæsection cakes more quickly than the first: and the second to show that the character of the disease, as tonic or atonic, greatly influences the coagulation of the blood. In reference to the former point,

Exp. CXCIV.

From the arm of a female labouring under fever, blood was drawn to the amount of a pound and a half; a portion of which received in a tea-cup on its first effusion remained fluid for seven minutes; a similar quantity taken immediately before tying up the arm, was caked in three minutes and a half.

Exp. CXCV.

A man, the subject of incipient enteritis, lost about a pound of blood. Of two portions received, as in the preceding experiment, the first began to coagulate in seven minutes, the last in four.

Exp. CXCVI.

Blood was taken from the arm of a young man labouring under an hepatic affection, combined with fever. A gentleman standing by, was requested accurately to mark the time at which the vessels were filled, and that at which concretion took place.* On subtracting the the periods, the result was as follows :—

In the 1st cup	12′	25″
2	12′	0″
3	11′	20″
4	10′	5″
5	8′	20″

The vessels contained three and four ounces.

So regular an acceleration of the concretory process in proportion to the reduction of the vital powers is a matter of surprise, nor can it be expected frequently to occur. It is observable that the former periods approximate much more than the latter, and it is probable that if the depletion had been continued, the sixth cup would have concreted in little more than five minutes, and the seventh in two or three.

The principle of blood's speedy concretion in debility, is important in a curative point of view. The first natural check to hæmorrhage is known to be the formation of a clot on the mouth of the vessel. If the longer the hæmorrhage the less had been the disposition to form such a clot, the wounded on the field of battle, and those injured by common accidents, who cannot promptly procure the aid

* In some of our experiments on this subject I suspect the period of concretion was stated when a clot was first seen, and in others when fluidity was lost. As this difference, however, did not occur in the *contrasted* portions of blood, it does not affect the inference.

of a surgeon, must inevitably have perished. Even with the best professional assistance the flooding female must almost invariably have sunk. Happily, however, the reduction of the general system, and the fainting which results from bleeding, so decidedly dispose the blood to concrete, as well as diminish the impulse on the injured vessel, that death from hæmorrhage is comparatively rare.

With regard to the effects of a tonic and atonic state of the system on the concretion of the blood, I contrast the following experiments.*

Exp. CXCVII.

A vein was punctured in the arm of a young person labouring under pneumonic inflammation. Coagulation did not commence till the expiration of eight minutes, though, from the smallness of the vein, the blood trickled guttatim.

Exp. CXCVIII.

From the arm of a stout young man, affected with urticaria, half a pound of blood was taken. A portion received in a small cup, did not coagulate till the expiration of thirteen minutes.†

Exp. CXCIX.

A small cupful of blood from a young woman in the last stage of phthisis pulmonalis exhibited concretion in four minutes.

* Dr. G. Fordyce states, in his Practice of Physic, that "the inflammatory diathesis is marked by a hard, and for the most part, a strong, full, and frequent pulse; the blood when taken from the arm, more fluid, and *continuing longer fluid.*"

† So long a period is not usual, nor is it here adduced as an example of a general rule, concretion commonly taking place in healthy blood at the end of 5—8 minutes.

Exp. CC.

A female, aged 33, pallid and emaciated, yet labouring under febrile symptoms, lost about half a pound of blood. In a cup taken soon after the vein was punctured, coagulation commenced in four minutes and ten seconds.

The marked disparity between the periods of coagulation, when the system is under the influence of active inflammation, (CXCVII) or remains unbroken by disease, and when the vital powers are reduced, (CXCIX. and CC.) clearly points out the importance of the subject in a curative view. Whoever pays attention to the circumstance, will, I am persuaded, accede to the opinion, that *the speedy occurrence of concretion on the effusion of blood, affords a reason sufficiently cogent for the discontinuance of depletory measures.*

Blood has been occasionally taken where the vascular action was preternaturally high, yet the system considerably reduced; Exp. CXCIX. and CC., are instances. In these cases, *increased action* was combined with *diminished power;* the former tending to retard, the latter to accelerate, coagulation. Hence, though some variety occurred, I generally found concretion, under such circumstances, to take place in a medium time.

The completion of coagulation does not observe the same regularity as its commencement. The perfection with which serum is effused, and the time required for the process, varies greatly. In a case of petechiæ hæmorrhagicæ related by *Mr. Pretty* in the London Medical and Physical Journal, blood drawn showed no serum for eight hours, but afterward separated a small quantity.

Exp. CCI.

A poor woman, the subject of apoplexy, presented blood with a loose coagulum, and a deep fibrous coat.

In two days, serum 27, crassamentum 470; or as 10 to 174.

Though these and other experiments show considerable variety, I feel warranted in the general inference, that in diseases of great debility serum is effused slowly and in small quantity.

The non-coagulation of blood in the body after death has been discussed before. We now advert to the absence of coagulum in blood taken by venæsection. It would seem, from the observations of *Huxham* and others, that in some complaints blood does not in any degree separate its fluid. In malignant petechial fevers, he says, "the crasis is so broken as to deposit a sooty powder at the bottom of the vessel, the upper part being either a livid gore, or a dark-green, and exceedingly soft jelly." In some scorbutics, likewise, where a disposition to hæmorrhage exists, he remarks, that the blood does not coagulate, but becomes grumous. *De Haen* also saw the blood in a dissolved state. *Richerand** states, that the venous blood which issued from the stump of an old man whose arm was amputated, was similar to a weak dye of logwood, entirely dissolved, purple, and never underwent the true coagulating process. Neither, according to the statement of authors, does the blood coagulate in the plague. *Hewson* saw blood taken from a woman, the subject of fever ensuing on parturition, which did not separate into serum and crassamentum. Similar observations are scattered through medical writings. How far they ought to be considered as cases of non-coagulation, may perhaps be doubted? I suspect that in most of them, if the blood had been thrown on a filter more or less coagulum would have been found. If I may judge from

* *Elem. Phys.* by De Lys.

my own remarks, I should say, that instances of real fluidity of the blood are very rare.

How shall we explain the retardation and absence of coagulation? Do the altered qualities of the albumen and hæmatosine diminish or prevent the play of affinity or attraction which in healthy blood produces the crassamentum?

5. THE FIRMNESS OF THE COAGULUM of blood has been considered a distinctive mark of a tonic state of the system; its great tenacity, a characteristic of inflammation; and its looseness, a sure proof of debility. During inflammatory action in strong persons the coagulum of the blood certainly becomes more cohesive; less perhaps from the increase of fibrine, than the increased quantity and altered quality of the albumen. While some diseases, as those of great debility, may be conceived to reduce the affinity of this substance to hæmatosine, inflammation, and generally the tonic state of the system, seem to increase it.

Dr. Langrish, in his experiments on morbid blood,[a] used a glass tube resembling that of a thermometer, for ascertaining the tenacity of the crassamentum. He placed the bulb on a coagulum, and adding quicksilver, found what quantity was necessary to penetrate it. His glass was graduated, and hence he could readily notice "the degrees of cohesion." From his observations it appears, that in acute fevers, including cases of inflammation, the average of tenacity was about 36; that of tertian fever was 23: and that of quartans 17. He further remarked, that blood drawn from three healthy young men had crassamentum of but 9, 10, and 12

[a] *Modern theory of the Practice of Physic*, by BROWNE LANGRISH, M.D London, 1738. p. 67.

degrees of cohesion. *Dr. G. Fordyce* remarked, that the blood coagulates into a firmer or looser mass, generally in proportion to the strength of the system.* The same opinion was also repeatedly advanced by the late *Mr. Hey*.

In acute maladies, therefore, it is evident, that the coagulum is generally dense. We frequently, however, observe much benefit derived from bleeding, even when the crassamentum is soft and yielding; nor should we, in such cases, hesitate to repeat the depletion, if other circumstances indicate its propriety. *Dr. Watt*, in his case of diabetes, remarks that great advantage accrued from venæsection, though the coagulum was loose and black; and that on repeated evacuations of blood, the crassamentum became much firmer, and of a more natural hue. In a case of the same disease, related in Majendie's journal, by *Dr. Lefevre*, this observation on the character of the blood and the effect of bleeding is confirmed. Experiments, moreover, show that the density of the coagulum is not always proportionate to the thickness of the blood as a whole. I scarcely need add that bleeding is often beneficial in diseases where no signs of inflammation exist.

As the density of the coagulum has had a considerable effect in the treatment of disease, I shall advert to two or three points of fallacy on this subject. It is frequently found that the serum is slowly exuded; and hence, unless a due time elapse before examination, the coagulum is soft from the serum it contains. Here, upon the general principle, the practitioner would desist from further evacuations, concluding the system to be greatly reduced. Sometimes, also, from the adhesion of the coagulum to the side of the vessel, from the kind of vessel, or other

* Elem. Pract. Physic.

causes, the separation of serum is prevented for many hours, yet, on the removal of such attachment, or on the division of the coagulum, the serum is effused, and the crassamentum becomes firm. The size of the vessels has also a considerable effect on the exudation of serum, and consequently, on the density of the remaining coagulum. The fluid of blood received in a basin, is usually in greater proportion than that contained in a small cup, and of course, the cake in the latter is looser than that of the former. If, however, on the *division of the coagulum, at the expiration of from eight to twenty-four hours, there ensue no considerable effusion of serum, and the crassamentum remain extraordinarily firm,* I believe that *further depletion is fully warranted.*

6. THE PROPORTIONS OF SERUM AND CRASSAMENTUM are considerably affected by disease. The chronic maladies which for a long period reduce the supply of nutritious aliment, and the repeated bleedings which result from accident, or are employed as remedies, alike tend to increase the proportion of serum; for this fluid is comparatively easy, crassamentum difficult of restitution or re-production.* Assuming 10 parts serum to 13—14 crassamentum as the standard of health, let us examine some specimens taken from patients labouring under chronic and debilitating disease.

EXP. CII.

In a dyspeptic case, a small quantity of blood was drawn at the patient's request,—a female of pallid countenance, spare habit, and health long impaired. The

* See chapter VII. sect. 11. page 141.

blood looked thin as it flowed, and assumed a partial coat of size. Serum 250, crassamentum 285; or as 10 to 11·4.

Exp. CCIII.

Blood was taken from the temporal artery of a man, whose constitution was feeble, and employment sedentary. His complaint was ophthalmia, but, with this malady, the system did not appear affected. On weighing the blood two days afterwards, the serum and crassamentum were very nearly equal in quantity.

Exp. CCIV.

Blood was taken from a woman, aged 46, afflicted with hemiplegia: serum 420, crassamentum 520; or as 10 to 12·3.

Exp. CCV.

Blood was drawn from the arm of an emaciated female long the subject of ascites. At one venæsection, the serum was to the crassamentum as 10 to 11·2; at another as 10 to 12·9.

These indicate *an increase of serum, or in other words a reduction of crassamentum, in disease of an atonic character.*

Reversing the view, we select a few specimens of blood from patients labouring under acute disease.

Exp. CCVI.

Two or three ounces of blood were taken from the arm of a girl, aged seven, labouring under pneumonic inflammation. Serum 166, crassamentum 460, or as 10 to 28.2.*

* In this as well as the succeeding cases, care was taken that the serum had due time to exude.

Leeches were applied to the breast the succeeding day; and on the third, about four ounces of blood taken from the arm. Serum 819, crassamentum 1124; or as 10 to 13·7.

Exp. CCVII.

Blood was taken from the arm of a woman, who had been of a robust constitution, but was now affected with a mild gastritis, or what might be termed gastric fever. Serum 80, crassamentum 225; or as 10 to 28·1.

Exp. CCVIII.

In a case of decided peritonitis, on the second bleeding, and after the inflammatory action had been much reduced, the proportions in two vessels were found the succeeding day to be, in that first received, serum 24½, crassamentum 39; or as 10 to 15·9: the second, serum 22½, crassamentum 42; or as 10 to 18·6.

Exp. CCIX.

Continued fever. A female, aged 26. Serum 325, crassamentum 520; or as 10 to 16: at a second venæsection, serum 250, crassamentum 410; or as 10 to 16·2.

Exp. CCX.

Incipient peritonitis. A man aged 58. Blood buffed and cupped; serum 680, crassamentum 1180; or as 10 to 17·3.

Exp. CCXI.

Severe pleurisy. A female. Crassamentum in two vessels, the day after venæsection, appeared so large that I broke it in pieces, and allowed the blood to stand three days longer for the full exudation of the serum. Yet the numbers ultimately proved to be,

No. 1. Serum 1080, crassamentum 5580; or 10 to 54·9
... 2. ....1080, ... 5520; or 10 to 54·8

This is an increase of crassamentum to which I never knew another case approximate; though the previous experiments afford abundant parallel instances in a minor degree. We may, then, infer that *acute disease reduces the proportion of serum;—in other words, increases the mass of crassamentum.*

I refer to experiment CCVI. as showing the importance of reducing an inordinate proportion of crassamentum. From the first attack of the complaint to the second venæsection, the child was in great suffering, but no sooner was the proportion of crassamentum reduced to its healthy degree, than the pain was greatly relieved, and in twenty-four hours, the malady completely removed.

From *Langrish's* experiments before referred to, it appears, that the average of the relative quantities, was 10 serum to 33 crassamentum, in acute fevers; 10 to 25 in tertians; and 10 to 16 in quartans. It seems probable, that in these cases the serum had not been completely effused; for the proportion of crassamentum in Langrish's statements considerably exceeds that commonly found in similar diseases. *Dr. Mills*, in almost all the cases of acute fever which he records, found the serum to be in very small proportion to the crassamentum, though he does not appear to have weighed these constituents. In some acute maladies, scarcely a drop of serum is exuded; and, what is remarkable, in asthma, a like circumstance has been observed. *Morgan* states the case of a female labouring under this disease,—

a pulse small and quick, "with melting heat,"—in which a pound of blood separated but 2½ oz. of fluid; and at another bleeding, the crassamentum was scarcely covered with serum.[a] I once took about 6 oz. of blood from the arm of an elderly gentleman, affected with asthma, with a pulse of 140, soft and weak. Next morning, I was surprised to find the exuded serum not to equal a tea-spoonful; and scarcely did the coagulum, on its division, separate any additional quantity. Such cases I am inclined to consider as instances of defect rather in coagulation, than in the proportion of serum.

The doctrine, nevertheless, of the proportions of serum and crassamentum being dependent on the character of the disease, is subject to considerable modification. The experiments I have stated are fairly taken as an approach to a common average, but I have found several results of an opposite and apparently inconsistent bearing. The incongruity is removed in most cases by a reference to the constitution and general physical condition of the patient. This subject has been before discussed in chapter VII, section 10; and here I need only express my opinion, that in reference to this point of observation, as much depends on the robust or reduced state of the system, as on the character of the malady. Where the animal frame is weak and delicate, the proportion of serum is usually large; when the disease is unattended with fever or inflammation, the proportion is increased. On the combination, therefore, of these causes, the blood will generally be found preternaturally fluid. If, on the contrary, the system be strong and robust, the quantity of serum is comparatively small; if the affection be of the

a *Philosophical Principles of Medicine*, by Thomas Morgan, M. D. London, 1725, p. 120.

inflammatory character, the serum is considerably diminished. On the combination, therefore, of the latter circumstances, we may expect the fluid to form but a small proportion to the solid parts of the blood.

It is by no means an unfrequent occurrence, however, for debility to be combined with acute disease. In this instance, according to the opinion I have stated, two principles or causes, opposite in their nature, are tending to produce contrary results, and as the one exceeds the other we shall have a corresponding increase or diminution in the proportion of the serum. The following observation illustrates my view :—

Exp. CCXII.

A young woman, in an advanced stage of phthisis, lost from the arm a few ounces of blood. This, two days after subtraction, weighed, serum 345, crassamentum 540 ; or as 10 to 15·6.

Here there was *increased action* conjoined with *diminished power;* a pulse ranging from 120 to 140, and a constitution greatly reduced; the former tending to lessen the relative quantity of serum, and the latter to augment it. In consequence, we find the serum to be in the proportion of 10 to 15, while, had there existed no principle to counteract the agency of the high vascular action, it would probably have borne a relation only as 10 to 20—25.

In all examinations of the proportions of serum, great care must be taken that coagulation is complete. In some blood, as I have urged in a former section, the fluid quickly exudes ; in other specimens it flows for several days. The serum, moreover, is sometimes abundant in acute diseases, in cases of urgent inflammation, when the crassamentum has contracted with more than ordinary force. A man, for instance, was bled for arachnitis. In subsequently observing the blood, I was particularly struck with the density of the coagulum, and the quantity of fluid effused. *If, therefore, we find serum in large proportion, with the crassamentum extraordinarily firm, we must not infer debility or atonic disease. But if there is a great quantity of serum when the crassamentum is comparatively loose and yielding, we are, I believe, justified in that opinion.*

7. Of the appearances which the blood assumes in disease, that most regarded by modern observers is the crust of yellowish size or BUFF-COAT. This has usually been considered the criterion of inflammation, since its presence is commonly found in pleurisy, and acute rheumatism. In these and some other affections, the blood on flowing from the arm appears thin, and on remaining three or four minutes, throws off a blueish lymph, which quickly concretes into a dense sheet, much resembling the spurious membranes produced by internal inflammation. This tunic, allowing the coagulum to adhere to its inferior surface, gradually increases in thickness, till it reaches the one-twelfth or one-eighth of an inch, or even, according to *Huxham*, an entire inch.

In some cases the surface is concave, and this cupped appearance is greatest, I think, when the quantity of blood is small. In blood of a buffy constitution the formation of the tunic is considerably affected by the mode in which the fluid is abstracted. A small trickling stream will prevent the appearance of the sizy tunic. The kinds of vessel in which the blood is received will also have an effect in altering its character.* I have repeatedly remarked, likewise, that during high vascular action, the first cup from venæsection, and which remained fluid the longest, had a dense crust; the surface of the second had transparent spots; and the third, which concreted most quickly, had no appearance of size: nor is it unfrequent, on the contrary, to find the buff-coat absent in the first vessel, and developed in the second.

The specific gravity of blood which presents the buff-coat has been examined by *Dr. Davy* in eleven cases. "In five instances in which the buffy coat was slight, the specific gravities were, 1.047, 1.051, 1.054, 1.055, and 1.054. In five instances in which the buffy coat was moderately thick, the specific gravities were 1.044, 1.038, 1.052, and 1.056; and in one instance in which it was thick, the specific gravity was 1.057." [α]

Besides pleurisy and rheumatism, various other mala-

* Some years ago experiments were made at the Hotel Dieu, under the direction of Professor RECAMIER, on the coagulation and buff-coat of the blood, as influenced by the mode of drawing it. In *Johnson's Medico-Chirurgical Review*, for 1824, one is related as follows: "A man, 35 years of age, of athletic constitution, was selected. After a violent exertion, he was suffering much pain in the lumbar region. A vein was opened in each arm at the same instant. In the right arm the orifice in the skin was one line and a half (French) in length—that in the vein one line. The stream was continuous, and three inches in projection, rather weak. The bleeding was stopped at the end of two minutes. *Results*—The blood from the right arm presented no buff. The clot was of the ordinary consistence. The blood from the left arm presented a thin layer of buff, the clot and serum being similar to those of the blood from the other vein."

M. BELHOMME (the experimenter under M. Recamier) has made about one hundred and fifty experiments on blood drawn in health and disease. He has come to the conclusion that a medium orifice (one line in the vein), a strong, rapid, and continuous jet in the form of an arch, and a narrow vessel for the reception of the blood, are the circumstances most favourable for producing the buffy coat. In strongly inflammatory diseases, however, and in pregnancy, the buffy coat will appear in almost whatever way the blood is drawn.

α *Edinb. Journal*, xxxix. 246.

dies give rise to the buff-coat. In cases where the mineral, or even the vegetable acids, have been freely administered, it has been observed;* and *Dumas* found it on the blood of a dog that had been bled during extreme thirst. It has not unfrequently been seen in a high degree in the disease called scurvy; in diabetes also: and it is said to exist in the last stages of phthisis. Utero-gestation, in its healthiest state, rarely fails to give rise to it. The buff-coat, I believe, is not presented by acute disease of mucous membranes; nor by a number of other maladies, which are unaccompanied by inflammation of serous structures.

Dr. Stoker, of Dublin, from his own observations, as well as those of *Mr. Todd*, conceives that the colour and figure of the buff are characteristics of the seat of disease. In simple pneumonic inflammation they have remarked the crust to be cupped, and either white or tinged with bright red. "In simple forms of hepatic disease" it is comparatively dark, extremely yellow, deep, and generally flat on the surface. α

Rapidity of circulation generally exists when the blood is sizy; and we find with some surprize, that a sudden rise of circulation may produce, or rather be attended by, an equally sudden formation of size. The fact of healthy horses presenting this appearance after a gallop, has before been noticed. A gentleman who, though out of health, was attending regularly to the duties of an active profession, was suddenly seized after eating a dinner of sole, with urticaria, accompanied with violent action of the arteries, and excessive impulse in the head. The pulse at first only 80, was found in two or three minutes afterwards to be at 140. In great distress he

* Cruickshanks *on Acids in Lues.*

α *Pathological Observations,* p. 37, et seq.

had a vein opened in each arm, and about a pound and a half of blood abstracted. It was strongly buffed and cupped. If his previous gastric disorder had not generated the buff, we must consider the case one of extraordinary rapidity in the formation of this crust.

Quickness of circulation, however, though a general, is not a necessary attendant on the sizy state. I bled a patient labouring under pleurisy, whose pulse before venæsection was but 80; yet a fibrous tunic was formed. In another, the subject of peritonitis, with a pulse of 84, three vessels of blood had each a sizy crust.

Authors have long noticed this appearance, without adverting to its nature or explaining its formation. *Sydenham* particularly described it:* but *Gaber* † and *Hewson* appear to be the first who carefully investigated its nature and cause. *Hewson* attributed the sizy crust to the tenuity of the blood, and consequent subsidence of

* The passage is too curious to be omitted. Adverting to pleurisy, he says, "Nimirum sanguis (saltem ille qui post primam vicem extrahitur) ubi refrixerit, sevi liquati præ se fert speciem ad crassitiem satis conspicuam; ac superficiem habet veri *puris* æmulam, et tamen ab eo longè diversam utpote quæ fibris instar reliqui sanguinis, arctè contexitur, nec ad modum puris defluit, quin à reliquo divulsa discolor illa pars, formam cuticulæ tenacis et fibris refertæ exhibet; et fortasse nihil aliud est quàm Fibræ sanguineæ, quæ rubicundo ac naturali suò integumentò per præcipitationem exutæ, ambientis aëris frigore in membranam hujusmodi subalbidam concrevêre. At verò (ut id obiter attingam) animadvertendum est, quòd si sanguis é secta vena non recto flumine versus Horizontem prosiliat, sed per cutim repens perpendiculariter dimanet, utut celeri se proripiat gradu, sæpe tamen ad dictum colorem non accedit, cujus ego me causam nescire fateor. Neque æger ab istiusmodi sanguinis missione perinde levatur, ac si modò primùm descriptò fluat. Quinimo cùm hoc ipso modo extrahitur sive orificium angustius, sive quid aliud obstiterit, quo mimùs pleniori exeat gurgite, neque hic sanguis pleuriticorum sanguinem colore æmulatur, neque æger par exinde commodum capit. Observavi etiam, quod si sanguis recèns extractus, quocunque demum modo fluxerit, immisso digito agitetur, superficie rubenti ac florida, ut in aliis morbis quibuslibet, spectabitur."—*Opera Med.* p. 164.

† *De Humoribus Animalibus.*

the heavy red matter. This opinion is favoured by the apparent thinness of buff-coated blood, but is unsupported by analysis. *Fourcroy* considered the buff-coat as coagulated albumen, with a portion of fibrine : *Deyeux* as gelatine, or fibrine degenerated into gelatine. The ingenious *Bordeu* believed the sizy tunic to be the result of a sort of plethora, which he terms mucous cachexy. In fact, he seems to view it as an excess of nutritious matter. This opinion, and the observations connected with it, have received considerable attention, yet it is ill-defined, ill-supported by analogical reasoning, and quite devoid of corroboration from experiment.*

What is the buff-coat? Is it a new substance formed in the blood by disease? No: for examination proves the existence of its constituents in the blood, and shows displacement rather than creation. It is a network of fibrine enclosing serum.† When pressed, the buff-coat yields a

* Dr. John Davy seems to agree with Hewson in ascribing the buff-coat to tenuity of the blood. *Edinburgh Medical and Surgical Journal*, xxix. 245.

† This, though established by daily observation, is well illustrated by an experiment of Mr. Dowler's related in the *Medico-Chirurgical Transactions*, xii. 90.—" Eight ounces of blood were taken from a man labouring under rheumatism, and received into two four-ounce vessels; the one being allowed to rest, in order that a buffy coat might form; while the blood in the other was kept in motion, to prevent its formation. After the lapse of twenty-four hours, the blood that had been kept agitated was washed, in order to separate the fibrine, which, when dried, weighed twelve grains. The four ounces contained in the other vessel, being washed, and the fibrine collected and dried by the same means (having, however, the buff previously removed,) afforded only six grains; consequently, it might reasonably be supposed that the six grains wanting were existing, in some form or other, in the buffy crust. This was found to be the case; for when it was submitted to pressure, a large quantity of fluid escaped; the mass became evidently of a fibrous structure; and, when dried, it weighed precisely six grains, making, together with the six which were obtained from the crassamentum of this blood, a number equivalent to that produced by the blood in the other vessel. The fluid thus pressed from the buff became solid by the application of heat, and consisted of common serum."

fluid which has all the properties of serum, and a substance which cannot be distinguished from fibrine.* The serum drawn from the sizy crust is stated by *Dowler* and *Gendrin* to contain a larger proportion of albumen than the serum of the rest of the blood. We found a result directly the reverse of this, viz.:—in 1000 parts of serum spontaneously exuded 134 parts of solid matter, while that of the same blood squeezed from the sizy crust yielded but 65. The buff-coat is of greater specific gravity than the serum, a fact which I should not have noticed had it not been denied. The ratio of its solid and liquid elements, however, varies considerably. It is found in some cases to be mostly fluid; in others, it is tough and compact like leather. Yet this contrast of appearance of tenuity depends partly, I believe, on the period at which it is examined. As a specimen of the constituents of the buff-coat, I detail the following:

Exp. CCXIII.

To ascertain the proportions of solid fibrine and solid matter in serum of the buff-coat, it was carefully separated from the crassamentum; and after the serum had been squeezed from the fibrine, each was placed on a sand bath, and, when dry, their quantities estimated.

* Blisters produced by cantharides are generally mere serous exudations; but sometimes they present a substance analagous to the buff-coat. This is often very thick, and consists of serous cells intersected by fibrous bands. It is most frequently formed on the second or third application of a blister before the healing of the first.

Should the fibrine be too abundant in the blood, would not such application of blisters tend to reduce it? In cases of pneumonic and some other inflammations, it seems highly advisable to draw off fibrine, the material of pleuritic adhesion, by blisters repeated for three days in succession, rather than to use them at the usual intervals.

Dry solid fibrine,	18
Dry solid matter from the serum	26
Water, or loss by evaporation,	56
Buff-coat	100

Since, then, the buff-coat is composed of fibrine retaining serum, is fibrine especially abundant in sizy blood? *Dr. Davy*, from his observations, infers that there is no constant relation between the appearance of this tunic, and the proportion of fibrine; yet his tabular report shows, that in every case but one the proportion of fibrine in buffed blood exceeded, and in most cases greatly exceeded, that which he marks as the healthy standard.* From all the examinations we have made, I infer without hesitation, that buff-coated blood contains a considerably greater proportion of fibrine than healthy blood. In illustration, I may refer to the 12th section of chapter VII.

How is the sizy tunic produced? The question is not difficult of solution, if we bear in mind three circum-

* I quote his table from the *Edinburgh Journal*, xxix. 248.

		Specific gravity.	Dry Fibrine per cent.
"1.	Healthy blood,	1.052	·15
2.	1st. Portions slightly buffed,	1.054	·37
	2d. After loss of 24 oz. do.	1.053	·34
3.	1st. pretty strongly buffed,	1.044	·47
	2d. after 24 oz. less buffed,	1.042	·37
4.	1st. slightly buffed,	1.055	·13
	2d. after 2½lbs. do.	1.054	·13
5.	1st. slightly buffed,	1.054	·17
	2d. after 24 oz. less buffed,	1.052	·16
6.	Buffy coat thick,	1.038	·34
7.	1st. moderately buffed,	1.058	·36
	2d. after 28 oz. strongly buffed	1.057	·40
8.	Slightly buffed,	1.056	·40
9.	Moderately buffed,	1.052	·39
10.	Slightly buffed,	1.051	·41
11.	Do. buffed,	1·047	·36"

stances : first, the increased quantity to which we have just adverted, of fibrine in sizy blood; secondly, the apparent identity of the buff-coat, and the spurious membranes of disease; and thirdly, the absence of the sizy crust in a perfectly healthy and unexcited state.

From the blood are formed all the animal structures, and not less, of course, the adhesions of pleurisy than the fibres of muscle. It is obvious that provision must be made for such formations, and we find it in that great laboratory, the circulating system.

Fibrine distributed in health in small proportion for the reparation of muscular and other structures, is increased for more extensive formations, and especially for adhesive inflammation; and from this excess of fibrine the blood drawn exhibits a fibrous crust. The greater specific gravity of the united hæmatosine and albumen, explains alike the general situation of the tunic and the prevention of a sizy coat by agitation of the blood. The immediate source from whence the superabundant fibrine is derived and the thickened state of the serum in contact with it, will be noticed hereafter.

The observations on the firmness of coagulum, and the proportions of serum and crassamentum, &c., are the result of former inquiries, and most of them appeared in the edition of 1819. Since that period we have not prosecuted this branch of the subject, but have endeavoured by analysis to ascertain the changes in proportion which take place in the elements of blood. The preceding sections of this chapter have had reference chiefly to obvious and sensible variations; the succeeding will chiefly regard those concealed changes which require more minute examination.

8. We have found great diversity in the SOLID CONTENTS of the blood. Its thickness is sometimes diminished, but much more frequently increased.

(A) The thinness of blood, or the reduction of its solid matter is occasionally seen, but not to a great extent, in that vascular congestion which does not depend on increased action of the heart and arteries. A female, aged 20, of sedentary habits, inclined to be fat, but of a remarkably exsanguineous and pasty complexion, was bled for amenorrhœa. The blood yielded 152 pints of hæmatosine and albumen, while the standard of health is 157·8. A married female (Crowther) of the age of 46, very fat, was bled on account of the plethora, which threatened her head and chest. Her blood yielded 149 of hæmatosine and albumen. From the slight degree of reduction, however, in these cases, and from their rarity, I believe that the blood of persons who live well and enjoy the comforts of life, is seldom made thinner by disease.

(B) The proportion of solid contents in the blood is remarkably increased in almost all those diseases for which venæsection is prescribed. Of sixteen cases taken in succession from ordinary practice, in which the elements of the blood were carefully examined, only one failed to show an augmentation of its density. Of the rest, six had a proportion between 160, the healthy standard, and 200; seven between 200 and 300: and two between 400 and 500. The wide difference, however, between the last and the others, indisposes us to receive them as ordinary specimens. They were, in fact, cases of cholera, in which water had been drained from the blood by large and continued evacuations. Rejecting these specimens, and a third that was subsequently taken from one of the cholera patients, we find the twelve cases present the following numbers:—

246·7	163·3	205·7
238·3	174·7	182·1
268·3	252·7	216·1
181·3	173·9	192·2

When we take the average, we find the increase scarcely less than one-third of the whole quantity.

In what elements is this remarkable increase? Chiefly in the albumen, sometimes in a small degree in the fibrine, possibly also in the hæmatosine. Whatever may be the result of investigations as to the quantity of salts in morbid blood, *Dr. Stevens*' notion of the absence of salt being the cause of the dreaded fevers of the West Indies, reminds one of a simile of *Lucian*—" a child peeping through the mask of a giant." The serum in the cases I have referred to, did not show a uniform increase in its specific gravity, or the solid contents on exsiccation: nor did these bear a regular relation to the solid contents of the crassamentum. Indeed, they rather exhibited an inverse proportion. The following table shows the proportions of solid matter in the serum and the crassamentum of the same respective specimens of blood:—

SOLID CONTENTS OF

Serum.	Crassament.	Serum.	Crassament.
39·5	207·2	43·0	130·9
33·7	204·6	38·0	167·7
35·1	146·2	47·8	134·3
46·0	117·3	34·2	181·9
33·4	141·3	26·4	165·8
20·9	231·8		

Standard of health. Serum 42·3. Crassamentum 118·3.

The apparent contrast between the solids of the serum and crassamentum is explained, when we remember that the proportion in the former is calculated not for 1000 of serum, but for 1000 of blood; and that when the serum had imperfectly exuded, the crassamentum would necessarily present a larger quantity of dry constituents, while the serum would have proportionally less.*

(c) The quantity of fibrine has been considered to bear a proportion to the acute character of the disease. *Dr. Whiting*, however, has found a great augmentation of this constituent even in purpura hæmorrhagica, a disease of extreme debility. There is, perhaps, a fallacy in the argument. The quantity of fibrine bears probably a relation rather to the extent and nature of disease, than to the state of the constitution. Where there is increased action without reduction of power, the fibrine, I believe, is not increased; but where these circumstances are conjoined, it is. Hence in second and third bleedings of the same patient, the blood frequently contains more fibrine than in the first. In a most severe and fatal case of pulmonic inflammation,† the proportion of fibrine was found to increase with the debility of the patient, and the advance of disease.

* The thickness of the blood has been repeatedly adverted to by old authors, but as their opinion was devoid of the basis of examination and experiment, and was connected, moreover, with other hypotheses equally unfounded, it has received little attention from the moderns. Was it from the influence of this notion that diluent drinks were once so liberally prescribed?

† Though unconnected with my subject, I beg to observe, that the distinctions between pleurisy and pneumonia are, *in a majority of cases*, neither practical nor useful, and that where these are considerable bronchitis rarely fails to accompany them. Pleurisy *sometimes*, indeed, exists alone, and produces extensive effects, without disease of the lung or air-tube, but I doubt the existence of pneumonia without pleuritis, and of extensive pulmonic inflammation without bronchitis. Auscultation gives no diagnostic mark of

An increase in the proportion of fibrine is, however, by no means marked or uniform, except in acute rheumatism and pleurisy. In these diseases we find it rise from the ordinary standard of 2·8 to 4·, 5, and sometimes almost to 6 parts in 1000 of blood: according to *Dr. Whiting*, it is even found as high as 9·7. I offer a list of specimens, having marked with an asterisk those taken from patients affected with the diseases alluded to.[a]

1·7	3·8	4·0	2·9
4·6	3·5	2·5*	2·4
3·6	2·8	1·9	2·9
4·6	3·1	1·8	5·5*
3·2*	3·3*	4·5*	4·3
3·4	3·8	3·8	3·7

I deduce one interesting observation from our comparison of analyses, viz. that in all cases in which the proportion of fibrine was considerably above the normal

the early stage of pleurisy, nor does practice, I think, require a distinction between this disease and pneumonia. The case alluded to (happily rare in its result) commenced with severe bronchitis, and the stethoscope soon detected pneumonia. From stethoscopic examinations we were able to prognosticate with precision the appearances found after death, the kind, the stage, and the seat of the several diseases. This knowledge had assured us of their extent and urgency, and caused prompt and energetic treatment; but this treatment applied, of course, to pulmonic inflammation, not to bronchitis, pneumonia, or pleurisy singly. I write this note for the young auscultator, who has failed in distinguishing complicated disease. Admirable as the work of Laennec is, and of the highest pathological and practical value, I venture to remark that he has refined too much.

[a] Mr. Thackrah had not fulfilled his intention of compiling either of the above tables: I have therefore arranged them from the records of his experiments. In one case of pleurisy, which he has not referred to, the proportions of fibrine and solid contents of serum are stated to have been 7·2 and 42·0; in another 7·25 and 49·8; and in a third 7·21 and 49·8, in 1000 parts of blood. Ed.

standard, the solid matter in the serum was below the average of health. This fact will be best exhibited in a table.

Fibrine.	Solid contents. of serum.	Fibrine.	Solid contents. of Serum.
4·6	33·7	4·0	33·4
4·6	21·1	4·5	14·0
3·4	33·9	3·8	38·8
3·8	35·1	5·5	26·4
3·5	24·7	4·3	40·2

Standard of health, fibrine 2·8. Solid contents of serum 42·3.

May we not suppose, from these experiments, that albumen is taken from the serum for the formation of fibrine? In buff-coated blood, where the fibrine is in larger proportion, we have found that the ratio of albumen and hæmatosine, though more abundant than in healthy blood, is less than in that of blood from inflammation, where no buff is presented. Thus if a patient be bled to-day for pneumonic inflammation, the blood without buff shall present albumen and hæmatosine to the amount of 220 in 1000 parts. Blood taken from the same patient for increasing inflammation will probably give the buff-coat, and with it albumen and hæmatosine only 180 in 1000 parts. Though the amount of increase in the fibrine in such a case will be much less than the loss of albumen, yet, supposing this latter substance to be the basis of the former, as we know not the proportions which might be requisite, it will not be considered a valid objection against the fact that an increase of fibrine is regularly accompanied by a loss of albumen in the serum. To this contrast of proportions I have not met with an exception.

That the blood in cholera is thicker than natural, has been shown by *Dr. O'Shaughnessy* and other writers,* but the general increase of solid matter in ordinary acute disease has not, so far as I am aware, been noticed by preceding authors, much less established by numerous and careful experiments.† This discovery appears to be of the greatest practical importance. The approaching restoration of humoral pathology to a place in the councils of medicine, demands a more accurate knowledge than is now generally possessed of the source of all secretions. If the state of the blood have a great,—nay, the greatest effect on the nature and character of urgent disease, and if in such cases the blood be found considerably thickened, or in other words to have lost a large quantity of water, one great object, it is evident, must be to supply fluid and restore the proportions of health. This, however, we shall discuss a few pages further on.

(D) Some observations made in 1825 led me to remark an increase of albumen in the serum of blood drawn in acute disease. *Dr. Traill*, of Liverpool, has published some statements of the same character.

* * * *

* MAJENDIE thought the blood of a mad-dog to be thickened; but I suspect the appearance he noticed should rather have been referred to impeded coagulation. He says, "je fis une saignée à l'animal, et je vis le sang si epais qu'il semblait être privé de sérum, du moins celui que je recueillis n'en donnait, pour ainsi dire, pas de trace une heure après sa coagulation."

† HEWSON believed the blood to be attenuated in inflammation.

(E) Peculiar substances have sometimes been found in blood drawn during disease. *M. Denis* has discovered chloresterine, and traces of silica, manganese, and alumine. *Signor Bizio* has described a substance under the name of "eritrogene," which he found in morbid blood. *Dr. Traill* detected oil in blood drawn during hepatic disease; and from a recent publication with which I have been favoured by *Dr. Paine*, of New York, it appears that *Dr. Gale* showed oily matter floating on the surface of cholera blood.

In reference to diabetes we have a negative observation to make. *MM. Vauquelin* and *Segalas D'Etchepare*, after a minute examination of the blood of a woman whose urine yielded much sugar, could not find the smallest particle of this substance in the blood. *Dr. Prout* also denies the existence of sugar in the blood of diabetes mellitus.

Urea has been repeatedly detected in the blood.

*   *   *   *

In conclusion, I will now discuss the principal pathological and therapeutic inferences that may be drawn from the foregoing chapters,—the points especially which bear on the practice of medicine and surgery.

What is "inflamed blood?" The phrase is constantly used by the profession, and the ideas connected with it influence practice to a powerful extent; yet the nature of the thing is unknown, and the application of the principle must obviously be uncertain and dangerous. Nay, I venture further to affirm, that in reference to acute internal diseases, three-fourths of the victims of mal-practice perish from the consequence of the usual ideas entertained of "inflamed blood." "Bleed daily as long as ever you see the blood inflamed," was the direction of a naval surgeon to his assistant. The order was strictly obeyed, "and thus," added this gentleman, "I sent many a brave fellow to a watery grave."

What, in a practical point of view, is "inflamed blood?" Is it that covered by a buffy and cupped surface? Certainly not: unless scurvy is inflammation; and debility, tonic disease. The fibrous tunic is known to present itself in cases of the greatest vital debility,—cases in which the loss of but a moderate quantity of blood would hurry the patient to his tomb; while it is often, nay commonly, absent in maladies, where severe inflammation is combined with the greatest constitutional strength.* In affections moreover, which require the repeated abstraction of blood, it has often been remarked, that the buff-coat has not appeared on the first or second bleeding, yet on the third or fourth it has been copiously exhibited. It has been shown also that the size of the stream, and the form of the vessel into which it is received, promote or prevent its appearance. What shall we say, then, of the doctrine of this tunic characterizing inflammatory action, and warranting the repeated abstraction of blood,—the doctrine, I mean, unlimited and unguarded? If the observation and the reasoning of the ablest experimentalists had a due effect in forming the opinions of medical practitioners and influencing their conduct, this dangerous opinion had been long since exploded. "As for those," says *Huxham*, "who will neither read nor reason, but practice by rote and prescribe at a venture, I must seriously advise them, at least, to peruse the sixth commandment."

What is inflamed blood? The preceding sections have, I trust, established several points which distinguish blood drawn during acute inflammation, both from blood which has been abstracted in health, and that which has been presented by diseases of debility. Briefly recapitulating those points, I would urge, that blood in acute inflammation, and generally in diseases of excitement, without marked prostration of strength, has three principal characteristics.

* De Haen, *cum multis aliis,* has pointedly made the observation, "Imo in morbis maxime inflammatoriis, in nullo sanguine, quoties cunque misso, aliquoties crusta ulla est."—*Ratio Medendi,* III. c. ii.

"Vidi, dum sanissimis hominibus vena secaretur quotannis verno tempore, sæpius crustam adesse. Imo in homine debili, cui ad hæmoptysin præcavendam, qua laboraverat, sanguis mittebatur singulis tribus mensibus, vidi semper crustam illam adesse. Adfuit ergo talis sanguinis diathesis, licet nulla inflammatio adesset. Et contra in validissimis inflammatoriis morbis aliquando nulla talis crusta in sanguine apparuit."—Van Swieten *in Aph. Boerh.* 384.

1. A greater duration of its fluidity after being taken from the vein.

2. A larger and denser crassamentum: and

3. The albumen uniformly and greatly increased, and the water consequently diminished.

The therapeutic inference is obvious and important. Our great aim in the treatment of inflammation should be to dilute the blood. For effecting this object two principal modes occur—bleeding, and the introduction of aqueous fluids into the circulation.

Bleeding is well-known to abate the action of the heart and arteries, to reduce the excitement of the nervous system, and, through these united agencies, to promote, to a certain extent, the secretions. But another and not less remarkable effect has been overlooked or unknown,—I mean the *dilution* of the blood which remains. That hæmorrhage increases the tenuity of the blood was shown in the tenth section of Chapter VII; and an explanation of the cause was there offered. I have here only to urge the recollection of the fact.*

* I have had occasion so frequently to mention BLEEDING, that I fear a junior reader might be inclined to look upon it almost as a catholicon. The most powerful of therapeutic agents, it nevertheless often requires the greatest caution, both in the degree and the mode of its employment. Profuse bleedings are rarely required in this country, perhaps in none. The works of the zealous and energetic DR. JACKSON exhibit an extraordinary system of depletion; in West Indian diseases, this "*heroic remedy*" being often carried to the extent of ℔iv of blood at one venæsection. How far peculiarities of disease produced by climate and the state of the soldiers' constitutions, might require this copious abstraction, I cannot determine; but in fevers of this country, a moderate bleeding, properly performed, effects all the good, and avoids all the danger, of a profuse one. "But," it may be asked, "what is the danger?" Not generally the immediate effects of syncope,—not death during the operation, though we have known this occasionally to happen: but an impaired state of the constitution long after the removal of the disease. The blood remains reduced in quality. The hæmatosine especially seems

The introduction of aqueous liquids into the circulation by diluting the blood, has a marked effect on the nervous system. The mad-dog almost immediately loses his violence and becomes prostrate in debility. And experi-

never to be fully regenerated. In several instances I have remarked for years the state of persons, particularly females, who have been profusely bled at short intervals for acute disease. They remain pallid for life, enfeebled, with respiration hurried on the slightest exertion; and often also with continual threatenings of an inflammatory attack, and begging for venæsection, not only as a preventive of such attack, but for the relief or removal of various distressing feelings. A very intelligent physician with whom I was intimate, and who conceived that the lancet was the only remedy to be relied on in the cure of urgent inflammation, had his daughter attacked with peritonitis. "Determined," as he afterwards told me, "to continue the bleeding till the pain subsided, we kept up the stream till she fell into complete syncope. We had then to make a pulse with brandy. On her rallying, the pain had not gone: we, therefore, took more blood; and at length she expressed relief, and is now slowly recovering." The recovery was indeed slow, and the change in the young lady's appearance was most marked. Her bloom was gone, never to return: and her health and strength were not restored several years after, and remain broken, I believe, to this day. Need I add that a modified treatment, as one moderate bleeding, and mercury to affect the mouth, would have subdued the peritoneal inflammation, with at least equal certainty, and without permanent injury to the health? There are peculiarities of constitution which ought powerfully to controul the amount of blood abstracted.

Nor is it only in venæsection that excess may be committed. Leeches are often applied in such numbers as to leave injury for life. "This child," a mother will remark, "always looks pale and ill. We had so many leeches applied when she was ill in the croup, some years ago, that she has never regained her colour, nor I think ever will. She is a poor puny ailing creature."—There is no comparison, I admit, between the evil of death by croup and permanent ill-health from its cure; but is croup cured by bleeding? Is it the only, or even the most powerful remedy?

In adults, also, it appears to me that leeches are sometimes excessively employed. For internal visceral inflammation, and even an inflammation of a joint, two or three dozen leeches are not uncommonly prescribed. Might not all the benefit be obtained by a much smaller amount of depletion, supported and succeeded by other measures? And does not this extensive draining of blood permanently reduce the vis vitæ?

On the mode of taking blood I will make a brief remark. Notwithstanding the recommendation given by some practical men that blood should be drawn off slowly, so as to abstract completely the pabulum of acute disease, I menters have noticed in various cases, both of health and disease, the prompt reduction of excitability and energy consequent on an extensive dilution of the blood.*

Fluids may be introduced into the circulation either directly or indirectly. The direct mode, that by injection into the veins, was discussed in a former section. In one disease only, as far as I am aware, has it been extensively tried with a curative view, and its effects on the whole have not been such as to recommend its use in other maladies.

decidedly adhere to the older recommendation of a full stream, (*Pemberton* says ℥viii should flow in three minutes,) the sitting or erect posture, and the consequent early induction of syncope. The state of the pulse, the countenance, and the skin must be diligently watched during the operation. A remarkable impression is sometimes made on the nervous system even by a small loss of blood, and then the continuance of the bleeding might most seriously affect the patient. On the other hand faintness occurring before a moderate quantity of blood is obtained, should not generally cause us to tie up the arm. A cordial and rest in a horizontal posture for a few minutes will restore the circulation, and enable us to obtain the blood we intended. Whatever may have been our previous idea of the quantity which the patient ought to lose, it should be subject to the alteration which circumstances during the operation may require. The reaction which ensues after copious venæsection, and the anomalous but distressing sensations which sometimes accompany it, and which require cordials or opiates, are subjects which the young operator should be acquainted with and remember. In doubtful cases I have often found great advantage from taking a small tea-cupful of blood in a full stream, and closing the orifice while I observed the period of its concretion. When this has been rapid I have immediately bound up the arm, when slow I have proceeded with depletion, and never, so far as I remember, with subsequent regret. In ordinary diseases it is seldom necessary or advisable to perform venæsection more than once. In cases of simple congestion, on the other hand, a sudden and full bleeding is ill borne, and two or three moderate abstractions of blood, in a stream comparatively small, will be found a more beneficial mode of treatment.

* "J'avais souvent observé dans mes expériences sur la pléthore aqueuse artificielle, que toutes les fonctions des animaux qui l'éprouvent, sont très sensiblement affaiblies, et particulièrement celles du système nerveux."—
MAJENDIE.

Majendie, from his experiments on another subject, inferred the extreme importance, when fluids are taken into the circulation, of having them thin and fine, "*tamise* par les agens d'absorption." No such filtration, I presume,—no such elaboration can be performed by art as that effected by the mouths of living vessels, and hence, perhaps, purest water injected directly into the circulation may act as a foreign body. Whether this idea is or is not worth notice, we cannot doubt that the indirect is the most natural and easy mode of exhibition.

The advantage of copious drinks in the treatment of acute disease seems to have been known to the older practitioners, though they were unacquainted with the principle, and knew nothing from experiment of the real state of the blood. They were probably influenced by careful observation of *juvantia* and *lædantia*. We find *Sydenham* commonly prescribing in acute diseases three pints or more of emulsion or ptisan, to be taken in twenty-four hours. The practice has since fallen into disuse. Diluents indeed are permitted, nay recommended in acute diseases; but no large quantity of drink is enjoined. Formerly, moreover, medicines were administered in very bulky, now they are given in very small and concentrated forms: and in this practice we have, of course, lost much of dilution.

With regard to chronic maladies, I have not facts and observations sufficient to warrant the application of the principle of dilution. In congestion and its effects, dilution seems calculated rather to injure than improve. Some disorders are certainly aggravated by copious drinks; as those of the stomach classed under the title of dyspepsia. In almost all these, diluents increase flatulence and oppression, and on the contrary a great reduction of the quantity of drink has a marked effect in restoring the vigour of digestion. Is it in such disorders that the absorbents of the stomach, in common with the other vessels of this organ, are so debilitated that they cannot take up, as in health, the fluid which we drink, and that this fluid is necessarily ejected into the intestines, there irritates the nerves, and thus occasions the formation of air?[α]

On the other hand, cutaneous eruptions are decidedly benefited by dilution. To this principle diet-drinks probably owe much of their efficacy; and the mineral waters, often of such marked power, though the quantity of their salts is insignificant, act chiefly by the quantity of water imbibed.[β]

α Is it not rather that when the functional powers of the stomach are weakened, a large quantity of liquid food more readily undergoes chemical changes, (attended with the formation of gas) which in health are prevented by the powers of life? Ed.

β For further copious details of alterations in the blood during disease, it is scarcely necessary to refer the reader to Dr. Copland's valuable *Dictionary of Practical Medicine*. Art. Blood. Vol. I. 180. Ed.

APPENDIX.

APPENDIX I.

FROM the paper of *M. Gaspard*, in *Majendie's Journal of Physiology*, are quoted his inferences on the introduction of pus into the circulation;[a] and in illustration, his sixth, seventh, and ninth experiments.

"1°. Le pus introduit dans les vaisseaux sanguins, à petite dose, peut y circuler sans causer la mort, pourvu qu'après avoir déterminé un trouble considérable des fonctions, il soit expulsé de l'économie, au moyen de quelque excrétion critique, surtout de l'urine ou des matières fécales.

"2°. Mais introduit plusieurs fois de suite, en petite quantité, chez le même animal, il finit par causer la mort;

"3°. A plus forte raison, il la détermine encore plus vite, quand il est injecté dans les veines, à une dose trop forte; et alors il cause diverses phlegmasies graves, des péripneumonie, cardite, dysenterie, etc.;

"4°. Il est susceptible d'être absorbé, quoique cependant il cause l'inflammation des membranes séreuses et du tissu cellulaire avec lesquels il se trouve en rapport;

[a] Referred to at page 175.

"5°. La plupart des symptômes qui s'observent dans toutes les fièvres lentes ou phthisiques, semblent pouvoir être rapportés à la présence du pus dans l'économie, puisque dans tous ces cas il y a toujours suppuration abondante et profonde, avec trouble général dés sécrétions. (1)" *page* 7.

"Exp. 6e. Le 21 Septembre, 1808, j'ai introduit dans les veines d'un autre chien de taille médiocre, mais robuste, une demi-once du même pus un peu vieux et plus putride que celui des experiences précédentes. Mais presque aussitôt après, l'animal éprouva, comme les autres, des vomissemens avec des efforts violens, qui ramenèrent même, à plusieurs reprises, des excrémens solides, et moulés, comme dans l'iléus; ensuite il offrit des symptômes nerveux effrayans: l'égarement de la vue, une sensibilité exaltée, des soubresauts involontaires de tout le corps, des accès convulsifs suivis d'abattement, hoquets, hurlemens douloureux et courts, démarche vacillante et sans but apparent, sorte de délire furieux, puis soif ardente, dyspnée, battemens du cœur secs, vibrants, et ressentis dans toute la poitrine, etc.; Cet état dura environ deux heures, et le chien expira dans les convulsions terribles, sans avoir éprouvé d'évacuations critiques. A l'*ouverture* du corps encore chaud, sang veineux très coagulable, ne laissant point séparer de sérosité par le repos; péricarde contenant un peu de liquide épanché; ventricule gauche du cœur très épaissé, enflammé, offrant à sa surface interne des taches de couleur de lie de vin,

"(1) Il me paraît très-probable que plusieurs autres fiévres lentés, telles que celles, qui ont lieu dans le cancer ulcéré, les hydropsies anciennes, le ptyalisme mercuriel, les maladies gangréneuses; comme encore dans l'ivresse, et aprés les exces de table, &c. ....; dépendent aussi de la présence de diverses substances étrangères résorbées dans le sang."

formées par une espèce de pellicule concrétée qui ne disparut qu'après des frottemens et de longues lotions; les autres organes out paru sains." * * *

"Exp. 7^{e}. Le Septembre, 1808, j'ai introduit, par la membrane séreuse du testicule, dans l'abdomen d'un petit chien, deux gros environ de pus, sans qu'il en soit résulté de douleur trés vive; mais ensuite, vomissemens avec efforts extrêmes; évacuation d'urine, fièvre, dyspnée; puis, au bout de trois heures, abdomen rénitent, rétracté et trés douloureux à la pression, comme dans la perétonite, et mort après de nouveaux vomissemens, douze heures après l'injection. A l'*ouverture* du corps, péritoine rougeâtre, un peu enflammé, et contenant plus d'une once de sérosité sanguinolente inodore; membrane muqueuse intestinale un peu rouge et enflammée."
* * * *

"Exp. 9^{e}. Le 28 Septembre, 1808, j'ai injecté du pus dans la plèvre gauche d'une petite chienne, et il s'en est suivi une gêne très douloureuse de la respiration, avec apparence de pleurésie. Cependant, vingt heures après, les symptômes étant bien moins intenses, et la mort ne paraissant pas devoir en résulter certainement, je tuai l'animal. A l'*ouverture*, je trouvai, également, dans la cavité des deux plèvres enflammées et recouvertes de flocons albumineux un liquide séro-sanguinolent inodore; les poumons étaient sains."

Tom. II. p. 4—6.

We also add a few remarks on the effects which an administration of certain poisonous substances by the stomach, induce on the blood.

The influence of *Opium* in full doses we have repeatedly endeavoured to ascertain; but our experiments have produced no very accordant or satisfactory result.

Exp. CCLXV.

Six grains of extract of opium were given to a whelp, and after some hours, as no evident effect had been produced, a dram of tincture of opium. The animal became stupid and cold, and in that state was bled from the aorta. Though the stream was full, concretion took place immediately. The colour of the blood was thought to be brighter than natural.

Exp. CCLXVI.

To a younger puppy were given six grains of extract of opium, which induced coma and clonic spasms. At the end of eight hours the effect did not appear to have diminished, and punctures were made in the femoral and carotid arteries, but no blood could be obtained from them. The animal was then pithed, the aorta immediately opened, and the blood flowed in a full bright stream. It concreted immediately.

These experiments tend to show that arterial blood of an animal under the influence of opium, concretes quickly, and is not darkened in colour. The state of debility which the narcotic produced readily accounts for the rapidity of coagulation.

Exp. CCLXVII.

A dram of powdered opium was given to a small bitch; and an hour after, when the animal appeared in a state of asphyxia, blood was taken from the jugular vein, but a full and free stream could not be obtained. The blood was very dark, and concreted in forty-five seconds. At the end of twenty-four hours the serum effused was in the proportion of only 10 to 44·1 crassamentum.

Exp. CCLXVIII.

To a dog was given half a dram of extract of opium, and in two hours the animal appearing to be completely under the influence of the narcotic, a wound was made in the neck, and one of the carotid arteries divided. The blood had its natural scarlet hue, and began to coagulate in two minutes. Thirty-five hours after, the proportions were found to be, serum 570, crassamentum 1176, or as 10 : 20·6.

The latter two experiments do not support the idea of concretion being rapid. Exp. CCLXVII., however, accords with the preceding ones in showing that the hue of arterial blood is not altered, while in that last-quoted (CCLXVIII) venous blood appeared darkened by the influence of opium. We have generally observed the effusion of serum in blood taken from an opiated animal, to be slow and imperfect.

Exp. CCLXIX.

A suicide, who had taken a quantity of crude opium, was bled six or seven hours afterwards. Blood from the jugular vein was stated by the operator to have concreted immediately. As the quantity obtained from this source was small, the temporal artery was opened. Its blood presented the hue of venous. Respiration, at this period, was defective, and attended by a peculiar sound. Subsequently blood was taken from the brachial vein : it did not concrete till the expiration of five or six minutes. In the effusion of serum nothing unusual was remarked.*

Opium, I conceive, will affect the character of the blood rather as it acts on the respiratory and nervous systems, than as it directly contaminates by absorption. Hence, perhaps, varied and apparently inconsistent experiments may be reconciled by a more careful regard to these systems and their contrasted states of excitement and exhaustion. The subject, however, remains for investigation.

On the effects of *Lead* introduced by the stomach, we have recorded two experiments.

Exp. CCLXX.

Twelve drams of carbonate of lead were given to a middle-sized dog. In an hour he vomited. He was then pithed, and blood immediately received from a vein in the abdominal integuments. It was observed that the blood did not begin to concrete for fifteen minutes, and was not a jelly till the expiration of twenty. Blood taken from the vena portæ began to concrete in twelve minutes, and was not firm till seventeen.

Exp. CCLXXI.

Twelve drams of carbonate of lead were given to a large dog. Two hours afterwards blood was taken from the jugular vein: It concreted in 1′ 10″. The animal was then pithed, and blood taken in a full stream from the vena portæ: It concreted immediately. The serum was slowly and imperfectly exuded.

* I relate the observations of this case, though I was not present at the time blood was abstracted, nor have I entire confidence in the accuracy of the details.

The former case shows a remarkable retardation of concretion, the latter no retardation. Our subsequent examination of the alimentary canal may perhaps explain the inconsistency. In the first dog the mucous membrane of the stomach was decidedly inflamed, and the intestines contracted and thickened; in the latter there was no more redness than is commonly found in the alimentary canal of these animals. I suppose, therefore, that the carbonate of lead, which is a very active poison, had produced little effect on the constitution of the second, while it had produced much on that of the first. I suspect, too, although our notes do not state it, that the dog experimented on in CCXLXX had been taking the mineral for some days previously. Further examination would *perhaps* show, that when lead fully affects the constitution, it produces such a change in the chemical relations of its fluids as to retard the coagulation of the blood.

II.

[The following experiments are selected from Mr. Thackrah's note-book, and though not quoted in the preceding work, appear to the Editor too valuable to remain unpublished.]

1.α

To ascertain the quantity of fluid in an animal, a living dog was weighed, and then bled to death. It was afterward skinned, and the carcase chopped into small pieces and washed till every appearance of blood was removed. All the fragments and the skin were next dried carefully on a sand-bath, and when the moisture had entirely evaporated, the following proportions were ascertained.

378·4	Solid.
621·6	Fluid.
1000·0	

2.β

Blood was received as it flowed (principally in drops) from the neck of a fowl, and after it had stood sixty hours the serum was poured off from the crassamentum, which was then broken up and allowed to remain at rest for twenty-four hours more, when the serum which had exuded was again poured off. On evaporating these two specimens of serum in a sand-bath, their solid and fluid contents were found to be

	First exudation.	Second.
Solid	112·2	155·5
Water	887·8	844·5
Serum	1000·0	1000·0

The serum in both instances was nearly pure.

α Referred to at page 30. β See page 41.

3.α

To ascertain if exposure to oxygen gas changes the colour of fibrine, some of this substance, obtained from the crassamentum of sheep's blood, and washed in distilled water till it was perfectly white, was placed in a gallipot on a pneumatic trough, under a bell-glass, which had a stop-cock at its upper extremity. A stream of oxygen gas was passed under the bell-glass and allowed to escape by the stop-cock for fifteen or twenty minutes. The cock was then shut and the fibrine allowed to remain in an atmosphere of oxygen gas during twenty-four hours, after which the gas was expelled and replaced by a fresh supply in a similar manner. At the expiration of forty-eight hours the colour of the fibrine was not altered, except in having become perhaps a shade darker than at first. Still, however, it was white.

4.β

Blood was received from the jugular vein of a dog into a bottle, which was corked, allowing a small bubble of air to remain included with the blood. After coagulation, that surface of the crassamentum which had been in contact with the air was of a florid colour; the rest was of a dull red hue.

5.

To corroborate the previous experiment, blood was taken from the jugular vein of another dog in a similar manner, and a larger bubble of air allowed to remain. The bottle was then corked and placed in a horizontal position, exposing an oval surface of blood to the air. After twelve hours it was examined. The serum had separated from the crassamentum, and on the latter there

α See page 46.

β Experiments 4 to 9 alluded to at page 48; see also Nos. 12 and 43 to 50.

was an oval patch of the same form as the air-bubble, more florid in colour, but in no other respect differing from the even surface of the cylindrical mass.

6.

The jugular vein of a dog was opened, and blood received into a gallipot which was allowed to remain at rest for twenty-four hours. At that period the serum had exuded, and the upper surface of the crassamentum was of a bright vermilion hue. The crassamentum was then taken out and placed in another vessel with the opposite surface upwards: in twenty-fours after no change of colour could be observed.

7.

Blood was received from the neck of a calf into a gallipot, and as soon as it concreted the clot was turned with its convex surface upwards. In twenty-four hours the upper surface was scarlet, the under a dull purple colour. The position of the crassamentum was then again reversed, but no change of colour took place in either.

The blood of a patient was also treated in a similar way, and with the same result.

8.

So soon as blood taken from the arm of a patient had concreted, the clot was placed on a filter. Thirty-six hours afterwards, the whole of the mass was of a dull purple colour, and on examining all its surfaces no appearance of scarlet could be detected.

9.

Air was blown through blood contained in a gallipot as soon as it had been drawn from the neck of a sheep, and the process continued until the blood had coagulated. The crassamentum was then cut into, and was of a much brighter colour throughout than blood taken at the same time into other vessels. It appeared also to concrete more quickly.

10.[α]

A plethoric young man was bled in the arm on account of a blow on his head. The pulsation of the carotids was full and violent. Six ounces of blood was received into a gallipot; then a specific gravity bottle filled; and afterwards another six-ounce gallipot. The stream was not large, but flowed regularly throughout the eight or ten minutes required for filling the vessels. After thirty-six hours the blood was analysed, and found to contain as follows:—

	First-drawn.	Last-drawn.
Solid matter	241·2	231·5
Water	758·8	768·5
Blood	1000·0	1000·0

11.

Blood was received from the arm of a patient into four gallipots successively. The first and last drawn were placed on a sand-bath that the proportion of water in each might be ascertained.

	First.	Last.
Solid matter	233·2	236·4
Water	766·8	763·6
Blood	1000·0	1000·0

12.[β]

Blood was drawn from the neck of a stuck calf, and as soon as the clot had formed it was turned, leaving the

α See page 142.

β Referred to at page 47. See also experiments 4 to 9.

convex surface upwards. In twenty-four hours the upper surface was a bright scarlet colour, the under a dull purple.

13.α

Blood from the neck of a fowl was examined sixty hours after being drawn. The serum was poured off; the crassamentum broken up; and after twenty-four hours more, the liquid again poured off. The proportions were ascertained to be,—

			Fibrine	2·8
Serum	432·7	or 10 : 13·1	Dry contents in serum	53·2
Crassamentum	567·3		Hæmatosine & alb. ...	82·4
			Water	861·6
	1000·0			1000·0

14 to 21.β

Experiments on the specific gravity of the blood of animals.

Oxen, ...	1015·9	.. 1053·4	... 1028·5	... 1043·8
Sheep, ...	1023·1	... 1054·3		
Duck, ...	1109·8			
Hog, ...	1061·0			

22 to 25.

Solid contents left by evaporating the blood of animals.

Horse	19·3 per cent.
Oxen........................	18·6 ... 18·2
Sheep	18·6

α See page 154. β Referred to at page 156.

26 to 41.α

Temperature of concretion in the serum of oxen and sheep.

Oxen.			Sheep.		
160°	157°	148°	142°	148°	140°
158°	156°	154°	140°	144°	
155°	148°	140°	142°	150°	

In several other animals this point was regarded, but as the temperature was found to be generally 150°—160°, no record of the experiments was preserved.

42.β

Analysis of the blood of a sheep.

Fibrine	2·6
Dry contents of serum Hæmatosine and albumen	196·6
Water	800·8
	1000·0

*Effects of different substances on the colour of the clot.*γ

43.

Blood was received from the arm of a patient into an equal quantity of milk. In twenty-four hours the under surface of the crassamentum was a dull red colour, and the upper a beautiful scarlet, but paler than that of blood which had not been diluted with milk. The clot was larger than usual.

44.

A similar experiment to the last. The upper surface of the crassamentum was pink, and the under a dull purple.

α See page 156. β See page 160. γ See also experiments 4 to 9.

45.

Blood flowed from the arm of a patient into a vessel containing urine. It did not appear to coagulate. In a few minutes the upper surface of the clot appeared of a bright vermilion hue. On being stirred it became dark, but after standing for a few minutes more, the scarlet colour returned, and again disappeared on the blood being stirred. This was often repeated at intervals, with the same results. It was then put aside for thirty-six hours. At the expiration of that period a clot had formed at the bottom of the gallipot, with the fluid above it perfectly clear, so that the bright vermilion hue of the crassamentum was distinctly visible. On removing the clot it was found to be exceedingly soft. No part except the surface was of a scarlet colour; the rest was of the usual dark purple.

The urine was tested previous to the abstraction of blood and found to be strongly alkaline.

46.

A small portion of crassamentum was dissolved in an ounce of distilled water, and a stream of hydrogen gas passed through the solution for four hours. No alteration in colour took place.

47.

A piece of crassamentum was mixed with about forty times its weight of water, and carbonic acid gas passed through it for fifteen minutes. The colour became slightly darker.

48.

A similar experiment with chlorine gas. The solution became gradually of a light brown colour. After standing for thirty-six hours a light straw-coloured sediment was deposited, resembling albumen coagulated by nitric acid. The fluid still retained its slight brown hue.

49.

A portion of crassamentum was mixed with a solution of two parts of sulphuric acid in eight parts of water. Heat was applied to 160°, and continued for six hours. The liquid was then filtered. The solid mass when dried was a dark brown or black substance, similar to that from the washings of unadulterated crassamentum.[a]

50.

Blood was received from the arm of a patient into a gallipot containing white of egg: it fell to the bottom of the vessel. Eighteen hours afterwards the colour of the blood appeared darker than usual: small streaks, however, which were observed passing through the white of egg, were of a brighter hue. A clot had formed at the bottom of the gallipot which was uniform in colour throughout, except where one or two patches appeared, resembling buff-coat, and at these points the crassamentum firmly adhered to the white of egg. The serum floated on the surface of the latter—it was not milky. Blood drawn from the arm of the same patient into another vessel was not buffed, nor the serum milky.

Effects of some substances on coagulation.

51.

Blood from the neck of a calf was received in a gallipot and placed in a jar of carbonic acid gas. Temperature of the blood 90°. It was fully coagulated in three minutes.

[a] For copious details of the effects of various acid and other substances when mixed with blood, see Dr. Burrows' *Gulstonian Lectures* in *Medical Gazette*, July 12, 1834. p. 502. Ed.

52.

Blood drawn from the arm of a patient into a bottle was placed on a mercurial trough. It coagulated in seven minutes. The bubble of air which was in the bottle did not appear to be at all enlarged after coagulation. Blood received at the same time into a gallipot coagulated in eight minutes.

53.

Blood was received from the arm of a female into a vessel containing a similar quantity of a solution of muriate of soda. It did not appear to coagulate, but on pouring off the liquid a few hours afterwards, a sediment was found at the bottom of the vessel. This was placed on a filter, and in two or three days traces of fibrine only could be detected.

54.

Liquor potassæ was poured on blood taken from the arm of a patient. It became of a black colour, but did not coagulate. On subsequent dilution with water, very little solid matter could be obtained by the filter.

55.

Blood was received into a gallipot about one-third filled with tincture of digitalis. It did not appear to concrete ; but on stirring the mixture twenty-four hours after with a glass rod, a loose clot had formed. Four days after there seemed to be no disposition to putrefy. The mixture was diluted with three times its bulk of water, and on passing it through a filter, portions of loose black coagulum were retained. A week afterwards the fluid part was dark but inodorous.

56.

A similar experiment with equal parts of magnesia and water, instead of tincture of digitalis. The blood did not appear to coagulate, and no clot was observed twenty-four hours after being drawn. On filtering, a substance was obtained resembling magnesia coloured by blood and mingled with very minute portions of cruor. The fluid, after being kept a week, gave out an unpleasant acidulous smell.

57.

Blood from the arm of a patient flowed into a gallipot, which was immersed in a freezing mixture; and a thermometer placed in the blood, showed the temperature to be 30°. After half an hour, the thermometer still at 30°, the blood had not coagulated. It was then removed into a room at 57°, and in thirty-six hours it had separated into serum and crassamentum; the former was clear, the latter very slightly buffed.

A vessel of a similar size was filled at the same time, and remained at a temperature of 57°: it concreted in six minutes. After thirty-six hours, the contents of both vessels were weighed, and the proportions found as follows:—

	Blood which had been cooled.	Blood at 57°.
Serum	371·9	363·6
Crassamentum	628·1	636·4
	1000·0	1000·0

58.

Two vessels were filled with blood from the jugular vein of a dog; the wires of a briskly-acting galvanic trough were introduced into one, and delicate thermometers placed in both. No difference could be observed in the fall of temperature of each. The galvanized blood did not coagulate.

Blood was then taken from the femoral artery of the same animal, and treated in a similar manner, with a like result.

The experiment was again repeated both with venous and arterial blood, but no difference was observed except that a small clot had formed on the ends of the wires.

Twenty-four hours afterwards, the blood through which the galvanic fluid had passed, had in no instance coagulated, but still remained a fluid mass.

Miscellaneous experiments on the blood.

59.

A graduated glass tube containing rather more than three and a half cubic inches was filled with carbonic acid gas over water, excepting a few lines of water which were allowed to remain in the tube. The end was then closed and the tube shaken. After a partial vacuum had formed by absorption of the gas in the water, the lower end of the tube was opened in a vessel containing serum from the blood of a sheep, which instantly rose in the tube. It was again agitated, and again immersed in serum, for several times in succession, until the serum had absorbed an inch of carbonic acid gas.

60.

An inch and a quarter of thick stale washings of crassamentum were shaken in a glass tube with two inches and a quarter of nitrogen gas. There was no absorption of the gas, nor any change of colour in the liquid.

61.

A similar experiment with atmospheric air, and a similar result.

62.

A little of the dried washings of crassamentum was mixed with two ounces of water, and a small piece of potassa fusa. When heated over a spirit-lamp great part of the solid matter became dissolved, and the whole was much softened. A portion of the fluid was further diluted with water, and on addition of nitric acid the albumen was immediately precipitated.

63.

Washings of crassamentum were reduced to the same specific gravity as serum containing red particles, although the washings were much darker in colour. On the addition of nitric acid to both, the crassamentum afforded a larger quantity of precipitate than the serum.

64.

Immediately after a rabbit had been struck on the neck blood was taken from the left side of the heart, from the vena cava, and the vena portæ. When examined through a powerful microscope the globules were thought to be smaller in the arterial blood than in that from the veins.

III.

[The following observations occur in an unconnected state, among Mr. Thackrah's MSS. They are evidently minutes which he had made for a second preface or preliminary discourse to the enlarged edition he was preparing, of this his favourite work. It would be injustice to suppress any of his remarks, and I give these in the detached form in which they present themselves among my friend's papers. Ed.]

The first edition of this inquiry, published in 1819, has been out of print for the last ten years. The delay of the present has resulted from my anxiety to improve, correct, and enlarge; but the pressure of other engagements had allowed me few and short intervals for investigations I would make.

The work, imperfect as it is, has been one of great labour and of much thought. The experiments are much more numerous than appear in the text. Many not printed have been made to prove a fact which occupies the shortest paragraph; many have failed from accidents; and many are not recorded from their unsatisfactory or negative results. In the execution of these experiments I owe very much to the intelligence, attention, and zeal of my pupils, Messrs. Breary, Corsellis, Whytehead, Dobson,

*   *   *   *

1. We have operated on the largest quantity of blood we could conveniently procure.

2. This blood has been taken when a full stream has been established.

3. We have had reference to vessels, temperature, &c.

4. We have especially avoided, on making or recording an experiment, any reference to the result expected: and in many, perhaps in most cases, we have previously entertained no opinion on the subject. When a subject has been started for examination, the plan of inquiry has been written in full; the points of particular importance stated; and then a considerable number of experiments made according to this plan, and carefully recorded, without an examination of the bearings of each.—Thus to determine the effects of plethora and inanition on the character of blood, experiments were made and successively recorded, before any examination of their bearings or any deductions formed. *Then* the points of agreement and disagreement were noted, and points on which suspicion of inaccuracy was entertained, re-examined, and doubtful points made the subject of new and varied experiments. The results of many of our inquiries are exactly what the physiologist would expect; I have, nevertheless, thought them worthy of record. Where facts and principles are daily contested, and when there appears a disposition in some men to object for sake of objection, or to obtain ephemeral distinction by impugning the doctrines which great research has established, additional proofs, even of acknowledged facts and principles, will scarcely be considered superfluous.

Two things are to be avoided in inquiries like the present. 1st. The admission of facts and principles, which, though generally admitted, have not been based on observations sufficiently numerous and careful.

2nd. Rejection of facts and principles because they are believed. An effort to attract notoriety by attacking the acknowledged doctrines of physiology, and by coupling the name of the aspirant with that of him whose researches have raised him to eminence

* * * *

The experiments which appear on the face of this work are by no means all that have been performed. The experiments recorded in our notes, and generally minutely recorded, besides the cases of failure of which no note was preserved, amount to [nearly four hundred, of which more than three hundred and seventy are quoted in this volume. Ed.] The details of all these might make a book, but would weary rather than instruct or interest the reader.

* * * *

Much I wished to do more, and much I wished to do better, but the broken health which obliges me to abandon my obliges me also to shorten my inquiries.

* * * *

[With this affecting broken sentence,—the latest probably which fell from Mr. Thackrah's pen,—the editor closes his labours.]

EDWARD BAINES AND SON, PRINTERS, LEEDS.