

Appendix to Dr. Knox's anatomical and physiological memoirs / [R. Knox].

Contributors

Knox, Robert, 1791-1862.
University of Glasgow. Library

Publication/Creation

[Edinburgh] : [Printed by Andrew Shortrede], [1837]

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APPENDIX

TO

DR KNOX'S

ANATOMICAL AND PHYSIOLOGICAL
MEMOIRS.

CONTENTS:

I. OBSERVATIONS ON THE TÆNIA SOLIUM.

II. PHYSIOLOGICAL INQUIRY INTO THE NATURAL HISTORY
OF THE HUMAN PULSE.

APPENDIX

DR. KNOWLTON

RESEARCHES AND OBSERVATIONS

ON THE

RESEARCHES

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As the two following Papers, and more especially the second, have been so frequently referred to in my Anatomical and Physiological Memoirs, I have thought it might be convenient for the reader to have them printed as an Appendix to No. 1st. They originally appeared in the *Edinburgh Medical and Surgical Journal*; the memoir on the Tænia in 1821, and that on the Differential Pulse and its diurnal revolution in 1815.

ROBERT KNOX.

May 1, 1837.

THE HISTORY OF THE
ROYAL SOCIETY OF LONDON
FROM THE YEAR 1660 TO 1700

The history of the Royal Society of London, from the year 1660 to 1700, is a subject of great interest and importance. It is a subject which has attracted the attention of many of the most distinguished historians and philosophers of the age. The history of the Royal Society is a history of the progress of human knowledge, and of the triumph of reason over superstition and ignorance. It is a history which has been the subject of many of the most valuable and interesting works of the age. The history of the Royal Society is a history of the progress of human knowledge, and of the triumph of reason over superstition and ignorance. It is a history which has been the subject of many of the most valuable and interesting works of the age.

OBSERVATIONS
ON THE
TÆNIA SOLIUM;
AND ON ITS
REMOVAL FROM THE HUMAN INTESTINAL CANAL
BY SPIRITS OF TURPENTINE.

SINCE the publication of the learned and elaborate work of Rudolphi on intestinal and other worms,* the numerous disputed points relative to the origin, growth, and propagation of these, seem to have been tacitly given up by medical men in favour of the opinions of that learned and ingenious author; or the inquiry, perhaps, has, from its abstruseness, been deemed beyond the present bounds of science. To the admirers of natural history, it would be superfluous to offer any apology for these brief remarks on one species of intestinal worm; and to the practical medical man, few inquiries can be more interesting, as its aim is to elucidate the origin and treatment of an exceedingly obscure and untractable complaint.

It has happened to me to witness the disease of *tape-worm* on an extensive scale, and even, to a certain extent, to experiment on its nature. The observations I have made seem to throw some additional light on this very obscure subject. Should it appear differently to the readers of your widely circulated Journal, I shall still feel satisfied in having performed my duty to science, by submitting these cases and remarks to the public.

The colony of the Cape of Good Hope is remarkable for the general salubrity of its climate. It enjoys, indeed, almost an immunity from the host of diseases to which, in other countries, mankind is exposed; to this there are a few exceptions, amongst

* Entozoorum, sive Vermium Intestinalium Historia Naturalis. Auctore C. A. Rudolphi.

which may be reckoned the occasional prevalence of *tape-worm*. It was long ago remarked by Dr Sparrman, that the colonists, or more particularly those inhabiting the banks of the great Fish River, "had very little knowledge of one of the most frequent and most troublesome disorders to which they were subject, and that was worms."* He observes, moreover, "that adults, and even elderly persons, seemed more generally troubled with the complaint than children."

This so great frequency of the disease amongst the colonists I have never observed, nor was my attention directed towards this subject, previous to October, 1819, when *tape-worm* became so general amongst the troops as to resemble an epidemic. The greater part of these men had just returned from a short though active campaign of a few weeks; they had been exposed to some rainy weather, and had remained for a short time encamped on the open field; yet these circumstances were productive of no sickness, nor did any ailment occur, excepting the singular prevalence of intestinal worms, and more particularly of *Tænia Solium*.

There are a few general observations which it may be useful to mention, previous to entering on the more detailed account of cases and remarks. The disease was not confined to any particular class or rank; intestinal worms, of one or other species, attacked as well those enjoying every possible comfort, as those exposed to all privations; they spared neither the young nor old, temperate nor debauched, male nor female.

Of the troops employed on the eastern bank of the great Fish River, the proportion of those who became affected with worms, to those who escaped this troublesome complaint, appeared to me about two to five; the *ratio* of those who remained in the colony differed from the preceding, and probably did not exceed one to four. The *Tænia Solium* and other worms, more especially *Ascarides lumbricoides*, appeared, *for the first time*, in most of the individuals attacked; they were almost all, without exception, healthy young men, accustomed to much exercise, constant discipline, and regular habits. The greater number of the individuals affected could not remember ever having passed worms, even from their earliest infancy to the period of their attack in 1819. Intestinal worms were passed at this time by men who had lived several years in the colony, and who had not previously been afflicted with them. In one case the gentleman had been *nine* years in the colony before the attack of the disease.

* Sparrman's Travels, vol. ii. p. 127.

CASES AND OBSERVATIONS.

CASE I. — Of a detachment consisting of about 86 men, 38 were found, on inquiry, to be affected with worms; of this number, two had the *Ascarides lumbricoides*, and the remainder *Tænia Solium*. The detachment was composed of young healthy men, previous to the appearance of the worms. Those affected became, generally speaking, emaciated and weak, low-spirited, pale, and unhealthy. They laboured under other symptoms, which will be more particularly described in a future case. The cure of all who chose to adopt the means was easily effected by small doses of the spirits of turpentine, after the failure of purgatives and various other remedies.

CASE II. — One of the above mentioned detachment, whose name I think was *Gardiner*, had resorted to a variety of methods in order to rid himself of so troublesome a complaint, but ineffectually. He had restricted himself entirely to a milk diet for about three weeks, with the effect of arresting the growth of the *tænia*, as few or none of the detached joints appeared in his stools during the continuance of this regimen; they reappeared immediately on his returning to the use of animal food. The spirits of turpentine were prescribed for him, in the dose of two drachms morning and evening. He, almost immediately after, voided by stool a portion of a *tænia*, consisting of about 240 distinct joints, each about three-fourths of an inch in length; the unpleasant harassing symptoms which generally accompany the presence of *tænia* in the human body immediately left him, and did not return.

CASE III. — A middle-aged man (a half-pay officer) had been affected with *tænia* for eight years, during which long period, an incredible quantity of drugs had been prescribed for the complaint, and taken without any permanent advantage. He had been under the care of several medical men, and his constitution was greatly impaired. During these eight years, he had been in the constant habit of passing detached joints of the *Tænia Solium* by stool, in great quantities, and his existence had become, in consequence of this and other complaints, truly miserable. Two drachms of spirits of turpentine in a little water were taken twice a-day, for four successive days. He now passed the *tænia* in large connected masses; the symptoms of disease disappeared, and his health returned.

CASE IV. — In this case, also an officer, the *tænia* appeared suddenly, whilst the individual enjoyed perfect health. He had never passed any worms previous to the present attack. The symptoms, as in most others, were acute headach, dyspnœa, fetid breath, grinding of the teeth, constant itching of the

nostrils, stiffness of the lower jaw, sensation of pain in the temporal muscles, increased flow of saliva, sickness and pain of stomach, pains in the chest and bowels, slight tenesmus, paleness, general debility, emaciation. The complaint was observed to be much aggravated by the use of animal food, and a vegetable diet was found effectual in repressing the growth of the *tænia*, but did not succeed in removing the disease. A powerful dose of jalap and calomel was prescribed, with the view of expelling, at least, the detached joints of the *tænia*, if not the worm itself. It proved useless in either way, for it neither expelled the worm entire, nor any of its detached joints. The cure was finally effected by the spirits of turpentine.

It would be tedious to enumerate more of the cases which occurred, as they bear to each other so close a resemblance; but there are circumstances regarding the action of the remedy employed, and the exciting cause of the complaint, which merit minute discussion.

It has been generally allowed by medical writers, that there is scarcely any part of medical science more obscure than the origin of intestinal worms. The works of the ancients contain no accurate observations on this interesting point, and the labours of later authors tended only for some time to involve the subject more and more in obscurity and error. Much was to be expected from the exertions of the natural historian, and from the aid of the microscope in the hands of men of science, accustomed to habits of correct observation, and to the impartial investigation of nature. This class of men *has* done much towards elucidating the nature of intestinal worms, or, speaking more generally, of *entozoa*, the name by which worms living within other animal bodies are distinguished. But great difficulties remain to be overcome, and the opinions maintained by the most accurate writer on this subject, (Rudolphi,) appear to me quite untenable.

A simple enumeration of the various causes which have been assigned for the production of intestinal and other worms, would convince an impartial reader, that such were inadequate to produce the effect, and were supported by men who did not give credit to their own doctrines. The extreme difficulty attending such investigations will be made manifest by remarking, that the so justly celebrated Linné entertained notions regarding the origin, habits, and growth of *tænia*, which Pallas and Rudolphi have since proved to be entirely erroneous. It is unnecessary here to discuss the opinions of a host of writers so ably combated and refuted by these authors. It seems preferable rather to inquire into the causes which gave rise to

intestinal worms, in so great a number of otherwise healthy individuals, — an inquiry fraught with interest to the medical practitioner ; since next to the removing disease when present, the most important subject for investigation is its exciting cause.

The best authors have remarked, that, for the production of intestinal worms, a certain predisposition of the body is necessary, which foreign writers call a "*diathesis verminosa*," and sometimes a "*dispositio verminosa corporis*." This they describe as originating in scanty, unwholesome vegetable diet, and hence the frequency of intestinal worms amongst the poor ; but they also assert, that debility, from whatever cause arising, may give origin to the same complaints. In confirmation of these opinions, they remark, that the inhabitants of low marshy countries, as Holland, are more subject than others to intestinal worms. All preceding theories having been disproved by minute microscopical inquiry, naturalists have admitted the preceding statements, and explained the generation of worms in the human body, by a process totally dissimilar to what is known to take place in the production of all other animals, and somewhat resembling the growth of mushrooms, from a *compost* employed for that express purpose. Nor do the careful expressions of Rudolphi diminish the difficulty, or explain away the absurdities attending the admission of such an *hypothesis*. "Organic particles," he observes, "are required for the production of *entozoa*, in the same manner as we find the classes of animals called *infusoria*, and *phytozoa*, and some *fungi*, are produced." In parts, he farther observes, which have been organized, (*partibus organicis*,) as well animal as vegetable, and which have been macerated in water for some time, innumerable animalcules called *infusoria* are known to arise, and to be produced by a process not unlike putrefaction.

It is with diffidence that I offer, in opposition to the opinions of such accurate observers of nature, the following objections : Debility presents itself to the medical practitioner daily in a hundred forms, without being accompanied by intestinal or other worms. On the contrary, these appear in the strongest constitutions, particularly exemplified by the cases detailed at the commencement of these remarks. The individuals alluded to were, with few exceptions, young, healthy, active men. The climate of the Cape is remarkably dry, forming, as it were, the counterpart of Holland ; and Hasselquist* says of Egypt, that the inhabitants are much afflicted with *tape-worm*, a practitioner in Cairo assuring him, that two-thirds of the inhabitants of that

* Hasselquist's Voyages and Travels, p. 386.

city were afflicted with them. Moisture, therefore, can have little effect in the production of *tænia*. Neither can a vegetable diet be blamed for its production, since many months had elapsed during which vegetables, bread excepted, formed no part of the diet of any of the individuals alluded to in the preceding cases.

As *tænia* and other worms are occasionally found in fish, some writers have asserted that they are thus transferred to the human frame. The observations of naturalists have shewn, that the *tæniæ* observed in fish and man differ essentially; and in addition, it may be remarked, that of the hundreds whom I have known to be affected with *tænia*, none had tasted fish for the period of at least six months previous to the appearance of the worms, and many not for years. It remains, therefore, to be explained, how so many, young, healthy, active men, became suddenly affected with *tænia*, for assuredly no theory hitherto offered on the subject of intestinal worms throws any light on their origin in the present instance. From the disturbance worms generally create in the human frame, we should be inclined to adopt the conclusion, very different from that of preceding writers, and forming almost an exact counterpart, namely, "That intestinal worms are not natural to man." This opinion is confirmed by the constant efforts nature makes to throw off the disease, — efforts more or less obvious in different individuals, but perhaps existing in all. The *Filaria medinensis* has been classed with the *entozoa*, though several writers allow that it naturally exists in the wells and rivers of warm countries.

Intestinal worms are introduced into the human frame mixed with the food or drink, and, finding in the intestines an apt place for their existence, they there live and thrive; but they do not arise from a weak and injured digestion, and a consequent combination of matter capable of converting itself into intestinal worms.* Were we satisfied with such explanations, the origin of man, and all other animals, would be readily accounted for, from "a peculiar arrangement and combination of organic molecules placed under peculiar circumstances;" the story of the serpent *Python* would be no longer a fable.

Perhaps the strongest objection hitherto offered to the opinion, that *entozoa* are not generated within the bodies of man and other animals, but are transferred to them through the medium of the *ingesta*, arises from the fact, well ascertained, that intestinal and other worms have nowhere been found excepting within the *viscera* of other animals. But to this it

* "Dissimilatio vermes gignit.

"Vermes ex dissimilationis processu in corpore animali debilitato
obtinente oriri statuit "

RUDOLPH.

may be observed, that the repeated errors committed by microscopical observers have brought this mode of observing natural phenomena into a certain degree of disrepute. The indefinite nature of the theory supported by late writers, that *entozoa* are generated within the *viscera* of man by a process our food undergoes when not properly digested, has retarded the advancement of knowledge relative to these animals. It has done worse : it has prevented minute inquiry into the immediate exciting cause of intestinal worms, into the particular article of food which more immediately produces them ; and it has spread widely a few vague notions, that unwholesome coarse food, whether animal or vegetable, when introduced into a weakened stomach, will at all times give rise to the disease of worms. It seems of importance, therefore, to reduce the subject to a few determinate points, and, by subjecting the inquiry to a stricter analysis, enable succeeding observers readily to prove or confute the existing theory.

It may, I think, be admitted, that intestinal worms (or the matter capable of producing them) can be introduced into the *viscera* of man and other animals only with the food or drink. That they do not arise from the latter, may be conjectured from the following fact, that individuals using, for a series of years, water drawn from the same sources, have not been afflicted with *tænia* or other worms ; nor did these appear until other causes, to which, with more probability, they might be referred, were also present.

The cases of *tænia* detailed in the commencement of this paper, occurred in a body of active healthy young men, subjected to the same discipline and privations, and using, with few or no exceptions, the same sort of food and drink. These circumstances merit peculiarly the notice of the professional reader, as it seldom happens that an experiment (for such it seems to me) can be made so extensively in civil life, where the habits of no two individuals resemble each other, — where the food and exercise of the sick are submitted to no precise rules, — and the dissimilarity in age, constitution, and previous habits of life, renders all general conclusions necessarily incorrect.

In men situated as our troops were in this short campaign, we must look to some general exciting cause of disease, the explanations hitherto offered of individual cases being totally inapplicable. The food of these men (for to it we must ultimately refer the disease) consisted of unwholesome meat, the flesh of overdriven, starved, and unhealthy oxen, reduced often to a mere skeleton, by fatigue, want of food, and exposure to the cold rains, which are observed to sicken and destroy most of the horned cattle and sheep which have not preserved their

condition throughout the winter. The use of this sort of food, from peculiar circumstances, was unavoidable. I shall not here dispute the point, whether the *tæniæ* were transferred, as completely formed animals, from the unwholesome and *diseased* meat, to the stomach and intestines of man, or are there generated; it is sufficient to have established the point, that they arise from the use of unwholesome animal food,—from the flesh of animals which have been diseased.

I have avoided, as much as possible, detailing any minute facts regarding the natural history of *tænia*, and other intestinal worms, supposing most practitioners in medicine to be acquainted with the leading *phenomena* of these animals. The following brief remarks I therefore submit to the naturalist, and claim his indulgence for any trifling error which may occur.

Various ridiculous opinions were formerly entertained regarding that variety of *tænia* called *Solium*, most of which have been successfully combated by the ingenious and accurate Rudolphi. It is now known that four individuals of the species *Tænia Solium* have been discharged by the same patient; hence the obvious impropriety of the name. The theory formerly upheld, regarding the indefinite growth of joints to supply the place of those detached from the body of the *tænia*, and passed by stool, has been rendered exceedingly doubtful. The *Tænia Solium* seems to be regenerated rather by an evolution of joints already existing, or by the production of new joints near that extremity of the worm where naturalists have proved the head to be placed. It is probable, therefore, that Rudolphi may be correct, when he states, that the joints of the *Tænia Solium* do not increase in number, but in length; or we may assent to the opinion of Pallas, who seems to think that there may be a reproduction of joints towards the head extremity of the animal.*

It is no longer disputed by naturalists, that in the small perfect *Tænia Solium* there are constantly found a distinct head and tail joint, which is not perforated by any vessel or tube; but as the animal, when suffered to come to maturity, secrete joints in great numbers, which pass off with the *feces*, it is obvious, should the observations of naturalists be correct, that the tail joint must *first* be detached, and unless regenerated at a future period, ought never afterwards to be found in the animal. This difficulty has been slightly touched on by preceding writers. It seems to prove that the longevity of the animal is considerable, and that its original habits must have become

* It is a remarkable fact, that no well authenticated case exists of worms being found in the intestines of the fœtus or infant at the breast.

altered by residing in the human intestines, for the secretion of the tail joint can hardly be considered as a natural process. A change of localities is observed to exert a certain though limited influence even over man and the class *mammalia*; but over animals of obscure vitality, and over the vegetable world, (to which *entozoa* bear close resemblance, the power of external agents knows no limits. By a change in their place of abode, by the slightest change of climate, or culture, they not unfrequently become so remarkably different from their original or parent stock, as with difficulty to be recognized. Hence it probably happens that the *tænia* has never been detected but in the intestines of man and other animals.

Intestinal worms infest graminivorous quadrupeds, and particularly the horse. They appear generally during winter, and quit him as soon as he is permitted to regain his flesh and strength on the spring pastures.* The presence of worms in the intestinal canal of man may be suspected when a few of the symptoms already enumerated are present; nor ought the practitioner to doubt the correctness of his diagnostic, on finding that a few doses of a purgative medicine do not bring off worms by stool. The *Tænia Solium* will betray its presence in the intestines by the separation and discharge of distinct joints. When other varieties of *tænia* are suspected, the patient ought to have recourse to *anthelmintics*. The *dyspnœa* occasioned by the *Tænia Solium* is a remarkable symptom, and may in some cases lead to an early detection of the complaint; but the superabundant secretion of *saliva* is connected with many disorders of the frame, and cannot be deemed a test either of the presence of worms, or of the recent use of mercury, but rather as a symptom of general bad health.

A variety of drugs and *methods* have been recommended for the expulsion of *tæniæ* and other *entozoa* from the intestines of man; these may be reduced to three heads.

1st. Mechanical remedies, as *pulvis stanni*; large doses of *aloes* taken in the form of pill, (which have been known to expel *tæniæ* from the intestines,) probably act in the same way. This method sometimes succeeds, but is exceedingly uncertain.

2dly. Drastic purgative medicines which occasionally are found successful in expelling the *tænia*, but whose operation is attended with disagreeable symptoms, and not unfrequently irreparable injury to the constitution. Milder purgatives are often exhibited, preceded by *bitters* and *aromatics*.

3dly, *Anthelmintics* or drugs which destroy intestinal worms,

* When young sweet grass cannot be obtained, the worms are generally expelled by a dose or two of melted fat, taken from the tail of the Cape sheep.

by being inimical to their vitality. Of these, the most efficacious is *spirits of turpentine*, given either alone, or compounded with empyreumatic animal oil, as in the celebrated preparation of Chabert. I have generally found, that from one or two drachms of ol. terebinth. given in a little water, morning and evening, for three successive days, were sufficient to destroy the *Tænia Solium*, (even in the most obstinate cases,) and cause it to leave the intestines without the aid of any purgative medicine. The practice, however, of exhibiting a little castor oil about noon of each day, is unobjectionable, and may be useful, as the turpentine never purges the patients. The narcotic effects produced by turpentine on *tænia* and other worms may be judged of by the fact, (ascertained by several experiments,) that when one drachm only has been taken, and the patient has declined continuing its use, an immediate stop is put to the secretion of separate joints by the *tænia*; the vigour of the worm is destroyed; it sickens, and in proportion to its unhealthy state, does the patient regain his health and strength. Should the turpentine not be repeated, the worm recovers slowly, and after the lapse of an uncertain period, (two or three weeks,) the usual secretion of joints recommences. A large dose of spirits of turpentine, (from four to eight drachms,) produced in many patients unpleasant symptoms, such as intense headach, vertigo, and a confusion of ideas, amounting to temporary delirium. Neither wine nor spirits should be drank during the use of turpentine, lest strangury be produced. Food ought to be avoided on the mornings of using the medicine, nor should any be taken in the evening. The turpentine has been found most rapid in its effects, when taken about 8 or 9 A.M.

Oil of turpentine has been used with success in cases of *tænia* on the Continent, and more particularly in Germany, for at least fifty years. To it the celebrated preparation of Chabert owes its remarkable efficacy. *Ascarides vermiculares* are easily expelled by the same drug; and it is probable that no intestinal worm will remain in the intestines, if the system be kept under the influence of oil of turpentine for a few days.

Many alarming symptoms have been known to arise from the presence of worms in the intestinal canal, particularly nervous disorders, as epilepsy and convulsions. It is not improbable that *chorea* may often depend on the presence of worms in the intestines, and its removal by the continued use of purgative medicines would seem to confirm this conjecture. I have seen a high degree of nervousness existing in many patients affected with worms, and have found that this nervous state disappeared with the removal of the exciting cause.

March 16, 1821.

PHYSIOLOGICAL INQUIRY
INTO THE
NATURAL HISTORY OF THE HUMAN PULSE.

On the Relation subsisting between the Time of the Day and various Functions of the Human Body; and on the Manner in which the Pulsations of the Heart and Arteries are affected by Muscular Exertion.

To the Editor of the Edinburgh Medical and Surgical Journal.

SIR,—I have ventured to submit to you for your approbation some observations and experiments on a subject, which, for some time, has occupied a considerable share of my attention. The matter itself will, perhaps, at first sight, appear to many rather curious, than interesting or useful. But it ought to be remembered that observations, in themselves apparently of little direct importance, have, in the progress of time, been found much to benefit science.

I may here briefly mention the circumstances which first induced me to investigate opinions, to the correctness of which so many had unlimitedly subscribed. Annoyed like others with the interminable disputes concerning the *stimulant* and *sedative* powers of foxglove, I resolved to make a few experiments on that celebrated drug. A variety of perplexing circumstances soon convinced me of the necessity there was, correctly to ascertain the various conditions of the healthy pulse, particularly

as regards muscular exertion, diet, &c. This knowledge, however, I found was not to be attained in a short time, nor without considerable labour. Contrary to my expectations, that which I deemed of minor importance, or merely preparatory to other researches, became an extended subject of inquiry.

Whilst conducting the experiments about to be related, I have often almost despaired of reducing to general laws the endless variety which the functions of the animal economy present to every observer. But remarking, amidst this seemingly inextricable confusion, a certain degree of regularity, I was induced and encouraged to prosecute the inquiry, convinced that we ought never to disregard those signs of order which nature manifests, whether they regard animate or inanimate beings.

It is almost needless for me to observe, that every precaution has been taken to avoid error. If, throughout the essay, this desirable object be attained, it is principally attributable to the assistance of my friend Dr James Thomson, continued with unremitting zeal and friendship, during a course of experiments both tedious and delicate.*

SECTION I.

1st, When we recollect that it is during the course of the day that all those circumstances, which tend to accelerate the pulse, are generally combined and present, the proposition that the morning pulse of a person enjoying health is quicker than the evening one, must seem almost a paradox; at all events, so hostile to what appears probable or plausible, that the clearest evidence of its truth must be brought forward before we could expect to gain a single proselyte to this opinion.

In detailing the results of my observations and experiments on this subject, a certain degree of licence is necessarily made use of, which, however, does not at all affect the conclusions I have drawn. In these experiments, it is not proposed to submit the functions of the body to rigorous calculation. In physiology, as well as in medicine, a near approximation to the truth is the only rational end we can possibly propose. Numbers are used, both because they render our ideas more precise, and cause to

* The subject of the experiments is about 22 years of age, of a moderate height, and somewhat muscular; his constitution may be called irritable,—by which is meant only, that it is easily excited by stimulants of almost every kind. He has not laboured under any serious indisposition for a great number of years, nor is he conscious of any hereditary or acquired tendency to disease in any organ.

be distinctly perceived the limits to which we should wish to extend these doctrines. The variation in different individuals must necessarily be immense;—for that the candid will make proper allowance.

Dr Cullen (whose name is not merely celebrated, but venerated in medical science) talked of a natural acceleration of the pulse, which, according to him, happens twice a-day, and resembles, in a distant manner, a febrile paroxysm. Now, amidst many hundred experiments, performed under a great variety of circumstances, I could never perceive any such phenomenon. The time of the day at which the first spontaneous acceleration is supposed to happen, is noon. My own pulse never shewed any symptoms of such acceleration, but, on the contrary, always diminished in velocity. Thus,

Ex.	11, A.M.	Pulse	72
	12, noon,		71
	1, P.M.		65
	Half past 1, P.M.		64

The other paroxysm, according to the same author, occurs in the afternoon, and that, too, totally independent of any excitement by food, &c. This opinion is equally erroneous with the former.

Ex.	3, P. M.	Pulse	68
	4,		66
	5,		64
	6,		62
Ex.	Half past 3, P. M.	Pulse	67
	Half past 4, P. M.		67
	5, P. M.		64
	Half past 5,		63
	6, P. M.		63
	Half past 6,	Pulse	62
	7, P. M.		62
	Half past 7,		60
	8, P. M.		58

These experiments are in direct opposition to the statements of Cullen; yet, whoever carefully peruses that author's works, more anxious to discover truth than, by partial views and misrepresentations, to detect contradictions, will be convinced that Cullen's notions are partly just and partly erroneous; that delicate and accurate observation is blended throughout with supposition, assertion, and hypothesis.

2d, This quickness of the arterial pulsations, observable in the morning, is at present very generally disbelieved; yet nothing can be more easy than to ascertain the fact experimentally. Perhaps the following instances, taken at random from a great number of others, may go far to settle the point. In the

following Table the pulse was reckoned in the sitting posture, (this is always to be understood, unless the contrary be expressed) before breakfast, and a short time after rising from bed; and, in the evening, after a light supper, and generally some spirituous liquor, taken, however, in small quantity, and much diluted. The power of alcohol in raising the pulse is well known. The period is the summer time.

TABLE.

About 9, A. M.		About 12, P. M.	
68	68	63	63
65	74	70	70
68	70	62	66
68	69	61	62
67	65	59	63
62	70	63	72
70	71	62	72
70	68	61	72
64	76	66	62
The average result of these is 68.5		The average is 64.38.	

To render the result of this table the more striking, it is necessary for me to point out a few circumstances, to which considerable attention ought to be paid. In the morning, the pulse was reckoned immediately almost on rising from bed, and always before food of any kind was taken. Every circumstance, therefore, conspired to reduce the pulse. On the other hand, the evening pulse was reckoned after the many exertions undergone during a long day,—after exercise of mind and body,—after food and drink; the pulse is, notwithstanding, considerably less frequent than in the morning, when every thing favourable to a rapid state of the circulation was carefully guarded against. But the pulse is not only in general quicker in the morning than in the evening; it is also more excitable; *i. e.* the same quantity of food or drink, the same degree of exercise, shall be found to exalt the pulse more in the morning than at any other period of the day or night; and the capability of being excited may, generally speaking, be said to diminish from a very early hour until the same hour next morning. The *data* on which this assertion rests are pretty numerous, though not so complete as I could have wished them to be. To accomplish this, would have required a complete change of my mode of life, which I found at the time impracticable.

TABLE

Shewing the different states of the pulse, as reckoned at three different periods of the day, viz. after breakfast, dinner, and supper. The experiment was made during summer.

After breakfast, always before 10, A. M.	After dinner, before 5, P. M.	Evening, between 10 and 12, P. M. generally near 12.
Pulse 66	Pulse 68	Pulse 63
68	71	70
69	73	62
66	69	61
69	71	59
69	74	62
70	76	65
64	66	63
80	80	68
73	80	70
75	76	76
80	76	68
74	84	60
74	72	63
78	72	59
75	72	62
76	80	66
73	76	62
Average 72.	Average 74.22, &c.	Average 64.388, &c.

The conclusions to be drawn from this table are obvious. To render them still more so, I may observe, that the breakfast is generally a moderate one, consisting of coffee, bread, and eggs; the dinner always of animal food principally, and a small quantity of vegetables; to which was often added a little spirits or porter; and, notwithstanding the vast difference between the stimulating powers of these two meals, the morning pulse is inferior to the one after dinner by only two beats; the difference would have been ten or fifteen, had the case been reversed, at least I am induced to think so from some experiments which I afterwards instituted directly to ascertain the point. In the evening the pulse was reckoned after supper, light, indeed, but certainly equal to the breakfast in stimulant power, and in some measure more so, because spirituous liquors very generally supplied the place of coffee. Notwithstanding this, the evening pulse is found to be, on an average, nearly eight beats lower than the morning one; a difference by no means inconsiderable, and sufficiently warranting the conclusions I have drawn from it.

The following table was drawn up, in order to ascertain the

effects which moderate exercise in the morning would produce on the pulse, reckoned, however, in such a manner as not to be immediately affected by it.

TABLE.

A walk of three miles before breakfast. Pulse after breakfast.	Pulse after dinner, as usual.	Pulse after supper, as usual.
74	76	62
81	78	62
80	75	62
83	78	64
80	75	66
78	74	64
Average 79.33.	Average 76	Average, 63.3 &c.

3. My experiments have not yet enabled me to ascertain, with precision, the hour when the pulse begins not only to be actually more rapid, but also to acquire a greater capability for action, manifested by the exhibition of any stimulant. This, with myself, perhaps takes place about 3 A. M.; but there is every reason to believe that the time varies with the individual, the season, climate, and perhaps, though this for many reasons is improbable, with the mode of life.

4. This increased capability for action, occurring regularly in the morning, is even of greater importance than the *actual state* of the circulation; the former may be made the subject of very delicate experiments; the latter we know to be exposed to a thousand variations, from causes which have been in part developed. While experimenting on this subject, there occurred to me a case, altogether of so interesting a nature, that I cannot refrain from giving it in detail. It affords a remarkable proof of the truth of the doctrines brought forward in this essay; at the same time, independent of this consideration, from the rarity of its occurrence, it appears to be worthy of record.

M. C. aged 4. The account we received from the parents of this child was, that she continued in good health until about six months after birth, when a blueness of the surface was remarked, particularly on any exertion, and accompanied with perpetual difficulty of breathing. In this state she continued to grow, and became remarkably tall of her age. She was observed, however, to get daily worse; that is, the paroxysms of threatened suffocation became more frequent, during which the whole surface of her body appeared almost black. A strong

beating was constantly to be felt in the region of the heart, and she was sometimes convulsed; muscular debility great. At her death she measured three feet four inches. The body continued warm for five hours after death.

DISSECTION.

1. *External appearances.* The countenance, feet, and body, were not all discoloured; the arms and fingers retained their usual dark colour.

2. *Thorax.* The lungs seemed sound, though dark-coloured; heart perhaps larger than usual; *foramen ovale* pervious, and pretty large. The *aorta* arose from both ventricles in such a manner, that the ventricles communicated with each other, and with the cavity of the vessel, which was much enlarged; its *vasa vasorum* distended with red blood. From the right ventricle there arose a pulmonary artery, very small in its diameter, (perhaps the size of a small goose quill) and feeble in its coats; it possessed all the characters of the artery, such as its valves and division into two branches. Not the smallest vestige of a *ductus arteriosus*; it seemed never to have existed. The passage from the left ventricle into the artery, common to both, was more direct than that from the right. Every where the arteries were full of dark blood, not coagulated.

3. The abdominal viscera were of a very dark colour. The head was not examined.

Before leaving the house, the father of the child related to us a circumstance to which we all paid particular attention. The child, he informed us, had always been very fretful and peevish, (perhaps from indulgence,) and apt to have frightful paroxysms when spoken to harshly; but these happened much more frequently in the morning; indeed, he observed that she was always ill in the early part of the day; so much so, that the smallest exertion, an angry word, or even a cup of tea, would instantly bring on a paroxysm, each time threatening destruction. This tendency to these fits decreased from morning till about 5, P.M., from which time she generally continued well till bed-time. The father detailed this with great minuteness, and seemed to wish for an explanation.

It will be easy to anticipate to what cause I would ascribe this increase of disorder in the circulation during the forenoon. So disproportionate was the pulmonary artery to the aorta, it was with difficulty that life could be carried on; but, in the morning, when the susceptibility of the arterial system was

greater, the inequality must also have been increased to such a degree as almost to have destroyed life. I may also remark that she died about 9, A.M.

5. To be convinced that an opinion, very different from that now brought forward, is the one more generally received, we have only to turn to the writings of a few celebrated physiologists. Thus, for example, in the "*Physiologie Positive*" of Fodéré, a work of merit, we find the following remarkable passage:—"Relativement à la différence du jour et de la nuit, on remarque que le pouls de l'homme adulte bat de 60 à 65 fois par minute, au commencement du jour, et qu'il va continuellement en augmentant jusqu'à battre 80 fois dans le même temps, chez les plus excitables, sur la fin de la journée; dans la nuit, les pulsations diminuent de nouveau, jusqu'au matin, où elles se trouvent revenues insensiblement au nombre de 60 à 65."* In order to have an approximation to the truth, we have only to reverse the above statement. If we regard as accidental circumstances the accelerations of the pulse, occasioned at various times by our diet, and that, too, according to the caprice of the individual, the pulse shall be found gradually to diminish in velocity from an early hour until midnight, and generally later. This difference I have found to be great in winter, but less, though still existing, in summer, as the above experiments, performed during that period, sufficiently prove; and from this I am inclined to believe, that heat has no inconsiderable share in the production of the phenomenon, though totally inadequate solely to produce it. This gradual excitation in the number of the pulsations Fodéré conceives to be owing to the combined effects of exercise, of the action of the internal and external senses, of light, heat, purer air, &c. Violent exercise towards evening will certainly produce a considerable excitation of the pulse, but less than what takes place in the morning, in consequence of the same degree of exercise; neither will any degree of heat, nor quantity of food, raise the pulse in the evening to the height at which it is in the morning, almost without any such excitants, provided they be not used in an immoderate degree.† Thus, after fasting till 8, P. M. I found my pulse to be 58; at 9, P. M. after eating a moderate dinner, pulse 58.

The common mode of experimenting with unequal quantities of food, taken at various hours of the day and night, was evi-

* *Essai de Physiologie Positive*, Tom. i. p. 190.

† In experimenting on this point, care must be taken that the acceleration of the pulse, occasioned by indigestion, be not confounded with the natural healthy pulse, produced by the simple stimulation of the food.

dently insufficient to establish the principle in its full extent. I endeavoured, accordingly, to submit to a particular regimen, and examine the state of the pulse under the influence of a diet always similar at every hour of the day. This decisive experiment could not be pushed any length; so difficult a matter did I find it to break through, at once, those habits which a great number of years had firmly established.

6. "All these things (continues Fodéré) singularly facilitate the return of the venous blood towards the heart; and this is one of the causes of the evening paroxysm which takes place in all fevers." But, having denied that any such gradual increase of the pulse exists, we need scarcely stop to say, that this cannot be one of the causes of the evening paroxysms in fever.

This opinion appears to be borrowed from the celebrated Cullen. That author, however, increased the facility of explanation, by describing two exacerbations or augmentations of the pulse; one about mid-day, the other towards evening. This explains, in his opinion, the occurrence of the double paroxysm in hectic fever.* The whole is a pure *hypothesis*, which seems partly to have arisen from supposing that the pulse was governed by the same laws in health and disease. So far as my observations go, the order of nature in disease is entirely reversed, and the observations of Cullen, Fodéré, and others, are applicable only to the unhealthy. That this acceleration of the pulse (which is really an aberration from the regular laws of nature) actually happens in fever, the testimony of Cullen, generally so accurate in the observance of disease, will sufficiently establish; and that this universally takes place in phthisis, we have, unfortunately, too many opportunities to verify.† It was from the sinking of the pulse towards evening that I ventured, independent of other circumstances, to prognosticate favourably in the case of a child labouring under typhus fever. It was this which induced me to hope that some of the functions had begun to resume their natural order, and that recovery was a probable event. Nor was I deceived; by proper attention the patient from that day rapidly amended.

7. The physiologists who invented the various diurnal accelerations of the pulse, one, two, or more, just as suited their fancy or necessity, will perhaps find little difficulty in explaining the gradual diminution of the pulse, should the phenomenon appear to them to be correctly stated. With me, I confess, the cause is yet excessively obscure, because I have not found it connected

* First Lines of the Practice of Physic, vol. i. c. 862.

† Beddoes' Essay on Consumption, p. 252.

with any circumstance, to the influence of which I might ascribe it. Experiment shewed me that no previous exhaustion from labour, excess in food, drink, &c. rendered the diurnal diminution of the pulsations more evident than usual; the want of food, perhaps, hastens and increases it, but certainly does not prevent or retard it. Neither is sleep the cause of the restoration of the susceptibilities and velocity of the morning pulse, since the negation of that does not at all destroy the *excitability** of the sanguiferous system; as manifested by the application of the accustomed stimulants.

August 30th, 1813, the day being moderately warm, I walked, between 1 and 11, P.M. a distance of nearly forty miles. Not having much appetite, retired to rest about 1, A.M. after drinking a little coffee, but slept none, perhaps owing to over-fatigue. Next morning (31st) about 7, my pulse was 80, and rather feeble; after breakfast, before which I took a small glassful of spirits, my pulse rose to 104. I was not feverish, and performed a journey that day of twenty-seven miles, at a tolerable pace.

To what, then, are we to attribute this daily diminution of velocity in the functions of the sanguiferous system? Its existence as a law of the sanguiferous system has been demonstrated experimentally; and I shall endeavour to shew, that a similar revolution daily takes place in several other functions of the human frame. To this conclusion Dr Cullen arrived, merely from observing the daily returns of sleep and watching, of appetites and excretions, and the changes which regularly occur in the state of the pulse. Of these changes he had, however, no clear ideas. When he proceeds to combine this diurnal revolution with the phenomena of fever, he unquestionably offers a conjecture, extremely probable, ingenious, and perhaps original; but unfortunately there results from this fine idea, nothing but conjecture and hypothesis, attributable principally to an incorrectness in the observance of the phenomena.

8. It required few experiments to convince me, that animal food raised the pulse much more than vegetable; the excitation of the pulse by wine is still greater, and that from spirituous liquors greatest of all. By these circumstances, but more especially by diet, the regularity in the diurnal revolution in the pulse, is, as might have been anticipated, much disturbed. But it was impossible to perform these experiments without remarking, that something similar happened to various other functions. Thus, beyond all doubt, our perceptions in the

* I use this word merely to avoid circumlocution.

early part of the day are clearer, our minds more acute, our whole intelligence more active. The functions of the stomach seem also much stronger at this time than towards evening. Feverish, restless nights, are the invariable attendants on late meals, which injure in the *ratio* of their quantity.

I have repeatedly remarked, that digestion went on more easily in the morning than in the evening. Three or four times have I been induced, (sometimes compelled,) whilst following some favourite sport in the country, to defer the taking food until evening; a greater or less degree of fever and restlessness, in proportion to the quantity of food taken, has uniformly followed such indulgence.

It was long with me a problem difficult of solution, why digestion should go on laboriously during the evening, when the actions of the muscular system were almost entirely suspended; more especially when I recollected, that the opinions of most, perhaps all physiologists, led us to conclude, that rest greatly favoured the digestion of our food. The objection, that during sleep, in which state the repose of the body is complete, digestion, nevertheless, is generally ill performed, seemed at first insurmountable; and it is not a little remarkable, that those physiologists who have so repeatedly stated the fact, have as constantly failed to note the objection. On observing that practical men expressly stated the necessity of rest for the right performance of the function of digestion, and experiencing daily the truth of the observation, I was convinced that the above objection was rather inexplicable than hostile to the opinions of physiologists, which I then, and still do consider as strictly correct. But this phenomenon is no longer difficult of explanation, if a daily revolution in the functions of the stomach be demonstrated by experiments, or even rendered probable by analogy.

On this subject, the opinion of those, whose profession it is to train men to the performance of great muscular feats, when they speak the truth, is of much more consequence than that of any medical man. Experience has taught them, that the evening is not a proper time for the digestion of the food; and accordingly we never find any substantial meal taken by their pupils after 5 P. M.; indeed they lay it down as a rule, that on going to bed, the stomach should have as little to do as possible. Thus it is recommended* to sup about 9 o'clock on a chicken, or some food that is nourishing, not gross. In another place,† you must retire early to rest, on a supper of runnet-milk, or

* Sinclair's Code of Health, vol. ii. p. 163.

† Ibid. p. 112.

milk-pottage. Again,* two meals a-day, viz. at 8 A. M. and 5 P. M. But these hours are rather later than the ones laid down by Jackson, who says,† “they breakfast upon meat about 8 o'clock, and dine at 2. Suppers are not recommended, but they may take a biscuit and a little cold water about 8 o'clock (I never heard of a more moderate supper,) two hours before they go to bed.”

The object kept in view during the training, is to enable the human frame to acquire the utmost degree of vigour consistent with health. To accomplish this, they employ the organs at the time when they ought to be employed, that is, during the early part of the day. “The exercise is always begun early in the morning, in summer at five; in winter at half-past six, or as soon as it is light. We prefer rising early in the morning, indeed it is indispensable.”

Strictly speaking, this increase in the functions of the body, may be more properly called an augmentation and diurnal revolution in the functions of the nervous system. But this is too indefinite a term; no two individuals attaching precisely the same meaning to it; and we shall therefore consider the above facts, brought forward by Mr Jackson, as proofs of an increase in the powers of the muscular system, without offering any conjecture on what that peculiarly depends; whether it be connected with increased energy of the brain, or be totally independent of it.

I shall close this part with a single additional observation. It is this: The regular decrease in the powers of the stomach is not dependent on any previous exertion of that organ, for I have found that a dinner taken at a late hour, with or without previous exhaustion, was always digested painfully and laboriously, with feverish nights, distressing dreams, and, instead of refreshment, farther exhaustion. This arises not from the food stimulating at that time the system more powerfully than in the morning. If we may judge from the pulse, it stimulates the body much less at midnight than at nine in the morning; but it arises from this, that the powers of the stomach are more languid; it does not digest the food taken into it; and should a feverish night follow, it is neither wonderful nor inexplicable. Hence we see the propriety of no function being much employed during the evening; not because it will greatly excite the pulse, and so produce fever (ridiculous idea! the excitation of the pulse does not produce or constitute fever); on the contrary, the stimulation is actually less, it is almost as nothing, so far as

* Sinclair's Code of Health, vol. ii. p. 104.

† Ibid. p. 94.

regards the pulse; but the phenomenon is occasioned by this, that all the organs are less powerful, less capable of exertion; in short, less able to perform their functions, or undergo fatigue.

A remark made by Cullen also illustrates, in some degree, the doctrine I have brought forward: "It is indeed to be observed, that in almost every person the taking of food occasions some degree of fever; but I am persuaded this would not appear so considerable in a hectic, were it not that an exacerbation of fever is present from another cause; and accordingly, the taking of food in the morning has hardly any sensible effect." Here we see what so seldom happens, all the facts throwing light on the doctrine, and it on them; for although the morning be the time when the action of food over the pulse is greatest, yet is there no febrile state excited. On the contrary, in the evening, as Cullen has remarked, a febrile paroxysm occurs in hectic, independent of food, aggravated perhaps by its presence, but whose real cause has totally escaped him.

9. Nor does this law seem confined to the functions of the brain, stomach, muscular, and arterial systems,—it extends, if I mistake not, to that of the lungs. Dr Prout found, "that the quantity of carbonic acid gas, formed during respiration, is not uniformly the same during the twenty-four hours, but is always greater at one and the same part of the day than at any other, that is to say, its *maximum* occurs between 10 A. M. and 2 P. M., or generally between 11 A. M. and 1 P. M.; and its *minimum* commences about half-past 8 P. M., and continues nearly uniform till about half-past, 3 A. M.)* The same gentleman observes, that the quantity of carbonic acid given off during respiration, bears no proportion to the numerical state of the pulse; in fact, he imagines that most carbonic acid is given out when the pulse is least frequent. My own experiments undoubtedly disprove this idea. The greatest quantity of acid, according to Dr Prout, was given off during the forenoon, when the pulse is, in general, higher, and always more easily excited by any exertion; we may almost say, that the capabilities of the arterial system are at that time greater; and the importance of this remark, as it regards *secretions*, must be obvious. Besides, from what I can judge of the tables given by Dr Prout, they refer principally to the afternoon and evening, unless some of the tables be wrong marked. Notwithstanding this, I am much inclined to agree with him in this, that the quantity of carbonic acid given off during respiration is not particularly connected with, at all events not dependent on, the state of the circulation.

* Annals of Philosophy, vol. ii. p. 330.

His experiments on the state of the respiratory organs after exercise—after the taking of spirituous liquors—during a mercurial course*—in short, after every thing which could excite the circulation, shew this in a decided manner. These experiments, however, require repetition. Many of them are too scanty to enable us to draw any certain conclusion from them, whilst others are contrary to all analogy.

The opinions contained in the excellent essay of Dr Prout do not seem to invalidate the conclusions which I have ventured to draw from my experiments: they amount to this—that all the functions, or at least many of them, are more vigorous in the morning than in the evening; that their capability for action is certainly greater; and that this increase in the functions commences at a much earlier hour than is generally imagined.

Were it lawful for me to speculate, in this experimental age, I would venture to support an opinion, at present, I allow, somewhat antiquated, and very *unfashionable*, that early rising may be conducive to long life, as it most certainly is to the perfect enjoyment of all our faculties. It was from repeated violations of all these dietetic maxims that I first perceived their importance; perhaps by a similar experience alone can others be convinced of their value.

Before concluding this section, it is my duty to observe, that on mentioning the results of some of these experiments to a medical friend, he assured me that experiments, performed by him about ten years ago, led to conclusions which were, in his opinion, extremely similar to mine. This circumstance, he observed, was very satisfactory to him, as my experiments had been performed, and my conclusions drawn, without any communication of ideas: it will be equally so to me, if I find the results exactly to correspond,—results of experiments performed by individuals so opposite in habits, temperament, and opinions.

SECTION II.

In this section we propose considering the effects of muscular motion on the pulsations of the heart and arteries. In what manner these pulsations are augmented in number, vigour, fullness, &c. by exercise, or, to speak more generally and correctly, by muscular motion, it is perhaps impossible, in the present state of our knowledge, to say. It seems, however, probable, that farther research may shortly lead to notions more precise than the

* Dissertatio de Copia Acidi Carbonici, &c. Andrea Fyfe auctore, Edin. 1814.

ones we at present possess. Our concern is with the fact itself; viz. that, by muscular action, the pulsations of the heart and arteries are augmented in power, velocity, &c. This fact, apparently so unproductive, and, by reason of its perpetual occurrence, so little apt to excite attention, shall yet, on examination, be found to throw some light on the physiology of the human body, and may perhaps assist in exonerating it from a charge so lavishly and inconsiderately bestowed,—that it is a science destitute of fixed principles; or, at least, that a knowledge of the laws by which the animal economy is regulated, is, in a great measure, placed beyond the sphere of human intellect.

At all times it must have been observed, that muscular exertion, almost of any kind, but more especially violent exercise, increased greatly the powers of the heart and arterial system; but that this extended even to the slightest muscular motion, such, for example, as is made use of during a change of posture, does not appear to have been suspected, or if so, its importance has been greatly overlooked. We may reduce the section to the following heads:

1mo, The most powerful stimulant which can be applied, in order to increase the action of the heart, is exercise. Walking at the rate of four miles an hour, requires at least a pulse equal to 132 per minute; and the time of the day, and the continuance of the exertion, less affect the rate of pulsation than one would *a priori* imagine. When I say, that walking at such a rate requires a certain increase in the arterial pulsations, I do not mean to assert, that an equal increase must necessarily occur in every individual. These numbers are added only to render the subject more definite; in short, as a single example,—as a proof that the increase is great, even in the strong and healthy. We shall immediately see how very differently the debilitated are affected. This high excitement is not followed by proportional exhaustion, in so far as regards the arterial system; a fact singular enough, since it is at variance with certain laws of the economy, supposed to rest on a secure foundation. Other organs certainly follow a different mode, and exhibit, after any increase in their functions, signs of exhaustion or weakened energy. Such a law seems to hold with regard to the nervous system, in particular with the digestive organs and the respiratory.*

2do, No stimulant which I have hitherto tried, has excited the pulse so much even as moderate exercise.

3tio, Various observations have rendered it probable, that the increase in the number of the arterial pulsations accompanying

* Annals of Philosophy for November, 1813, p. 328.

muscular motion, is greatly influenced by the debility or weakness of the individual. Were it allowable to apply the rigorous language of calculation to a science which cannot be called *exact*, we would say that such increase is in the direct *ratio* of that debility. Hence, in fever, the slightest change of posture shall often produce an incredible velocity of pulse. Persons who have suffered much from loss of blood, or by chronic complaints, cannot bear the erect posture for any length of time; and hence, in the debilitated, who, it is well known, are very subject to faintings, the slightest muscular motion, by inducing or necessitating a rapid motion of the blood, (in them too rapid,) shall give rise to that distressing accident.

As exercise increases, so other stimulants diminish the frequency of the pulse in the debilitated, at least generally. But as there may be various kinds of *debility*, in each of which the pulse may be differently affected by exercise, and as this increase of the pulse, when it does take place, is often accompanied with irregularity, a very extensive series of experiments is required, before we can implicitly agree with the above rule. On this latter principle, even in its present state, we may explain, I think, satisfactorily, many of the supposed stimulant effects of foxglove, which drug debilitates, directly and greatly, most *systems* of the animal economy.

4to, The time of the day has a very considerable effect on the augmentation of the arterial pulsations by moderate exercise. As this relation has already been considered at some length, I shall here confine myself to the statement of a few general results. During the morning, the mere change of posture from the horizontal to the erect, shall increase the pulse by about 15 or 20 beats. At mid-day, this increase shall be 10; and, in the evening, 4 or 6. The effects produced by the sitting posture, assumed after the horizontal, are not half so considerable as those occasioned by the erect posture. The above is the manner in which the arterial pulsations are affected by posture, at different periods of the day; and though the results here stated may be greatly magnified in some, and equally diminished in others, it may, I think, be laid down as a general rule, that similar laws constantly regulate the healthy pulse.

From the above observation, we readily perceive, of how little consequence the details of physicians are, regarding the diseased pulse. If the slightest change of posture can in an instant excite the pulse by 50 or 60 beats, how easily may the medical man deceive himself and others! how often, without a previous knowledge of these facts, may drugs seem to benefit the sick, when they are inert, or actually pernicious.

5to, The increase of the pulsations occasioned by change of posture, may shortly prove a valuable *asthenometer*. There are others, but they do not seem so certainly to indicate debility as the above. Some of them, indeed, are more calculated to detect disease than simple debility. These *asthenometers* are* the hot-bath, the cold-bath, or cold air, also the non-excitation of the pulse by stimulants, supposed to happen in those habituated to spirituous liquors. Some may allege, and perhaps with justice, that this shall be found to indicate rather a state of disease than debility. This condition of the arterial, or perhaps nervous system, which renders the pulse non-excitabile by stimulants, is supposed to arise from other causes besides the abuse of spirituous liquors. "Une observation très remarquable que j' ai eu occasion de faire, c' est que, lorsque la sensibilité a été émoussée par une affection chronique, les opiacées finissent par n' avoir plus de prise sur l' économie animale."† I have seen one case which favoured somewhat the above opinion—the case of a young man, who, without doubt, laboured under hereditary predisposition to consumption. His pulse, on one occasion, I found to be not at all affected by a very great quantity of spirituous liquors. The opinion remains to be proved or refuted by additional observation. Indeed, as it is customary with those labouring under chronic complaints to resort to the use of narcotics, (this practice had not been adopted in the case just mentioned,) it is not to be wondered at that this class of drugs should at last cease to produce their wonted effects.

Somewhat connected with this subject is the detection of commencing disease in the lungs, or pulmonic debility. This, according to Beddoes,‡ is indicated by a continued high pulse. "When consumption is advancing, it will be more frequent than natural, and, in general, much more frequent towards the close of day." The whole subject is novel, and deserves attention.

As my only wish in prosecuting these experiments has been to correct a few notions regarding the physiology of the human body, and to advance that estimable science, I shall feel gratified with an examination of my experiments, whether that lead to a refutation or to a confirmation of the opinions maintained throughout this essay.

* Beddoe's Hygeia.

† Nouveaux Elémens de Therapeut. et de Mat. Med. par Alibert, tom. ii. 506.

‡ Essay on Consumption, p. 252.

EDINBURGH:
Printed by ANDREW SHORTREDE, Thistle Lane.

suppl.

J. Percy Esqre.
with the Authors kind regards.

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X

INAUGURAL DISSERTATION
ON THE QUESTION,
HOW FAR ARE
SECRETION AND NUTRITION
DEPENDENT ON
NERVOUS INFLUENCE?

SUBMITTED TO THE
MEDICAL FACULTY
OF THE
University of Edinburgh,
IN CONFORMITY WITH THE RULES FOR GRADUATION,
BY AUTHORITY OF THE
VERY REVEREND PRINCIPAL BAIRD,
AND WITH THE SANCTION OF THE
SENATUS ACADEMICUS.

BY
CHARLES CHADWICK,
OF LEEDS,

EXTRAORDINARY MEMBER OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH,
AND CANDIDATE FOR THE
DEGREE OF DOCTOR IN MEDICINE.

EDINBURGH :
PRINTED BY JOHN STARK.

MDCCCXXXVII.

Book the Doctor's Book

INAUGURAL DISSERTATION

ON THE QUESTION

HOW FAR THE

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DEPENDS ON

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EDINBURGH :

PRINTED BY JOHN STARR.

MDCCLXXII

TO

JAMES SYME, Esq. F. R. S. E.

PROFESSOR OF CLINICAL SURGERY IN THE UNIVERSITY OF EDINBURGH,

THE FOLLOWING ESSAY

IS RESPECTFULLY INSCRIBED,

IN ADMIRATION OF HIS PROFESSIONAL EMINENCE, AND

IN GRATEFUL ACKNOWLEDGMENT OF HIS VALUABLE INSTRUCTIONS

AND REPEATED ACTS OF KINDNESS AND ATTENTION,

BY HIS PUPIL AND FRIEND,

THE AUTHOR.

TO

JAMES BYRNE, Esq. F.R.S.E.

FOR HIS PRACTICAL REPORT IN THE TREATMENT OF LEPROSY.

THE FOLLOWING ESSAY

IS RESPECTFULLY INSCRIBED,

IN ADMIRATION OF HIS PROFESSIONAL VIRTUES, AND

IN GREATEST ACKNOWLEDGMENT OF HIS VALUABLE INSTRUCTIONS

AND REWARDED WITH DEEREST KINDNESS AND ATTENTION,

BY HIS PUPIL AND FRIEND,

THE AUTHOR.

TO

JAMES WILLIAMSON, Esq. M. D.

SENIOR PHYSICIAN OF THE LEEDS GENERAL INFIRMARY, AND LECTURER
ON THE PRACTICE OF PHYSIC IN THE SCHOOL OF MEDICINE,

THIS ESSAY

IS, BY KIND PERMISSION, DEDICATED

BY HIS FRIEND AND FORMER PUPIL,

THE AUTHOR,

WHO WOULD THUS EVINCE HIS GRATITUDE FOR THE VALUABLE

ASSISTANCE AFFORDED HIM IN THE PROSECUTION OF HIS

PROFESSIONAL STUDIES.

TO

JAMES WILLIAMSON, Esq. M. D.

PROFESSOR OF THE LECTURES ON MEDICINE AND SURGERY
IN THE HOSPITAL OF THE UNIVERSITY OF EDINBURGH

THIS ESSAY

OF THE NATURE AND CAUSES OF
THE DISEASES OF THE LIVER
BY
JAMES WILLIAMSON, Esq. M. D.

AND AUTHOR

OF THE LECTURES ON THE NATURE AND CAUSES OF THE DISEASES

OF THE LIVER, AS DELIVERED AT THE HOSPITAL OF THE UNIVERSITY OF EDINBURGH

IN THE YEAR 1801.

FOR

DR MADDEN, SURGEON,

EXTRAORDINARY MEMBER OF THE ROYAL MEDICAL SOCIETY
OF EDINBURGH;

WHOSE KIND COMPANIONSHIP HAS EMINENTLY CONTRIBUTED

TO RENDER THE HOURS OF STUDY AGREEABLE,

AND THOSE OF RELAXATION PROFITABLE;

AND WHOSE FRIENDSHIP WILL EVER ENDEAR THE REMEMBRANCE

OF HIS ACADEMICAL RESIDENCE,

THIS DEDICATION PAGE IS AFFECTIONATELY RESERVED,

WITH THE BEST WISHES FOR HIS FUTURE SUCCESS,

BY THE AUTHOR.

HOW FAR ARE
FOR

SECRETION AND NUTRITION
DR. MADDEN, SURGEON.

DEPARTMENT OF

EXPERIMENTAL MEDICINE OF THE NATAL MEDICAL SOCIETY

NERVOUS INFLUENCES

WHICH KIND OF CONNECTION HAS EXISTENTLY EXISTED

TO RENDER THE NERVOUS SYSTEM

In the bodies of all animals in which a certain
function is known to be performed, there is found ex-
isting a species of apparatus, either forming or
acting organs and occupying determinate stations
in more diffusely spread throughout the system.
The organs which are scattered throughout the
body, differing as widely from each other in their
appearance, character, and purpose, as from the
vital organs whence they are derived. The
organs which perform these important functions
vary greatly in their anatomical constitution, pre-
serving in one part the appearance of a compound
and solid gland, whilst in another they only of-
fer the appearance of a simple extended membrane.
They differ so much, as they certainly do, in their gene-
ral appearance, anatomists inform us that their
essential constitutions are not very dissimilar.

HOW FAR ARE
SECRETION AND NUTRITION
DEPENDENT ON
NERVOUS INFLUENCE ?

IN the bodies of all animals in which a circulation is known to be performed, there is found existing a species of apparatus, either forming distinct organs and occupying determinate stations, or more diffusely spread throughout the system, whereby certain fluids are separated from the blood, differing as widely from each other in their appearance, character, and purposes, as from the vital stream whence they are derived. The organs which perform these all-important functions vary greatly in their anatomical constitution, presenting at one part the appearance of a compact and solid gland, whilst at another that only of a simply extended membrane. Yet though differing so much, as they certainly do, in their general appearance, anatomists inform us that their necessary constituents are not very dissimilar. It

is not for me to enter on the discussion as to what constitutes a secreting organ, and whether in it it is absolutely necessary that an excretory duct should form a part. We know that similar processes go on in some membranes whose minute glands cannot strictly be said to have proper excretory ducts, and also over the whole surface of the body, as well as in those which possess these tubes. We know likewise that in all these processes blood-vessels, conveying an abundant supply of their proper contents, are essentially required; and it is also ascertained, with a considerable degree of certainty, that in all these cases nerves are distributed to these organs, excepting, perhaps, in some of the very lowest in the scale of animals, where the fact of the existence of a nervous system has not been fully proved, though, I think, with a tolerable degree of reason, inferred. As to the part performed by the blood, physiologists are at once agreed, from it are separated those fluids which it is the function of each organ to afford; but how these changes are brought about, and what share the nerves have in the perfection of the process, constitutes one of the most mysterious and difficult questions to which the enquirer can direct his attention. Notwithstanding its alarming perplexity, it is the one I propose to make the subject of enquiry in the following essay, and without further preface I enter upon the task. I purpose, in considering this subject, to notice, in

the first place, the various experiments which have been performed by several physiologists, and which appear to bear upon, and in any way elucidate the subject. I shall also endeavour to ascertain from what source has arisen that diversity of opinion which authors have been led to entertain. Afterwards I shall state the ideas I have myself gathered whilst engaged in the study of the subject; and if possible, in the last place, draw some conclusion on this most interesting point, thus answering, to the best of my ability, and as far as the present advance of physiological research will allow, the question under notice. In pursuing the subject, however, considerable difficulty will have to be encountered, not merely from the intricacy of the subject itself, but likewise from the very loose and imperfect manner in which I find this branch of science has been treated of by those learned individuals who have elucidated, by their enquiries, so many physiological difficulties, and to whose names, as authorities, we are accustomed to look with deference and respect. They will be found in many instances referring to this subject with comparative neglect, or founding their opinions on what those studying it must be compelled, in this age of close research, to pronounce as insufficient data.

The influence, real or supposed, exerted by the par vagum on the secretion of the juices of the stomach, during digestion, has long been referred

to as a proof that the process generally depends on the action of the nerves. Most probably, from the ease with which these nerves can be exposed in the neck, during their passage to the stomach, they have come to be a favourite object of experiment. Many and various are the means which have been adopted for destroying or suspending their action, but the conclusions derived, are, I fear, liable to considerable objection. But without anticipating, I shall endeavour to give a brief outline of what has hitherto been done by physiologists on this part of the subject. It is not my intention to introduce all the published experiments relating to this point, as that would be totally inconsistent with the limits of an essay of this kind, but merely a few of the most important, from which an idea may be formed of their value as an argument in considering the question under present notice.

Experiments shewing the effects of section of the par vagum are of very ancient origin; perhaps, however, those performed with a view of ascertaining the changes produced by this operation on the respiratory functions have the highest claim to antiquity. With this part of the enquiry, however, I have not to interfere, but to those authors who have mentioned the effects on digestion, I may, I imagine, with propriety here refer. According to a paper of Messrs Breschet and Milne-Edwards, Baglivi is the first who notices this class

of effects of injury of the par vagum. On consulting his work, I find that he refers to Willis,* who makes mention of the injurious effects of ligature of these nerves on the digestive functions. Shortly after him Valsalva † notices similar effects produced on these organs by like operations. Baglivi ‡ states that, on dividing the eighth pair, he found that respiration became difficult, and that whatever was taken for some time after such an operation was vomited. On dissection after death, which usually took place about the twelfth day, the œsophagus was full of undigested food, which had been more recently eaten, which circumstance he attributes to the inability of the animal to swallow perfectly. Brunn § mentions that, in his experiments when the par vagum on both sides was divided, the dog on which the operation was performed would eat nothing but a little grass, which was found in the stomach, after death, unchanged, along with matters having the appearance of true fæces. Haller appears, as far as I have been able to ascertain, to have paid little attention to the influence of the nervous system on secretion. He gives, however, a few experiments on the par vagum, along with their results, which have some connection with the present part of our enquiry.

* Willis de Nervorum usu, Tom. i. cap. 24.

† Valsalva Opera omnia, curante Morgagni.

‡ Baglivi Opera omnia. Antwerp, 1715.

§ Brunn, Experimenta circa Ligaturas Nervorum.

These generally were, that after fruitless attempts to vomit, along with very difficult respiration, the animals perished a few hours after the operation, and on dissection, putrefaction of the contents of the stomach was discovered. He states, that his observations contradict those of Brunn, as to the pain occasioned by the operation, the length of time the animals lived after it, and the conversion of the contents of the stomach into fæces, which the latter imagined he detected.* I cannot here help expressing my regret that this able physiologist did not pay more attention to this enquiry. Blainville found that, on dividing the nerves some secretion did take place, but that the digestion of food was wholly suspended. Dupuy† has recorded that when the eighth pair of nerves is divided in the neck in various animals, as sheep, horses, &c. the œsophagus is found to be filled with unaltered food, as also is the stomach, on the contents of which no change appears to take place. He found that when the trachea was artificially opened, so that a free supply of atmospheric air was afforded to the lungs, the animals usually lived for seven or eight days, and at length appeared to die from affection of the digestive organs alone. When, however, this precaution was not taken, they perished in a few hours from congestion of the lungs, the air passage being closed

* Haller, Opera Minora, Vol. i.

† Journal de Medecine, 1816, Vol. xxxvii.

from paralysis of the laryngeal muscles supplied by the recurrent branch of the par vagum.

The experiments and observations of Le Gallois * on the division of this nerve were principally made in regard to the changes produced on the respiration. He, however, appears to believe, that digestion is suspended, and that if death did not take place from other causes, the derangement produced in the exercise of this function would be sufficient to produce that event.

Sir B. Brodie, in endeavouring to investigate this subject experimentally, found, from the speedy death caused by the division of the eighth pair in the neck interrupting the respiratory functions, that it was necessary to avoid, by some means or other, this source of fallacy. He had previously ascertained, that when a dog was poisoned with arsenic, a considerable effusion of fluid was found always in the stomach. He accordingly administered some of the deleterious drug to an animal, and also introduced some into a wound made in the thigh, and afterwards, in both cases, divided the branches of the par vagum upon the œsophagus, just as they pass from it, upon the surface of the stomach. In these cases, the breathing was not in the slightest degree altered from the natural state, and the poison took effect with the usual phenomena, excepting the secretion of the fluid, which was entirely wanting. Hence he appears

* Le Gallois sur la Principe de la Vie.

to infer, that the secreting power of the stomach is interrupted by the section of these nerves, and he observes,* “we cannot venture to deduce from these any positive conclusions respecting the necessity of the nervous influence to the secretions in general, but as forming one link in the chain of an interesting, but difficult physiological investigation,—the circumstances which have been mentioned, may be considered as possessing some value.”

Sir B. Brodie and Dr W. Philip turned their united attention to this subject, and the results of their enquiry were published in the Philosophical Transactions. Some of their experiments showed that after division of the nerves, when the ends were left in contact, or even when the cut extremities had considerably retracted within the muscular substance of the neck, digestion was in a great measure completed. But again in others, when they took the precaution to turn back the divided ends of the nerves from each other, little or no perfectly digested food was found on examination after death. The great mass had the appearance of masticated food without any other change, however long the animal was allowed to live. †

Dr Wilson Philip proves that the motions of the alimentary canal, meaning by this the stomach

* Philosophical Transactions, 1814. † Ibid. 1822.

and intestines, are wholly independent of the nervous system. From the experiments of Dr Hastings, conducted under his (Dr Philip's) own inspection, (and published also separately by Dr H. himself,*) he concludes that when the par vagum is divided, the digestion of food is suspended, and that this arises from the non-secretion of the gastric juice. He states that when the division was made below the situation where the recurrent branch is given off, the difficulty of breathing did not supervene so rapidly as when made above. Rabbits were usually made the subject of these operations, two being employed for the purpose, one for comparison, which was always placed in the same situation as the other, excepting as regards the injury of the nerves. The one was killed upon the death of the other, and the respective states of their stomachs compared. Efforts to vomit always occurred sooner or later during the experiments. In those animals which were fed after the operation, it ensued immediately, whilst in those which had eaten previously, the consequence was delayed until sometime afterwards.

This he considers as a powerful argument in his favour. For in the first case, says he, the internal coat of the stomach is not defended from the irritation of the crude food by the proper secretion which would convert the surface of the alimentary mass into chyme; whilst in the latter,

* Journal of Science, Vol. xi.

some gastric juice having been secreted before the power of the nervous influence has been destroyed, a certain time must elapse before the chyme thus formed can be propelled onwards, and the stomach subjected to that unaccustomed irritation which causes the action of vomiting.*

Dr Clarke Abel, in prosecuting the enquiries of the last mentioned author, in regard to the effect of galvanism on secretion conveyed through the nervous system, found, on dividing the par vagum in the neck, that the process of digestion was suspended from the loss of the secretory power of the stomach. †

Broughton, in a number of experiments, performed on different animals, ascertained that when food of various kinds was given both before and after the operation, that first taken was partially digested, whilst the latter had undergone no change. On some of them the operation produced little or no distress for several hours, but that after this time they were greatly affected, and usually perished within the space of twenty-four hours. ‡

Magendie, himself, formerly attributed the derangements of the digestion to the effects produced on the gastric secretion, by the interruption of the

* Wilson Philip's experimental enquiry into the laws of the vital functions.

† Medical and Physical Journal, Vol. xliii.

‡ Journal of Science, Vol. xi or Magendie's Journal, Vol. i.

nervous action ; but it appears from his more recent publication that he now ascribes it to the disordered state of the respiratory function, caused by the operation in question, and therefore that it is only a secondary effect of such lesion.*

Messrs Breschet, Milne-Edwards, and Vava-sour have published a number of experiments, in which they confirm the results obtained by Wilson Philip, as to the effects of simple section of the par vagum, as well as of section, when the ends of the divided nerves were turned away from each other. Moreover, to obviate the objection of Magendie, they divided the œsophagus of a guinea pig, just before its passage through the diaphragm, ensuring in this way the section of all the nervous branches ramifying upon it, finding it otherwise impossible to separate all the nerves without this precaution. The animal died after an interval of eight hours, having previously appeared lively and without pain. Neither inflammation nor effusion was found in the abdomen, yet still, however, the food remained unaffected by the action of the stomach. † The above were published in an early number of the Archives Generales de Medecine, but in a subsequent part of that work they attribute the results to paralysis of the muscular fibres of the stomach. The function of the par

* Précis Elementaire de Physiologie, a translation.

† Archives Generales de Medecin, Tome ii.

vagum, is, according to their account, to preside over the motions of that organ.*

Paetsch has given an account of some experiments, in which he cut through the œsophagus, between the diaphragm and stomach, and found that the digestion of food previously taken was entirely suspended, although the animals shewed little or no signs of uneasiness for several hours after the operation. His friend, Augustus Schultze, Professor of Physiology in the University of Fri-burg, found precisely opposite results in similar experiments, for, according to his account, the process of digestion was, not only not interrupted, but even performed with greater rapidity.†

Messrs Leuret and Lassaigne object strongly to any experiment in which the abdomen or chest is opened, as, from the severity of the operation, no precise results can be obtained. Their first trial was upon a horse, in whose neck they divided these nerves. Digestion was suspended, but this they attribute to the inflammation of the stomach which ensued in this case, as in some future experiments the food was found perfectly digested. Analysis shewed the chyme and chyle formed to be similar to that under ordinary circumstances. A great number of gentlemen witnessed these experiments, and were convinced of the accuracy of the results; among whom was M. Dupuy, who con-

* Archives Generales de Medecine, Tom vii.

† Dissertatio Inauguralis. Gottingæ.

fessed the error of his previously published observations. They conclude that digestion can be completed independently of the par vagum.*

Dr Ware, in some experiments intended to prove the inaccuracy of Dr Wilson Philip's theory of the identity of the nervous influence and galvanism, found that, on dividing the eighth pair, digestion was entirely suspended.†

To conclude the notice of this class of experiments I may briefly mention some which I performed myself during the last summer. They corroborate those which have been already noticed, where the suspension of the digestive functions was the result obtained. After the operation the animals were always allowed to live for a time, sufficiently long for digestion, under ordinary circumstances, to have proceeded some length. Little pain was manifested during this time. Difficulty of breathing was often, though not invariably, considerable, and the effusion of fluid into the bronchi moderate. Other rabbits, for these were the animals made use of, were always kept for comparison, placed under precisely the same circumstances, excepting the division of the nerves, which obviated in a great measure the objection urged by some, that the pain of the operation is at any time sufficient to suspend digestion.‡

* Recherches Physiologiques et Chimiques pour servir à l'Histoire de la Digestion.

† London Medical and Surgical Journal, Vol. i.

‡ Vide Appendix.

M. Brachet, in a great number of experiments, performed on various animals, and related in his late work on the ganglionic system, confirms the above observation in every respect ; but whilst he believes the secretion to be under the immediate power of the nervous system, yet he does not allow this to the par vagum, but ascribes to it, amongst numerous other offices, that of presiding over the motions of the organ.*

In estimating the value of these experiments in regard to the question under consideration, and of the conclusion which may be drawn from them, numerous difficulties obstruct that simplicity of deduction which all positive physiological inferences require. There are three separate functions which the par vagum may discharge in its distribution on the stomach. It may alone be of use in influencing or regulating the secretion of the gastric juice ; on it may depend those sensations with which physiologists state the organ to be endowed ; or lastly, its office may be to call into action the peristaltic movements of the viscus. And I find, in the various authors whom I have consulted, these three different theories advocated, in explanation of the suspension of the digestive function, which now, from the numerous experiments recorded in proof of the fact, must, I think, be allowed, by all candid enquirers, to be the con-

* Recherches experimentales sur les fonctions du systeme nerveux ganglionaire.

sequence of division of these nerves. To fix upon the correct one of these three theories constitutes not a slight portion of the difficulties of the subject, for if we agree with those who are inclined to admit the first hypothesis, the question is immediately asked, how it happens that, whilst this secretion is suspended, that of the mucus of the lungs is so greatly increased? and does it not appear strange, that from the same cause two such decidedly opposite consequences should arise? We shall be told likewise, by those advocating the third theory mentioned, that for a certain time the secretion does go forwards, as is proved by the existence of a layer of chyme on the surface of the alimentary matter, which, say they, would be continued, did not the paralysis of the organ prevent the application of fresh unchanged food to the secreting surface. As to the first of these objections, although it is stated, by almost all experimenters on the subject, to be the fact that the secretion from the internal mucous surface of the air-passages is much increased, and though not prepared positively to deny it, yet I am inclined to think, from my own observation, that this is more apparent than real. The deception arises, I imagine, from the air breathed being mixed, by the violent respiratory efforts, with the fluid already there, thus giving it a frothy appearance, which might easily be mistaken for augmentation of its quantity. In confirmation of this idea I have found that when

only slight efforts were made in respiration, the fluid discovered was considerably less. * To the second of these objections it has, with some degree of plausibility, been answered, that the secretion might have taken place before the operation, and immediately after the reception of the food into the stomach, or, as it has more hypothetically been explained by others, as owing to the action of the nervous power which remained in the nerves below the place of section. The former of these explanations appears to be strengthened by the observation of Dr W. Philip, already mentioned, † regarding the time at which vomiting comes on, when the animals are fed before and after the operation. To those who are inclined to embrace the second theory, the following serious objection will occur. For a perfect sensation, it is requisite that a communication should exist between the part affected and the brain—how does it happen, then, that, when these nerves are divided, simple mechanical or galvanic irritation of the cut extre-

* My friend, Dr John Reid, who is still investigating this subject, informs me, that “ he is at present inclined to believe that this *serous frothy* effusion is the *result* of the severe dyspnœa which generally precedes death, and not the *cause* of it, as is usually imagined.” He says, “ the grounds on which I have adopted this opinion are these, *1st*, In my experiments the extent of this effusion appeared to be proportionate to the extent of the dyspnœa preceding death; and *2dly*, This effusion appears to me to differ in no respect from that found in the lungs in all cases of death where severe and protracted dyspnœa have been present.”

† Vide pp. 9, 10.

mities causes the function to be properly discharged? The third theory involves another much disputed point in physiology, viz. the dependence or independence of the muscular contractions on the nerves, and accordingly as the one or other of these opinions is held, will the answer given to this part of the enquiry be regulated. Wilson Philip, as has already been stated in this essay, affirms that the motions of the intestines are perfectly independent of the nervous system, and for this opinion he has the authority of the illustrious Haller, and numerous other distinguished physiologists. In addition to these, I may here shortly advert to the opinion of Magendie, who conceives that this effect upon the stomach is produced secondarily, by the derangement taking place in the respiratory organs; for he states, that he found, when the par vagum was divided below the branches sent to the lungs, a perfect digestion took place. Several experiments of very able physiologists, some of which have been already quoted, contradict this result; neither has the opinion elsewhere received any material support. M. Brachet states in regard to it, that, from the numerous branches into which these nerves divide in the chest, M. Magendie must have failed in dividing them all. But it is time that I should leave this part of my subject; and I imagine that little more need be advanced, under this head, to prove the perfect complexity in which it is involved as con-

ned with the stomach. If the question under consideration is capable of being elucidated by any series of experiments, it must be by one, performed on some of the numerous secreting organs of the body, whose functions are less complicated, than those of the viscus which has hitherto occupied our attention.

It has been found by numerous trials, that the kidneys may be exposed by cutting into the abdomen, and yet the animal survive several hours, and, notwithstanding the severity of the operation, it is ascertained, that the shock thus caused to the system is not sufficient to suspend the secretion of urine. That this is the case may easily be proved by emptying the bladder at the time of the operation, and afterwards, when death has taken place, or a sufficient time has been allowed to elapse, examining the state of the organ.

The kidneys are supplied with nerves from the renal plexus, which is composed principally of branches of the sympathetic system, having, however, communication with the cerebro-spinal nerves.

I shall now briefly notice some of the experiments which I have collected from various authors, illustrating the effects of injury of the different parts of the nervous system on the urinary secretion, believing, as I do, that these are, in the present state of our knowledge, better calculated than any others we possess, to clear up, in some mea-

sure at least, part of the difficulties by which we are so completely surrounded.

Naveau, in his inaugural dissertation, gives the following as the result of some of his experiments : Having previously ascertained that rhubarb administered to dogs might be easily detected in the urine, he divided the nerves going to the kidneys, and emptied the bladder. He gave the animal rhubarb, and in the space of eight hours the bladder was found filled with a purple-coloured urine, which was otherwise much changed in character. Urea was considerably less in quantity, and albumen much increased. Lint dipped in the fluid was tinged red by the colouring matter of the blood which was contained in it, but not the slightest trace of rhubarb could be detected. After this the secretion continued gradually to lose its characteristic properties, until at length it contained nothing but colouring matter of the blood suspended in a clear and thin watery fluid. This experiment was repeated by him several times with precisely similar results. He also states that division of the par vagum slightly affects this secretion, for although when rhubarb is administered it cannot be detected in the contents of the bladder, yet the natural constituents of the fluid remain the same. When the spinal marrow was divided at the first dorsal vertebra, the secretion continued, and rhubarb given was speedily detected in it, but the characters of the fluid were slight-

ly changed. When the spinal chord was destroyed from the place of section, above-mentioned, to the sacrum, little change was observed, but when the destruction was carried further up into the medulla oblongata, respiration instantly ceased, and, although artificially carried on for thirteen minutes, no secretion was formed. The brain was next gradually removed, and here likewise the secretion was not affected. Immediately, however, on the medulla oblongata being injured, respiration ceased, and although this function was again artificially maintained, the same result as before mentioned occurred.*

Brodie informs us that, on removing the brain, the secretion of urine was suspended, although artificial respiration was maintained. In repeating the experiment he found the same result, together with a considerable reduction of the animal heat. He concludes, that, when the influence of the brain is cut off, the secretion is arrested, as also the formation of animal heat, notwithstanding that the usual changes in the appearance of the blood are effected by respiration.†

Westrumb relates three experiments which appear to have been carefully performed. He divided the spinal marrow at the first vertebra, and maintained artificial respiration, by which means the temperature of the body was kept at the na-

* *Dissertatio Inaug. circa urinæ secretionem*, Halæ, 1818.

† Croonian Lecture, *Philosophical Transactions*, 1812.

tural standard. He injected into the stomach in the first two cases ferrocyanate of potassa, and into the third rhubarb, neither of which could be detected in the urine, although evidence of their presence in another part of the body was yielded by the proper tests.*

Having found numerous references to the opinions and experiments of Krimer, a German physiologist, on this subject, I feel sorry that I have not been able to obtain more than a brief notice of the conclusions at which he arrives.† They appear, however, so exactly similar to those already given from the thesis of Naveau, that I shall omit their repetition. Drescher likewise, in his inaugural dissertation, having referred to the observations of Krimer, confirms them in every respect.‡ Brachet states that division of the par vagum does not prevent the secretion of urine from being formed, though he adds there was some difference in the quantity and colour of the fluid of two dogs in which he divided this nerve at the same time. After division of the spinal chord in the neck, M. Brachet informs us, that the secretion proceeded, and when he combined this operation with the one last mentioned, the animal lived forty-seven minutes, during which time nearly an ounce of urine was secreted. In several separate instances, he divided all the nervous branches going to the kidneys,

* Journal Complementary, Vol. xvi. † Ibid. Vol. xxv.

‡ Dissertatio Inauguralis de Systemate Uropoëtico.

but still found that a fluid was separated from the blood, of a rose colour, and having a distinctly urinous smell. Again having proceeded as in the last case, he in addition introduced a tube into the renal artery, and then divided the vessel completely upon it, by which means the circulation was perfectly maintained, whilst the continuity of nervous communication was as perfectly destroyed. The animal in this case lived four hours, during which time three ounces of a red liquid had passed into the bladder, which in all respects resembled blood. No wound in the kidney could be detected to account for this. He proved by another set of experiments, that this mode of operating did not suspend the circulation through the organ, for on making incisions into it, in various parts, perfect jets of arterial blood were yielded from the divided surfaces.*

I have thus endeavoured to collect the results of the principal experiments, which have hitherto been used as arguments on the subject under consideration. I have already stated my belief that those first detailed, and which have hitherto been principally dwelt on by physiologists, are too complicated in their results, to allow a positive conclusion to be derived from them. Although the others which are afterwards mentioned, are perhaps better calculated for this end, yet I would not have

* *Recherches experimentales sur les fonctions du systeme nerveux ganglionaire*, par M. Brachet.

it understood that I entertain the belief, that, by any facts hitherto recorded, a positive decision can be arrived at. In bringing this essay to its conclusion, I will first state some of the arguments by which it is determined that secretion is, in any degree, under the influence of the nervous system, and next examine the extent to which this influence is exerted.

I have only to mention the effects of violent mental affections on the various secretions,—as of grief on that of the tears, of fear on that of the perspiration and urine, of the stimulus of savoury odours on the flow of saliva, and lastly, of the increased production of milk in the female breast on the sight of her infant,—to convince the most sceptical that the different processes, by which these fluids are produced, are liable to suffer changes from the action of the nervous system. If these will not suffice, I refer to the results of some of those experiments first mentioned in this essay, which, though I have already deemed insufficient to determine the more difficult part of our enquiry, furnish, I imagine, in themselves abundant evidence that my conclusions in this particular are not incorrect. But it is almost superfluous to proceed further in arguing this part of the question, as I scarcely anticipate that any material objection can be raised against the view I have here taken. Those authors who are quite decided in their disbelief of the necessity of the nervous power in se-

cretion, are perfectly willing to admit the exertion of an occasional influence. Thus Dr Alison, in a very able paper published in one of the numbers of the Journal of Science, says, when referring to some previously performed experiments of Mr Brodie, "when Mr Brodie concludes from these experiments that the suppression of the secretion was to be attributed to the division of the nerves, and that the secretions of the stomach and intestines are very much under the control of the nervous system, his inferences must command general assent."* "Functional secretion," says Mayo, "is to a remarkable degree influenced through the nervous system." † Bostock too, confesses, that "it is sufficiently obvious that the organs of secretion in the higher orders of animals are very much under the influence of the nerves, and in many cases materially affected by them." ‡

But do these secretions depend on some influence communicated by the nerves to the elaborating organs? can they, or can they not, take place without this transmitted agency? The principal objections which have been raised to the possession of this power by any portion of the nervous system will now fall to be considered. The first I shall notice is one to which great importance has been attached by its promulgators; and I must

* Journal of Science, Vol. ix. p. 113.

† Outlines of Physiology, 3d edition, p. 93.

‡ Elementary System of Physiology, Vol. ii. p. 426.

allow, that, if all were perfectly agreed as to its correctness as an ascertained fact, the question might be considered as decided. It is stated that in a considerable number of the lower animals, and in the entire vegetable kingdom, no traces of a nervous system have hitherto been discovered by the most minute anatomical investigations. Thus Bostock declares that, if a good instance of secretion, performed in animals where a nervous system has not hitherto been detected, can be furnished, the controversy is at once decided. If then this distinguished physiologist had followed out this line of argument, used in the former, and still adopted in the later edition of his work, and had cited an instance of this kind, his choice might probably have fallen upon some one of those animals in which a nervous system, truly of a more simple character, has lately been discovered. For since the time when this argument was first advanced, nerves have been distinctly detected in some classes of animals where their existence was not previously suspected, which circumstance proves at once that the controversy considered in this manner is any thing but decided. To this, however, it may be replied, that many still exist to which the remark of Bostock strictly applies; but as we have seen patient and successful investigation overcoming in a great measure this powerful barrier, and, arguing from the uniformity invariably observed by nature in all her works,

we may infer, I think, with considerable propriety, that future well-directed research will establish the existence of nerves in those which remain. Then, in regard to vegetables, independently of the fact of their belonging to a different natural kingdom, in which all the vital processes are performed with greater simplicity, and in which many belonging to animals are altogether deficient, we may state that the analogy is not strictly perfect, and, therefore, although admissible as a corroborative, can scarcely be used as a decisive argument. I have much pleasure in being able to add the authority of Roget in support of the above argument, who, after observing that the organic affinities which produce secretion, and all those unknown causes which effect the nutrition, development, and growth of each part, are placed under the control of the nervous power, goes on to remark, that "as the functions of plants are sufficiently simple to admit of being conducted without the aid of muscular power, still less do they require the assistance of nervous energy."* I make no use of the statement which has, I am aware, been ventured by some vegetable physiologists, that nerves do actually exist in this kingdom, as by M. Dutrochet and M. Brachet, the latter of whom declares his opinion that they belong to the ganglionic system, for the fact of

* Bridgewater Treatise, Vol. ii. p. 357.

such existence cannot, in the present state of our knowledge, be positively asserted. Another argument made use of on this side of the question, is the occasional production of fœtuses, in which a portion or portions of the nervous system have been found deficient, and in which the different parts of the body had attained a full size, inferring justly from this fact, that the various secretions have been duly performed. The instance of this species of malformation most frequently quoted, is that published some years ago by Mr Lawrence.* In this case, the child lived a few days after birth, and amongst others, the secretion of urine was duly performed. Besides this, numerous others have been from time to time recorded, of a similar nature, but in these as in that detailed by Mr Lawrence, more or less distinct traces of a nervous system have generally existed.

Positive experiments are likewise confidently appealed to in support of this doctrine, as, for instance, that of Bichat on one of the testicles of a dog, the nerves going to which he is said to have divided. Inflammation terminating in suppuration succeeded to the operation, which of course, as he observes, obscured the result of the experiment; but he insinuates that the very fact of the formation of matter taking place, would lead him to conclude that the nerves are not necessary to the seminal secretion, as this, the formation of matter, is an

* *Medico-Chirurgical Transactions*, Vol. v.

analogous process. But it may be inquired, did M. Bichat divide those numerous branches of nerves which closely accompany the spermatic artery, and which, if he did not, might suffice for the performance of the analogous function of which he speaks? * Mr Mayo, too, formerly published an experiment, in which he is stated to have divided the nerves of the kidney, after which the natural secretion proceeded. † This statement is, I imagine, sufficiently answered by the very numerous and satisfactory experiments already detailed, ‡ which, from the apparent accuracy with which they were performed, must be deemed more worthy of attention than the single one in question, in the performance of which some fallacy must have existed. The probability is, that Mr Mayo, seeing some fluid collected in the bladder, deemed this a sufficient proof that the secretion had been duly performed; whereas, had he carefully examined the nature of the contents, he would have found them resembling in character that described in the thesis of M. Naveau.

Again, the state of patients when labouring under paralysis has been adduced as a further argument, for it is said that, under these circumstances, secretion is continually performed, and that inflamma-

* Anatomie Generale, Vol. iv. p. 604.

† Outlines of Physiology.

‡ Of Naveau, Brachet, &c. p. 19. et seq.

tion and suppuration may occur, the latter of which, as we have before stated, is termed an analogous process. Instances are also recorded where, in complete paralysis of the lower half of the body, erection and ejaculation of the semen have occurred; and Bichat, when arguing this point, relates the case of an individual in this state having contracted a gonorrhœa. But what do these instances prove? Certainly nothing for the purpose they are intended. To do this it must be allowed that all nervous power was suspended; but the very fact of the venereal appetite existing at all, and influencing the generative organs, denies that this could have been the case.

The progress of the formation of the chick in ovo, has yielded also a very formidable objection against the nervous theory of secretion. It is stated that the secretory processes "go on in the chick before any vestige of brain or spinal marrow can be traced, as also in the early part of the existence of the human foetus, when the brain and nerves appear incapable of performing their functions."* I, however, apprehend that this argument will, in a great measure, be deprived of its weight, when it is remembered that, if allowed, it must likewise be admitted in regard to the formation of the various parts of the animal from the blood; for it is ascertained that the blood-vessels are distinct-

* Alison, in Journal of Science, Vol. ix.

ly formed before any of the fluid which afterward circulates in them can be accurately observed.

But I have arrived at that part of my essay, where I think it requisite to sum up the arguments, which appear to conduce to the establishment of the nervous theory of secretion, as it has been denominated by Bostock.

Bordeu affirms that the number of nerves usually distributed to glands are more than sufficient to ensure the life of the part ; neither, says he, are these organs very sensitive, nor are they endowed with motion, yet, allowing that some are required for each of these purposes, several remain to answer some other end, “ and are they not employed in secretion ?”* Though this manner of stating his argument may be considered as very ingenious, rather than strictly correct, yet I am inclined to attach some weight to the anatomical distribution of the nerves in favour of this theory. It is generally allowed by physiologists, that the secretions are performed by the minute arterial, and not by the venous capillaries. Now, anatomists inform us that arteries and excretory tubes, to their minutest ramifications, are abundantly supplied with nervous branches, particularly of the sympathetic system, whilst they do not appear to be so connected with the veins. † What, then, can be the use

* Recherches Anatomiques sur les Glandes, p. 341.

† Lobstein de Nervi sympathetici humani fabrica, usu et morbis, p. 103.

of this abundant distribution? They cannot serve the purpose of assisting in the circulation, else we might infer that they would be found upon the veins: and Dr Allen Thomson tells us, in an able paper on the circulation, lately published, "that although the circulation in the small vessels is obviously liable to be modified by the state of the nerves in their neighbourhood, or perhaps by affections of the nervous system in general, there is no reason to consider the capillary circulation as more immediately dependent on the nervous influence than is the action of the heart."* The known effects of opium, in suspending the secreting functions generally over the body, will serve likewise in support of this doctrine, more especially when the method in which it acts upon the system is remembered. It is through the nerves alone that this very powerful drug produces its effects; and we find that, whilst the economy is labouring under its action, secretion generally is either arrested, or at any rate materially diminished. Lobstein relates the case of a friend of his own whose hair turned white in a few days, from the great mental anxiety occasioned by the burning of his house; and numerous other instances are to be found on record of similar occurrences. Now the peculiar colouring matter of the hair is allowed to be the result of a secretion taking place in the

* Cyclopædia of Anatomy and Physiology, Article Circulation.

glandular apparatus, seated at the roots, and the manner in which this change was produced can only have been through the medium of the nervous system, and must therefore be regarded as a corroborative proof of the nervous theory. But by far the most cogent arguments, on this side of the question, are those derivable from the very important experiments already detailed, as performed by Naveau, Krimer, Brachet, and others. The first mentioned of these gentlemen, it will be remembered, found that substances introduced into the stomach, as rhubarb, which in the natural state may be easily detected in the urine by well established tests, soon after its administration, could no longer be so on division of the nerves going to the kidney; moreover, that the secretion gradually ceased to manifest its usual properties, until at length nothing but the serous part of the blood, with a little of the colouring matter, was separated; the result merely of a mechanical process of transudation. The result in these cases is the effect of an apparent and sufficient cause, and to me appears inexplicable on any other grounds than those to which it is here referred. The experiments of M. Brachet seem to confirm the above in every respect, and even go further in proof of the point in question; but these will require confirmation by other experimenters before they can be implicitly trusted. If, however, the whole of this series be found to be correct,

and I see no reason to doubt the accuracy of the results of the former part of it, we need, I apprehend, no further proof of the decided part taken by the nerves in the process of secretion. I have hitherto purposely avoided all reference to that theory which has been advocated by Dr Wilson Philip, of the identity of the nervous influence and galvanism, which, though one of an unusually interesting nature, could not have been introduced here, without increasing the thesis to an unwarrantable length. Moreover, the two questions, viz. that which forms the subject of this paper, and the one to which allusion has just been made, are, I consider, perfectly distinct, at any rate in the state in which the latter at present exists. If, however, that identity should in process of time be satisfactorily proved, then these experiments, in which secretion, already suspended by the destruction of the nervous communications, was found to be re-established by galvanism transmitted along the divided nerves of the organ, will come to be powerful arguments on this side of the question. As, however, I have not been able to find sufficient data for the establishment of this fact, and for the reasons above-mentioned, I consider myself justified in having omitted a more lengthened detail of them.

It now remains for me to state the opinions which I have been led to entertain by the study of this subject; but the previous observations contained in

the essay render it almost unnecessary to perform this part of my duty. The evident inclination which I have to adopt the theory of the dependence of the secretion on the nervous influence must have been easily detected. But I feel myself compelled to admit that the subject still requires a considerable share of close investigation, and it has always been allowed to be one of the most mysterious and difficult of elucidation in the whole range of physiological science. The arguments, too, on the opposite side, must, in common candour, be allowed to be most powerful. Yet, nevertheless, I regard the theory which I am thus inclined to adopt as one progressively gaining strength, which it will continue, with the general advance of physiology, to do until established on a firmer basis than that on which it at present exists. In support of this idea we have the recent discoveries of nervous systems in the lower classes of animals, which it must be remembered takes away at every step a strong hold of the opposing theory.

We have likewise the recent, and, if correct, very important experiments of Brachet, which, I imagine, only require confirmatory repetition almost to decide the controversy.

The observations of this physiologist, from some cause or other, which I am at a loss to explain, are viewed with general distrust in this country. This may arise from the results of almost all his experiments, too exactly supporting, what appear

to have been his pre-conceived notions on the subject; but whatever may be its cause, as the results which he has published are, as will have been already concluded, of a very important nature, as connected with our present subject, I cannot help coinciding with the hope expressed in a late excellent review of M. Brachet's work, that "some of our British physiologists will be induced to turn their attention to this department of physiology, with a view of settling, as far as experiment can settle, the doubtful questions connected with it."*

But is not this the proper place for enquiring to what part of the nervous system should this power of superintending, as it were, the secretory functions be ascribed? I believe that this question might be easily avoided, on the ground that it is not strictly included in my present enquiry, the object of that being to ascertain whether such a power does exist, and not to point out its particular location. Thus, however, I am not anxious to effect my escape, but am willing to admit that at present it cannot be answered satisfactorily. Various situations appear to have been indicated for the residence of this influence, as the different observations which have been already noticed in the foregoing pages, and which need not here again be enumerated, will at once suggest. Present appearances, however, favour the idea, which has long been entertained by many, that through the

* Edinburgh Medical and Surgical Journal, Vol. xxxvi.

ganglionic system this power has its means of operation. When, however, the first of these questions shall have been answered, then the second will open out a most inviting field for interesting enquiry.

I have now only, in conclusion, briefly to notice the second part of my subject, and to explain the reasons why its consideration has not formed a more prominent feature in the pages of this essay. Its omission has not been one of accident but rather of design; for regarding, as I do, the process by which the solid textures of the animal machine, when worn out by the continued performance of their various functions, are constantly renewed, one, as essentially belonging to the order of secretion, as that of the urine or saliva, I was inclined to believe that its introduction might, in some measure, complicate the subject, and thus tend to obviate that degree of clearness which it has been my object to attain. I need scarcely adduce proofs of the similarity which I have just mentioned; they are performed by the same system of vessels, and are, as far as is yet known, regulated by the same laws. In this view I am supported by numerous physiologists; thus Haller regarded nutrition, and characterized it as "*omnium simplissima secretio*;" and in this his authority has been deservedly respected.

Few experiments have hitherto been made respecting the direct influence of the nervous power

on this function. Those of Dupuy, Veterinary Professor in the University of Alfort, appear to have been the principal, in which he extirpated the superior cervical ganglia from the neck of a horse. He found that after this operation the animal might live many days, and even weeks, and that the principal effects were obstruction of the pupil, redness of the conjunctiva, general wasting of the body, and an eruption of a mangy nature, affecting the entire surface. Hence he concludes, that these nerves exercise considerable influence over the nutritive functions.* The effects of the different diseases of the nervous system on nutrition, as in paralysis, one of the consequences of which is wasting of the parts affected, have long been noticed, and adduced by some as arguments on this question, but they have on the other hand been explained by "the total inactivity of the parts affected."† Some observations which have been made, among others, I believe, by Magendie, on the eye, in cases in which the part of the fifth pair of nerves supplying that organ has been either divided, or its trunk diseased, have been likewise adduced, in which the first symptom was drying of the conjunctival surface, and subsequent rupture of the cornea from ulceration, terminating in discharge of the contents of the eye-ball. These facts, however, are

* Journal de Medecine, Vol. xxxvii. p. 340.

† Alison's Outlines.

far from sufficiently numerous or authentic to serve as decisive arguments, and, indeed, those last related appear rather to belong to the former part of the essay, and might perhaps have been more properly introduced there, as another proof of the suspension of secretion on injury of the nerves supplying the secreting part. Difficulties of a very perplexing nature present themselves to the performance of any series of experiments for elucidating this question, and it must be rather the analogy, if the similarity for which we have just been contending be admitted, on which arguments must at present, and I imagine likewise in future, be founded, establishing at the same moment, when the merits of the question shall come to be more fully understood, the dependence or independence of the two functions of secretion and nutrition on the action of the nervous influence.

APPENDIX.

EXPERIMENT I.

July 13, 1836. 1 $\frac{1}{4}$ P. M.—Two rabbits, previously kept fasting for twenty-four hours, were fed for half an hour on vegetables, and at a quarter to two the par vagum was divided on each side above the recurrent, and about an inch in length removed. Immediately after the operation the breathing appeared difficult, but soon became more placid, though still evidently affected. In about an hour the respiration again laborious, and attended with a slight noise, which increased up to four o'clock. In the second rabbit, the nerves were exposed, and lifted from their places, but not divided. It appeared quite unaffected by the operation. At 8 P. M. Noise attending inspiration of the first somewhat increased. The second is quite lively. The heart's action in both unaffected from the first. Six hours and a half after the operation both killed and immediately examined.

The stomach of the second, was not fully distended with the masticated food of a dark-green colour; a portion towards the lesser curvature was semifluid, of which some had passed into the duodenum.

In the first the stomach was more distended, and the contents externally of a darker hue, and throughout of one degree of consistence, resembling more nearly the natural state of the vegetable. The entire mass was dry. The duodenum and œsophagus empty.

The lungs in neither case were congested, but if at all, it rather applied to the second than the first. Neither trachea nor bronchi contained any appreciable quantity of fluid.

The bladder in the first much distended. On careful examination the nerves were found distinctly divided.

EXPERIMENT II.

July 16.—Two rabbits, previously starved for twelve or sixteen hours, were subjected at 8 P. M. to the same operation, after being fed as before. The one, whose nerves were divided, suffered from difficulty of breathing, which continued for half an hour, when they were left for the night. No attempts to vomit were observed. Next morning it was found dead.

On examination the stomach was distended, but not fully. The contents were little changed throughout, but on the surface was a layer of lymph-like matter. The œsophagus was partially filled.

The lungs were covered with black patches, and frothy mucus in the bronchi and trachea in considerable quantity. The mucous membrane of

the latter was highly injected. The nerves had been perfectly divided. The second rabbit was not killed, as it would not have afforded a fair comparison.

EXPERIMENT III.

25th July, 7 $\frac{1}{2}$ A. M.—A portion of the eighth pair from each side removed as before without the rabbit being previously fed. Breathing much disordered, and refused food. 8. 30'. Still refuses food; several ineffectual attempts to vomit. 9. A little dandelion leaf eaten, when held close to its mouth. Breathing afterwards more thick, and attended by louder noise. 9. 40'. Loud noise still continues, and refuses food. 10. Respirations regular, but with much noise. 1. P. M. Breathing still with much noise, rather quick. Does not move about. 7. Noise during respiration increased. 7. 15'. Killed by a blow on the head. Contrary to expectation stomach found full of apparently digested food. Lungs natural, containing little mucus. Œsophagus empty.

EXPERIMENT IV.

25th July, 8. A. M.—A rabbit fed after fasting, and nerves divided, without any portion being removed. The cut ends were left, as much as possible, in contact. Respiration much disturbed, accompanied by a gurgling sound. 9. 30'. Respiration laborious with little noise. 9. 50'. Respirations

irregular, without noise. 10. 50'. Appears lively, but breathing irregular. 1. P. M. More lively than either of the others operated on at the same time. Ate some dandelion, which had been accidentally left in its way. 3. P. M. Symptoms little altered, but if at all is more quiet. 4. P. M. No change, has again taken food. 7. 40'. Killed. Stomach quite distended; contents of black colour, unaltered in consistence, but that recently taken quite green. Duodenum empty. Œsophagus filled with green recently swallowed food. Lungs natural in appearance, containing little or no mucus. Trachea empty.

EXPERIMENT V.

25th July, 8. 15'. A. M.—In another rabbit previously allowed to feed freely, the nerves were divided, and portions removed, as in Experiments 1. and 2. Respiration increased in frequency, and accompanied by a slight purring noise. 9. 50'. Respiration quick, and very laborious, but with less noise, chiefly abdominal; one or two ineffectual attempts to vomit. 10. 50'. Much the same as at last report; one or two attempts to vomit. 1. P. M. Remains quiet. Breathing more difficult, but with less noise, still attempting fruitlessly to vomit. 4. Breathing if at all changed more difficult. 8. P. M. Killed.

Stomach found distended. More appearance, however, of digestion at the pyloric extremity. Central portions of the mass quite unaltered.

Duodenum and œsophagus empty. A little mucus in the trachea and bronchia, as also in the lungs.

EXPERIMENT VI.

25th July, 8. 20'. A. M. The nerves having been divided as in the last experiment, the trachea was opened, and a quill tube was introduced and secured. The respiration was, however, more difficult than in the two other rabbits. 9. A. M. Stands with its neck extended, apparently to facilitate the breathing, which is very difficult. The tube removed with some relief. 9. 30'. Breathing more easy. 9. 55'. Breathing again exceedingly laborious, with slight noise. 10. 50'. Vomited a slight quantity of mucus mixed with blood. 1. P. M. Breathing laborious; has again vomited some clear fluid. 3. Breathing continues laborious. 4. Difficulty of respiration increasing. 6. 30'. Died suddenly, having, from accounts given, appeared previously very lively.

Examined at 7. P. M. Stomach much distended, contents covered with a film of grayish white matter. Interior of the mass unaltered. Œsophagus empty. Lungs dark-coloured, much congested. Air-passages filled with a frothy mucus. Duodenum contains a turbid fluid, having the appearance of bile and mucus.

Development of the Organ of Circulation
 in the Embryo and Fetus, as also in the Adult

BY
 JOHN W. COLEMAN, M.D.

IN THE ORGAN OF CIRCULATION

BY THE FACULTY OF THE UNIVERSITY OF
 CAMBRIDGE

BY JOHN W. COLEMAN, M.D.

JOHN W. COLEMAN, M.D.

JOHN COLEMAN & SON, NORTH BRIDGE

PRINTED BY JOHN W. COLEMAN, M.D.

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PRIZE THESIS.

INAUGURAL DISSERTATION

ON THE PRESENCE OF

AIR IN THE ORGANS OF CIRCULATION,

SUBMITTED TO

**The Medical Faculty of the University of
Edinburgh,**

IN CONFORMITY WITH THE RULES FOR GRADUATION,

BY AUTHORITY OF

THE VERY REV. PRINCIPAL BAIRD,

AND WITH THE SANCTION OF

THE SENATUS ACADEMICUS.

BY

JOHN ROSE CORMACK,

PRESIDENT OF THE ROYAL MEDICAL SOCIETY OF EDINBURGH, &c.

AND CANDIDATE FOR THE

DEGREE OF DOCTOR IN MEDICINE.

EDINBURGH :

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HODGES AND SMITH, DUBLIN.

MDCCCXXXVII.

THE HISTORY OF THE

ROYAL BARRACKS

OF EDINBURGH

AND THE HISTORY OF THE

ARMY

OF GREAT BRITAIN

BY

JOHN BURNET

ESQ.

A FINE COPY OF THIS BOOK IS BEING

FORWARDED TO THE

OLD FOR

John Percy Esq
with the author's
best respects.

August, 1827.

"FAIRE LADYE! THAT AYRE IS MOST KILLINGE. IT HETH A
STRANGE AND SUBTILE INFLUENCE O'ER THE HART."

Old Play.

TO THE REV. JOHN CORNACK, D.D.

MINISTER OF STOW.

THIS INAUGURAL DISSERTATION

IS AFFECTIONATELY DEDICATED

BY HIS SON,

THE AUTHOR.

TO

THE REV. JOHN CORMACK, D.D.

MINISTER OF STOW,

THIS INAUGURAL DISSERTATION

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CONTENTS

INTRODUCTION..... 1

CHAPTER I

Examination of the Physiological Changes which take place
in those Cases in which Death is the almost immediate
result of the Admission of Air..... 3

CHAPTER II

Circumstances which modify the Effects. Observations on
Cases in which Death did not take place, or was a secondary
result..... 22

CHAPTER III

On the Cause of the Entrance of Air into Veins divided during
operations; with some Considerations on the Means most
likely to Avert Death in such cases..... 32

CHAPTER IV

Remarks upon the Generation of Air in the Living Body,
especially in the Blood Vessels, with Observations on the
Consequences which may result from its presence there..... 40

CONTENTS.

	Page
Introduction,.....	1

CHAPTER I.

Examination of the Physiological Changes which take place in those Cases in which Death is the almost immediate result of the Admission of Air,.....	3
--	---

CHAPTER II.

Circumstances which Modify the Effects. Observations on Cases in which Death did not take place, or was a secondary result,.....	22
--	----

CHAPTER III.

On the Cause of the Entrance of Air into Veins divided during operations; with some Considerations on the Means most likely to Avert Death in such cases.....	32
---	----

CHAPTER IV.

Remarks upon the Generation of Air in the Living Body, especially in the Blood Vessels, with Observations on the Consequences which may result from its presence there,...	46
--	----

EXPERIMENT

The amount of physiological knowledge derived from experiments on the inferior animals would undoubtedly have been more extended and accurate than it really is had experimenters been more desirous to give exact accounts of what they saw, and less anxious to turn important facts these, and make generalizations of facts. It is in the mental, that the thing which has led some men distinguished men to commit another and a rough more or more error than that in which themselves have now been made—I allude to a practice which the French in particular are chargeable, viz. that of describing their experiments in inflated language, using fanciful and exaggerated expressions in recording the effects of the various physiological agents they have employed; thus rendering their observations in a great measure useless, indeed in some cases it may be said, than useless.

It is unnecessary slaughter of animals might certainly be avoided, were experimenters more careful to record their observations in that plain unvarnished language which alone

INTRODUCTION.

THE amount of physiological knowledge derived from experiments on the inferior animals, would unquestionably have been more extended and accurate than it really is, had experimenters been more desirous to give exact accounts of what they saw, and less anxious to form ingenious hypotheses, and hasty generalizations of facts. It is to be lamented, that this itching after novelty has led some most distinguished men to commit another and a much more serious error than that to which reference has now been made—I allude to a practice with which the French in particular are chargeable, viz. that of describing their experiments in inflated language, using fanciful and exaggerated expressions in recording the effects of the various physiological agents they have employed; thus rendering their observations in a great measure useless, indeed in some cases it may be, worse than useless.

Much unnecessary slaughter of animals might certainly be avoided, were experimenters more careful to record their observations in that plain unvarnished language, which alone

is suited to scientific details. This observation has been naturally suggested by the consideration of the subject of the following Essay.

Bichat exceedingly exaggerated the deleterious effects produced upon the animal economy by the introduction of air into the veins, asserting that a bubble of air introduced into a vein occasioned immediate death. This statement the subsequent experiments of Nysten, Magendie, Dr. Blundell, and others, have proved to be utterly erroneous. Nysten found that a small quantity of all the various gases with which he experimented, including nitrous gas and sulphureted hydrogen, might be introduced into the circulation without occasioning death,* and as will appear from experiments afterwards to be detailed, I have injected large quantities of air into the veins of dogs and rabbits, without the manifestation of any decided effect.

For the sake of perspicuity, it seems necessary to arrange the great variety of topics to which our attention is demanded, under some leading heads, and with this view the following Essay is divided into four chapters, each being to a certain extent a separate Essay.

* Rech. de Physiol. et Chimie Pathol. p. 152. Paris, 1811.

INVESTIGATION,

&c.

CHAPTER I.

EXAMINATION OF THE PHYSIOLOGICAL CHANGES WHICH TAKE PLACE IN THOSE CASES IN WHICH DEATH IS THE ALMOST IMMEDIATE RESULT OF THE ADMISSION OF AIR.

WHEN air or any gas is introduced into the circulation *in sufficient quantity to cause rapid death*, the symptoms which precede the fatal event, are in general pretty uniform, as are also the appearances observed on dissection. The animal suddenly falls down, utters some cries, and speedily expires in convulsions. The most striking and constant phenomenon observed after death, is the presence of air and frothy blood in the heart, and also very frequently in every part of the circulatory apparatus.

There are a number of cases on record of patients dying on the operating table, from air entering a divided vein; and many other instances of sudden and unexpected death in similar circumstances, which were at the time of their

occurrence considered inexplicable, may, probably, with correctness, be referred to this cause; and some instances of sudden death after parturition are now occasionally explained on the supposition of air entering by the uterine sinuses. The not unfrequent occurrence of these untoward events renders our subject one of peculiar interest, independent of that which must ever be excited by the examination of so curious a point in physiology; for the important inquiry is naturally suggested, can nothing be done in such emergencies? But this question cannot be satisfactorily answered at present; for till we have seen the order in which the vital processes are arrested, we cannot hope to be able to discover a method of averting this catastrophe.

The baneful effects of the injection of air into the blood vessels were known to Wepfer and others among the older pathologists,* but it was Dupuytren who first pointed it out as the cause of some of those sudden deaths which take place during surgical operations, and since his observations on this subject have been published, a number of such cases have been described by surgeons in Great Britain, on the Continent, and in America.

Wepfer killed an ox by blowing into the jugular vein, and is believed to be the first who made such an experiment.

As far as I am aware, the physiological effects caused by the introduction of air into the veins, were first made the subject of experiment in this country by Brown Langrish, and described by him to the Royal Society of London about the year 1746. This enthusiastic experimental physiolo-

* The following, among the older authors, have noticed this subject, viz. Wepfer, Redi, Bohn, Vander, Camerarius, Brunner, Harder, Sproegel, Lieutand, Morgagni, Spallanzani, Valsalva, Rudolphi, &c.

gist arrived at the conclusion to which Nysten so long afterwards came, viz. that the circulation is arrested in the heart.

Langrish, upon opening the thorax of a dog, which he had killed in a few seconds by “propelling sulphurous air towards the heart” through the jugular vein, found the right auricle and ventricle greatly distended with air and almost destitute of blood. The cavities of the left side were collapsed. Believing “that the death of the dog was owing to the resistance which the air gave to the return of the blood through the venae cavae, and not to any particular action of the sulphureous fumes on the blood itself,” he threw a similar quantity of pure air into the jugular vein of another dog, and found that death ensued as suddenly as in the previous case.* As there is no account given of the post mortem appearances, we may conclude that they were similar to those found in the first experiment.

In more recent times the subject has been investigated by Bichat, Magendie, Nysten, Piednagel, Leroy, Wing, and others. There is a great deal of discrepancy of opinion among these authors, and we shall now examine the respective merits of the various theories which they have propounded.

Bichat believed that death was occasioned by the contact of air with the brain; Nysten maintains that the arrestment of the vital functions depends upon the distension of the right side of the heart with air—that death commences at the right side, and that the cessation of the contractions of the left is a secondary result. Leroy at one time supposed that emphysema of the lungs is the sole cause of death; but he afterwards modified this opinion, and sug-

* Physical Experiments upon Brutes, p. 152. Lond. 1746.

gested the three following ways in which death may take place from the introduction of air into the veins: 1st, By affecting the sensibility of the brain, or by acting mechanically upon that organ: 2d, By producing sudden emphysema of the lungs: and, lastly, By depriving the heart of arterial blood. Piednagel conceives that a change in the structure of the lungs is the principal cause of death.*

Magendie at one time held that emphysema of the lungs was one of the causes of death; but as he does not allude to it in the most recent publication where he notices this subject, it seems probable that he now agrees with Nysten, especially as he brings forward nothing but what indicates such an acquiescence.

The opinion of Bichat, that the air acts as a poison on the brain, appears to have been the result of hasty or inaccurate observation; an opinion which we seem fully warranted in expressing, when we find this distinguished physiologist stating so egregious an error as that a single bubble of air produces an instantaneously fatal result. This theory is based upon the following facts and observations: 1st, The circulation goes on for some time after the introduction of air into the veins: 2d, Air propelled upon the brain through one of the carotids causes death: 3d, Convulsions precede death: 4th, The venous capillaries are full of blood mixed with air; and, fifthly, He considers the cases of sudden death mentioned by Morgagni, in which air was found in the vessels of the brain, as strengthening his theory.

Subsequent inquiries appear to give almost no support to

* Journ. de Physiol. Tome ix p. 79, et seq

the views of Bichat ; and indeed these statements furnish but a poor foundation for the sweeping theory which he has attempted to rear upon them. Perhaps his strongest fact is, that death is preceded by convulsions ;—phenomena which must unquestionably arise in consequence of an impression made upon the central organs of the nervous system. But then, it may be fairly asked, are not these convulsions *secondary* results ? They occur in almost every case of violent death, and in numerous instances can be clearly traced to a primary affection of the heart or lungs. Leroy alleges that convulsive movements are rarely to be witnessed. It may occasionally happen that they are not seen, but I am perfectly satisfied that, in at least the great majority of cases, they do take place. In most of the cases in the human subject, which occurred during operations, there is express mention made of them, (as well as in experiments made upon animals) ; and when the circumstance is not noticed we have no right to conclude that it did not take place. I have been assured by my friend Mr. Dick of the veterinary school of this place, that although he has killed many horses by blowing air into the veins, he does not recollect of a single instance in which death was not preceded by violent convulsions. But be this as it may, the presence of the phenomenon in question cannot be held as proving that the brain is the organ primarily affected ; for it is obvious, that in whatever manner the supply of blood to the brain is suddenly diminished convulsions follow.

The notion of some, that a sudden emphysema of the lungs is the cause of death, at first sight appears plausible, and is certainly very ingenious ; but upon examina-

tion it will, I think, be found untenable. When a large quantity of air was thrown directly into the right auricle by Piednagel and Magendie, the former states that respiration all at once ceased, and the heart's pulsations became strong and rapid. On dissection, the lungs were emphysematous and the right side of the heart distended with air, the left cavities containing only a little frothy blood.

Piednagel gives the following account of the manner in which he supposes these appearances to be produced. He conceives that the air contained in the air cells, by means of its pressure prevents that mixed with the blood in the minute ramifications of the pulmonary artery from passing onwards; that this resistance which the heart meets with in attempting to propel the blood through the lungs, acts as a stimulus upon it, and by redoubling the force of its contractions, breaks up the tissue of the lungs. It is urged as an argument in favour of emphysema being the cause of death, that air when introduced gradually and in small quantities produces very slight effects. But it is obvious that it by no means follows, that when death does suddenly take place from the entrance of a large quantity of air, it is caused by emphysema of the lungs. In the first place, emphysema is far from being a constant appearance, as will appear from cases and experiments to be detailed; and when found it can be accounted for in a much more simple way than that proposed by Piednagel.

Dr. Wing killed a rabbit in a few seconds by the forcible injection of air into the external jugular vein, and upon opening the chest the lungs were found collapsed; and besides it is stated, that in every respect they were in

a natural condition.* I have suddenly distended the right auricle in the manner described by Magendie and Piednagel, but emphysema of the lungs was hardly ever observed, though the experiment was often repeated. In one or two cases the structure of the lungs certainly seemed somewhat broken up, but this might be, and in all probability was, produced by the convulsive attempts at respiration which preceded death, just as happens in many cases of poisoning with strychnia and other substances.

No one, so far as I am aware, has attempted to shew that the amount of emphysema found in animals killed with air, is sufficient of itself to produce a sudden and violent death. Till this is done, the theory of Leroy, and others on this subject, must be considered visionary, since all the facts with which we are acquainted, are obviously opposed to it.

It is impossible to distinguish between old and recent emphysema, so that in some of the instances in which it has been found, there is reason to suppose that it may have been of some considerable standing. We know that people with very extensive emphysema of the lungs, may attain an advanced old age without necessarily suffering any great inconvenience, but what bears still more upon this point is, that extensive and sudden emphysema is often occasioned by a fit of whooping cough, and yet we see no symptoms produced analogous to those caused by the entrance of air into the veins. From all these considerations, then, the hypothesis of Leroy, Magendie, and others on this subject, appear wholly unsupported by facts, and must consequently be abandoned.

* Boston Med. and Surg. Journ., as quoted in the *Lancette Française*. Mars, 1835.

That the air sometimes proves fatal, by depriving the heart of its supply of arterial blood through the coronary arteries, appears to be a very visionary doctrine, and I cannot recollect any facts which give it even a semblance of truth.*

With a view of elucidating the question proposed for consideration in this chapter, the following experiments were performed.

Experiment.—The subject of this experiment was a horse about seventeen hands high, and in pretty good condition, but which was condemned to death on account of an incurable cancer of the foot. A tube, a quarter of an inch in diameter, was introduced into the left jugular vein. The man who blew, filled his chest twice, and continued to blow for a minute. He then stopped on account of the symptoms of uneasiness which the animal exhibited. In a few seconds the horse staggered and fell, and in three minutes from the commencement of the introduction of air, was quite dead. During the period he survived after falling, he made great and violent efforts to inspire, and during that time, was strongly convulsed, the convulsions commencing soon after he fell. It was computed that he lost about eight quarts of blood, which is the quantity usually taken at an ordinary venesection.

Sectio.—Air was found in every visible vein over the whole body. The chest was opened an hour and a half

* Another very questionable doctrine is that of Pigeaux, who denies that the entrance of air is the sole cause of death, and considers that much of the deleterious consequences may be attributed to the injury sustained by the small nervous branches. Surely this is absurd. *Gaz. Med.* 28 Mars 1833.

after death, when the lungs were seen collapsed, and in no degree emphysematous. All the cavities of the heart, but particularly the right auricle, were distended, and had a tense elastic feel. The right side was first examined. The auricle was enormously distended, and upon a small opening being made with the scalpel, frothy blood, with which this cavity seemed to be entirely filled, instantly rushed out. The greater part of the ventricle was filled with fluid and coagulated blood, but there were also some in a frothed state. The left auricle contained frothy blood, and some coagulated masses. In the left ventricle the quantity of air was just sufficient to make its existence appreciable; but there was a great quantity of blood, both fluid and coagulated in this cavity.*

Experiment.—A tube about equal in diameter to a crow quill was inserted into the right jugular vein of a rabbit. In the course of two minutes I introduced three or four full expirations. During this time the animal lay quite tranquil, and did not struggle in the least, but the breathing was difficult, and the heart's action feeble and fluttering. Just as I desisted from blowing, I observed some slight convulsive movements of the limbs, and in a few seconds more they recurred. Respiration had now ceased, and there were no more convulsions. The thorax was immediately laid open. Great venous congestion everywhere presented itself. The heart was enormously distended. Upon puncturing the right auricle and ventricle, air unmixed with blood issued forth, and in the auricle there was a good deal of

* Dr. John Reid, and Mr. William Scott were present at the performance of this experiment.

frothy blood. The froth was not nearly of so light a consistence as in the former experiment, owing probably to the blood and air not having had sufficient time to be thoroughly agitated together. The left side of the heart contained fluid blood. It is worthy of remark, that the irritability of the heart was almost extinct. Even though rapidly relieved of its load of blood and air, the contractions excited by pricking it with the knife were unusually trifling. In the horse again, the contractions of the heart continued very forcible long after it had been cut out of the body. The lungs were next examined, and were found to be quite healthy. They were collapsed, and in no degree emphysematous. In the vena cava and some of the larger abdominal veins, bubbles of air were observed, but in most of the other vessels examined, none could be detected.*

Since in both of these experiments, the structure of the lungs was in no degree broken up, we must refer the cause of death either to the heart or to the brain. Now, in both instances, the convulsions indicating an affection of the brain, appear to have been secondary results, and the immediate cause of death, the stoppage or derangement of the heart's action, in consequence of an over distension of its walls. It is probably in the agonies of death, and in those cases where the heart's action at that time becomes quickened, or, as is frequently the case, assumes an irregular or vermicular movement, that the frothing of the blood takes place. The appearance is by no means uniform, and I have seen the blood and air wholly unmixed.

The following experiment shews, that air may be thrown

* My friend Mr. Thomas R. Scott assisted me in performing this experiment.

into the jugular vein with great violence, and yet no emphysema be produced.

Experiment.—A tube was inserted in the jugular vein of a dog, and a large quantity of air introduced suddenly and with great force. The time occupied in this process was about six seconds, and at the end of this time, the vein was so distended with air, that no force employed caused any more to enter. I was just proceeding to tighten a ligature round the vessel, when the animal began to struggle, and after uttering some cries, expired in twelve seconds from the time the introduction of the air commenced.

Sectio.—The chest was immediately examined. *The lungs presented their usual appearance.* The heart was greatly distended, and from its engorged state was contracting very feebly. The right side of the heart was full of light frothy blood. The inferior *cava* contained little blood, but was much distended with air, as were to a greater or less extent most of the venous trunks of any considerable size. The coronary veins presented the same appearance as the inferior *cava*. In the veins of the hind legs there were only a few bubbles of air, but it was observed that all over the body there was great congestion of the venous system. There was no air in the left side of the heart. The obvious explanation of these phenomena is, that the air had been sent directly through the right auricle into the inferior *vena cava* and coronary veins; for its absence from the left side of the heart clearly indicates that what was found in the veins had not made the round of the circulation. From this experiment then it appears that very great force may be used without producing any em-

physema, and it may be added as a circumstance worthy of note, that there was but little frothy blood in the pulmonary artery.*

The phenomena both before and after death described as having been seen in those individuals who have died suddenly on the operating table, owing to the accidental admission of air, fully bear out the views now stated. The first case of the kind which attracted general attention, happened in Paris in 1818. It is particularly interesting and valuable, from the circumstance that the appearances detected on dissection were quite different from those expected:—the belief being that the right sac of the pleura would be found full of air.

Beauchere was removing a tumour from the right shoulder. When detaching the last adherent portion with the bistoury, a peculiar sound was suddenly heard, similar to that caused by the entrance of air through a small opening into the thorax of a living animal. It was the opinion of all present that the pleura had been wounded. The patient exclaimed, “*mon sang tombe dans mon cœur ! je suis mort !*”—he became pale, his head fell backwards, the eyes were fixed, and he could not distinguish objects. Respiration was easy but loud, and seemed to be performed chiefly by the left lung; the movements on the right side of the chest being very

* One not acquainted with the appearance usually presented by the lungs of rabbits, might easily suppose that there was emphysema when that organ was nevertheless in its ordinary condition. For what seems to be the healthy state of the rabbit's lungs bears a very close resemblance to emphysema in the human subject. It is therefore not so satisfactory to make this experiment on a rabbit. In the above experiment I was assisted by my friends Messrs. Thomas R. Scott, and William Scott.

feeble. The pulse was extremely small, frequent, hard, and irregular. The whole body was covered with a cold sweat, and he had some convulsions. All restorative measures failed, and the patient died a quarter of an hour after the fatal cut had been given.

On examining the body eighteen hours after death, the chest was found to contain a quantity of red coloured serum. The lungs were free from all disease;—they crepitated and filled both thoracic cavities. There was no opening into the right pleura. On examining the seat of the operation, it was found that the jugular vein had been wounded. In fact, a portion of this vessel (half its calibre, and an inch in length,) had been cut out. The wound in the jugular, terminated just as it joined the subclavian vein. The superior vena cava contained no blood; its internal membrane was red. The pericardium was filled with serosity. None of the cavities of the heart contained any blood. The left side seemed to be in a natural state;—perhaps the ventricle was a little thickened. The walls of the right cavities were flabby, very thin, pale, and of a much greater calibre than those of the opposite side. The brain presented a grey appearance, and all its blood vessels of a size sufficient to be visible, were distended with air. The aorta, crural arteries, and inferior vena cava contained air mixed with blood.

An event of a similar nature to that now detailed, happened at the Hotel Dieu in Paris in 1822, when Dupuytren was removing a tumour from a girl's neck. A sound similar to that heard in the former case, led the operator to remark that had he been cutting in the neighbourhood of the air passages he would have supposed that he had made an opening into some of them. No sooner had he said this,

and at the same time given the last stroke of the knife which concluded the separation of the tumour, when the patient exclaimed, *I am dying*, was seized with a general trembling, and quickly expired.

All the usual methods resorted to for the recovery from syncope and asphyxia were tried without any success, though persevered in for several hours.

Sectio.—The body was examined twenty-four hours after death, and the right auricle was found distended with air so as to give it an elastic tension, and when an incision was made through its parietes, the air escaped in great quantity without any admixture of blood; it nevertheless contained a small quantity of uncoagulated blood. The other cavities of the heart (which were healthy,) the arteries and veins throughout the body, and the membranes of the brain, contained fluid blood mixed with air. The lungs were red, pliable, crepitant, elastic, and perfectly healthy. There was no wound in the trachea. The serous membranes of the brain were thin and transparent, without serosity and without injection. Its tissue was firm, uninjected, and with the colours well marked. Red spots were observed on the stomach, some of them evidently owing to injection of the capillaries. The muscles were firm and red.

In neither of these cases are there any facts of importance to support the hypothesis of Bichat. The presence of air in the vessels of the brain sinks into insignificance, when we discover that it is also found in every minute artery and vein all over the body. The existence of emphysema of the lungs in Beauchere's case, may fairly be considered as purely accidental. In the operation cases detailed, as well as others of a similar nature on record, it was in all probability produced by

the artificial respiration had recourse to. It is almost impossible to perform this process, without to a greater or less extent breaking up the air cells. Both in my own hands and in others more expert, I have uniformly seen some amount of emphysema produced when endeavours were made to recover animals by means of artificial respiration. Perhaps it is from serious injury being done to the lungs, that so few infants resuscitated by this method survive more than a few days; and there is much reason to fear, that the rude apparatus frequently employed by unskilful persons for the recovery of those apparently drowned, is the cause of so little advantage being derived from artificial respiration in such cases.

The following case may be classed along with those of Beauchere and Dupuytren already described. When Mr. Barlow of Blackburn, Lancashire, was removing a tumour from the side of the neck of a delicate lady, and was "proceeding to dissect aside the skin to get at the basis of the tumour, a sudden and unexpected hissing gurgling noise rushed obviously from a large divided empty vein, and the patient expired instantly, without either sigh, groan, or struggle, and every effort to restore animation was fruitless. This unexpected event," Mr. Barlow goes on to remark, "was truly appalling to all present, for scarcely an ounce of blood was lost on the occasion, and her death was then wholly attributed to a state of debility and syncope, which opinion I acknowledge remained unchanged till I accidentally met with the case of Dupuytren."*

A fatal case of a similar nature occurred to Dr. Warren of Harvard University, when removing a cancerous mam-

* Med. Chirurg. Trans., vol. xvi., p. 29.

ma. The vein through which the air entered was the subscapular. As the phenomena preceding death were quite similar to those already described, and as there was no *post mortem* examination, a detail of the case would throw no additional light on the subject, and is consequently omitted.*

There are various circumstances which render it possible that in some instances in which women die unexpectedly after parturition, and when all seems going on well, death is owing to air entering the circulation by means of the open mouths of the veins communicating with the uterine sinuses.† These orifices, immediately after the separation of the decidua, are very large. They have been made the subject of investigation by many modern as well as old anatomists and obstetricians, and upon the whole the various descriptions correspond. Burton says, that the uterine sinuses “in the ninth month of gravitation are so large as to admit the end of the biggest finger; and their orifices that open into the cavity of the womb, will at the same time admit the end of the little finger.”‡

Now the uterus not unfrequently contracts and expands alternately with considerable energy after the expulsion of the fœtus, and it is quite reasonable to suppose that air may sometimes be sucked into the gaping mouths of the uterine

* Medical Gazette, vol. xii., p. 270.—Case of Nancy Bunker.

† Olivier, in the article *Air* in the *Dict. de Med.*, (2nd edit.) asks whether those cases of unexpected death after parturition may not be explained in this way.

‡ *New System of Midwifery*, p. 19. Ed. 1751. Dr. R. Lee claims the discovery of these openings. The above quotation shews that he is in error in doing so. *Vide Med. Gaz.*, vol. xii., p. 202, where priority of discovery is discussed.

vessels, in sufficient quantity to prove fatal to a female exhausted with the fatigues of labour.

Le Gallois mentions three female animals, upon which he was making experiments with a view of observing the effects of abstinence and loss of blood, in which sudden death took place from air entering the circulation by the uterine veins.*

Baudelocque† states, that in examining the bodies of patients who had died of uterine hæmorrhagy in the hospital of *La Maternité*, he constantly found a gaseous substance in the arteries. This he assigns (apparently without any reason) as the cause of the convulsions which precede death. He states that he has no doubt but that the gas is spontaneously generated. This opinion may or may not be correct, for it is at least possible that the air may be admitted from without, since the bleeding depends upon the imperfect closing of these sinuses; and as in cases of uterine hæmorrhagy after labour, convulsive contraction and dilatation of the uterus is not uncommon, the possibility of death being occasioned by the air drawn in by this sucking power is at least entitled to consideration. It must be admitted, however, that with the scanty light which the records of pathology are yet able to throw upon this point, it would be rash to give any decided opinion upon the subject.

In a case recorded by Leclerc (and which occurred in the practice of Dubois and Boyer), we can hardly conceive that the air was drawn in by the process above suggested. Madame B——, a delicate lady, who had for some time suffered from a dull pain in the left inguinal region, was

* Ann. Hebdom. de Med., vol. iii., p. 183. Paris, 1829.

† Traite des Hemor. Internes de l'Uterus, p. 66. Bruxelles, 1832.

suddenly seized with hæmorrhagy from one of the uterine vessels, and died in the course of three hours. Upon examination of the body, various morbid appearances were noticed ; but what particularly demands attention is, that there was no blood in the heart, that the inferior *vena cava* contained air alone, and that the peritoneum was in some places emphysematous.*

There is no mention made as to the interval which elapsed between death and the examination, nor is it stated at what season of the year the event took place ; so that it does not appear with what degree of probability the presence of air in the blood-vessels may be explained by supposing that decomposition of the blood had taken place. It must also be remembered, that in death from hæmorrhagy, it is not unusual to find air in the veins. Mery states, that if the blood of an animal be drained out from an opening in the inferior *vena cava*, the veins fill with air in proportion as they empty of blood, and that the air comes from the smaller into the larger veins.† Nysten says that this is by no means a constant result, and that it depends upon the size of the opening made in the vessel.‡ I have repeated Mery's experiment, but found no air at all in the circulation, though the aperture in the vein was large.

Another question suggested by the above considerations is, whether air generated within the body during life is not sometimes the immediate cause of death ; but this topic is not here discussed, because it appears that it may be more naturally attended to afterwards.

* Archives Gen. de Med., xviii., p. 281.

† Mem. de l'Acad. des Sciences, an. 1707. p. 167.

‡ Nysten, op. cit. p. 4.

In conclusion then, it may be stated as clearly established, that when a quantity of air, by entering the circulation, proves suddenly fatal, the immediate cause of the arrestment of the vital functions is the inability of the right side of the heart to contract and expel its elastic contents; and therefore, all the phenomena which follow are consequences of this first cause.*

* Accidents in consequence of the entrance of air into the circulation, have occurred to the following operators: Sir A. Cooper; Mr. Barlow of Lancashire; Mr. Simmonds of Manchester; Dupuytren, Beauchere, Clemot, and Roux, of Paris; Drs. Mott and Stevens of New York; Dr. Warren of Harvard, U. S.; Dr. Castara of Lunéville; Delpech of Montpellier; Graefe of Berlin; Goullard of Lyons; and doubtless to many others, by whom the cases have not been put on record. Indeed it is surprising, that so many of these accidents should have been published, during the comparatively short period that has elapsed, since Dupuytren first turned the attention of the medical world to the subject.

CHAPTER II.

CIRCUMSTANCES WHICH MODIFY THE EFFECTS. OBSERVATIONS ON CASES IN WHICH DEATH DID NOT TAKE PLACE, OR WAS A SECONDARY RESULT.

It has already been stated, that very considerable quantities of air may be introduced into the circulation without producing death. The concurrent testimony of a variety of experimenters establishes this beyond the possibility of doubt. Sometimes the patient to whom the accident happens, or the animal experimented upon, suffers only transient inconvenience, or makes a complete recovery, and in other instances death ensues so late as some days after the admission of the air. Two very interesting subjects of investigation are thus presented to our notice. In the first place, what are those circumstances which thus modify the result? And then, what is the nature of the modifications which occur under these varying circumstances?

First, then, let us attend to the different modifying causes. These appear chiefly to be referable to three heads: *1st*, the quantity of air admitted;—*2nd*, the rapidity of its admission;—and, *3rd*, the situation of the orifice through which it enters. For the sake of perspicuity, each of these points will be separately illustrated.

1st, That *the result is modified to a very great extent by the quantity of air admitted*, it is unnecessary to insist upon at any length. The experiments of Nysten, Magendie, Wing, and others, clearly shew that it is only when introduced in considerable quantity that there is a fatal issue. This I have satisfied myself of by repeated experiments. The same fact has also been stated by Dr. Blundel, in his memoir on the transfusion of blood. It is probable that when slowly introduced, the oxygen is either in whole or in part absorbed, and the volume of the elastic fluid thus materially diminished. That such absorption does really take place there can be no doubt, for the experiments of Dr. Christison distinctly prove that the oxygenation of the blood is a purely chemical process, and that even out of the body venous blood absorbs a large quantity of oxygen, and changes its purple for a florid hue.*

Nysten infers from various experiments that some of the injected gas may be thrown off by the lungs.

It appears, however, that a large quantity of air may enter the heart, and nevertheless no such phenomena be manifested, as took place in those cases and experiments already detailed. This statement is founded upon the following

Experiment. Six or seven full expirations were injected through a small tube into the jugular veins of two dogs. A little difficulty of breathing ensued, but this soon passed away. At the end of half an hour the animals were killed, having up to that time seemed pretty lively, and exhibited

† Edinb. Med. and Surg. Journ. Jan. 1831.

almost no symptoms of uneasiness. Upon examining the chest immediately after death, the lungs were in both instances found collapsed, and devoid of emphysema. The heart particularly in one instance was greatly distended with air, and had an elastic feel. In both instances, when the scalpel was thrust into each of the cavities of the heart, a little air unmingled with blood rushed out with a whizzing noise. Not the slightest trace of frothing could be detected—a circumstance which by the way, it may be remarked, tends to corroborate the notion formerly suggested, that the frothing of the blood is caused by the irregular motions of the heart which sometimes immediately precede or follow death.*

In the experiments described in the first chapter, almost the whole calibre of the vein was occupied with the tube, and death ensued in a very few minutes, whereas in the cases of the two dogs just detailed, a good deal of blood was allowed to flow down the vessel along with the air, and hence it was, I apprehend, that the baneful effects were so little apparent.

2d, It may be stated as a general conclusion, that *the greater the rapidity with which the air is introduced, and the greater the diameter of the tube through which it passes, so much the more sudden and deleterious is the result.* This appears obvious from various statements already made, and it is therefore unnecessary in this place to supply additional illustrations.

In those cases of operation in which a fatal termination almost immediately ensued, it seems evident, that owing to

* My assistants in performing this experiment were Drs. J. Y. Simpson, and J. H. Pollexfen, and Mr. William Scott.

a thickening of the coats of the vein, or from other causes afterwards to be stated, the vessels were unable to collapse; and if thus kept in a patulous state, it is easy to conceive how rapidly the right side of the heart would become distended with air; for it is clear, that when a vessel in such a state is cut across, air, and air only, would pass down it.

The effect produced *when great force is used in blowing*, is well illustrated by the experiment upon a dog, mentioned in the first chapter, (p. 13.) In that case, the right side of the heart was suddenly and violently distended with air, and death took place with the greatest conceivable rapidity.

It was probably owing to the great force used, that the following experiment proved fatal; for certainly the small quantity of air injected was not of itself sufficient to produce the result. M. Bassereaux, when assisting Dr. Bretonneaux to inject pus into the jugular veins of dogs, happened upon one occasion, imperfectly to fill the syringe. The canula was introduced into the vein, and the piston pressed down; but before the liquid had entered the circulation, (and dissection shewed that not a drop of pus had been injected) both gentlemen heard a distinct "*gargouillement*." The experiment was instantly stopped, and *in half a minute the animal fell down* on its side, in strong convulsions. The respiration was laboured and stertorous, and *death took place in two minutes*. Upon dissection, *only a few bubbles of air were found in the right auricle*, which was almost entirely gorged with blood. The principal object of mentioning this case is, that unless explained in some such way as has now been done, it might possibly be adduced in favour of the doctrine of Bichat.

3d, The only other modifying circumstance proposed for

consideration, is *the situation of the orifice through which the air enters.*

Magendie made the curious discovery, that when air is injected into one of the branches of the *Vena portæ*, no inconvenience seems to result from the operation. This must obviously depend upon some change which the air undergoes in its passage through the portal circulation. It is either absorbed, or so intimately mixed with the blood, as to be rendered incapable of causing any impediment to the circulation.

Experiment. I threw a quantity of air into one of the mesenteric veins of a rabbit, and in eight minutes afterwards killed the animal. The liver was in a state of almost complete anæmia, and upon slicing it, minute bubbles of air appeared at every point on the incised surfaces. There was no air apparent in the heart, nor in any part of the body, except the liver.*

Having thus very briefly considered the principal circumstances which modify the effects resulting from the admission of air into the circulation, it still remains for us to attend to the precise nature of the modifications presented under these varying circumstances.

We have seen that quantity modifies the result to a very great extent; but to what extent I am unable to state, as I have killed one animal with much less than was required to destroy another, apparently similar in strength and size. As a vast number of experiments would have been necessary, before any thing like a satisfactory average result

* Assistants in this experiment—Mr. Thomas R. Scott, and Mr. William Scott.

could be obtained, I was not long in perceiving the necessity of (in the meantime at least) abandoning this part of the investigation. But this was done with less reluctance, as the point did not appear to be of very essential importance, and seemed to be one which, whatever trouble might be bestowed upon it, could after all only be elucidated to a certain extent.

Very violent effects may be produced by the injection of air into a vein, and yet the animal operated upon may spontaneously recover. For example, Dr. Wing threw by degrees a considerable quantity into the jugular vein of a sheep. At each stroke of the piston of the syringe, a gurgling sound was heard in the heart, there was slight difficulty in breathing, and strong convulsions. The experiment occupied ten minutes. When the animal was released at the end of this time, he manifested a desire to eat, and mixed with the rest of the flock.* Similar experiments have been previously described by Nysten and others.

Those cases which prove fatal after the lapse of some days, afford a most interesting and important subject of inquiry. In some instances death is owing, as Nysten's experiments prove, to an affection of the lungs. This able experimenter states, that when the air is not in sufficiently large quantity to put a stop to the vital functions, by arresting the contractions of the right side of the heart, it is forced into the minute ramifications of the pulmonary artery, thus producing such obstruction as causes bronchitis, which may terminate in death ; and it appears that this event sometimes happens so late as the third or fourth day. In a case belonging to this class, the following are the *post mortem*

* Boston Med. and Surg. Journ., as quoted in *Lançette Française*, Mai 1835.

appearances described, the examination having been made two hours after death. The pleuræ presented their natural appearance, but the lungs were of a greyish colour, mixed with brown spots, somewhat gorged with blood, and much distended with frothy mucus. There was not a single bubble of air in the heart, nor in any of the blood vessels. Both ventricles contained blood, and in these cavities there were small yellow semi-transparent concretions. Nysten observed a similar state of the lungs in repeated experiments of the same nature.*

I was anxious to verify these experiments of Nysten, but was never lucky enough to obtain a case at all resembling those which he details as perishing from secondary thoracic symptoms, though I had several instances of spontaneous restoration to health, after the injection of a large quantity of air. There was one rabbit in which I fully anticipated something such as he describes, but complete and speedy recovery took place. This is an account of the

Experiment.—A large quantity of air was injected into the jugular vein. Very violent effects were produced, so violent indeed, that the animal seemed for twenty minutes to be in a moribund, or at least in a very critical state. To our astonishment, however, it gradually came round, and in about an hour the respiration, which was at first very laboured, became tolerably natural, and the rabbit was soon little out of sorts. In a few hours afterwards the breathing, as far as I could judge, was quite natural, and the animal ate food with avidity. I kept it for sixteen days, and never observed anything wrong, excepting an abscess which formed in the seat of the operation.

* Nysten, op. cit. p. 36, &c.

Upon dissection, immediately after killing it, the lungs were found to be perfectly healthy, and there was no air in any of the blood vessels.*

It is the opinion of some, that death may take place after the lapse of some days from the time the air has entered the vessels, and yet be attended with all the striking phenomena usually presented when the fatal event is an immediate consequence. I am not aware of any facts which at all justify such a supposition, but since the notion is entertained by some, it is proper to mention it in this place.

The following case is interesting, though unfortunately it does not throw any very positive light upon this part of the subject, since, by assuming either view as correct, the phenomena may be satisfactorily explained. It is as much, therefore, with a view of shewing that although the most formidable symptoms have been manifested, and immediate death seems almost inevitable, the patient may notwithstanding speedily rally; thus pressing upon our attention the important fact, that if the heart be in any way sufficiently disencumbered of the air, it may regain its natural action, and the circulation, after having been all at once violently disordered, and almost arrested, may in a few minutes go on as before the accident. This points out the important practical bearing of our inquiries, and holds out a good ground of hope that some useful rules may be suggested, by an attentive consideration of this branch of the subject.

But to proceed.—M. Roux, when removing a tumour situated on the cellular sheath surrounding the common carotid artery, jugular vein, and pneumogastric nerve,

* I was assisted in this experiment by Mr. T. R. Scott, and Mr. W. Scott.

made an opening into the jugular vein. Upon inspection after death, it was found that the vein had been wounded transversely, and that on account of the morbid thickening of its walls, the inferior aperture was gaping. At the moment the operator lifted up the tumour to enable him to dissect it out, a sort of whistling noise was heard, like that produced by the entrance of air into the empty receiver of an air pump. The patient uttered a plaintive cry, and became greatly agitated—the contractions of the heart were hurried, the pulse was weak, respiration long and laboured, and at length, after one protracted inspiration, followed by a short and hasty expiration, symptoms of approaching death were manifested. By preventing the admission of any more air, and the employment of frictions, and stimulants to the nostrils, and dashing water on the face, the circulation and respiration were restored. The dissection was discontinued, and the tumour enclosed in a double ligature. All went on well till the seventh day, but on that morning the patient suffered from oppression, difficulty of speech, then from coma, and died during the night.

Sectio. The eighth pair of nerves were uninjured. Both lungs were slightly congested, and emphysematous spots were observed under the pulmonary pleura. The cavities of the heart were empty. On puncturing the abdominal and thoracic aorta in different places, a great many bubbles of air escaped, mixed with bloody serum, and the same could be observed, but in less quantity, in the iliac arteries.* There was no air in the vessels of the brain.

The narrator of this case asks whether the non-occurrence

* Abridged from the Dublin Journ. of Med. and Chem. Science, vol. iv. p. 475. Vide also *Fuyderat's* account of this case in the Journ. Univ. et Hebdom. Tome xi. p. 165.

of death till the seventh day was owing to the small quantity of air admitted during the operation, or to a fresh quantity having got in immediately before death, owing to the inferior opening of the vessel still remaining gaping on the surface of the wound. He conceives that the frothy serum found in the bronchial cells, and the emphysematous spots noticed under the pulmonary pleura, render the latter supposition plausible, but even granting that the emphysema was produced by the air which entered the wounded vein, it is just as likely to have been caused at the operation as afterwards, and as for the frothy serum, it is by no means an unusual *post mortem* appearance in most diseases. The absence of the laboured breathing and other characteristic phenomena described as ushering in death, from the admission of air into the veins, throws a good deal of obscurity over the nature of the case, and appears to render it highly probable that there were some causes operating which do not appear in the narrative. Might not the oppression and coma depend on something quite different from air in the circulation, such as phlebitis, or inflammation of some important organ? Be that as it may, the air might have entered, owing to the slipping of the bandages, immediately before, or to a certain extent during the death struggles.

It appears, at all events, that although sudden death does not follow the admission of air, and immediate danger be averted, there are disasters still to be dreaded, the most important of which are inflammation of the lungs, and the admission of more air, to which may perhaps also be added the danger of phlebitis, in consequence of the injury done to the vein in the operation, or owing to the violence of the measures adopted to prevent immediate death.

CHAPTER III.

ON THE CAUSE OF THE ENTRANCE OF AIR INTO VEINS DIVIDED DURING OPERATIONS ; WITH SOME CONSIDERATIONS ON THE MEANS MOST LIKELY TO AVERT DEATH IN SUCH CASES.

THE only condition absolutely necessary for the admission of air during an operation is, that the vein be kept in a patulous state, for when in this condition, the movements of the chest in respiration (assisted perhaps by the tendency in the right auricle to form a vacuum during its diastole) are sufficient to cause the air to enter. This may at least be inferred from Sir David Barry's experiments. He shewed, that during inspiration a suction influence is exerted upon the blood in the veins entering the chest. Now when this suction is exerted upon the contents of a wounded vein, *incapable of collapsing from disease of its coats or any other cause*, these contents, whatever physical properties they present, must be under the influence of the same law which was pointed out by Sir David Barry with regard to the blood in unopened vessels. Should the vein be completely severed, it is evident that air, and air only, will pass down to the heart ; but if, on the other hand, it be merely wounded, there will be a mixture of blood and air.

The patulous state of the vein may depend on two causes :—1st, The vessel may be gaping in consequence of a rigidity of the coats caused by disease; or, 2nd, It may be kept open by the constrained position of the patient, or the manipulations of the operator and his assistants. M. Berard, I am aware, ascribes the phenomenon chiefly to the anatomical peculiarities of the vessels, independent of disease.* He has shewn that the large veins near the heart have fasciæ closely adherent to their coats, and that these fasciæ are attached to bones. This connexion, he believes, is of such a nature as to prevent the collapse of the veins. One decided objection to this explanation is, that the entrance of air by no means always follows a wound of these vessels; and seeing that the admission of air is not a necessary consequence of a wound of a vein provided with such a fascia, how can it be established that this fascia is in any one instance the cause of the entrance of air? The anatomical facts, then, pointed out by Berard cannot be regarded as at all explaining why veins sometimes remain uncollapsed, in spite of the suction power of the heart and chest, and the influence of atmospheric pressure; but they may, however, be viewed as to a certain extent favouring the occurrence of the phenomenon.

It is not, however, only by the great vessels in the neck that air has been known to get in. Air entered the saphena of a patient upon whom Dupuytren was operating; and when Clemot was removing a tumour from the breast of a female, it entered a divided vein, and in consequence of this, the woman died a few hours after the operation. Upon dissection, the right side of the heart was ascertained

* Archives Générales de Méd. Juin., 1836.

to be distended with air.* “ In an attempt which I made,” says Dr. Mott, “ to remove the parotid gland in an enlarged and schirrous state, the facial vein where it passes over the base of the lower jaw was opened, in dissecting the integuments from the tumour in the early stage of the operation, before a single artery was tied. At the instant this vessel was opened, the attention of all present was arrested by the gurgling sound of air passing into some small opening. The breathing of the patient immediately became difficult and laborious. The heart beat violently and irregularly, his features were distorted, convulsions of the whole body soon followed to so great an extent, as to make it impossible to keep him on the table. He lay upon the floor in this condition for near half an hour, as all supposed *in articulo mortis*.

“ As the convulsions gradually left him, his mouth was permanently distorted, and complete hemiplegia was found to have ensued ; an hour or more elapsed before he could articulate, and it was nearly a whole day before he recovered the use of his arm and leg.”†

In most cases on record, we may infer, from the descriptions given, that there was a diseased state of the coats of the

* *Lançette Française*, 30th Nov. 1830. Clemot there details the particulars of two other accidents of a similar nature, which occurred in his own practice. In one case he was dissecting out a tumour from the axilla, and in the other tying the subclavian artery. Goullard of Lyons, when removing indurated glands from the axilla, wounded the axillary vein. Only a small quantity of blood was lost ; but in an instant the patient became pale, the muscles of the face were convulsed, hiccup came on, which in a few minutes was followed by death. There was *no post mortem* examination. *Duplat Gaz. Méd.*, Dec. 1833.

† *Med. Chirurg. Journ. Lond.* vol. xvi. p. 33.

vessels ; but at the same time, a peculiar and constrained position of the parts might, as has already been stated, keep a vein in such a state as to allow of a large quantity of air being sucked into it. Such is the explanation which must be given of the appearances observed in the case of a student who committed suicide in this city some months ago, by cutting his throat. There was almost no blood lost ; and upon examination, the heart was found to contain air in all its cavities. I am informed there was no thickened condition of the veins of the neck observed.

In the transfusion of blood, air may be, and doubtless often has been injected into a vein ; but if the operation is performed with very ordinary dexterity, it is difficult to imagine how a quantity sufficient to produce any disastrous consequences could be thrown in. Dr. Blundel, who has carefully examined this point, says, that “it seems probable that the entrance of a few drachms of air into the vessels would be attended with considerable distress, and even danger ; but it must be recollected, that if the operation be carefully performed by a competent person with a proper instrument, there can be no risk lest air should enter the vessels in large quantities ; and the probability is, that a bubble or two of air would occasion little if any inconvenience.”*

The interest and importance of a physiological investigation are greatly enhanced, when we enter upon it with a hope of being able to deduce useful rules of practice from our observations, and it is the belief of the author that his subject is possessed of this attraction. It has appeared,

* Ashwell's Practical Treatise on Parturition. Appendix by Dr. Blundel, p. 538. London, 1828.

from what has already been advanced, that the deleterious effects resulting from the entrance of air during operations is purely mechanical, and that the death of the patient depends simply upon the contractions of the right side of the heart being arrested, or greatly impeded, in consequence of the presence of that elastic fluid. Hence it is naturally suggested, that the means most likely to afford safety to the patient will consist in relieving the heart with the greatest possible expedition—an inference, it may be remarked, which points out the practical importance of studying the order in which the vital functions become arrested.

We are also strengthened in the hope, of being frequently able in this way to save the patient, by a knowledge of the astonishing advantages which result from opening the jugular vein in certain cases of poisoning, in which the deleterious agent produces a sudden but merely transient arrestment of the contractions of the heart, and where death seems to result from this state being rendered permanent, in consequence of the distention which has taken place during the period of temporary inaction. We generally find that in such cases, by timely relieving the congestion of the heart, its contractions are in the course of a few seconds renewed, and speedily go on with nearly their usual regularity; but if this be not done, the animal generally dies very quickly, the circulation being greatly impeded, or wholly arrested by the mechanical distention of the heart with blood. The intervention of the valve does not prevent a reflux current, as might be supposed, so that this important principle admits of easy application, both in cases of engorgement with blood, as well as in those in which air is the distending agent.

Haller, both in his *Physiology*, and in his treatise on the *Movement of the Blood*, distinctly states that the right side of the heart can be emptied by opening the jugular vein. He believed this to be entirely owing to derivation of blood, and as in no way dependent upon the contractions of the heart. This view was also adopted by Spallanzani, who repeated the experiments of Haller. The flow of blood from vessels opened at a distance from the centre of the circulation, certainly appears to depend entirely upon derivation; but when large veins near the heart are opened, there seems also to be another cause in operation. This was first stated by my friend Dr. John Reid, in a paper which he published in the 127th number of the *Edinburgh Medical and Surgical Journal*. He conceives that the unloading of the heart, when the jugular vein is cut into, depends upon two causes; one of them being the derivation of Haller, and the other the contractions of the right side of the heart. In cases of extreme engorgement, the latter cause can only come into operation, as a consequence of the former.

“At each contraction,” Dr. Reid remarks, “the heart attempts to force a certain quantity of blood along the vessels connected with it, and as there is no *vis a tergo* to prevent the action of the heart moving the blood along the veins in its immediate neighbourhood, a certain quantity is forced out through the opening in the jugular.” Of the accuracy of this I am perfectly satisfied, having witnessed the phenomenon in the experiments detailed by Dr. Reid, and also in the cases of different animals which I have since killed in various ways.

I have endeavoured by a detail of experiments to show the advantage resulting from a copious depletion by the ju-

gular vein, in cases of poisoning with Creosote.* I have since extended the inquiry to other poisons, and in particular have made farther observations for the purpose of elucidating this point more fully with regard to Creosote and Prussic acid—substances which in large doses unquestionably prove fatal, by producing a sudden arrestment or derangement of the movements of the heart.

Since the cause of death, both in poisoning with these substances, and from the entrance of a large quantity of air into the right side of the heart is the same, viz. the inability of the heart to overcome the distention, it does not seem inappropriate to detail the following

Experiment, which illustrates most beautifully the beneficial effects of copious depletion, when a poisonous dose of Prussic acid has been administered. A free opening was made in the jugular vein of an ordinary sized pointer dog, and hemorrhage prevented by means of pressure. A dose of Prussic acid was then administered, of such strength as to contain about a drop of the real acid. The animal became almost immediately affected, uttered some faint cries, in twenty seconds fell down and lay for a few seconds motionless on the floor. He then began to struggle as if in the agonies of death; but the vein now broke out, and the blood flowed in a rapid and copious stream. Immediately the dog shewed signs of returning vigour. He raised his head, then in a few seconds looked around him, and his eyes, which were formerly dim and suffused, regained their natural lustre. From this time he rapidly recovered, and be-

* Treatise on Creosote, pp. 84, 85, and 92. Edinburgh, 1836.

gan to lick up his own blood. At the end of an hour he did not seem much out of sorts, and was then killed, the experiment being regarded as perfectly satisfactory. *

That it really was so, I have not the slightest doubt. I have seen at least fifty or sixty dogs killed outright with Prussic acid—with doses often smaller, but seldom larger, than a drop of the real acid. Much of this slaughter was also witnessed, (about eighteen months ago,) at the police office of this city, by my friends Dr. J. Reid, Dr. J. Y. Simpson, and Mr. Skae, as well as by various others. Never did we there see such a recovery take place as that which I have now described, and it was only strong and powerful animals that survived the administration of the poison more than a very few minutes.

Experiments similar to the above were made with Creosote, and the results were almost identical with those observed when Prussic acid was employed. †

In conducting this investigation, it was found essential to the success of the operation that the blood should be very speedily and copiously abstracted; and this remark is equally applicable to the relief of the heart from air—that

* In the performance of this remarkable experiment, I had the assistance of my friends, Dr. Pollexfen, and Mr. William Scott.

† There are cases of blows on the head, (as is stated by Dr. J. Reid, in the paper already quoted,) in which the contractions of the heart are not renewed by unloading its right side. It must be borne in mind, however, that in this class of cases many anomalies present themselves, depending apparently, to a certain extent, upon the degree of shock sustained by the nervous system; but it is to be explained more fully, I think, by reference to a fact which must strike every one who performs experiments upon animals,—that the persistence of the contractility of the heart in individuals belonging to the same species, (and killed in exactly the same way,) varies very much in duration.

is, it must be done immediately and thoroughly. Nysten says, that he recovered two dogs from a moribund condition, by forcing the air out of the heart, through an opening in the jugular vein, by means of pressure on the thorax. This is certainly a very clumsy mode of proceeding, and I should think, even in dogs, not always likely to succeed. At all events, in the human subject, such a method could be of little or no avail. The recommendation here given, (which is also that of Magendie,) is certainly that which the operator ought, if possible, to follow when the untoward accidents occur, of which frequent mention has already been made. Magendie recommends the air to be drawn from the heart by applying the mouth to the divided vessel, or by pumping it out with a syringe, if that instrument should happen to be within reach.*

It is evident that the surgeon ought not, even for a few seconds, to delay his attempts to relieve the heart; and that, consequently, in most cases, he will, if he is anxious to proceed with the necessary expedition, have recourse to sucking with his mouth.

Though convinced of the advantage which results, in the majority of cases, from the removal of the air from the heart, I must admit that in a few instances, my attempts to recover animals in this way proved abortive. This ought, probably, to be attributed to too large a quantity of air having been introduced, or to some other accidental circumstance, producing a state beyond that of imminent danger, from which it was possible to relieve the animal. In spite of these unsuccessful efforts at resuscitation, I can confidently state, as a fair deduction from experiments, that

* *Leçons de la vie*, p. 60. A Paris, 1836.

in general, the method recommended, if promptly had recourse to, and judiciously conducted, will prove successful.

I think the experiment more generally succeeds when, along with the air, a considerable quantity of blood is evacuated. In support of this opinion, the following case is subjoined: When Bouley Junior, veterinary surgeon at Paris, was bleeding a horse for pneumonia, having ceased to compress the vein, he heard a peculiar sound, which, however, did not particularly attract his attention, as he had on former occasions remarked a similar occurrence. The operation was concluded, and the animal sent back to the stable, where it was immediately seized with general trembling. Respiration became laboured and plaintive, the pulse small, irregular, and very quick; and then we are told, that groaning deeply, he fell down as if struck by a thunder bolt. For sometime Bouley was unable to account for these symptoms; but upon recollecting the sound which was heard when he relinquished his pressure of the vein, he concluded that it might be owing to the entrance of air. Under this impression, the vein was caused to bleed afresh. As the blood flowed the animal revived; and in half an hour after the accident, he was restored to the state in which he was prior to the operation, and in three days resumed his usual work. Magendie states, that an analogous case happened to Girard at the veterinary school of Alfort.*

The advantage which resulted from the loss of blood in this case is very obvious; and may not recovery in the following instance be traced to a similar cause? When Mr. Simmonds was removing a large tumour from the left

* Journal de Physiol. Tome i. p. 197.

side of the neck of a woman in the Manchester Infirmary, in 1791, he divided the internal jugular vein. "The torrent of blood," he says, "pouring out from so large a cavity, and the noise occasioned by the rushing in of air, added to other appearances, formed a picture more frightful than I ever beheld." The patient recovered; and there is nothing said about the injurious consequences of the admission of air; from which we may presume, that none were apparent. It is merely stated, that the patient experienced no morbid affection of the head from the obliteration of the vessel.*

It naturally occurs, when reflecting upon the practical inference to be drawn from these cases, that the advantage to be derived from the abstraction of blood from the vessel which has been divided, may be prevented, or that even greater danger may be incurred, by hazarding the admission of an additional quantity of air into the right chambers of the heart. If the vessel is gaping, owing to disease of its coats, perhaps the safest plan would be to open the largest healthy vein in the neck which may be within reach; for from what has already been stated, the chance of saving the patient depends almost entirely upon the speedy and effectual unloading of the heart. Should the air have gained access, merely from the temporary gaping of the vessel, occasioned by the position of the patient during the operation, there can then be no objection, not only to sucking the air out by it, but also to giving the additional relief to the heart, which would be afforded by the derivation of blood, and the contractions of the auricle.

* Medical Facts and Observations, vol. viii. p. 23.

If the state of the parts render it necessary immediately to apply pressure to the wounded vein, and should it be impossible or inexpedient to attempt relief, by opening any of the great vessels near the heart, the next best thing which can be done, is to relieve the circulation by some less direct method, which may be readily accomplished either by venesection at the bend of the elbow, or by opening the temporal artery. Dr. Warren, of Harvard University, has published a case in which there is every reason to believe that the patient was saved by the latter operation. When this gentleman was removing a cancerous tumour from the left side of the face and neck, in a case in which all the neighbouring tissues were involved in the disease, a very distinct sound was heard resembling the passage of air through water. The patient became faint, his countenance livid, respiration stertorous, and convulsions were observed. The wounded vessel (which was a small vein running across the neck) through which the air had rushed, was immediately compressed, and the temporal artery opened, when the blood issued forth in abundance. During the first twenty minutes there was a very marked abatement of the alarming symptoms. He continued, however, in a state of insensibility, for two hours and ten minutes longer, when he awoke, as if from sleep, and on the following morning was in his usual state, with the exception of some soreness over the thorax, and a headache. The operation was afterwards completed; and when the wound had nearly healed, the patient was dismissed at his own request.* The time during which he was in the hospital after the accident,

* Medical Gazette, vol. xii. p. 269, where Dr. Warren's paper from the American Journal of Medical Science is given.

is not stated ; but it must have been considerable, since it appears that seven days intervened between the first and second operations.

Artificial respiration, frictions, the application of stimuli to the nostrils, and especially the cold effusion, may in certain cases be used as subsidiary means ; but they are certainly not entitled to that primary importance which is generally assigned to them by those who have written on this subject. No time ought to be trifled in blowing and rubbing, but the root of the evil should be at once laid siege to.

Warren has very strange notions regarding the treatment to be adopted. After recommending both the external and internal use of ammonia, dashing cold water on the face, the introduction of a tube into the glottis, or through an aperture between the thyroid and cricoid cartilages, for the purpose of carrying on artificial respiration, and so on, he proceeds to say—"an attempt to pump the imbibed air from the heart from the internal jugular vein, by means of a syringe, is an operation that cannot be recommended, since it appears more likely to allow the entrance of a farther quantity of air, than to abstract that already admitted." In reply to this, it is sufficient to say, that the heart can be much more readily relieved by a method different from that which he condemns. But he goes on to make a most extraordinary suggestion in the next sentence, where it is said, "A proposal which might at first sight seem scarcely more plausible than that mentioned, might be made with some hope of advantage. The introduction of a liquid into the veins, has been often attended by the revival of the patient in cases of cholera, though rarely with ultimate success. In this accident, the vital powers not having received that

lesion which is the result of an exhausting disease, it may be hoped that a successful revival might sometimes be effected by means of the injection of the saline solution into the veins.* There can be little doubt, but that the injection of a fluid into the veins would increase and not diminish the danger; but what analogy exists between cholera patients, and the class of cases at present under consideration, it is not easy to divine. Is a man, writing in any work, but especially in one which is to be extensively circulated among the junior members of the profession, justified in making such a recommendation, without previously putting it to the test of experiment upon the inferior animals, or at least having some plausible theoretical argument to adduce in its support?

The measures which the surgeon ought to adopt with a view of guarding against the admission of air during an operation, are very obvious. When operating on parts extensively diseased, or when cutting in the vicinity of the large veins in the neck, he must pay special attention to the position of the patient, and when obliged to divide a large vein, he ought not to be unprepared for the accident taking place. With a view of averting it, he ought, before making the hazardous incision, to request the patient to take a full inspiration, so that the vessel may be divided during expiration.

Should it happen, however, that in spite of every precaution, a dangerous quantity of air gain access, not an instant must be lost in adopting the measures formerly pointed out, as the most likely to save the life of the patient.

* Cyclop. of Pract. Med. and Surg., Article, *Air*, p. 266. Philadelphia, 1834.

CHAPTER IV.

REMARKS UPON THE GENERATION OF AIR IN THE LIVING BODY, ESPECIALLY IN THE BLOOD VESSELS, WITH OBSERVATIONS ON THE CONSEQUENCES WHICH MAY RESULT FROM ITS PRESENCE THERE.

PRETERNATURAL accumulations of air take place in almost every part of the body. They have been noticed in situations where air ought naturally to be found, as well as in those where it ought not to exist—in organs which communicate, and in those which do not communicate with the external air. It is known to accumulate in the bladder—the uterus—the cavities of the pleura—peritoneum—pericardium—arachnoid—tunica vaginalis—in the cellular tissue all over the body,—and in the parenchyma of organs. De Boisement, in his excellent Thesis on Pneumatoses,* mentions a case of gaseous matter being formed within the synovial membrane of the knee joint, and from the analogy between synovial and serous membranes, it is curious that no similar case (as far as I can discover) should be on record.†

* Recherches sur les Pneumatoses, par A. Briere de Boisement. A Paris, 1825, p. 24. *Thesis, No. 201, quarto.*

† My friend Dr. Duncan has informed me of a case somewhat similar to that mentioned by De Boisement. It occurred in the practice of the late Dr. James C. Gregory, and under the observation of Dr. Duncan. The patient was a woman of thirty-six years of age. Her

In many animals air is an ordinary and natural secretion. Such is the gaseous matter contained in the swimming bladder of the fish ; but what is more to our purpose, in many of the cold-blooded animals, air appears to circulate along with the blood. Reichel frequently observed globules of air in the blood of frogs ;* and Spallanzani saw the same in that of salamanders.† Blumenbach states that he has seen air vesicles so frequently in the blood of amphibia, and fishes, that he considers them to be constantly present.‡ Similar observations have been made by Redi, Caldesi, and Morgagni.§

Anormal formations of air are by no means uncommon, both in the inferior animals and in the human subject, in certain states of disease, and they sometimes exist without any other morbid phenomena being apparent. Bags containing air have been found in the abdominal cavities of healthy pigs ; and occasionally air is contained in cysts in the human subject.||

In hysteria, air frequently accumulates in very large quantities in the stomach and intestines ; and in peritonitis the formation of gaseous matter is a common occurrence.

disease was cholera. In consequence of the injection of the saline solution, phlebitis supervened. During life it was suspected that within the cavity of both knee joints there was accumulated a mixture of air and pus, and upon dissection a few hours after death, such was proved to be the case. In this instance, however, it is probable that the gaseous matter was produced by the decomposition of the pus.

* De Sanguine motu experimenta, p. 16. Leipsic, 1767.

† Experiences sur la Circulation, &c. p. 158.

‡ Kleine Schriften zur Vergleich. Phys. und Anat. und Naturgesch. gehörig, parag. 71.

§ Morgagni de Sedibus et Causis Morb. lib. i. 2, p. v. § 22.

|| Dict. de Méd. Article, *Kystes*.

Air may accumulate in the urinary bladder, the uterus, and in other situations, without indicating a state of general disease, and consequently is of trifling importance. The distention of the uterus with air, however, has occasionally given rise to awkward errors, from the affection being mistaken for pregnancy. I was lately informed by a medical friend, of a woman residing near Edinburgh, to whom such a misfortune happened. The baby clothes were ready, and she was in the daily expectation of being brought to bed. One morning, however, having stooped to pick up something from the floor, to her no small astonishment and dismay the contents of the uterus passed off in one continued stream of air.*

Sudden impressions made upon the mind occasionally produce an immediate disengagement of a large quantity of air. Frank mentions a case of a lady, who, on hearing a false report of the death of her husband, fainted, was seized with convulsions, and at the same time became enormously tympanitic. Lobstein, in quoting this case, gives another of the same kind. A man, after a hearty meal, received a piece of bad news. Suddenly his digestion became disordered, and air was formed within his body to so great an extent, as actually to suffocate him.† Hypochondriacs are very subject to the formation of large quantities of gaseous matter within the stomach and intestines.

Spontaneous emphysema has often been observed to a greater or less extent as a sequel of certain epidemics. We

* An analogous case is related in the *Lancet*, vol. ii. p. 355. Bianchi has recorded a similar case in the *Journ. de Méd. de Paris*, 1756. Vide also Astruc *Traité des Maladies des Femmes*.

† Lobstein *Traité d'Anatomie Pathol.* Tome i. p. 156. A Paris, 1829.

are told by Frank that an epidemic fever which prevailed in Germany in 1772, and another which raged in Bobbio, (a small town in Italy,) in 1789, frequently terminated in a general emphysema. The same author mentions the case of a young lady of Vienna, who used to become emphysematous during every paroxysm of a tertian ague.*

Dr. Sickel gives an account of a woman, who, without any external injury, was suddenly seized with emphysema over the whole body. No bad consequences resulted, and on the following day, the affection had completely disappeared.† The narrator assigns as the cause of the phenomenon that a short time before its occurrence, the woman had eaten some mustard seed, eruca, and a sausage.

It has been stated by Baillie, and other observers, that when air is found in the stomach or intestines of a dead animal, it may generally be also detected in the small blood vessels of these parts. Baillie, after detailing a case of "emphysema not proceeding from any local injury," gives it as his opinion, "that the air was formed from the blood itself, by some peculiar arrangement of its parts, and conducted into the cells of the cellular membrane by very small vessels."‡

Andral§ and other eminent pathological authors speak of gaseous matter being formed in the circulatory apparatus. Lobstein also considers this as established; and he considers gaseous secretions, in whatever part of the body they may be found, as *vital products*, resulting from

* Cyclop. of Pract. Med. Article, *Emphysema*. Lond.

† Sicelii Decad. *quart.* 1744, p. 487.

‡ Transactions of a Society for the Improvement of Medical Knowledge, vol. i. p. 202.

§ Andral Précis d'Anat. Pathol. Tome i. p. 52?. Paris, 1829.

some peculiar nervous influence, engendered in the minute ramifications of the nerves embracing the capillaries;* and Gaspard employs the term "*gazeification vitale*" in his essay on the subject.† The experiments of Magendie and Girardin clearly shew that the gas secreted by the intestines is not the result of any chemical decomposition, but is truly a vital secretion. After many careful observations, they found that the nature of the aliment did not influence the nature of the gas which was produced, but that this was invariably the same. In the smaller intestines the gases detected were carbonic acid, hydrogen, and nitrogen; and in the lower bowels they found the same gases, with the addition of sulphuretted hydrogen.‡ From all these considerations, then, it seems manifest, that although in many cases, accumulations of gas within the body owe their origin to chemical decomposition, yet nevertheless they are sometimes the products of a vital action.

Whatever be the explanation of the fact, it is quite certain that emphysema frequently takes place without any lesion, and that in such cases upon dissection air is found in the blood vessels.

There are some curious instances, however, on record, in which there seems at least a very strong probability that during life a very large quantity of air was contained in the blood-vessels. A man twenty-five years of age, who had been ill for fifteen days, was admitted into the Hôpital Cochin of Paris, "with symptoms of typhus fever; he also complained of pain in the left thigh; and whilst he was in a state of delirium, said he had been bitten on the knee

* Lobstein, op. cit. Tome i. p. 162.

† Gaspard Dissert. Physiol. sur la Gazeification vitale. Paris, 1812.

‡ Rech. Physiol. sur les gas intestinaux, § 8. Paris, 1814.

by a dog. The limb was most attentively examined, but not the slightest trace of such an accident could be discovered. The thigh and scrotum were much swollen. He died on the following day. On dissection, eight hours after death, the surface of the body was found soiled by blood which had transuded through the integuments, and some blood had also been discharged from the nose. The whole body was emphysematous, but the left inferior extremity was so to a very high degree. It was double its natural size, of a brown colour, and covered with numerous phlyctenæ, some black, of great extent, and collected in clusters, from which escaped a reddish serous fluid, mingled with a quantity of gas; others white, from which nothing but air escaped. When the limb was pressed with the hand, crepitation was distinctly heard, the abdomen was much distended with gas, and in the intestines were observed those alterations which are so common in cases of typhus fever. Bubbles of air filled the vessels of the pia mater and the left vena saphena. The lymphatic ganglions of the mesentery were enlarged, and contained gas, which took fire from the flame of a taper, and produced an explosion; the same phenomena also followed the exit of the air, which was contained in the legs, thighs, and scrotum.”* Weber mentions a case of aneurism containing air.†

According to the experiments of Krimer it appears, that if the blood be pressed out of a portion of an artery, and that portion be isolated by two ligatures, it will soon become distended with air.‡ If this statement be correct, it seems

* Lond. Med. Phys. Journ. June 1831, as quoted in Article *Emphysema*, Cyclop. of Pract. Med. Lond.

† Annotationes Anat. et Phys. p. 6.

‡ Versuch einer Physiol. des Bluts, § 177, 185. Leips. 1823.

to favour the opinion, that the internal coat of an artery has the power of secreting air.

That such is the case we are inclined to believe; and to prevent repetition, the arguments upon which this opinion is founded are here summed up. 1st, Air is found circulating in the vessels along with the blood in certain cold-blooded animals. 2d, Air has been found unmixed with the blood in the human species after death, when from the history of the case there is every reason to suppose that it was in the blood-vessels during life.

Air is secreted by serous membranes, to which the inner coat of an artery bears a close resemblance; and this, when viewed in connexion with the experiments of Krimer, is an argument of considerable weight.*

Another argument might be drawn from these cases of sudden death, in which the only thing found which can at all explain this event, is the presence of a large quantity of air in the blood-vessels. Sometimes the gaseous matter is found only in the vessels of the brain, and to this class of cases Morgagni has given the name of *gaseous apoplexy*. At other times we find the heart distended with frothy blood. From what has been stated above, it is fair to entertain the idea, that the sudden evolution of air within the blood-vessels may occasionally give rise to symptoms similar to those produced by its injection into the veins. This hypothesis is strengthened by the fact, that the formation of air in the living body sometimes takes place with extreme rapidity.

Cases of sudden death, in which the heart is described as

* In Burdach, vol. v. p. 209. is the absurd fancy that air is naturally secreted in the heart and origin of the large vessels, because they empty themselves of their blood without being able to remain collapsed.

having been found distended with air, are not so numerous as those in which the air has been found only in the vessels of the brain. One very remarkable instance is given by Morgagni. "A fisherman of Venice, upwards of forty years of age, and the subject of dyspepsia, was seized when in his boat with an affection of the abdomen, apparently from flatulence, to which he had been previously liable, and suddenly expired." "The body was examined on the following day. *Dissection*.—The abdomen was tumid from gas, with which the stomach and intestines were inflated." "The heart was large and flaccid; and both of the ventricles, and the right auricle, contained frothy blood." Most of the veins, the pulmonary artery, the aorta and carotids, also contained a frothy mixture of air and blood. The scrotum was greatly inflated with air.* Pechlin gives a case of a somewhat similar nature,† upon which Nysten remarks—"On reconnoit que la mort doit etre spécialement rapportée à la distension du ventricule pulmonaire par un gaz qui empechoit le sang venieux d'y arriver."‡ Unfortunately the symptoms which immediately preceded death are not detailed. Ruysch gives an account of a woman who died suddenly, and in whom the heart was found distended with air.§ A similar case is described by Grætz, and is quoted by Morgagni (Lib. i. ep. v. § 20.) and Nysten, (p. 7, and p. 174.)

The following are perhaps two of the most important of these cases of which we have any account. The one is given

* Cooke's Morgagni, vol. i. p. 80. Lond. 1822 from Morgagni, v. 17.

† Observ. Physico-Medicae. Obs. lvii. p. 135. Hamburg, 1691.

‡ Opera Omnia, 1737, p. 9.

§ Mr. Percival, veterinary surgeon in the 1st Life Guards, has published an account of a horse which died suddenly when under treatment

by Nysten upon the authority of his friend Dr. De Jaer. The subject of the observation was a shoemaker of forty-five years of age, who had for the last fifteen years of his life been afflicted with spasmodic asthma, and had generally about seven violent accessions during the year, which came on without any premonitory symptoms. At the commencement of one of the exacerbations, he was brought to the "Hôpital Cochin" of Paris, where he died on the third day. The body was opened twelve hours after death, when it was yet warm. The left ventricle and the arteries contained no blood, but the right auricle and ventricle, and the whole venous system were distended with frothy blood. All the organs were in their natural state, and the muscles were very firm and red.

What gives additional importance to this case is, the short time which was allowed to elapse between death and the examination of the body.

The other case to which allusion has been made occurred to M. Laumonier, in the hospital of Rouen.—A woman, aged fifty-six, died suddenly from an attack of asthma, a disease to which she was subject. The body was examined twenty-four hours after death. The season was winter; and there was not the slightest trace of putrefaction. There was emphysema in various parts of the body, and the internal coat of the carotid artery had a red appearance, as if resulting from inflammation. The heart was very much distended, and its right cavities were filled with gaseous matter of a peculiar smell.

for a catarrhal affection, the heart of which, upon dissection, was found enormously distended with air. (*Veterinarian*, vol. x. p. 65. Lond. Feb. 1837.) It is exceedingly to be regretted that there is no mention made of the time which elapsed between death and dissection.

In the Medical Observations and Inquiries there is a singular case mentioned, which I am somewhat inclined to classify with the above. The patient during life was sensible of a noise within the thorax resembling “a stream of water passing over obstructions, or passing over a narrow confined place. He was subject to exacerbations, and when he varied his position the circulation became confused, and as it were wholly carried on in a corner of his heart, which at such times beat with a whizzing noise. During his illness he felt the greatest pain when the noise seemed least; so that when speaking of his situation he had a common expression that *Gush was his friend*; while Gush stood by him he should live.”—At last he died. Upon dissection the pericardium was found loaded with fat. The heart *in situ* was longer and more pointed than usual for its size. The right auricle was much enlarged, and very thin, bearing strong marks of inflammation. The ventricle having lost its usual firmness and colour, was so transparent, as in a manner to admit an inspection into its very substance. *Upon cutting into these two cavities a considerable quantity of air rushed out, and upon laying them both open they appeared as totally void of blood as if they had been washed clean. The interstices between the chordae tendineae were full of air bubbles.* A variety of less remarkable morbid appearances were noticed. Unfortunately it is not stated at what period after death the inspection was made, so that the case standing, as I believe it at present does, *per se*, is deprived of much of its value.*

Without laying any weight whatever upon this last case,

* Med. Observ. and Enq. vol. vi. Case of M. Houlder, by Mr. R. B. Cheston, surgeon at Gloucester. Communicated by Dr. William Hunter.

it does not seem too hypothetical, to suggest that in the cases which occurred in the "Hôpital Cochin" and the Hospital at Rouen, the immediate cause of death was the presence of a large quantity of gaseous matter in the heart. We have seen that violent mental emotions sometimes occasion the disengagement of large quantities of air. Is it possible that the rapid evolution of this elastic fluid is ever the immediate cause of those instantaneous deaths which frequently follow sudden and immoderate emotions of joy or grief?

Death it would appear sometimes takes place from the sudden disengagement of gas within the vessels of the brain, constituting the *gaseous apoplexy* of Morgagni and other authors; but a mere reference to this subject must suffice, as the appropriate limits of a Thesis have I fear been already exceeded. The idea of Morgagni was, that the cause of death was the pressure exerted upon the brain by the air; but Bichat thought that in some cases the quantity of air found is so small as not to give plausibility to this explanation, and contended that the life of the brain was destroyed by some peculiar and subtle action exerted upon it.*

All that can be said with confidence, then, in reference to this subject, is, that there are various facts and arguments which render it exceedingly probable, that owing to the sudden formation of air within the blood-vessels, death may be produced either by arresting the contractions of the right side of the heart, or by producing a fatal action on the brain.

* Sur la Vie et la Mort, Part ii. Article 2de, § 2.

Mr Percy

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with Mr Atkinson's resp

THERMOMETRIC,

BAROMETRIC,

AND OTHER TABLES,

FOR THE

RECIPROCAL CONVERSION OF THE NUMBERS OF THE SCALES IN MORE FREQUENT USE.

CALCULATED BY

GEORGE ATKIN, Esq.

EXTRACTED FROM THE EDINBURGH NEW PHILOSOPHICAL JOURNAL.

ADAM & CHARLES BLACK, EDINBURGH.

MDCCCXXXVII.

THERMOMETRIC TABLES

English Feet and Inches Calculated by GEORGE ATKIN
 French Feet and Inches, Meters, and Millimeters; Tables of
 Thermometric Tables, Barometric Tables, Tables of Forces,
 &c.

THERMOMETRIC TABLES, Nos. I, II, III.

The following Tables are for the purpose of converting the degrees of the three thermometric scales into one another. When the quantity to be converted is a whole number, its value is ascertained by simple inspection of that table in which the proposed scale stands in the first column. Thus, to convert $+85^{\circ}$ R. into degrees of Fahrenheit and Centigrade, by Table I we find:

$$+85 \text{ R.} = 233.25 \text{ F. and } 166.25 \text{ C.}$$

Again, if -65° C. were the given number, we find in Table II:

$$-65 \text{ C.} = -85.0 \text{ F. and } -75.0 \text{ R.}$$

Lastly, to convert $+42^{\circ}$ F. into the corresponding scales, in Table III we find:

$$+42 \text{ F.} = +44.2 \text{ R. and } 5.56 \text{ C.}$$

If there be fractional parts annexed to the proposed number, by having recourse to the table of proportional parts, the value of the part is ascertained, and must be added to the number found in the body of the table; observing that, when the signs are alike, the sum is taken when unlike the difference. The signs of the fractional parts are the same in all the scales.

Example

1. Convert $+44.2^{\circ}$ R. into degrees of Fahr. and Cent.			
By Table I. $+44 \text{ R.}$	$= +131.00 \text{ F.}$	$+ 55.00 \text{ C.}$	
$+ 0.2$	$= 0.15$	0.20	
<hr/>	<hr/>	<hr/>	
44.2 R.	$= 131.15 \text{ F.}$	55.20 C.	
2. Convert -10.4° R. into degrees of Fahr. and Cent.			
-10 R.	$= -58.00 \text{ F.}$	-50.00 C.	
$- 0.4$	$= - 0.9$	$- 0.5$	
<hr/>	<hr/>	<hr/>	
-10.4 R.	$= -58.9 \text{ F.}$	-50.5 C.	

Thermometric Tables; Barometric Tables; Tables of Toises, French Feet and Inches, Metres and Millimetres; Tables of English Feet and Inches. Calculated by GEORGE ATKIN, Esq.

THERMOMETRIC TABLES, NOS. I. II. III.

THE following Tables are for the purpose of converting the degrees of the three thermometric scales into one another. When the quantity to be converted is a whole number, its value is ascertained by simple inspection of that table in which the proposed scale stands in the first column. Thus, to convert + 85 R. into degrees of Fahrenheit and Centigrade, by Table I. we find :

$$+ 85 \text{ R.} = 223.25 \text{ F. and } 106.25 \text{ C.}$$

Again, if —65 C. were the given number, we find in Table II.

$$-65 \text{ C.} = -85.0 \text{ F. and } -52.0 \text{ R.}$$

Lastly, to convert + 42 F. into the corresponding scales, in Table III. we find :

$$+ 42 \text{ F.} = + 4.44 \text{ R. and } + 5.55 \text{ C.}$$

If there be fractional parts annexed to the proposed number, by having recourse to the table of proportional parts, the value of the part is ascertained, and must be added to the number found in the body of the table; observing that, when the signs are alike, the sum is taken when unlike the difference. The signs of the fractional parts are the same in all the scales.

Example.

1. Convert + 44.2 R. into degrees of Fahr. and Cent. :

By Table I.	+ 44 R.	=	+ 131.00 F.	+ 55.00 C.
	+ 0.2	=	0.45	0.25
	44.2 R.	=	131.45 F.	55.25 C.

2. Convert —40.4 R. into degrees of Fahr. and Cent. :

—40 R.	=	—58.00 F.	—50.00 C.
— 0.4	=	— 0.9	— 0.5
—40.4 R.	=	—58.9 F.	—50.5 C.

3. Convert -9.6 R. into degrees of Fahr. and Cent. :

$$\begin{aligned} -9 \text{ R.} &= +11.75 \text{ F.} & -11.25 \text{ C.} \\ -0.6 &= -1.35 & -0.75 \end{aligned}$$

If the proposed quantity extends to hundredths or thousandths of a degree, it may be converted by shifting the decimal point of the proportional part either one or two places to the left. For example :

4. Convert $+52.246$ C. into degrees of Fahr. and Reaum. :

$$\begin{aligned} +52 \text{ C.} &= 125.6 \text{ F.} & 41.6 \text{ R.} \\ +0.2 &= 0.36 & 0.16 \\ +0.04 &= 0.072 & 0.032 \\ +0.006 &= 0.0108 & 0.0048 \\ \hline +52.246 \text{ C.} &= 126.0428 \text{ F.} & 41.7968 \text{ R.} \end{aligned}$$

BAROMETRIC TABLES, Nos. IV. V. VI.

1. Required the value in French inches and lines, and English inches, of a column of mercury 729.7 mill. in height. By Table IV.

	Inch.	Lin.	
729 mill.	$=$	26	11.16 French.
			28.701 English inch.
$+0.7$	$=$	0.315	0.0275
729.7	$=$	26	11.475
			28.7285

Tables V. and VI. are used in the same manner, and serve for the conversion of French inches and lines and English inches into one another, and also into millimetres. By these tables a conversion may be made at a half or third of any of the numbers, by doubling or trebling the given quantity, and then a half or third of the result will be the quantity required.

TABLES VII. to XIV.

Are for converting the different French and English measures into one another, and are used in the same manner as the preceding.

Cent.	Fahr.	Reaum.	-9.6 R. = $+10.40$ F. -12.00 C.	Cent.	Fahr.	Reaum.
100.00	212.00	80.00		100.00	212.00	80.00
99.00	211.80	79.80		99.00	211.80	79.80
98.00	211.60	79.60		98.00	211.60	79.60
97.00	211.40	79.40		97.00	211.40	79.40
96.00	211.20	79.20		96.00	211.20	79.20
95.00	211.00	79.00		95.00	211.00	79.00
94.00	210.80	78.80		94.00	210.80	78.80
93.00	210.60	78.60		93.00	210.60	78.60
92.00	210.40	78.40		92.00	210.40	78.40
91.00	210.20	78.20		91.00	210.20	78.20
90.00	210.00	78.00		90.00	210.00	78.00
89.00	209.80	77.80		89.00	209.80	77.80
88.00	209.60	77.60		88.00	209.60	77.60
87.00	209.40	77.40		87.00	209.40	77.40
86.00	209.20	77.20		86.00	209.20	77.20
85.00	209.00	77.00		85.00	209.00	77.00
84.00	208.80	76.80		84.00	208.80	76.80
83.00	208.60	76.60		83.00	208.60	76.60
82.00	208.40	76.40		82.00	208.40	76.40
81.00	208.20	76.20		81.00	208.20	76.20
80.00	208.00	76.00		80.00	208.00	76.00
79.00	207.80	75.80		79.00	207.80	75.80
78.00	207.60	75.60		78.00	207.60	75.60
77.00	207.40	75.40		77.00	207.40	75.40
76.00	207.20	75.20		76.00	207.20	75.20
75.00	207.00	75.00		75.00	207.00	75.00
74.00	206.80	74.80		74.00	206.80	74.80
73.00	206.60	74.60		73.00	206.60	74.60
72.00	206.40	74.40		72.00	206.40	74.40
71.00	206.20	74.20		71.00	206.20	74.20
70.00	206.00	74.00		70.00	206.00	74.00
69.00	205.80	73.80		69.00	205.80	73.80
68.00	205.60	73.60		68.00	205.60	73.60
67.00	205.40	73.40		67.00	205.40	73.40
66.00	205.20	73.20		66.00	205.20	73.20
65.00	205.00	73.00		65.00	205.00	73.00
64.00	204.80	72.80		64.00	204.80	72.80
63.00	204.60	72.60		63.00	204.60	72.60
62.00	204.40	72.40		62.00	204.40	72.40
61.00	204.20	72.20		61.00	204.20	72.20
60.00	204.00	72.00		60.00	204.00	72.00
59.00	203.80	71.80		59.00	203.80	71.80
58.00	203.60	71.60		58.00	203.60	71.60
57.00	203.40	71.40		57.00	203.40	71.40
56.00	203.20	71.20		56.00	203.20	71.20
55.00	203.00	71.00		55.00	203.00	71.00
54.00	202.80	70.80		54.00	202.80	70.80
53.00	202.60	70.60		53.00	202.60	70.60
52.00	202.40	70.40		52.00	202.40	70.40
51.00	202.20	70.20		51.00	202.20	70.20
50.00	202.00	70.00		50.00	202.00	70.00
49.00	201.80	69.80		49.00	201.80	69.80
48.00	201.60	69.60		48.00	201.60	69.60
47.00	201.40	69.40		47.00	201.40	69.40
46.00	201.20	69.20		46.00	201.20	69.20
45.00	201.00	69.00		45.00	201.00	69.00
44.00	200.80	68.80		44.00	200.80	68.80
43.00	200.60	68.60		43.00	200.60	68.60
42.00	200.40	68.40		42.00	200.40	68.40
41.00	200.20	68.20		41.00	200.20	68.20
40.00	200.00	68.00		40.00	200.00	68.00
39.00	199.80	67.80		39.00	199.80	67.80
38.00	199.60	67.60		38.00	199.60	67.60
37.00	199.40	67.40		37.00	199.40	67.40
36.00	199.20	67.20		36.00	199.20	67.20
35.00	199.00	67.00		35.00	199.00	67.00
34.00	198.80	66.80		34.00	198.80	66.80
33.00	198.60	66.60		33.00	198.60	66.60
32.00	198.40	66.40		32.00	198.40	66.40
31.00	198.20	66.20		31.00	198.20	66.20
30.00	198.00	66.00		30.00	198.00	66.00
29.00	197.80	65.80		29.00	197.80	65.80
28.00	197.60	65.60		28.00	197.60	65.60
27.00	197.40	65.40		27.00	197.40	65.40
26.00	197.20	65.20		26.00	197.20	65.20
25.00	197.00	65.00		25.00	197.00	65.00
24.00	196.80	64.80		24.00	196.80	64.80
23.00	196.60	64.60		23.00	196.60	64.60
22.00	196.40	64.40		22.00	196.40	64.40
21.00	196.20	64.20		21.00	196.20	64.20
20.00	196.00	64.00		20.00	196.00	64.00
19.00	195.80	63.80		19.00	195.80	63.80
18.00	195.60	63.60		18.00	195.60	63.60
17.00	195.40	63.40		17.00	195.40	63.40
16.00	195.20	63.20		16.00	195.20	63.20
15.00	195.00	63.00		15.00	195.00	63.00
14.00	194.80	62.80		14.00	194.80	62.80
13.00	194.60	62.60		13.00	194.60	62.60
12.00	194.40	62.40		12.00	194.40	62.40
11.00	194.20	62.20		11.00	194.20	62.20
10.00	194.00	62.00		10.00	194.00	62.00
9.00	193.80	61.80		9.00	193.80	61.80
8.00	193.60	61.60		8.00	193.60	61.60
7.00	193.40	61.40		7.00	193.40	61.40
6.00	193.20	61.20		6.00	193.20	61.20
5.00	193.00	61.00		5.00	193.00	61.00
4.00	192.80	60.80		4.00	192.80	60.80
3.00	192.60	60.60		3.00	192.60	60.60
2.00	192.40	60.40		2.00	192.40	60.40
1.00	192.20	60.20		1.00	192.20	60.20
0.00	192.00	60.00		0.00	192.00	60.00

TABLE I.

Reau.	Fahr.	Cent.	Reau.	Fahr.	Cent.	Reau.	Fahr.	Cent.	Reau.	Fahr.	Cent.
-60	103.00	-75.00	+1	+34.25	+1.25	+62	+171.50	+ 77.50	+123	+308.75	+153.75
59	100.75	73.75	2	36.50	2.50	63	173.75	78.75	124	311.00	155.00
58	98.50	72.50	3	38.75	3.75	64	176.00	80.00	125	313.25	156.25
57	96.25	71.25	4	41.00	5.00	65	178.25	81.25	126	315.50	157.50
56	94.00	70.00	5	43.25	6.25	66	180.50	82.50	127	317.75	158.75
55	91.75	68.75	6	45.50	7.50	67	182.75	83.75	128	320.00	160.00
54	89.50	67.50	7	47.75	8.75	68	185.00	85.00	129	322.25	161.25
53	87.25	66.25	8	50.00	10.00	69	187.25	86.25	130	324.50	162.50
52	85.00	65.00	9	52.25	11.25	70	189.50	87.50	131	326.75	163.75
51	82.75	63.75	10	54.50	12.50	71	191.75	88.75	132	329.00	165.00
50	80.50	62.50	11	56.75	13.75	72	194.00	90.00	133	331.25	166.25
49	78.25	61.25	12	59.00	15.00	73	196.25	91.25	134	333.50	167.50
48	76.00	60.00	13	61.25	16.25	74	198.50	92.50	135	335.75	168.75
47	73.75	58.75	14	63.50	17.50	75	200.75	93.75	136	338.00	170.00
46	71.50	57.50	15	65.75	18.75	76	203.00	95.00	137	340.25	171.25
45	69.25	56.25	16	68.00	20.00	77	205.25	96.25	138	342.50	172.50
44	67.00	55.00	17	70.25	21.25	78	207.50	97.50	139	344.75	173.75
43	64.75	53.75	18	72.50	22.50	79	209.75	98.75	140	347.00	175.00
42	62.50	52.50	19	74.75	23.75	80	212.00	100.00	141	349.25	176.25
41	60.25	51.25	20	77.00	25.00	81	214.25	101.25	142	351.50	177.50
40	58.00	50.00	21	79.25	26.25	82	216.50	102.50	143	353.75	178.75
39	55.75	48.75	22	81.50	27.50	83	218.75	103.75	144	356.00	180.00
38	53.50	47.50	23	83.75	28.75	84	221.00	105.00	145	358.25	181.25
37	51.25	46.25	24	86.00	30.00	85	223.25	106.25	146	360.50	182.50
36	49.00	45.00	25	88.25	31.25	86	225.50	107.50	147	362.75	183.75
35	46.75	43.75	26	90.50	32.50	87	227.75	108.75	148	365.00	185.00
34	44.50	42.50	27	92.75	33.75	88	230.00	110.00	149	367.25	186.25
33	42.25	41.25	28	95.00	35.00	89	232.25	111.25	150	369.50	187.50
32	40.00	40.00	29	97.25	36.25	90	234.50	112.50	151	371.75	188.75
31	37.75	38.75	30	99.50	37.50	91	236.75	113.75	152	374.00	190.00
30	35.50	37.50	31	101.75	38.75	92	239.00	115.00	153	376.25	191.25
29	33.25	36.25	32	104.00	40.00	93	241.25	116.25	154	378.50	192.50
28	31.00	35.00	33	106.25	41.25	94	243.50	117.50	155	380.75	193.75
27	28.75	33.75	34	108.50	42.50	95	245.75	118.75	156	383.00	195.00
26	26.50	32.50	35	110.75	43.75	96	248.00	120.00	157	385.25	196.25
25	24.25	31.25	36	113.00	45.00	97	250.25	121.25	158	387.50	197.50
24	22.00	30.00	37	115.25	46.25	98	252.50	122.50	159	389.75	198.75
23	19.75	28.75	38	117.50	47.50	99	254.75	123.75	160	392.00	200.00
22	17.50	27.50	39	119.75	48.75	100	257.00	125.00	161	394.25	201.25
21	15.25	26.25	40	122.00	50.00	101	259.25	126.25	162	396.50	202.50
20	13.00	25.00	41	124.25	51.25	102	261.50	127.50	163	398.75	203.75
19	10.75	23.75	42	126.50	52.50	103	263.75	128.75	+164	+401.00	+205.00
18	8.50	22.50	43	128.75	53.75	104	266.00	130.00			
17	6.25	21.25	44	131.00	55.00	105	268.25	131.25			
16	4.00	20.00	45	133.25	56.25	106	270.50	132.50			
15	-1.75	18.75	46	135.50	57.50	107	272.75	133.75			
14	+ 0.50	17.50	47	137.75	58.75	108	275.00	135.00			
13	2.75	16.25	48	140.00	60.00	109	277.25	136.25			
12	5.00	15.00	49	142.25	61.25	110	279.50	137.50			
11	7.25	13.75	50	144.50	62.50	111	281.75	138.75			
10	9.50	12.50	51	146.75	63.75	112	284.00	140.00			
9	11.75	11.25	52	149.00	65.00	113	286.25	141.25			
8	14.00	10.00	53	151.25	66.25	114	288.50	142.50			
7	16.25	8.75	54	153.50	67.50	115	290.75	143.75			
6	18.50	7.50	55	155.75	68.75	116	293.00	145.00			
5	20.75	6.25	56	158.00	70.00	117	295.25	146.25			
4	23.00	5.00	57	160.25	71.25	118	297.50	147.50			
3	25.25	3.75	58	162.50	72.50	119	299.75	148.75			
2	27.50	2.50	59	164.75	73.75	120	302.00	150.00			
1	29.75	-1.25	60	167.00	75.00	121	304.25	151.25			
0	+32.00	0.00	+61	+169.25	+76.25	+122	+306.50	+152.50			

PROPORTIONAL PARTS.		
Reau.	Fahr.	Cent.
0.1	0.225	0.125
0.2	0.450	0.250
0.3	0.675	0.375
0.4	0.900	0.500
0.5	1.125	0.625
0.6	1.350	0.750
0.7	1.575	0.875
0.8	1.800	1.000
0.9	2.025	1.125

TABLE II.

Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.
-75	-103.00	-60.0	-2	+28.4	-1.6	+71	+159.8	+56.8	+144	+291.2	+115.2
74	101.2	59.2	1	30.2	0.8	72	161.6	57.6	145	293.0	116.0
73	99.4	58.4	0	32.0	0.0	73	163.4	58.4	146	294.8	116.8
72	97.6	57.6	+1	33.8	+0.8	74	165.2	59.2	147	296.6	117.6
71	95.8	56.8	2	35.6	1.6	75	167.0	60.0	148	298.4	118.4
70	94.0	56.0	3	37.4	2.4	76	168.8	60.8	149	300.2	119.2
69	92.2	55.2	4	39.2	3.2	77	170.6	61.6	150	302.0	120.0
68	90.4	54.4	5	41.0	4.0	78	172.4	62.4	151	303.8	120.8
67	88.6	53.6	6	42.8	4.8	79	174.2	63.2	152	305.6	121.6
66	86.8	52.8	7	44.6	5.6	80	176.0	64.0	153	307.4	122.4
65	85.0	52.0	8	46.4	6.4	81	177.8	64.8	154	309.2	123.2
64	83.2	51.2	9	48.2	7.2	82	179.6	65.6	155	311.0	124.0
63	81.4	50.4	10	50.0	8.0	83	181.4	66.4	156	312.8	124.8
62	79.6	49.6	11	51.8	8.8	84	183.2	67.2	157	314.6	125.6
61	77.8	48.8	12	53.6	9.6	85	185.0	68.0	158	316.4	126.4
60	76.0	48.0	13	55.4	10.4	86	186.8	68.8	159	318.2	127.2
59	74.2	47.2	14	57.2	11.2	87	188.6	69.6	160	320.0	128.0
58	72.4	46.4	15	59.0	12.0	88	190.4	70.4	161	321.8	128.8
57	70.6	45.6	16	60.8	12.8	89	192.2	71.2	162	323.6	129.6
56	68.8	44.8	17	62.6	13.6	90	194.0	72.0	163	325.4	130.4
55	67.0	44.0	18	64.4	14.4	91	195.8	72.8	164	327.2	131.2
54	65.2	43.2	19	66.2	15.2	92	197.6	73.6	165	329.0	132.0
53	63.4	42.4	20	68.0	16.0	93	199.4	74.4	166	330.8	132.8
52	61.6	41.6	21	69.8	16.8	94	201.2	75.2	167	332.6	133.6
51	59.8	40.8	22	71.6	17.6	95	203.0	76.0	168	334.4	134.4
50	58.0	40.0	23	73.4	18.4	96	204.8	76.8	169	336.2	135.2
49	56.2	39.2	24	75.2	19.2	97	206.6	77.6	170	338.0	136.0
48	54.4	38.4	25	77.0	20.0	98	208.4	78.4	171	339.8	136.8
47	52.6	37.6	26	78.8	20.8	99	210.2	79.2	172	341.6	137.6
46	50.8	36.8	27	80.6	21.6	100	212.0	80.0	173	343.4	138.4
45	49.0	36.0	28	82.4	22.4	101	213.8	80.8	174	345.2	139.2
44	47.2	35.2	29	84.2	23.2	102	215.6	81.6	175	347.0	140.0
43	45.4	34.4	30	86.0	24.0	103	217.4	82.4	176	348.8	140.8
42	43.6	33.6	31	87.8	24.8	104	219.2	83.2	177	350.6	141.6
41	41.8	32.8	32	89.6	25.6	105	221.0	84.0	178	352.4	142.4
40	40.0	32.0	33	91.4	26.4	106	222.8	84.8	179	354.2	143.2
39	38.2	31.2	34	93.2	27.2	107	224.6	85.6	180	356.0	144.0
38	36.4	30.4	35	95.0	28.0	108	226.4	86.4	181	357.8	144.8
37	34.6	29.6	36	96.8	28.8	109	228.2	87.2	182	359.6	145.6
36	32.8	28.8	37	98.6	29.6	110	230.0	88.0	183	361.4	146.4
35	31.0	28.0	38	100.4	30.4	111	231.8	88.8	184	363.2	147.2
34	29.2	27.2	39	102.2	31.2	112	233.6	89.6	185	365.0	148.0
33	27.4	26.4	40	104.0	32.0	113	235.4	90.4	186	366.8	148.8
32	25.6	25.6	41	105.8	32.8	114	237.2	91.2	187	368.6	149.6
31	23.8	24.8	42	107.6	33.6	115	239.0	92.0	188	370.4	150.4
30	22.0	24.0	43	109.4	34.4	116	240.8	92.8	189	372.2	151.2
29	20.2	23.2	44	111.2	35.2	117	242.6	93.6	190	374.0	152.0
28	18.4	22.4	45	113.0	36.0	118	244.4	94.4	191	375.8	152.8
27	16.6	21.6	46	114.8	36.8	119	246.2	95.2	192	377.6	153.6
26	14.8	20.8	47	116.6	37.6	120	248.0	96.0	193	379.4	154.4
25	13.0	20.0	48	118.4	38.4	121	249.8	96.8	194	381.2	155.2
24	11.2	19.2	49	120.2	39.2	122	251.6	97.6	195	383.0	156.0
23	9.4	18.4	50	122.0	40.0	123	253.4	98.4	196	384.8	156.8
22	7.6	17.6	51	123.8	40.8	124	255.2	99.2	197	386.6	157.6
21	5.8	16.8	52	125.6	41.6	125	257.0	100.0	198	388.4	158.4
20	4.0	16.0	53	127.4	42.4	126	258.8	100.8	199	390.2	159.2
19	2.2	15.2	54	129.2	43.2	127	260.6	101.6	200	392.0	160.0
18	0.4	14.4	55	131.0	44.0	128	262.4	102.4	201	393.8	160.8
17	+1.4	13.6	56	132.8	44.8	129	264.2	103.2	202	395.6	161.6
16	3.2	12.8	57	134.6	45.6	130	266.0	104.0	203	397.4	162.4
15	5.0	12.0	58	136.4	46.4	131	267.8	104.8	204	399.2	163.2
14	6.8	11.2	59	138.2	47.2	132	269.6	105.6	+205	+401.0	+164.0
13	8.6	10.4	60	140.0	48.0	133	271.4	106.4	PROPORTIONAL PARTS.		
12	10.4	9.6	61	141.8	48.8	134	273.2	107.2	Cent.	Fahr.	Reaum.
11	12.2	8.8	62	143.6	49.6	135	275.0	108.0	0.1	0.18	0.08
10	14.0	8.0	63	145.4	50.4	136	276.8	108.8	0.2	0.36	0.16
9	15.8	7.2	64	147.2	51.2	137	278.6	109.6	0.3	0.54	0.24
8	17.6	6.4	65	149.0	52.0	138	280.4	110.4	0.4	0.72	0.32
7	19.4	5.6	66	150.8	52.8	139	282.2	111.2	0.5	0.90	0.40
6	21.2	4.8	67	152.6	53.6	140	284.0	112.0	0.6	1.08	0.48
5	23.0	4.0	68	154.4	54.4	141	285.8	112.8	0.7	1.26	0.56
4	24.8	3.2	69	156.2	55.2	142	287.6	113.6	0.8	1.44	0.64
-3	+26.6	-2.4	+70	+158.0	+56.0	+143	+289.4	+114.4	0.9	1.62	0.72

TABLE III.

Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.
-100	-58.66	-73.33	-35	-29.77	-37.22	+30	-0.88	-1.11	+95	+28.00	+35.00
99	58.22	72.77	34	29.33	36.66	31	0.44	0.55	96	28.44	35.55
98	57.77	72.22	33	28.88	36.11	32	0.00	0.00	97	28.88	36.11
97	57.33	71.66	32	28.44	35.55	33	+ 0.44	+ 0.55	98	29.33	36.66
96	56.88	71.11	31	28.00	35.00	34	0.88	1.11	99	29.77	37.22
95	56.44	70.55	30	27.55	34.44	35	1.33	1.66	100	30.22	37.77
94	56.00	70.00	29	27.11	33.88	36	1.77	2.22	101	30.66	38.33
93	55.55	69.44	28	26.66	33.33	37	2.22	2.77	102	31.11	38.88
92	55.11	68.88	27	26.22	32.77	38	2.66	3.33	103	31.55	39.44
91	54.66	68.33	26	25.77	32.22	39	3.11	3.88	104	32.00	40.00
90	54.22	67.77	25	25.33	31.66	40	3.55	4.44	105	32.44	40.55
89	53.77	67.22	24	24.88	31.11	41	4.00	5.00	106	32.88	41.11
88	53.33	66.66	23	24.44	30.55	42	4.44	5.55	107	33.33	41.66
87	52.88	66.11	22	24.00	30.00	43	4.88	6.11	108	33.77	42.22
86	52.44	65.55	21	23.55	29.44	44	5.33	6.66	109	34.22	42.77
85	52.00	65.00	20	23.11	28.88	45	5.77	7.22	110	34.66	43.33
84	51.55	64.44	19	22.66	28.33	46	6.22	7.77	111	35.11	43.88
83	51.11	63.88	18	22.22	27.77	47	6.66	8.33	112	35.55	44.44
82	50.66	63.33	17	21.77	27.22	48	7.11	8.88	113	36.00	45.00
81	50.22	62.77	16	21.33	26.66	49	7.55	9.44	114	36.44	45.55
80	49.77	62.22	15	20.88	26.11	50	8.00	10.00	115	36.88	46.11
79	49.33	61.66	14	20.44	25.55	51	8.44	10.55	116	37.33	46.66
78	48.88	61.11	13	20.00	25.00	52	8.88	11.11	117	37.77	47.22
77	48.44	60.55	12	19.55	24.44	53	9.33	11.66	118	38.22	47.77
76	48.00	60.00	11	19.11	23.88	54	9.77	12.22	119	38.66	48.33
75	47.55	59.44	10	18.66	23.33	55	10.22	12.77	120	39.11	48.88
74	47.11	58.88	9	18.22	22.77	56	10.66	13.33	121	39.55	49.44
73	46.66	58.33	8	17.77	22.22	57	11.11	13.88	122	40.00	50.00
72	46.22	57.77	7	17.33	21.66	58	11.55	14.44	123	40.44	50.55
71	45.77	57.22	6	16.88	21.11	59	12.00	15.00	124	40.88	51.11
70	45.33	56.66	5	16.44	20.55	60	12.44	15.55	125	41.33	51.66
69	44.88	56.11	4	16.00	20.00	61	12.88	16.11	126	41.77	52.22
68	44.44	55.55	3	15.55	19.44	62	13.33	16.66	127	42.22	52.77
67	44.00	55.00	2	15.11	18.88	63	13.77	17.22	128	42.66	53.33
66	43.55	54.44	-1	14.66	18.33	64	14.22	17.77	129	43.11	53.88
65	43.11	53.88	0	14.22	17.77	65	14.66	18.33	130	43.55	54.44
64	42.66	53.33	+1	13.77	17.22	66	15.11	18.88	131	44.00	55.00
63	42.22	52.77	2	13.33	16.66	67	15.55	19.44	132	44.44	55.55
62	41.77	52.22	3	12.88	16.11	68	16.00	20.00	133	44.88	56.11
61	41.33	51.66	4	12.44	15.55	69	16.44	20.55	134	45.33	56.66
60	40.88	51.11	5	12.00	15.00	70	16.88	21.11	135	45.77	57.22
59	40.44	50.55	6	11.55	14.44	71	17.33	21.66	136	46.22	57.77
58	40.00	50.00	7	11.11	13.88	72	17.77	22.22	137	46.66	58.33
57	39.55	49.44	8	10.66	13.33	73	18.22	22.77	138	47.11	58.88
56	39.11	48.88	9	10.22	12.77	74	18.66	23.33	139	47.55	59.44
55	38.66	48.33	10	9.77	12.22	75	19.11	23.88	140	48.00	60.00
54	38.22	47.77	11	9.33	11.66	76	19.55	24.44	141	48.44	60.55
53	37.77	47.22	12	8.88	11.11	77	20.00	25.00	142	48.88	61.11
52	37.33	46.66	13	8.44	10.55	78	20.44	25.55	143	49.33	61.66
51	36.88	46.11	14	8.00	10.00	79	20.88	26.11	144	49.77	62.22
50	36.44	45.55	15	7.55	9.44	80	21.33	26.66	145	50.22	62.77
49	36.00	45.00	16	7.11	8.88	81	21.77	27.22	146	50.66	63.33
48	35.55	44.44	17	6.66	8.33	82	22.22	27.77	147	51.11	63.88
47	35.11	43.88	18	6.22	7.77	83	22.66	28.33	148	51.55	64.44
46	34.66	43.33	19	5.77	7.22	84	23.11	28.88	149	52.00	65.00
45	34.22	42.77	20	5.33	6.66	85	23.55	29.44	150	52.44	65.55
44	33.77	42.22	21	4.88	6.11	86	24.00	30.00	151	52.88	66.11
43	33.33	41.66	22	4.44	5.55	87	24.44	30.55	152	53.33	66.66
42	32.88	41.11	23	4.00	5.00	88	24.88	31.11	153	53.77	67.22
41	32.44	40.55	24	3.55	4.44	89	25.33	31.66	154	54.22	67.77
40	32.00	40.00	25	3.11	3.88	90	25.77	32.22	155	54.66	68.33
39	31.55	39.44	26	2.66	3.33	91	26.22	32.77	156	55.11	68.88
38	31.11	38.88	27	2.22	2.77	92	26.66	33.33	157	55.55	69.44
37	30.66	38.33	28	1.77	2.22	93	27.11	33.88	158	56.00	70.00
-36	-30.22	-37.77	+29	-1.33	-1.66	+94	+27.55	+34.44	+159	+56.44	+70.55

TABLE IV.

Millim.	French		French Lines.	English inches.	Millim.	French		French Lines.	English inches.
	In.	Lines.				In.	Lines.		
720	26	7.17	319.17	28.347	759	28	0.46	336.46	29.882
721		7.62	319.62	28.386	760		0.90	336.90	29.922
722		8.06	320.06	28.426	761		1.35	337.35	29.961
723		8.50	320.50	28.465	762		1.79	337.79	30.000
724		8.95	320.95	28.504	763		2.23	338.23	30.040
725		9.39	321.39	28.544	764		2.68	338.68	30.079
726		9.83	321.83	28.583	765		3.12	339.12	30.119
727		10.28	322.28	28.623	766		3.56	339.56	30.158
728		10.72	322.72	28.662	767		4.01	340.01	30.197
729		11.16	323.16	28.701	768		4.45	340.45	30.237
730	26	11.61	323.61	28.741	769		4.89	340.89	30.276
731	27	0.05	324.05	28.780	770		5.34	341.34	30.315
732		0.49	324.49	28.819	771		5.78	341.78	30.355
733		0.94	324.94	28.859	772		6.22	342.22	30.394
734		1.38	325.38	28.898	773		6.67	342.67	30.434
735		1.82	325.82	28.937	774		7.11	343.11	30.473
736		2.27	326.27	28.977	775		7.55	343.55	30.512
737		2.71	326.71	29.016	776		8.00	344.00	30.552
738		3.15	327.15	29.056	777		8.44	344.44	30.591
739		3.60	327.60	29.095	778		8.88	344.88	30.630
740		4.04	328.04	29.134	779		9.33	345.33	30.670
741		4.48	328.48	29.174	780	28	9.77	345.77	30.709
742		4.93	328.93	29.213	PROPORTIONAL PARTS.				
743		5.37	329.37	29.252					
744		5.81	329.81	29.292					
745		6.26	330.26	29.331					
746		6.70	330.70	29.371					
747		7.14	331.14	29.410					
748		7.58	331.58	29.449					
749		8.03	332.03	29.489					
750		8.47	332.47	29.528					
751		8.91	332.91	29.567					
752		9.36	333.36	29.607					
753		9.80	333.80	29.646					
754		10.24	334.24	29.686					
755		10.69	334.69	29.725					
756		11.13	335.13	29.764					
757	27	11.57	335.57	29.804					
758	28	0.02	336.02	29.843					
					Millim.	French lines.		English inches.	
					0.1	0.045	0.0039		
					0.2	0.090	.0078		
					0.3	0.135	.0118		
					0.4	0.180	.0157		
					0.5	0.225	.0196		
					0.6	0.270	.0236		
					0.7	0.315	.0275		
					0.8	0.360	.0314		
					0.9	0.405	.0354		
					1.0	0.450	0.0393		

TABLE V.

French				Millim.	French				Millim.	PROPORTIONAL PARTS.		
In.	Lin.	Fr. Lin.	English inches.		In.	Lin.	Fr. Lin.	English inches.		French Lines.	English inches.	Millim.
26	1	313	27.799	706.128	27	7	331	29.397	746.736			
	2	314	27.887	708.384		8	332	29.486	748.992			
	3	315	27.976	710.640		9	333	29.575	751.248	0.1	0.0089	0.2256
	4	316	28.065	712.896		10	334	29.664	753.504	0.2	0.0178	0.4512
	5	317	28.154	715.152	27	11	335	29.753	755.760	0.3	0.0266	0.6768
	6	318	28.243	717.408	28	0	336	29.841	758.016	0.4	0.0355	0.9024
	7	319	28.332	719.664		1	337	29.930	760.272	0.5	0.0444	1.1280
	8	320	28.420	721.920		2	338	30.019	762.528	0.6	0.0533	1.3536
	9	321	28.509	724.176		3	339	30.108	764.784	0.7	0.0622	1.5792
	10	322	28.598	726.432		4	340	30.197	767.040	0.8	0.0710	1.8048
26	11	323	28.687	728.688		5	341	30.285	769.296	0.9	0.0799	2.0304
27	0	324	28.776	730.944		6	342	30.374	771.552	1.0	0.0888	2.2560
	1	325	28.864	733.200		7	343	30.463	773.808			
	2	326	28.953	735.456		8	344	30.552	776.064			
	3	327	29.042	737.712		9	345	30.641	778.320			
	4	328	29.131	739.968		10	346	30.730	780.576			
	5	329	29.220	742.224	28	11	347	30.818	782.832			
	6	330	29.309	744.480	29	0	348	30.907	785.088			

TABLE VI.

English inches.	French		French Lines.	Millim.	English inches.	French		French Lines.	Millim.
	In.	Lines.				In.	Lines.		
27.1	25	5.12	305.12	688.31	29.9	28	0.65	336.65	759.43
27.2		6.25	306.25	690.85	30.0		1.78	337.78	761.97
27.3		7.37	307.38	693.39	30.1		2.90	338.90	764.51
27.4		8.50	308.50	695.93	30.2		4.03	340.03	767.05
27.5		9.63	309.63	698.47	30.3		5.15	341.15	769.59
27.6		10.75	310.75	701.01	30.4		6.28	342.28	772.13
27.7	25	11.88	311.88	703.55	30.5		7.41	343.41	774.67
27.8	26	1.01	313.01	706.09	30.6		8.53	344.53	777.21
27.9		2.13	314.13	708.63	30.7		9.66	345.66	779.75
28.0		3.26	315.26	711.17	30.8		10.78	346.78	782.29
28.1		4.38	316.38	713.71	30.9	28	11.91	347.91	784.83
28.2		5.51	317.51	716.25	31.0	29	1.04	349.04	787.37
28.3		6.64	318.64	718.79	PROPORTIONAL PARTS.				
28.4		7.76	319.76	721.33					
28.5		8.89	320.89	723.87					
28.6		10.01	322.01	726.41					
28.7	26	11.14	323.14	728.95					
28.8	27	0.27	324.27	731.49					
28.9		1.39	325.39	734.03					
29.0		2.52	326.52	736.57					
29.1		3.64	327.64	739.11					
29.2		4.77	328.77	741.65					
29.3		5.90	329.90	744.19					
29.4		7.02	331.02	746.73					
29.5		8.14	332.14	749.27					
29.6		9.27	333.27	751.81					
29.7		10.40	334.40	754.35					
29.8	27	11.52	335.52	756.89					
					English In.	Fr. Lines.	Millim.		
					0.01	0.1126	0.254		
					0.02	0.2252	0.508		
					0.03	0.3378	0.762		
					0.04	0.4504	1.016		
					0.05	0.5630	1.270		
					0.06	0.6755	1.524		
					0.07	0.7881	1.778		
					0.08	0.9007	2.032		
					0.09	1.0133	2.286		
					0.10	1.1259	2.5399		

TABLE VII.—TOISES.

Toises.	Metres.	English Feet.	Toises.	Metres.	English Feet.
1	1.94904	6.39459	200	389.80726	1278.91832
2	3.89807	12.78918	300	584.71089	1918.37748
3	5.84711	19.18377	400	779.61452	2557.83664
4	7.79615	25.57837	500	974.51815	3197.29580
5	9.74518	31.97296	600	1169.42179	3836.75496
6	11.69422	38.36755	700	1364.32542	4476.21412
7	13.64325	44.76214	800	1559.22905	5115.67328
8	15.59229	51.15673	900	1754.13268	5755.13244
9	17.54133	57.55132	1000	1949.03631	6394.59160
10	19.49036	63.94592	2000	3898.07262	12789.18321
20	38.98073	127.89183	3000	5847.10893	19183.77481
30	58.47109	191.83775	4000	7796.14524	25578.36642
40	77.96145	255.78366	5000	9745.18155	31972.95802
50	97.45182	319.72958	6000	11694.21786	38367.54963
60	116.94218	383.67550	7000	13643.25417	44762.14123
70	136.43254	447.62141	8000	15592.29048	51156.73284
80	155.92290	511.56733	9000	17541.32679	57551.32444
90	175.41327	575.51324	10000	19490.36310	63945.91605
100	194.90363	639.45916			

TABLE VIII.—FRENCH FEET.

French Feet.	Toises.	Metres.	English Feet. and Inch.		French Feet.	Toises.	Metres.	English Feet and Inch.	
			Feet.	Inch.				Feet.	Inch.
1	0.16667	0.32484	1	0.7892	200	33.33333	64.96738	213	1.8366
2	0.33333	0.64968	2	1.5784	300	50.00000	97.45182	319	8.7550
3	0.50000	0.97452	3	2.3675	400	66.66667	129.93575	426	3.6733
4	0.66667	1.29936	4	3.1567	500	83.33333	162.41969	532	10.5916
5	0.83333	1.62420	5	3.9459	600	100.00000	194.90363	639	5.5099
6	1.00000	1.94904	6	4.7351	700	116.66667	227.38757	746	0.4282
7	1.16667	2.27388	7	5.5243	800	133.33333	259.87151	852	7.3466
8	1.33333	2.59872	8	6.3135	900	150.00000	292.35545	959	2.2649
9	1.50000	2.92355	9	7.1026	1000	166.66667	324.83938	1065	9.1832
10	1.66667	3.24839	10	7.8918	2000	333.33333	649.67877	2131	6.3664
20	3.33333	6.49679	21	3.7837	3000	500.00000	974.51815	3197	3.5496
30	5.00000	9.74518	31	11.6755	4000	666.66667	1299.35754	4263	0.7328
40	6.66667	12.99358	42	7.5673	5000	833.33333	1624.19692	5328	9.9160
50	8.33333	16.24197	53	3.4592	6000	1000.00000	1949.03631	6394	7.0993
60	10.00000	19.49036	63	11.3510	7000	1166.66667	2273.87569	7460	4.2825
70	11.66667	22.73876	74	7.2428	8000	1333.33333	2598.71508	8526	1.4657
80	13.33333	25.98715	85	3.1347	9000	1500.00000	2923.55446	9591	10.6489
90	15.00000	29.23554	95	11.0265	10000	1666.66667	3248.39385	10657	7.8321
100	16.66667	32.48394	106	6.9183					

TABLE IX.—FRENCH INCHES.

French Inches.	Toises.	Millimeters.	English Inches.
1	0.01889	27.070	1.0658
2	0.02778	54.140	2.1315
3	0.04167	81.210	3.1973
4	0.05556	108.280	4.2631
5	0.06944	135.350	5.3288
6	0.08333	162.420	6.3946
7	0.09722	189.490	7.4604
8	0.11111	216.560	8.5261
9	0.12500	243.630	9.5919
10	0.13889	270.699	10.6577
11	0.15278	297.769	11.7234
12	0.16667	324.839	12.7899

TABLE X.—FRENCH LINES.

French Lines.	Toises.	Millimeters.	English Inches.
1	0.00116	2.256	0.0888
2	0.00231	4.512	0.1776
3	0.00347	6.767	0.2664
4	0.00463	9.023	0.3553
5	0.00579	11.279	0.4441
6	0.00694	13.535	0.5329
7	0.00810	15.791	0.6217
8	0.00926	18.046	0.7105
9	0.01042	20.302	0.7993
10	0.01157	22.558	0.8881
11	0.01273	24.814	0.9770
12	0.01389	27.070	1.0658

TABLE XI.—METRES.

Met.	Toises.	French			English		Met.	Toises.	French			English	
		Feet.	In.	Lines.	Feet.	Inch.			Feet.	In.	Lines.	Feet.	Inch.
1	0.51307	3	0	11.296	3	3.3708	200	102.61481	615	8	3.200	656	2.1580
2	1.02615	6	1	10.592	6	6.7416	300	153.92222	923	6	4.800	984	3.2370
3	1.53922	9	2	9.888	9	10.1124	400	205.22963	1231	4	6.400	1312	4.3160
4	2.05230	12	3	9.184	13	1.4832	500	256.53704	1539	2	8.000	1640	5.3950
5	2.56537	15	4	8.480	16	4.8539	600	307.84444	1847	0	9.600	1968	6.4740
6	3.07844	18	5	7.776	19	8.2247	700	359.15185	2154	10	11.200	2296	7.5530
7	3.59152	21	6	7.072	22	11.5955	800	410.45926	2462	9	0.800	2624	8.6320
8	4.10459	24	7	6.368	26	2.9663	900	461.76667	2770	7	2.400	2952	9.7110
9	4.61767	27	8	5.664	29	6.3371	1000	513.07407	3078	5	4.000	3280	10.7900
10	5.13074	30	9	4.960	32	9.7079	2000	1026.14815	6156	10	8.000	6561	9.5800
20	10.26148	61	6	9.920	65	7.4158	3000	1539.22222	9235	4	0.000	9842	8.3700
30	15.39222	92	4	2.880	98	5.1237	4000	2052.29630	12313	9	4.000	13123	7.1600
40	20.52296	123	1	7.840	131	2.8316	5000	2565.37037	15392	2	8.000	16404	5.9500
50	25.65370	153	11	0.800	164	0.5395	6000	3078.44444	18470	3	0.000	19685	4.7400
60	30.78444	184	8	5.760	196	10.2474	7000	3591.51852	21549	1	4.000	22966	3.5300
70	35.91519	215	5	10.720	229	7.9553	8000	4104.59259	24627	6	8.000	26247	2.3200
80	41.04593	246	3	3.680	262	5.6632	9000	4617.66667	27706	0	0.000	29523	1.1100
90	46.17667	277	0	8.640	295	3.3711	10000	5130.74074	30784	5	4.000	32803	11.9000
100	51.30741	307	10	1.600	328	1.0790							

TABLE XII.—MILLIMETRES.

Millim.	Toises.	French lines.	English lines.	Millim.	Toises.	French lines.	English lines.
1	0.00051	0.443	0.0394	60	0.03078	26.598	2.3622
2	0.00103	0.887	0.0787	70	0.03592	31.031	2.7560
3	0.00154	1.330	0.1181	80	0.04105	35.464	3.1497
4	0.00205	1.773	0.1575	90	0.04618	39.897	3.5434
5	0.00257	2.216	0.1969	100	0.05131	44.330	3.9371
6	0.00308	2.660	0.2362	200	0.10261	88.659	7.8742
7	0.00359	3.103	0.2756	300	0.15392	132.989	11.8112
8	0.00410	3.546	0.3150	400	0.20523	177.318	15.7483
9	0.00462	3.990	0.3543	500	0.25654	221.648	19.6854
10	0.00513	4.433	0.3937	600	0.30784	265.978	23.6225
20	0.01026	8.866	0.7874	700	0.35915	310.307	27.5596
30	0.01539	13.299	1.1811	800	0.41046	354.637	31.4966
40	0.02052	17.732	1.5748	900	0.46177	398.966	35.4337
50	0.02565	22.165	1.9685				

TABLE XIII.—ENGLISH FEET.

Eng. feet.	Toises.	Metres.	French			English feet.	Toises.	Metres.	French		
			Feet.	In.	Lines.				Feet.	In.	Lines.
1	0.15638	0.30479	0	11	3.114	200	31.27643	60.95850	187	7	10.836
2	0.31276	0.60959	1	10	6.228	300	46.91465	91.43835	281	5	10.254
3	0.46915	0.91438	2	9	9.343	400	62.55286	121.91780	375	3	9.672
4	0.62553	1.21918	3	9	0.457	500	78.19108	152.39725	469	1	9.090
5	0.78191	1.52397	4	8	3.571	600	93.82929	182.87670	562	11	8.508
6	0.93829	1.82877	5	7	6.685	700	109.46751	213.35615	656	9	7.926
7	1.09468	2.13356	6	6	9.799	800	125.10572	243.83559	750	7	7.344
8	1.25106	2.43836	7	6	0.913	900	140.74394	274.31504	844	5	6.762
9	1.40744	2.74315	8	5	4.028	1000	156.38215	304.79449	938	3	6.180
10	1.56382	3.04794	9	4	7.142	2000	312.76431	609.58899	1876	7	0.360
20	3.12764	6.09589	18	9	2.284	3000	469.14646	914.38348	2814	10	6.539
30	4.69146	9.14383	28	1	9.425	4000	625.52861	1219.17797	3753	2	0.719
40	6.25529	12.19178	37	6	4.567	5000	781.91076	1523.97246	4691	5	6.899
50	7.81911	15.23972	46	10	11.709	6000	938.29292	1828.76696	5629	9	1.079
60	9.38293	18.28767	56	3	6.851	7000	1094.67507	2133.56145	6568	0	7.259
70	10.94675	21.33561	65	8	1.993	8000	1251.05722	2438.35594	7506	4	1.438
80	12.51057	24.38536	75	0	9.134	9000	1407.43937	2743.15044	8444	7	7.618
90	14.07439	27.43150	84	5	4.276	10000	1563.82153	3047.94493	9382	11	1.798
100	15.63822	30.47945	93	9	11.418						

TABLE XIV.—ENGLISH INCHES.

Inch.	Toises.	French		Millim.	Inch.	Toises.	French		Millim.
		In.	Lines.				In.	Lines.	
1	0.01303	0	11.260	25.400	7	0.09122	6	6.817	177.797
2	0.02606	1	10.519	50.799	8	0.10426	7	6.076	203.197
3	0.03910	2	9.779	76.199	9	0.11729	8	5.336	228.596
4	0.05213	3	9.038	101.598	10	0.13032	9	4.595	253.995
5	0.06516	4	8.298	126.998	11	0.14335	10	3.855	279.395
6	0.07819	5	7.557	152.397					









