BOYLSTON

MEDICAL PRIZE DISSERTATIONS

FOR THE YEARS 1819 AND 1821.

EXPERIMENTS AND OBSERVATIONS

ON

THE COMMUNICATION BETWEEN THE STOMACH AND THE URINARY ORGANS,

AND

ON THE PROPRIETY OF ADMINISTERING MEDICINE BY INJECTION INTO THE VEINS.

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BOSTON:
OLIVER EVERETT AND JOSEPH W. INGRAHAM,
1821.

PRINTED BY JOSEPH W. INGRAHAM.

TO

DR. WILLIAM HOOKER, M. M. S. S.

DEAR SIR,

In presenting these Dissertations to the publick, I am naturally reminded anew of the many obligations which I owe to you, as my earliest professional instructer, and my constant friend. If you find in them any thing of that disposition to accurate observation and rational deduction, which it was the design and tendency of your instructions and example to teach, I doubt not you will regard them with a favourable eye.

That the questions, which I have here attempted to answer, are in themselves interesting and important, is sufficiently shown by the curiosity which they have excited, for a very long period of time; and I am encouraged, by the judgment which the Boylston committee have passed in their favour, to hope that the answers that I have given will not be thought wholly unworthy of the notice of the publick. Indeed they may be said to have been already in some measure published, in having been submitted to the examination of the committee, and their decision having been made known to the world.

There is a difficulty, of no small magnitude, in an experimental investigation, particularly of physiological questions, arising from the want of suitable subjects for experiment. Experiments must be performed on living animals. But the difference in habits and character, between man and the smaller animals, is so great, as to give a great degree of uncertainty to the deductions from the results of experiments made on them. Thousands of animals have died in the cause of physiological science, whose deaths have scarcely added any thing to our knowledge of the laws or properties of the human system. Experiments on our own race can never be performed to any considerable extent. If they are hazardous in their nature, they of course are never to be attempted, even if subjects could be found who would be willing to undergo them. And when they are not so, none but professional men can estimate the degree of inconvenience or risk to which they may be subjected by submitting to them. To obviate these difficulties, I have in the first dissertation made myself the subject of my experiments. This circumstance necessarily limited very much the number and diversity of the experiments. I trust, however, that, aided by what has before been done, and by the light which diseases have thrown upon the subject, they will be found sufficient to furnish a satisfactory answer to the question discussed.

In the second dissertation, the objections to experiments on animals could be in some measure obviated, because the nature of the subject admitted of a direct comparison of the effects produced upon different animals of the same species.

The importance of the question was sufficient to justify the sacrifice of a few of the less valuable animals ; especially since this was the only way in which it could, in the first instance, be investigated. For no man would be rash enough to risk an attempt of this sort upon an individual of the human race, until the experiment had first been made upon some other species of animal. Indeed the result has shown that even here, the analogy between the smaller animals and man, is not sufficiently strong to enable us to anticipate with much confidence the effects of an experiment upon one, because similar effects have been produced from the other.

I trust you will excuse the liberty I take in prefixing your name to these Dissertations ; and thus connecting it with the production of a pupil, who owes so much to your instructions and example.

With grateful and affectionate respect,

Your friend and servant,

THE AUTHOR.

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DISSERTATION I.

ON THE QUESTION

IS THERE ANY COMMUNICATION FROM THE STOMACH TO THE BLADDER, MORE DIRECT THAN THAT THROUGH THE CIRCULATING SYSTEM AND THE KIDNEYS?

WHICH OBTAINED THE BOYLSTON PREMIUM

IN 1819.

"Oculi mei non adeo Lyncei, ut hunc, nodum solvere queam."—RUYSCH.

DISSERTATION

ON THE COMMUNICATION BETWEEN THE STOMACH AND URINARY ORGANS.

HISTORICAL SKETCH OF OPINIONS UPON THE SUBJECT.

An opinion that there is some passage for fluids, from the stomach to the bladder, more direct than that through the circulation and kidneys, appears to have prevailed from the earliest periods of medical history. Hippocrates speaks of the passage of matter from the abdomen, through the veins, into the bladder; and evidently supposes a communication between the liver, and stomach, and urinary organs.* As other vessels were formerly called *veins*, besides those which return the blood to the heart, his language gives us no very

* "Quibus inter septum transversum, et ventriculum pituita concluditur, et dolorem exhibet, neque in alterutrum ventrem viam habet, iis per venas in vesicam pituita versa, morbi fit solutio." Hip. Aphor. lib. vii. Aph. 54. Fœsius' Latin edition. Frankfort, 1595, sect. vii. p. 360. See also "De Morbis." lib. iii. Ibid. sect. v. p. 47.

precise idea of his opinion as to the nature of the communication.

Galen expresses the same opinion more explicitly; and, in support of it, takes notice of the circumstance, that, in certain cases, fluids taken into the stomach are very speedily rejected by urine; much sooner, as he remarks, than the same fluids pass through the unobstructed canal of the intestines. This circumstance continues to be a leading argument upon the question; and has been quoted by almost every writer upon it, since the days of Galen, in proof of a direct passage from the primæ viæ to the bladder. Galen, as well as Hippocrates, attributes the communication to the veins; and points out more particularly, the vessels which he supposes to be concerned in it.* But since the discovery of the circulation of the blood by Harvey, it is unnecessary to observe, that there is no such communication between the blood-vessels of the kidneys and those of the stomach, as is here supposed.

* "At quod affatim, celeriterque ad vesicam urinæ defertur, id neque ad ventriculi, neque jejuni, neque tenuium intestinorum imbecilitatem, tanquam causam referendum est. Si enim ob id, quod potum sustinere non possunt, ad excretionem properant, quid obstat, quo minus per sedem excernantur?"

Quippe in libris de naturalibus functionibus jam ostensum est, jecur ex ventriculo per mesentericas venas, velut arbores a terra per radices, cibum ad sese allicere; sic etiam renes id, quod aquosum, in sanguine est." De Locis Affectis, lib. vi. Gesner's Omnia Opera Galeni. 1550. vol. ii. p. 953.

Alpinus quotes the opinion of Galen without addition, and then proceeds to a consideration of the different appearances of the urine as applicable to medical purposes.* This was a pursuit, in which the ancients were very much engaged. They examined the urine to discover the nature of diseases, and their indications of cure, more than to investigate its physiological properties. Although they did not carry their attention to this branch of pathology to so absurd a length, as that to which it was carried at a subsequent period, when the state of the urine was almost the only guide by which the physician was directed; yet they attributed a much greater importance to it, than it is allowed by modern physicians. Hippocrates and Galen, as well as Alpinus, wrote upon the several kinds of urine, considered in a pathological point of view, and the indications of their different appearances.

Fabricius ab Aquapendente speaks very confidently of the direct passage of fluids from the stomach to the kidneys and bladder. He supposes the fluids to exude from the stomach and other abdominal viscera into the cavity or sac in the abdomen, produced by the folding of the omentum, and to be transfused from this cavity into the urinary passages, by a species of absorp-

* Alpinus De Præsagienda, lib. vii. Boerhaave's edition. tom. i. p. 493.

tion through their coats. This opinion is founded principally upon the appearance of the depositions in the urine of persons who are recovering from pleuritis, and other inflammatory diseases; which he attributes to a transposition of matter from the inflamed part to the bladder. He also quotes Hippocrates and Galen for the observation, that the matter of abscesses in the lungs is sometimes discharged per urinam.* This theory, and the ingenious arguments by which it is supported, are sufficiently disproved by the discoveries of modern anatomy. It is now unnecessary to remark, that no such exudation or transfusion can take place, through the membranes of the living body.

Thomas Bartholinus appears to have been the first who attempted to exhibit and trace the course of the vessels, which were supposed to constitute the medium of the communication in question. Having been engaged in repeating the experiments of Pecquet, and extending his discoveries respecting the lacteal vessels, Bartholine

* "Sed et nos, et omnes medici observarunt, passimque observant; imo alias, cum thoracis venae sine pari anatomen administrarem, pleuriticus occurrit curandus, qui turbidas crassasque mingens urinas, subjugales appellatas, quales in malignis febribus apparent, cito sernatus est, egoque ed ex urinis meis auditoribus prædixi." De Omento. Opera. Anat. Fab. Lugdani, p. 126.

believed that he had discovered branches of those vessels sent off to the kidneys, and to the uterus, as well as the trunk to the subclavian vein. "They squeeze out the serum," he says, "being separated from the chyle in that light preparation, and endeavouring to expell, either into the reins hard by them, or into the emulgent arteries to which they send branches, or into the capsules of the Atrabilis destined for melancholie, or lastly into the doubling of the peritoneum, in which they abide."*

"The way is now clear," he adds, "and the shortest cut found, by which drinkers who like Promachus who wil drink foure gallons for his own share, or drink like Bonesus more than any man beside, who are not born to live but to drink, how they came to pisse this again so soon, and so abundantly." "For the ordinary way through the *liver*, *heart*, *arteries*, *emulgent veins*, *bladder*, is longer, and though Aquapendente take a great deal of pains to find a shorter through the *liver*, and *gastrick veins* of the stomach, and Piso and Comingius through the *spleen ;* yet he, finding the circulation, missed of his aim, and they, having found out ways in the conception of their own wit, cannot demonstrate them to the sense."†

* "Anatomical History concerning the Lacteal Veins, by Thomas Bartholinus," translation of 1653, p. 42. I have not seen the original treatise.

† Ibid. p. 43.

Diembroeck expresses some doubt about the nature of the communication, but is equally confident of the existence of some direct passage between the two classes of viscera. "These things," he says, "when formerly I seriously considered......I began to think that of necessity, besides the veins, there must be some other passages through which the more copious serum, and those hard substances, already mentioned, come to the bladder."* "However, for the better clearing of this difficulty, I would desire all anatomists, that they would use a little more than ordinary diligence, in search of these vessels, for the common benefit, to the end that what is now merely conjectured at, may come to be evident by solid demonstrations."†

In 1667, a letter was published in the London Philosophical Transactions, giving an account of a case in which air blown into the thoracic duct, was found to inflate the emulgent vein. This circumstance was considered by the author as evidence of a direct communication between that duct, or the primæ viæ, and the kidneys; but as it has never been confirmed by subsequent observations, it cannot now be regarded as of much weight in deciding the question. The same

* Diembroeck's Anatomy, translated by William Salmon, edition of 1694. book i. sect. xxxi. p. 122.

† Ibid. book i. sect. xxxii. p. 123.

Transactions for the next year, contain a paper by Dr. Fairfax, in which he supports the same opinion; and in confirmation of it, gives the particulars of a case where a bullet was voided by urine, which he supposed to have been swallowed.

In 1671, there were published in the Philosophical Transactions, almost the first experiments, which appear to have been performed for the purpose of deciding this question. They were the production of a French gentleman whose name is not mentioned. In these experiments, the ureters of a dog were tied, and the urine forced out of the bladder; and the animal being killed in a few hours after, the state of the parts was examined. At the first attempt, no urine was found in the bladder, after the ligatures were applied, and no swelling above the ligatures. The author attributes this circumstance to the injury of the organs by exposure to the air ; and in the second experiment took care to make only a very small opening in each side, for the purpose of applying the ligatures, and of emptying the bladder, and to close it immediately after that was done. Three hours after, a small quantity of urine was found in the bladder, " and the ureters above the ligatures seemed to be a little swelled." The experiment was not repeated a second time. Dr. Darwin says, that

Dr. Kratzenstein performed a similar experiment with the same result, and refers to Haller's Disputat. Morbor. ;* but I have not been able to consult this work of Haller's, and therefore know nothing of the details of the experiment.

Drelincourt, it is said, repeatedly tried the same experiment, taking care not to injure the other contents of the abdomen ; but no urine was found in the bladder, after the ureters were tied. The same was the result when he tied the emulgent arteries. The urine which was in the bladder at the time the ligatures were applied, was immediately evacuated, and some time after the animal died lethargick; but upon opening the bladder it was found perfectly empty."†

Bellini, in his elaborate treatise, "De Urinis et Pulsibus," says very little of the physiological properties of the urine, and scarcely mentions the supposed passage from the stomach to the urinary organs. But from the manner in which he speaks of the subject, it is manifest that he placed very little confidence in the opinion of its existence.‡

* Zoonomia, sect. xxix. 3.

† I have not seen Drelincourt's Anatomy, in which these experiments are given ; but have taken this account of them from Boerhaave's lectures, vol. iii. p. 155.

‡ Quoniam liquidum, et dura, quæ naturalem urinam constituunt, solvuntur, et libere confunduntur cum sanguine in

Etmuller refers to Bellini, but gives no opinion of his own, respecting the manner of the secretion or excretion of the urine.*

A paper by Morin was published in the memoirs of the royal academy of sciences of Paris, for 1701, in which, says Boerhaave, "this opinion was proposed by its author, with so many seeming advantages on its side, that he hoped it would be universally allowed without difficulty by all." "It was therefore the opinion of the learned members of that academy, that the urine properly so called, had a different passage, from that which is commonly known, into the bladder, by whose pores it was absorbed, without first passing through the kidneys."†

Morin describes two kinds of urine, one of which takes the course that has always been assigned to it, through the circulation and kidneys to the bladder; the other he supposes to be pressed from the aliment through the coats of the stomach into the cavity of the abdomen, as water is pressed out of a sponge by the hand, and

singulis partibus, in quibus humores aliqui secernuntur; nisi igitur omnes secretiones, vel earum aliquæ, vel aliquot naturali modo, procedant et perficiantur, urina naturalis reddi non potest, et ea statu recedet, prout feret alterarum separationum necessitas." Bellini "De Urinis," &c. Boerhaave's edition, p. 24.

* Omnia opera. vol. i. p. 364.

† Boerhaave's Lectures, iii. p. 136.

to be absorbed in a similar manner, by the force of the abdominal muscles into the bladder.* His theory, therefore, is very much like that of Fabricius, already quoted. The principal circumstance upon which Morin founds his opinion, is the facility with which water passes through the coats of the stomach, and those of the bladder, after death, when either of those viscera are

* "Les voies des premiéres urines [the watery urine,] sont les pores du ventricule, et peut-être des intestins, les interstices, qui sont entre les intestins dans la capacité du bas ventre, et enfin les pores de la vessie même ; et les voies des secondes urines, [common, coloured urine] sont les artères emulgentes, les reins, et les urètérs."—Mem. de l'Acad. R. des Sciences de Paris, 1701, Barryat's Coll. Acad. tom. i. p. 694.

"Ne concévra t-on pas bien plutôt qu'il en est des alimens nageans dans la liqueur, comme d'une éponge remplie d'eau, qui venant à être pressés entre les mains, laisse échapper l'eau dont elle est pleine, ... ; de même les alimens étant pressés par les parois du ventricule qui les embrasse, laissent échapper par ses pores une partie de la liqueur dans làquelle ils nagent, et ils en laissent échapper plus ou moins, suivant qu'il y a plus ou moins de liqueur, plus ou moins d'alimens, et suivant que la pression est plus ou moins forte."

"Or cette pression du ventricule n'est jamais assez forte pour exprimer toute la liqueur qui est mêlée avec les alimens ; mais elle est telle, qu'elle leur en laisse autant qu'ils en ont besoin pour aider à leur digestion ; et cette quantité de liqueur qui demeure avec les alimens, est celle qui dans la digestion dévient partie du chyle, passe par les artères et les veines, et est ensuite rendue chargée couleur, par la vessie ; et c'est ce que je nommerai les secondes urines."—Ibid. tom. i. p. 695.

separated from the body, and filled with that fluid. It is unnecessary, in the present state of anatomical and physiological knowledge, to observe, that this circumstance furnishes no criterion by which to judge of the state of things, during life. It is now sufficiently well established, that there are no pores in the coats of the hollow viscera, through which their contents can pass during life into the contiguous cavities. The absurdity of this theory is very fully shown by Boerhaave, in the remarks upon this subject in his lectures.

In the London Philosophical Transactions for 1709, there is a paper by Mr. Yonge, of Plymouth, giving an account of some hair that was passed by a young woman with her urine, which he considers as evidence of a direct communication between the primæ viæ and the bladder; but by what means, he does not attempt to explain. The hair was submitted to the microscopical examination of Lewenhoeck, whose remarks upon it are exceedingly curious, though not particularly connected with the present subject. Mr. Yonge attempts, in a second paper, the same year, to convince the incredulous, and establish his opinion, by quoting the opinions of different writers upon the subject, and by collecting a variety of accounts of extraneous substances which are said to have been passed by urine. He, however, gives no new facts of any importance. The validity of the argument, deduced from the occasional passage of these substances, will be examined in a subsequent part of this paper.

Several other cases of a similar kind are given in the Philosophical Transactions for a number of years following; but they contain nothing that is materially different in its character from those already mentioned.

Boerhaave very explicitly declares his disbelief of the existence of any such passage as has been supposed. He accounts for the great flow of urine in certain cases, by the size of the vessels of the kidneys, and the ready communication between the terminations of their arteries and the commencement of the urinary tubes. He quotes many facts in support of his explanation; and at the same time, comments upon the arguments in favour of an opposite theory.*

Darwin, on the other hand, with at least equal confidence, adopts the opposite opinion. His theory is, that liquids are taken up by the absorbent vessels of the stomach, and carried into those of the bladder, by the anastamoses of their branches; and from these by a retrograde action of the vessels, into the bladder.† This theory rests entirely upon the supposition of the

* Boerhaave's Lectures, vol. iii. p. 154, et seq.

† Zoonomia, sect. xxix. 3.

possibility of the absorbent vessels' taking upon themselves a retrograde action, by which the fluids in them will be conveyed backward through the valves to their origin. The only grounds upon which its author founds it are, his confidence that some passage exists, shorter than the course through the circulation, and his ignorance of any other than this. Darwin also gives two or three experiments, in support of the general position, that liquids taken in drink pass to the bladder, without going through the circulation; which will be more particularly noticed hereafter.

Sir Everard Home's papers, in the Philosophical Transactions for 1808 and for 1811, contain some very valuable experiments and observations, which are applicable to the question under consideration. These experiments show, in a manner which, so far as I know, has never been controverted, that a large portion of the fluids received into the stomach, are absorbed directly into the blood, without taking the comparatively circuitous route through the lacteal vessels. They also determine with more precision than had before been done, the length of time necessary for liquids to pass from the stomach to the bladder. These facts, although they shorten very considerably the route by which fluids may pass from the stomach into the circulating system, have no reference to any communication between that viscus and the bladder, which shall exclude the circulation and the kidneys.

I have thus noticed all the writers of any considerable importance, upon this subject, whose productions I have been able to procure. Santorini, Blegny, Borichius, Duverney, Grimaud, and a few others, are quoted, but I have been unable to obtain their works. Haller is also frequently referred to; but the reference is rather to the statements of facts which his works contain, than to any opinion or arguments of his upon this question.

It will be observed, that by far the greater part of the authors I have mentioned, are in favour of the affirmative side of the question. Indeed Boerhaave is almost the only one I have found, who has said any thing against it. Almost every modern systematick writer either takes no notice of the subject, or only mentions it to refer to those who have before written upon it. It should seem, therefore, that a great majority of physiologists have believed in the existence of a communication between the stomach and the bladder, more direct than that through the circulation and kidneys; or that they have considered the inquiry as of too little importance to excite their attention to it.

STATE OF THE ARGUMENT, FROM FACTS ALREADY PUBLISHED.

The leading, and by far the most important circumstances in favour of the existence of such a passage as is supposed, are the large quantities of urine which are sometimes passed, after taking large quantities of fluid into the stomach; the short time which in some cases intervenes between the time of taking and that of passing the liquids; and the similarity of the urine to the drink, in its appearance and character. In ordinary circumstances, the urine is a fluid of a character peculiar to itself, and apparently possesses at all times the same properties, except such variations as seem to arise from different degrees of dilution of its more active qualities, in the watery fluid, which always composes its principal part. In almost every respect, it seems totally unlike the drinks from which the fluids of the body must be chiefly supplied, and scarcely receives any modification from them; and although the quantity of urine, at all times, bears some proportion to that of the fluids received, the relation is so slight as to be hardly observable, unless the quantity of drink is entirely disproportionate to the wants of the system.

Yet there are many cases on record, in which large quantities of urine have been passed, within a very short time after similar quantities of drink have been taken, and possessing qualities exceedingly similar to it. The accounts of some of these cases are so extravagant, as to appear rather fabulous. But there are many others which are sufficiently authenticated; and the general results are confirmed by a simple experiment, which it is very easy for any person to perform for himself. Let any one take an unusual quantity of drink, and he will soon find his discharge of urine proportionably increased, or nearly so: and if the nature of the drink be such as to admit of easy detection, he will be able to discover traces of it in the urine discharged.

Bartholine quotes a considerable number of cases of this kind. "Saxonia," he says, "saw a nobleman of Venice, who, for some years together, drank always at dinner thirty-four glasses of wine, and yet quickly voided all."* "Zaccus Lusitanus," he adds, "tells us of one, who gave himself up to the pure juice of the grape, who did in a little time sup a great many cups, and those no small ones; but whatsoever he drank, he pissed it presently unchanged, both in smell, taste, and colour, and that which is beyond all belief, that drinking in the space of an hour twenty pounds of water, he did return it by urine most clear as

* History of the Lacteal Veins, p. 44.

he had drank it." "I care not for strange examples," he adds, "because I had experience of it lately in myself, in fits of a stone which sometimes like a hangman (that I may complain with Erasmus) does torture me, having drank a little Rhenish wine for a diuretick, afterwards it came out very clear in the chamberpot, alike both in colour and substance, and that this continued for some days unchanged."*

Many other such cases might be adduced from different authors, but it is unnecessary to multiply the examples. The same effect is produced upon the urinary organs, by the same cause, in health. Boerhaave says, "If a healthy person makes water, while fasting, and so soon as he is out of bed, the urine appears concocted, has a smell, a bitter saltness, and high yellow colour; if now the person drinks half a pint of warm spring-water, he will in a quarter of an hour's time, discharge urine almost inodorous, and more nearly resembling water; if again after this, as much more water be drank, the urine will be voided almost without smell or colour, and without any sediment. If thus water is continued to be drank in this manner, as I have sometimes done myself ten times, at last the water will be discharged little or nothing altered, flowing from the bladder as through a funnel, and with so great

* History of the Lacteal Veins, p. 45.

an inclination to excretion, as will scarcely permit the mind to influence the retentive organs."*

Until sir Everard Home's experiments in 1808, it was doubtful whether the speedy appearance of the urine in these cases was not the effect of a sympathetick excitement of the urinary organs, by the presence of a large quantity of liquid in the contiguous viscera, rather than of the passage of the same portions of fluid into those organs. Indeed it is not even now decided that this is not sometimes the case. But Mr. Home has shown that the same portion of fluid which is received into the stomach, will, under some circumstances, pass into the bladder, in a very short space of time. His experiments are so important, that I shall copy his account of them at length.

"Half an ounce of tincture of rhubarb, diluted in one and a half ounce of water, taken in the interval between meals, did not pass off by urine in less than an hour, and even then, was not in sufficient quantity to be discovered, till the test [caustick alkali] was applied.

"The same quantity was taken immediately before a breakfast, consisting of tea. In seventeen minutes, half an ounce of urine was voided, which, when tested, had a slight tinge. In thirty minutes, another half ounce was made, in which

* Boerhaave's Lectures, vol. iii. p. 125.

the tinge was stronger; and in forty-one minutes a third half ounce, in which it was very deep. In an hour and ten minutes, seven ounces were voided, in which the tinge of rhubarb was very weak, and in two hours, twelve ounces were voided, in which it was hardly perceptible.

"In six and a half hours the rhubarb acted on the bowels, and gave a decided tinge to the fæces; the urine made at the same time had a much stronger tinge than what was voided at one hour and ten minutes.

"In this experiment the rhubarb appeared to have escaped from the cardiac portion of the stomach; and in two hours ceased to pass through that channel; but was afterwards carried into the system from the intestines, and again appeared in the urine.

"This experiment was repeated on another person; the rhubarb was detected in the urine in twenty minutes. In two hours the tinge became very faint; and in five hours it was scarcely perceptible; in seven hours, the rhubarb acted on the bowels; and the urine made at that period became again as highly tinged as at first."*

In the same paper, and a subsequent one upon the same subject, Mr. Home has shown, I think very conclusively, that a very large portion at least of the fluids which are received into the

* Philosophical Transactions for 1808, p. 51, et seq.

stomach, are absorbed directly from that viscus; and do not, therefore, take the course of the chyle through the intestines, absorbents, and thoracic duct, to the heart. This discovery very considerably shortens the course, which was previously believed to be necessary for those fluids to take for them to become incorporated with the blood; and consequently, diminishes the necessity of supposing a communication more direct than through the circulation, in order to account for their speedy appearance in the urine.

The next most important argument, or at least one more frequently adduced than any other, to prove a direct communication between the stomach and the bladder, is derived from the occasional passage of hair, and other extraneous substances by urine. Cases are recorded, in which needles, bodkins, bullets, and some other articles besides hair, have been found in the bladder, or have passed the urethra. Bartholine quotes "the story of the Venetian virgin in Alexander Benedictus, and," as he says, "by others too is affirmed, which had swallowed a hair bodkin four inches long, and after four years piss'd it out of her bladder, envelop'd with calculous matter;"* and adds, "that nothing is more ordinary than pissing of hairs."†

* Anatomy of the Lacteal Veins, p. 49.

† Ibid. p. 50.

The Philosophical Transactions contain several papers upon this subject; and in almost every instance, the author considers it as nearly or quite certain, that the extraneous substance came from the stomach, by some route, of course, more direct than through the circulation and kidneys. The cases of Dr. Fairfax and Mr. Yonge have already been referred to. Others might be quoted were it necessary; but I presume the fact of their occasional occurrence is too well established, and too generally believed, to render any farther proof requisite.

It needs no argument to show that these substances could never have passed through the circulating system, to find their way into the bladder. But it seems not a little remarkable, that a circumstance of such exceedingly rare occurrence, should be considered as evidence of the universal existence of such a communication; and still more, that such large bodies should be supposed to have passed through vessels or pores, which are too small to be discovered by the minutest investigations of anatomists. The absurdity is still farther increased, by the length of time which these substances are always retained in the body. Four years, as in the case of the Venetian virgin, is a long time for a body to be detained in a passage so direct, as that which is supposed for the purpose of explaining these very phenomena.

The same kind of reasoning might be adopted to prove the existence of a direct communication between the stomach and almost every part of the animal system. Many cases, equally well established, might be quoted, to show that substances which were swallowed into the stomach, have been subsequently discharged through the integuments in remote parts of the body. Dr. Lysons, of Gloucester, gives a case in the Philosophical Transactions for 1769, of a girl who swallowed three pins, which were, long afterwards, discharged by an abscess in her left shoulder. Mr. Knight says, that sir Hans Sloane had a pin which had been swallowed by a young woman, and was afterwards taken out of an abscess in her thigh.* Mr. K. quotes this case in proof of a direct communication between the stomach and bladder, because the pin was encrusted with calculous matter. It is surprising, that it should not have occurred to him, that supposing the pin to have ever been in the bladder, it might have come there in the same manner, in which it afterwards passed from the bladder to the thigh.

The passage of extraneous solid substances from one part of the living system to another, is by no means confined to such as are received in-

* Philosophical Transactions, for 1741.

to the stomach. Dr. Lysons, in the paper just referred to, mentions a case, in which a needle that had been lodged in the left arm of a woman, six inches below the shoulder, was afterwards extracted from the right breast. Cases somewhat similar are not unfrequently met with in books and in practice. However difficult it may be to account for the transmission of the foreign substance, in these cases, without their producing more irritation and disease, no one will consider the occurrence as evidence of an open communication, by which they may pass without injury to the system. No more ought the passage of similar substances, from the stomach to the bladder, to be considered as proving a direct communication between those viscera.

It has never been proved that the hair, which is the most frequent of the foreign substances discharged by urine, ever entered the stomach. Lewenhoeck's comparison of the hair, in Mr. Yonge's case, to that which is found in the heel of a stocking which had been worn two or three weeks,* is the only attempt at proof, that I recollect to have seen. Indeed, since the discovery that hair is not unfrequently formed, in diseases of some of the organs contiguous to the bladder, particularly of the ovaria, I presume it

*.Philosophical Transactions, for 1685.

will no longer be contended that it is ever derived from any other source.

The experiment in the Philosophical Transactions, of the French gentleman already mentioned, has often been quoted with much confidence, as proving that some way exists for fluids to get into the bladder, besides through the ureters. Had the result of this experiment been fully confirmed, the question might have been considered as decided. But this is very far from being the case. This gentleman himself only obtained this result at one attempt, while the result was once directly of an opposite nature; and the observations of others have not by any means satisfactorily confirmed it. It is almost impossible to perform the experiment in such a manner as to be sure that the ureters are effectually obstructed, and the bladder completely emptied, without producing such an injury of the surrounding parts, by their exposure to the air, as to render it doubtful whether it is owing to the injury, or to the obstruction, if no urine is subsequently found in the bladder. It appears from Darwin's account of it, that in Dr. Kratzenstein's experiment, the only evidence that the bladder was completely emptied, was that a catheter was introduced, for the purpose of emptying it. Yet this experiment is quoted with much confidence, as proving the existence of the passage in question. On the other hand, the experiments of Drelincourt, already quoted from Boerhaave, give a directly opposite result; and this result is confirmed by the similar effect produced by tying the emulgent arteries. It is also farther established, by the appearances after death, in certain diseases, in which the ureters have been obstructed during life. Boerhaave says, "certain death follows, when a person has both kidneys, or both ureters obstructed by a calculus, or by any other cause; and on opening the bodies of such persons, after death, the bladder is found empty."*

Two cases of obstruction of both ureters are given in the London Philosophical Transactions. In the first, by Dr. Hardisaway, the entrance of the ureters was stopped by stones in the pelvis; but the state of the bladder is not mentioned.† In the other case, by Dr. Huxham, the passage was grown up in one ureter, and the other was entirely obstructed by stones in the pelvis and ureter. The woman had not passed any urine for fifteen days before death. The bladder was quite empty, and half rotten, and did not contain a drop of urine.‡

Dr. Darwin regards it as evidence of a direct passage, "that in some morbid cases, the urine

* Lectures, iii. p. 156.

† Philosophical Transactions, for 1723.

‡ Ibid. for 1744.

has continued to pass, after the suppuration or total destruction of the kidneys."* But from this circumstance, I shall draw an inference of a directly opposite nature, as will appear in a subsequent part of this paper. The same gentleman attempts to show by experiment, that substances which are taken in drink, are discharged by urine, without being conveyed through the blood. "A friend of mine," he says, "(June 14, 1772,) on drinking repeatedly of cold small punch, till he began to be intoxicated, made a quantity of colourless urine. He then drank about two drachms of nitre, dissolved in some of the punch, and eat about twenty stalks of boiled asparagus; on continuing to drink more of the punch, the next urine that he made was quite clear, and without smell; but in a little time another quantity was made, which was not quite so colourless, and had a strong smell of the asparagus: he then lost about four ounces of blood from the arm.

"The smell of asparagus was not at all perceptible in the blood, neither when fresh taken, nor the next morning, as myself and two others accurately attended to; yet this smell was strongly perceived in the urine, which was made just before the blood was taken from the arm.

"Some bibulous paper, moistened in the serum of this blood, and suffered to dry, showed no

* Zoonomia, sect. xxix. 3.

signs of nitre by its manner of burning. But some of the paper, moistened in the urine, and dried, on being ignited, evidently showed the presence of nitre."*

In another experiment, performed upon a person very much affected by diabetes, the result was as follows :

" November 1. Mr. Hughes dissolved two drachms of nitre, in a pint of a decoction of the roots of asparagus, and added to it two ounces of the tincture of rhubarb; the patient took a fourth part of this mixture every five minutes, till he had taken the whole. In about half an hour he made eighteen ounces of water, which was very manifestly tinged with the rhubarb, the smell of asparagus was doubtful.

" He then lost four ounces of blood, the serum of which was not so opake as that drawn before, but of a yellowish cast, as the serum of the blood usually appears.

" Paper dipped three or four times in the tinged urine, and dried again, did not scintillate when it was set on fire; but when the flame was blown out, the fire ran along the paper for half an inch; which, when the same paper was unimpregnated, it would not do; nor when the same paper was dipped in urine, made before he took the nitre, and dried in the same manner.

* Zoonomia, sect. xxix. 3.

"Paper dipped in the serum of the blood and dried in the same manner as in the urine, did not scintillate when the flame was blown out, but burnt exactly in the same manner as the same paper dipped in serum of blood drawn from another person."*

Mr. Home tried the same experiment, except that tincture of rhubarb alone was used to ascertain that the same fluid was present in the urine and the blood, which had been taken by mouth; but the result was very different. He says, "When blood is drawn from the arm of a person who has taken rhubarb in sufficient quantity to affect the urine, the serum is found to have a slight tinge from it, equal to that which one drop of tincture of rhubarb gives to half an ounce of serum, when added to it."†

It is much to be regretted that neither of these gentlemen has told us, with more precision, what length of time intervened between drinking the liquid which was sought in the blood, and drawing the blood for examination. It is at least probable that this circumstance alone would explain most of the difference of their results. Two drachms of nitre are not sufficient to impregnate the whole mass of blood, supposing it to have been mixed with that fluid; and therefore the

* Zoonomia, sect. xxix. 4.

† Philosoph. Transac. for 1808.

whole quantity might have passed through the veins, and yet have escaped detection, until it was separated from the blood by the kidneys, and appeared in the urine. It will be seen by the experiments which follow, that this explanation does not rest merely upon hypothesis, or conjecture.

On the whole it appears, that the principal ground for the very general belief of the existence of a communication between the stomach and bladder, more direct than through the circulation and kidneys, is the difficulty of otherwise accounting for the speedy appearance of fluids in the urine, which have just before been taken in drink. The most diligent research has never been able to discover the medium of such a communication; and no probable mode has ever been pointed out, by which it can take place. Every experiment, which has been performed, to show its existence, has been contradicted by others, performed under circumstances, which apparently entitle them to, at least, equal confidence. If then it should be made to appear, that all the phenomena connected with the rapid production of urine, can be explained by the natural functions of the organs which are known to exist, the necessity will be removed, of *supposing* such as have never been discovered.

OBSERVATIONS AND EXPERIMENTS.

In pursuing the investigation of this subject, it is not my intention to seek the passage in question, by any *anatomical* discoveries. After the laborious researches of Diembroeck, Ruysch, Malpighi, Lewenhoeck,* and other eminent anatomists, it would be presumptuous for me to expect to find what they could not find; or to regard my inability to discover it, as any additional proof of its non-existence. It is in this view, that I am disposed to say with Ruysch, in the words of my motto, "Oculi mei non adeo Lyncei, ut hunc nodum solvere queam."† My researches therefore have been purely physiological. Yet they are not on this account necessarily less conclusive. If the observations and experiments, which I now present, shall be confirmed by the judgment and experience of others, I flatter myself that the view which they will give of the subject under consideration will be such as to

* There is no direct evidence that all the anatomists, here mentioned, were ever engaged in seeking for a passage from the stomach to the bladder. But as they were engaged in researches upon the structure and functions of the kidneys, and other organs connected with them, at a time when this question excited a great deal of interest in the world, it is highly probable that they partook of the desire of determining it by their own observations.

† Thesaur. Anatom. iv. p. 41.

furnish a satisfactory and conclusive answer to the question proposed for this discussion.

The first object must be to ascertain, so far as it is practicable to do it, what length of time it would require, for fluids to be carried from the stomach to the bladder, through the medium of the circulation. This can never be determined with any considerable degree of precision; yet some approach to the knowledge desired may be made by estimating the rapidity and force of the blood in circulation.

The whole quantity of blood actually in circulation in an adult, cannot be estimated at more than from thirty to forty pounds, and is probably less. The quantity which the heart is capable of throwing out at each contraction of the ventricles is said, by Haller, to be about two ounces; and there are, in a middle aged person, at least, seventy pulsations in a minute. Making this the basis of our calculation; supposing there are forty pounds of blood, it will take four minutes and thirty-four seconds, to complete one entire revolution of any given portion of the blood, including its course through the lungs. If the quantity of blood be only thirty pounds, the same revolution will be accomplished in three minutes and twenty-five seconds. But it is not certain that the heart actually throws out at each contraction, the whole quantity of which it is capable. Suppose it to throw out only three fourths of the full quantity, making an ounce and a half at a pulsation; in this case, six minutes and fifty-two seconds will be required to complete the circulation of forty pounds of blood; or five minutes and one second for thirty pounds.

It is to be remembered, however, that the course from the place where the fluids of the stomach enter the blood, to the kidneys, is very far short of a complete circulation through the whole body. I shall here take it for granted, that the absorption of fluids directly from the stomach, is sufficiently established by Mr. Home. The distance from the stomach to the kidneys, through the circulation, then, is that from the stomach to the heart, through the lungs, back to the left side of the heart, and from the mouth of the aorta to the terminations of the emulgent arteries. This at the most, cannot be supposed to exceed one half of an entire revolution in the circulation; and is probably less than one fourth. The time requisite, therefore, as far as the circulation is concerned, for fluids to pass from the stomach to the kidneys, is not more than from two to three and a half minutes; leaving the remainder of the time, which actually intervenes between their being taken in drink, and ejected in urine, to their absorption in the stomach, and their secretion and excretion in the kidneys and bladder.

I do not pretend that this calculation gives with precision the length of time required for the purposes mentioned in it. But I am persuaded that the errours are upon the side of allowing too much, rather than too little time. Neither do I rely upon it as evidence that the fluids actually do pass through the blood, to go from the stomach to the bladder. I bring it forward here merely to show, that from the great rapidity of the circulation, the supposition of their passing in this way is not so absurd, as it seems generally to have been considered.

We have no data, by which to determine with accuracy how long the kidneys are occupied, under different circumstances, in accomplishing their secretion. Yet there are some facts, which go to show that they sometimes perform it very quickly; and there is no reason to suppose that the urine is detained in the pelves or ureters, or necessarily in the bladder, after it is secreted. If a person is suddenly exposed to violent cold, or to fright, or anxiety, very soon after having voided his urine, it often happens, that he will almost immediately pass a very considerable quantity more, which will be watery and colourless, without his having taken any fluid in the mean time. As this last urine is entirely different in its appearance, from that which preceded it, it must have been secreted after that was ejected. Indeed the structure of the kidneys, and the size of their vessels seem adapted to the secretion of a large quantity of fluid in a short space of time; especially when they are stimulated to more than usual activity, by the substances brought to them in the blood, such as are the fluids which admit of the most ready detection in the urine. The emulgent arteries, as Boerhaave observes, are much larger in proportion to the size of the organs they supply, than those of any of the other viscera.

We have next to inquire, what length of time is consumed in the absorption of the fluid from the stomach into the blood. But here we have nothing to guide us in the inquiry. I will only remark, therefore, that the theory of a direct passage to the bladder is equally subject to the same objection. For, as it is not pretended that there is any considerable vessel, by which fluids are conveyed from one viscus to the other, they must necessarily be absorbed; and no reason can be given why they should not be absorbed as speedily through the vessels which convey them directly into the blood, as in any other way.

So far then as any reliance can be placed upon this calculation, it appears that the course from the stomach to the bladder, through the circulation, is hardly less direct and open, than any, which can possibly be supposed to exist. That it is sufficiently direct to admit of an explanation of the phenomena, is all that is necessary for me to show.

I will now proceed to the experiments which I have performed for the purpose of ascertaining whether in fact there is any other course, by which the fluids of the stomach may pass off by urine.

EXPERIMENT I.

I had eaten my usual breakfast of coffee and roll at eight o'clock, besides which I had taken nothing since the evening preceding; and in the course of the morning, had taken a little moderate exercise. At eleven o'clock, the temperature of the room was 62° ; a thermometer under my tongue stood at 98° ; the pulse was seventy in a minute. The whole quantity of urine made since twelve o'clock of the night before, was seventeen and a quarter ounces.

At eleven, the bladder having been previously emptied, I drank a pound of water, of the temperature of 58°, in which was diffused two drachms of spirit of the gaultheria procumbens.

In ten minutes, three drachms of urine were voided. It had rather a lighter colour than that which was passed before; and had the usual urinous smell.

In thirty minutes, two and three quarter ounces more were voided. The colour was still lighter than the last; odour the same.

In forty minutes, voided four and a half ounces. It was nearly colourless; odour nearly the same, not at all that of the gaultheria.

In fifty minutes, voided four and three quarter ounces. It was perfectly transparent, and colourless; and had distinctly the odour of the gaultheria.

In an hour, voided five and a half ounces; which was colourless; and had the odour of the gaultheria, but less distinctly than the last.

In an hour and ten minutes, voided three and a quarter ounces. It had a very little more colour than the last; but had no odour perceptible.

In an hour and twenty minutes, voided three fourths of an ounce. It was very nearly of the same colour as that passed at thirty minutes; and had the usual urinous smell.

In an hour and thirty minutes, voided half an ounce. It had the same colour as that passed at ten minutes; and its odour was urinous.

The whole quantity excreted in an hour and a half, was twenty-two ounces and three drachms; six ounces and three drachms more than had been received into the stomach. In the eleven hours preceding, seventeen and a quarter ounces had been secreted, which is at the rate of an ounce and a half to every hour. So that in the hour and a half of the experiment, the secretion was *increased* three ounces and seven drachms, besides the quantity of fluid taken within that time.

EXPERIMENT II.

Having learned from Mr. Home's paper on the spleen, that tincture of rhubarb is readily discovered in the urine, by the aid of a caustick solution of potass, I made several trials, some of which were partly taken from his paper, to ascertain more particularly the effects of the test, and to serve as a standard of comparison. The following are the results.

1. In common urine, the alkaline solution produced no change.

2. Four drops of tincture of rhubarb, in four ounces of well-water, gave a decided yellowish tint; which was converted into a light wine-red by the addition of the alkaline solution. N. B. Sixty drops of the same tincture, and dropped from the same phial, weighed exactly half a drachm.

3. Four drops of tincture of rhubarb, added to four ounces of common urine, gave a slight

orange cast to the colour, which was increased by the alkali; but was not farther increased, by adding four drops more of the tincture. Eight drops more, gave a distinct orange colour.

4. Twelve drops of the tincture, added to six ounces of another portion of urine, more watery than the last, turned it to a lighter yellow. The colour was deepened by adding the alkaline test.

5. Eight drops of the tincture in four ounces of the colourless urine, which was made in the last experiment, gave a light yellow colour, which was changed by the test to a bright wine red.

6. One drop of the tincture in a drachm of serum of the blood, gave a yellowish tinge, which was a little deepened by the alkali. The change by the alkali was much more observable, with two drops to the drachm, and still more with three. In these cases, the alkali gave more of a reddish cast to the colour.

EXPERIMENT III.

The temperature of the room was 56°; that of my body, as exhibited by a thermometer under the tongue, 98°; pulse seventy-six in a minute. I had breakfasted as usual at eight o'clock, and had taken nothing else, since the evening before. The whole quantity of urine excreted in twelve hours, since eleven o'clock of the evening preceding, was sixteen ounces. It had the usual appearance, and underwent no change by the addition of the solution of potass.

I mixed an ounce by weight of tincture of rhubarb with fifteen ounces of water, and added thirty drops of tincture of opium, in order to prevent or retard the effect of the rhubarb upon the bowels. The temperature of the mixture was 51°.

At eleven o'clock, immediately after evacuating the bladder, I drank the whole of the mixture; and also three or four drops of volatile oil of cinnamon in about two drachms of spirit, to cover the taste of the rhubarb.

In ten minutes, a quarter of an ounce of urine was discharged. It was about the same colour as that passed before, and received no change from the test.

In twenty minutes, three fourths of an ounce was discharged. It was of a light yellowish colour, and was changed by the test to a dull reddish yellow. As soon as possible, I opened the median vein of the left arm, and took two and a quarter ounces of blood. This was completed in less than five minutes.

In thirty minutes, an ounce of urine was discharged. It was of a very clear, bright yellow colour, and was changed by the alkali to a bright wine-red.

In thirty-five minutes, I attempted to take more blood from the orifice in the vein; but without success.

In forty minutes, voided two ounces of urine. Its colour was a bright yellow, rather lighter than the last, very clear; and was changed by the test to a clear wine-red. Immediately after, I opened the basilick vein, and took an ounce of blood. This was accomplished in less than two minutes.

In fifty minutes, voided three and a half ounces of urine. It was very clear, and its colour a rather lighter yellow than the last. The red which was produced by the test, was also considerably fainter. I attempted to re-open the vein, but could obtain only a very small quantity of blood; and the cellular membrane was so extensively injected with blood, that I could not obtain a suitable place for another opening. As I could not bleed myself in the right arm, and had no assistant at hand to do it for me, I was therefore obliged to abandon the remainder of this part of the experiment.

In an hour, three and a half ounces of urine were discharged. It was very clear; but its colour was still lighter than the last, and the red, produced by the alkali, still fainter.

In an hour and ten minutes, voided two ounces. It had rather more colour than the last, and the test produced a deeper red; though the difference was not very considerable.

In an hour and twenty minutes, voided three fourths of an ounce. Its colour was a still deeper yellow, and rather more of an orange cast. The alkali changed it to a red, as deep and as clear as that passed at forty minutes.

In an hour and a half, voided an ounce and a quarter. The colour was still a brilliant yellow, but rather more inclined to that of natural urine. The red produced by the test was still more intense than the last.

In an hour and forty minutes voided an ounce and a quarter. Its appearance was about the same as the last, both before and after the test was added.

In two hours, an ounce and a half of urine was discharged, which still retained the same appearance and character.

At five o'clock, six hours from the commencement of the experiment, the urine was still of a deep yellow colour.

The bowels were not affected by the rhubarb, until about eight o'clock in the evening, and then only one slightly cathartick discharge took place.

The next day I carefully examined the blood which had been drawn. The serum of the two first portions was evidently tinged by the rhubarb; that of the second more so than the first.

It left the peculiar stain of rhubarb upon the glass. The colour in both was a little deepened by the test. The quantity of the third portion was too small to permit me to decide with confidence; but it appeared to be more deeply coloured than either of the others.

In this experiment, the quantity of urine discharged in an hour and forty minutes, was sixteen ounces: just equal to the quantity of fluid taken into the stomach, without making any allowance for the natural secretion of urine, during the time of the continuance of the experiment.

The decrease of the quantity of urine, and of its colouring matter, at about an hour from the beginning of the experiment, and their subsequent increase, entirely coincide with the results of Sir E. Home's experiments; and may perhaps be considered as affording some confirmation of his position, that the principal part of the fluids is absorbed directly from the stomach, while another portion takes the course of the lacteals with the chyle.

EXPERIMENT IV.

If the liquids taken into the stomach, pass directly to the bladder, without being mixed with the fluids of the body, it is obvious that a small quantity of colouring matter, sufficient only to give a decided tinge to the quantity of liquid received, will appear in the urine as soon after it is taken, as a much larger quantity. But if these liquids pass into the circulation, and are mixed with the mass of blood, the colouring matter must be too much diluted to be discovered in any of the animal fluids, until a sufficient time has elapsed for the kidneys to separate it from the blood, in the secretion of urine; which will then be tinged by it. The object of this experiment was to ascertain which of these circumstances actually takes place.

I had already observed (Experiment II, Nos. 3, 4, and 5,) that two drops of tincture of rhubarb were sufficient to give a decided tinge to an ounce of every kind of urine. But in order to make allowance for any fluids which it might casually meet with, I doubled the proportion of rhubarb, and put sixty-four drops of the tincture into sixteen ounces of a mixture of cider and water, about one fourth part of it being cider. That I might, at the same time, ascertain whether the coldness of the liquid in the stomach hastened or retarded the excretion of urine, I reduced the temperature of the mixture to 32°.

I had breakfasted upon coffee and roll, as usual, at eight o'clock; and had taken nothing else since the evening before. At twelve, when the experiment began, the temperature of the room was 44°; my pulse beat eighty in a minute; a thermometer under the tongue stood at 97°. The quantity of urine, passed in the twelve hours preceding, was eighteen and a half ounces.

I drank the mixture, abovementioned, precisely at twelve o'clock; and in ten minutes after voided one drachm of urine, which was not at all tinged by the rhubarb. I felt somewhat chilled by the coldness of the mixture; and the thermometer under my tongue was nearly, though not quite, a degree lower than at first.

In twenty minutes, half an ounce of urine was discharged. The colour was rather lighter than that of natural urine; and was not in the least degree affected by the alkaline solution.

In thirty minutes, voided an ounce and a quarter. It was lighter coloured and more watery; and was not changed by the test.

In forty minutes, voided two and three quarter ounces. It was clear, and very nearly of the colour of common rain-water. The colour was not affected by the test.

In fifty minutes, voided four ounces. Its colour was nearly the same as the last; but on adding the test, it was changed to a faint reddish hue, indicative of the presence of rhubarb.

In an hour, voided five ounces. It was very clear, and slightly inclined to a straw colour; which was changed by the alkali to a faint wine-red. By comparing the portion into which the alkali was poured with the remainder, the change was very conspicuous.

In an hour and ten minutes, voided two and a half ounces. It was very clear, and of a light straw-colour; and was changed by the test to a light wine-red. The colour, both before and after the addition of the alkali, was deeper than the last.

In an hour and twenty minutes, voided one ounce. It was of a bright straw colour; and was changed by the test to a deeper reddish yellow.

In an hour and a half, voided half an ounce. It was of a deep straw-yellow colour, and received a reddish tint from the alkali.

In two hours, passed two ounces. The colour was a bright yellow, very obviously produced by the rhubarb. It was changed by the test, to a deeper and more reddish yellow, like that of the third experiment, No. 3, except that it was deeper.

At twenty minutes past three o'clock, three ounces were discharged, which was of a bright yellow colour, but not so deep as the last. It was changed by the alkali to a more reddish yellow.

At four o'clock, another ounce was discharged, which was similar in its appearance to the last.

At seven o'clock, voided six ounces, which exhibited no trace of the rhubarb.

In this experiment, the increased discharge of urine evidently continued, until more than two hours after the liquid was taken. In the two hours, nineteen ounces and five drachms of urine were excreted, which is three ounces and a half more than the liquids taken, and nearly half an ounce more than the amount of these liquids added to the quantity of urine which would have been secreted if no such liquids had been taken; upon the supposition, that the secretion would have continued in the same ratio as it had done for the twelve hours preceding.

EXPERIMENT V.

The object of this experiment, besides its general effect in confirming or weakening the confidence to which the others are entitled, was to ascertain if the appearance of the rhubarb in the urine is hastened or retarded, by the quantity of liquid with which it was taken: and secondly to determine if it continues to be found in the blood as long as it appears in the urine.

I had taken nothing since the evening preceding, except my breakfast, which had been as usual at eight o'clock. At the commencement of the experiment, the temperature of the room was 44°. My pulse beat seventy-five in a minute. The amount of urine in the last fifteen hours was sixteen ounces.

I first opened the median vein of my left arm, and took an ounce of blood, to serve as a standard of comparison. At twelve o'clock, I drank undiluted an ounce of tincture of rhubarb, in which were thirty drops of tincture of opium; and to cover the taste, I took, at the same time, three or four drops of oil of cinnamon, in a very little common spirit.

In ten minutes, one drachm of urine was discharged, which was of a natural colour, and was not affected by the alkali.

In twenty minutes, voided a drachm and a half. It was obviously coloured by the rhubarb; and was changed very considerably by the alkali. Immediately after, I made another opening in the median vein, and took three ounces of blood. This was accomplished in less than two minutes.

At forty-five minutes after one, voided two and three quarter ounces of urine; and at five o'clock, four and a half ounces more, both of which were deeply coloured by the rhubarb.

At six o'clock, voided an ounce and three quarters. It was evidently tinged by the rhubarb, but less so than that which had been previously discharged. I immediately opened the median basilick vein, and took two ounces of blood.

No more urine was discharged until half past nine, in the evening, when five ounces were passed, at the same time that there was a cathartick discharge from the bowels. This urine was of the usual colour and appearance; and contained no trace whatever of rhubarb.

The next day, I compared the serum of the blood, which was drawn at the different times. That which was taken at twenty minutes, was very considerably coloured by the rhubarb, and received a deeper tinge by adding the alkali. The colour of this portion very nearly resembled that produced by two drops of the tincture of rhubarb in a drachm of pure serum, and was nearly as much changed by the test. The serum of the blood taken at six o'clock was not at all tinged, but appeared, in every respect, like that drawn before the rhubarb was taken.

Both the blood and the urine were, in this experiment, more deeply coloured in twenty minutes, than they were, in the same length of time, in the third experiment, in which the same quantity of rhubarb was taken in a large quantity of liquid.

The following conclusions appear to result from the facts observed in the foregoing experiments.

1. The speedy discharge of watery urine after taking a large quantity of liquid in drink, is not occasioned by sympathetick excitement of the

urinary organs, but by the actual excretion of the fluid received.

2. The same portions of fluid, which are received into the stomach, begin, under certain circumstances at least, to be collected in the bladder within twenty minutes from the time when they are taken.

3. When a large quantity of fluid is taken, the excretion of urine is greatly increased; but the increase does not bear any very exact proportion to the quantity of drink. The increased discharge begins in about twenty minutes, is at its height in about an hour, and terminates, generally, in less than two hours.

4. When a liquid which is coloured with rhubarb is taken, the colour of the rhubarb appears in the urine ; and its appearance in that excretion is not at all confined to the time of the increased discharge, which is occasioned by the quantity of fluids received.

5. When a liquid, which is of such a nature as to admit of ready detection in the animal fluids, is taken into the stomach, if the quantity is sufficiently large to affect the whole mass of blood, it will be found in the blood drawn from the veins as soon as in the urine; although it will continue to appear in the urine after it has disappeared from the blood.

6. When a quantity of colouring matter sufficient to affect the whole mass of blood is taken, mixed with a large quantity of liquid, the colour appears in the urine as soon, at least, as the excretion *begins* to be increased, and is at the deepest before the increase of urine is at its height; but if the quantity of colouring matter be only a little more than sufficient to tinge the amount of liquids taken, it does not appear in the urine until after a considerable quantity of watery urine has been passed, and does not give its deepest colour until some time after the excretion has diminished.

The urine, it is to be observed, is made up of all the refuse fluids of the body; and is the only secretion which is strictly feculent in its nature, and which answers no useful purpose in the system, after its separation from the blood. Hence the kidneys are so constructed, and are furnished with such a species of excitability, as to separate from the blood every fluid brought to them, which is not adapted to any farther use in the system. If, therefore, there is a large quantity of extraneous fluids in the blood, these organs are excited to greater action, and separate them, so that they become immediately visible in the urine. But if the quantity is smaller, it requires a longer time for them to be concentrated sufficiently to be discovered in that excretion. In the same manner, although the quantity of foreign liquids is at first large, so as to be readily distinguished in the blood, as well as in the urine; yet as they can only be separated by the kidneys, they must of course first disappear from the blood, while the urine is still affected by them, though in a slighter degree.

These remarks are intended to connect and explain the principal phenomena of the third, fourth, and fifth experiments. In each of these experiments the urine was, more or less, coloured by the tincture of rhubarb which had been taken. The ounce of tincture, taken in the third, and also that in the fifth experiment, was sufficient to have tinged thirty pounds of urine, so as to make the tinge very distinctly observable. This is probably much more than the quantity of blood which it could possibly be mixed with in the circulation, before it reached the kidneys. Consequently the first urine that was discharged, after the secretion was increased by the quantity of liquids which were received, was highly tinged by it. But after a sufficient time had elapsed, for the kidneys to have separated nearly all the rhubarb from the blood, the proportion which was left was too small to be rendered sensible, by any means which we have of detecting it, although a portion still remained in the urine; or the last portions of the rhubarb might have already left the blood, before its excretion from the body was fully completed. In the fourth experiment, in which a much smaller proportion of rhubarb was taken, the quantity was so small, that it was in a manner lost in the general mass of blood, until it was again collected by the secretory action of the kidneys.

In the same manner is to be explained most of the difference between the results of Dr. Darwin's experiments, and those of Sir Everard Home, which have been quoted. Either the quantity of nitre in Dr. Darwin's was too small to be detected, when disseminated through so large a mass of fluids as the whole circulating blood, or a sufficient length of time had elapsed, before the blood was drawn for examination, for it to have passed through the blood, and to have been secreted by the kidneys. This explanation does not indeed apply to all the phenomena mentioned in the second experiment quoted. But I think it can hardly be doubted that the author was mistaken in supposing there was no trace of the rhubarb in the blood. He says that the serum was different in its appearance from that drawn before, and was "*of a yellowish cast*," although he adds, "as the serum usually appears." It does not appear that he made any comparison of serum which was known to be tinged, with that which was not; or that he had any test to aid in the detection of the rhubarb.

7. The rapidity, with which fluids pass from the stomach to the bladder, is not increased by increasing their quantity.

In the fifth experiment, in which only an ounce of liquid was taken, the rhubarb appeared in the urine as speedily after it was taken as it did in the third, where a pound was taken. It is worthy of remark, however, that the substances which are particularly adapted to detection in the urine, and which have therefore been the subjects of experiment in regard to it, are all of them peculiarly calculated to excite the action of the urinary organs. Such, it is well known, is the case with nitre, rhubarb, and oil of turpentine. Hence it may fairly be presumed, that these substances require less time to pass from the stomach to the bladder, than such liquids as are commonly taken for drink.

8. It results from the whole, that the only mode by which fluids, received into the stomach, can pass into the bladder, is by absorption into the blood, and subsequent secretion by the kidneys.

If there were a more direct communication, the rhubarb, in the fourth experiment, would have been discovered in the urine as soon after it was taken as in the others; for the liquid which contained it would have passed on to the bladder, without being intermixed with the animal fluids. In such a case, too, there would have been a more exact correspondence between the quantity of liquid taken, and that which was discharged; and the rhubarb must have all, or nearly all, been carried out of the system with the extra discharge of urine: whereas it continued to colour the urine for several hours after nearly as deeply as before; and, in the fourth experiment, the tinge grew deeper as the quantity of urine declined.

But the most decisive circumstance is, that the rhubarb was found in the blood which was taken from the veins, as soon as it appeared in the urine, and as soon, too, as any increase of the excretion was produced. The rhubarb must have been carried farther, to reach the veins of the arm, than to reach the kidneys; since it must have gone to the extremity of the circulation in the limb, and have been returned from the hand. This circumstance completely refutes the contrary argument, derived from the rapidity and quickness of the production of urine, in certain cases; at the same time that it is, in itself, positive evidence that the fluids in question actually circulate in the blood before they go into the bladder.

It is no objection to this explanation, that some of the fluids which are received sometimes pass off nearly unchanged. If these fluids are present in the body in larger quantities than are requisite for the wants of the system, they are so far extraneous to it; and it is the peculiar province of the kidneys to separate, and throw off, all such fluids which are presented to them by the course of the circulation. It is not so much their office to change the nature of the fluids received into the system, as it is to convey out of the system such fluid substances as remain, and are superfluous in the blood, after the changes produced by the other organs, in accomplishing their purposes of nutrition and secretion.

In addition to this, the great afflux of foreign liquids, in certain cases, while it stimulates the vessels of the kidneys to great increased action, at the same time debilitates them, and renders them less capable of a careful selection of such portions as are only fit for excretion. It will hold true, I believe, of all the animal secretions, that when, from whatever cause, they are preternaturally increased, the fluids secreted are less perfectly matured in their composition than when they are produced by the natural action of their several organs.

The same remark will apply to many diseases of the glandular organs; in which, notwithstanding a very considerable change of structure, the quantity of their secretions is sometimes very much increased, although their quality is less perfectly adapted to the uses which they are to perform. Such, at least, is probably the case in regard to most of the larger glands. If the secretions were prevented from being accomplished, it is but reasonable to suppose, that the circulation would, at the same time, be obstructed in the organ, and of consequence a less quantity of blood would be carried into it. Yet there is generally evidence of a more than ordinarily active state of the circulation; and the blood vessels are enlarged. It may, perhaps, be said, that, in suppurative diseases, this increased quantity of blood goes to the formation of pus. But the quantity of pus is too small, while it is confined, to occasion so great a consumption of blood.

There are many cases of diabetes on record, in which the kidneys, as it appeared after death, have been extensively ulcerated, and their substance destroyed. Perhaps in most, if not all, fatal cases of diabetes, these organs might be found to be thus affected. If the explanation, which I have attempted, be correct, it accounts for the quantity and crude state of the urine in this disease. This explanation is confirmed by a remarkable case, in the Philosophical Transactions for 1747, by Mr. Glass, a surgeon of Oxford. It was a tumour in the abdomen of a young woman, resembling dropsy, from which, after death, "not a pint short of thirty gallons (by calculation two hundred and forty pounds) of water, limpid as urine, and not in the least fœtid, were drawn." There had been no symptom of dropsy, except the tumour. The water was contained in a sac, "resembling the uterus of a cow at the end of gestation," which filled the whole abdomen. From this sac, a duct, whose orifice easily admitted a large crow-quill, descended to the neck of the bladder. The emulgent vessels of the right side passed from the trunks of the vessels to this sac, in the same manner as they did in the left side, to the kidney, which was sound, and in its natural situation. No trace of a kidney could be found on the right side, unless this sac was it. It is manifest that the investing membrane of this sac secreted a much larger quantity of fluid, from the blood brought to it by the emulgent artery, than a sound kidney would have done in a healthy state of the body.

The mammæ, in women, are subject, though more rarely, to a disease perfectly analogous to the affection of the kidneys in diabetes; and Bartholine, and some other eminent authors, have thought, that they too are supplied with a communication from the stomach and intestines, more direct than the circulation. Indeed, the evidence, in favour of such an opinion, is very nearly the same as that in proof of a direct communication between the stomach and bladder. The mammæ, as well as the bladder, occasionally, though perhaps not so frequently, pour out very large quantities of fluid; and these fluids are, in either case, alike subject to be more or less modified by the nature of the ingesta. It is a fact, perfectly familiar to every physician, that an infant is often affected by the food of its nurse; so far even, that cathartics, taken by the nurse, not unfrequently operate upon the bowels of the infant. Yet one opinion has long since been abandoned as absurd, while the other seems to have been believed by far the greater part of physiologists.

No mode has yet been pointed out, that rests even upon plausible grounds, by which such a communication, as has been supposed, can exist. Scarcely any two of the multitude of authors, who have written to prove its existence, have agreed as to the manner of its existence. There is no theory upon this subject that explains the phenomena with any thing near the same facility as the course through the circulation. Were the two cases equal, therefore, in regard to positive testimony, the evidence would preponderate in favour of this course. But how far they are from being equal, sufficiently appears from what has already been said.

Another consideration, which might be adduced in support of the opinion, that there is no other communication than by the circulation, is the utter uselessness of such a communication, if it did exist. It appears altogether inconsistent with the wisdom, and perfection of design, which are every where displayed in the animal economy, to suppose that an organ should be formed for no other purpose but to carry off the fluids which are received into the body before they have been submitted to the action of the parts for whose nourishment a portion of them, at least, is designed. Such a mechanism would seem calculculated for the excesses, to which our race is but too prone, rather than accommodated to the real wants of the system. How much more reasonable is it to suppose that these fluids are only rejected, after they have been presented to the several organs for whose use they were designed, and such parts have been selected as may have been required to supply the demands of those organs.

Several other considerations might be mentioned in support of the same opinion. But it is needless to accumulate arguments upon the probabilities of the question, after attending to the facts which have been exhibited. If any confidence is placed in the results of the experiments detailed in this paper, performed with all the care and attention of which I am capable, and confirmed, as they are, by the experiments of others, and by the changes sometimes produced by disease, I think the conclusion is unavoidable, that the *only* communication between the stomach and the bladder, is through the circulation and kidneys.

DISSERTATION II.

ON THE QUESTION

CAN MEDICINAL SUBSTANCES BE SAFELY AND ADVANTAGEOUSLY INTRODUCED INTO ANIMAL BODIES THROUGH THE MEDIUM OF THE VEINS?

WHICH OBTAINED THE BOYLSTON PREMIUM

IN 1821.

" Timeo Danaos, et dona ferentes."—VIRG.

In the following paper, the question proposed for discussion is examined only in reference to emeticks and catharticks. The other classes of medicines furnish no distinctive marks of their operation, applicable to the smaller animals, sufficiently characteristick to enable us to compare the effects of the different modes of administering them; and therefore experiments with them cannot lead to any satisfactory result. It is obvious, however, that most of the remarks upon these two classes will apply with equal force to all the other medicines

DISSERTATION

ON THE PROPRIETY OF ADMINISTERING MEDICINE BY INJECTION INTO THE VEINS.

It sometimes happens in the course of medical practice, that a particular medicine is strongly indicated, at the same time that there is some powerful obstacle in the way of administering it. These cases are indeed rare; but when they occur, they are of a most serious nature, and call very urgently for some mode of relief. A mechanical obstruction in the œsophagus might perhaps be removed by vomiting, were it not that this same obstruction renders it impossible for an emetick to be taken: and still more frequently, the operation of a cathartick is prevented, when it is very much needed, in consequence of an irritable state of the stomach, which rejects every medicine that it receives. Hence we are led to the inquiry, whether some other mode of introducing medicines into the system be not practicable, and particularly whether this may not be done by injecting them into the veins.

This question seems to have first occurred to Dr., afterwards Sir, Christopher Wren, then Savillian professor in the university of Oxford, in consequence of the speculations and investigations respecting the transfusion of blood from one animal to another. As early as 1665, according to Mr Oldenburg, Dr. Wren injected medicinal substances into the veins of animals. His "experiments," says Mr. Oldenburg, "were made at different times, and upon several dogs. Opium, and the infusion of crocus metallorum were injected into the veins of the hind legs of these animals. The opium soon stupified, but did not kill the dog; but a large dose of the crocus metallorum induced vomiting and death in another dog."*

About two years after, a similar experiment was tried by signior Fracasati, professor of anatomy at Pisa. He injected some of the mineral acids into the veins of dogs, which was of course fatal to them. "Into the veins of another dog was injected some oil of sulphur; but he did not die, though this infusion was several times tried upon him. The wound being closed and the dog let go, he fell to gnawing some bones which he found with a great avidity, as if this liquor had caused in him a great appetite." "Another dog

* Philosophical Transactions, for 1665.

into whose veins some oil of tartar was injected, did not escape so well; for he suffered much, and after being greatly swollen, died."†

The first account of any attempt to inject medicinal substances into the human veins is contained in the Philosophical Transactions for 1667. It is entitled, "Some new experiments of injecting medicated liquors into veins, together with considerable cures performed thereby; communicated by Dr. Fabritius, of Dantzick," and translated by Mr. Oldenburgh from the original Latin.

"As we had a desire" (says Dr. F.) "to try what would be the effect of the surgical experiment of injecting liquors into human veins, three fit subjects presenting themselves in our hospital, we thought good to make the trial upon them. But seeing little ground to hope for a manifest operation from merely altering medicines, we thought the experiment would be more convenient and conspicuous from laxatives; which made us inject by a syphon two drachms of such a kind of physick into the vein of the right arm. The patients were these. One was a lusty robust soldier, dangerously infected with the venereal disease, and suffering grievous exostoses of the bones in his arms. He, when the purgative liquor was infused into him, complained of great pains in his elbows, and the little valves of

* Philosophical Transactions, for 1667.

his arm swelled so visibly that it was necessary by a gentle compression of ones fingers to stroke up that swelling towards the patient's shoulders. About four hours after, it began to work, not very troublesomely; and so it did the next day, insomuch that the man had five good stools after it. Without any other remedies, those protuberances were gone, nor are there any traces left of the abovementioned disease.

"The two other trials were màde upon the other sex. A married woman of thirty-five, and a servant maid of twenty years of age, had been both of them from their birth very grievously afflicted with epileptick fits, so that there were little hopes left to cure them. They both underwent this operation, and there was injected into their veins a laxative rosin, dissolved in an anti-epileptick spirit. The first of these had gentle stools some hours after the injection; the next day the fits recurred now and then, but much milder, and are since altogether vanished. As for the other, viz. the maid, she went the same day to stool four times, and several times the next; but by going into the air, taking cold, and being careless in her food, she died.

"It is remarkable that all three vomited soon after the injection, and that excessively and freely."*

* Philosophical Transactions, for 1667.

In the same transactions for the next year, there was published an "Extract of a letter written from Dantzick to the honourable Mr. Boyle, containing the success of some experiments of infusing medicines into the human veins."

"Mons. Smith," says this letter, "physician in ordinary to this city, having liberty granted him to try an experiment upon some persons desperately infected with the venereal disease, then in the publick hospital here, ventured the opening of a vein, and infusing some medicines into the blood. This was tried upon two persons, one of whom recovered, and the other died. Yet being since further encouraged by corresponding with some of the Royal Society in England, about a month since the said physician, assisted by Mons. Scheffler, another old practitioner in this city, repeated the experiment by infusing altering medicines into the vein of the right arms of three persons, the one lame of the gout, the other extremely apoplectick, and the third reduced to extremity by that singular distemper the plica polonica. The success of this, as Mons. Hevelius informs me, was, that the gouty man found himself pretty well next day, and shortly after went to work, it being harvest time, and has continued well ever since, leaving the hospital yesterday, and professing himself cured. The apoplectick patient has not had one paroxysm since;

and the several sores which the plica polonica had occasioned, are healed; and both these persons have been able to work at any time these three weeks."*

In 1690, it is said that Dr. Moulin injected an ounce and a half of crude mercury into the jugular vein of a dog. The animal did not apparently suffer very much at the time, but the fourth day he died.†

These are all the notices that I have been able to find, of any attempt to introduce medicines into the veins either of man or of other animals, until within a few years. I have quoted these, more as curiosities connected with this subject, than as furnishing any considerable aid towards deciding the question proposed for discussion. Although we are not told what particular articles of medicine were used in the experiments on human subjects, yet it is evidently altogether fanciful to suppose that any cathartick, by a single application, should suddenly cure the venereal disease, removing nodes from the bones; or that a laxative rosin dissolved in an anti-epileptick spirit should atonce cure epilepsy; or that alteratives should as suddenly cure the gout, apoplexy, or the plica polonica, merely be-

* Philosophical Transactions, for 1668.

† Ibid. for 1690.

cause these medicines were thrown into the veins, instead of being administered by mouth.

The extravagance of the effects imputed to the injections so entirely destroys the credibility of the accounts, that no reliance can be placed in them, except to show that the attempt has long since been made to take advantage of this mode of administering medicine. In every other point of view we must regard this as a new question, at least so far as the practicability of injections is to be determined. I shall therefore proceed to the investigation, without any reference to the extravagant opinions of those who began the inquiry almost two centuries ago; and endeavour to ascertain the benefits and the dangers to be expected from the practice of injecting medicines into the veins.

There is but little in the present state of physiological knowledge to aid in the inquiry whether medicinal substances can safely be introduced into the animal system by injection into the veins, or not. Whether the peculiar excitability of the coats of the blood-vessels will permit them, without injury, to receive and transmit fluids, which have not been first assimilated by the digestive organs, is not at all determined. And if it were, we are still ignorant to what extent the effects of fluids, thus introduced, would resemble those of the same substances, received in the ordinary manner through the stomach.

The consequences which are said to follow from the introduction of air into the veins, might seem to indicate a state of excitability, not very readily accommodated to the action of foreign substances. Bichat remarks, that "it has been generally and for a long time known, that when any quantity of this fluid is introduced into the vascular system, the motion of the heart is quickened, the animal is agitated, gives a cry of pain, is seized with convulsive motions, falls deprived of animal life, still lives organically for some time, and soon ceases to exist."* If a small quantity of air only is sufficient to produce these disastrous effects, this circumstance alone, whatever may be the action of other fluids, will be enough to render the practice of injecting into the veins extremely dangerous. For it seems hardly possible to introduce any fluid directly into a vein without the risk that some portion of air will enter with it.

Another circumstance which seems to show that the excitability of the different parts of the circulating system is peculiar to themselves, and that there is danger in disturbing the regularity of their actions, is the effect produced by the introduction of venous blood into the arterial sys-

* Researches on Life and Death, p. 148. Philadelphia ed.

tem. If the consequences are so fatal, which follow the application of a fluid so similar to the natural one, as to elude all attempts to detect any difference, except in colour, how much more injurious must be supposed to be the action of foreign substances, which have been scarcely at all assimilated to the animal system.

Another danger to be apprehended from injections into the veins, arising from their peculiar irritability, is an inflammation of the internal coat of the vein. We are taught in surgical treatises, that very severe, and even fatal inflammation, has sometimes been excited in a vein, by puncturing it with a dull or rusty lancent in bleeding. Such occurrences are doubtless very uncommon. But if they can ever be produced by so slight a cause, there would be much more reason to fear them, from the irritation which would be unavoidable in introducing foreign substances into the vein. This danger would be greatly increased, if the substance introduced were of an irritating nature, as most medicinal substances must be; especially when applied to so delicate a structure as that of the internal coats of the veins.

These considerations would lead us to regard the practice of injecting fluids into the veins as highly dangerous, or, at best, of very doubtful safety. But it is to be observed, on the other hand, that experiments on animals have not shown it to be so extremely hazardous. In many instances, liquids have been thrown into the veins without producing any peculiarly disastrous consequences, except such as might be regarded as the natural effect of the particular substance employed, independently of the manner in which it was administered. Fontana, Brodie, Magendie, and, more than all, Orfila, relate many experiments, in which medicinal and poisonous substances, in solution, were injected into the veins of animals. In many of them, the consequences were not so immediately fatal as to support the opinion, that the excitability of the veins is so peculiar as absolutely to forbid the introduction of foreign fluids; and, in some, the animal was not materially injured. These experiments, therefore, show, that in some of the smaller animals, and under some circumstances at least, these injections might be safely used.

It is not to be forgotten, however, that we are still ignorant how far the analogy between different animals extends, and to what degree similar results might be expected in the human system. The great dissimilarity in diet, habits, and mode of life, between man and the other animals, gives much reason to doubt, whether inferences, drawn from experiments on one, can justly be applied to the other.

In some instances, where there has been opportunity to make the comparison, the difference between man and the other animals, and among the several species of animals, in regard to the action of foreign substances, is found to be very great. The action of some of the poisons, when taken into the stomach of certain animals, is much less destructive than that of the same substance, when received into the human stomach. It is said that a dog will take, with little or no permanent injury, a quantity of poison which would be speedily fatal to a man. There is, also, a great difference among quadrupeds. Articles, which to some species of animals are highly poisonous, are to others perfectly innocent.

Still it is impossible to reject, entirely, the inferences which are drawn from the results of experiments on animals; or to consider them as wholly inapplicable to the laws of the human system. Since we have no direct evidence that foreign liquids, thrown into the human veins, are necessarily injurious, and have positive proof that in some animals at least, they are in some measure innocent, we are not to regard it as established, that the use of them would be so dangerous as in all cases to prohibit its adoption. It is therefore still a subject for investigation, under what circumstances, and to what extent, can foreign substances be safely introduced into the veins of the human body; if indeed they can be introduced at all.

But if it be allowed that medicinal substances may be introduced into the system by the veins, without injury, what reason is there to suppose, that the action of substances, thus introduced, will be the same, as it is when they are received through the medium of the stomach?

In regard to such medicines as evidently act only in consequence of their absorption, and their mixture and circulation with the blood, it would be natural to expect that the effects should be the same, whatever might be the manner in which they are received into the mass of circulating fluids. The action of mercurials on the salivary glands, for instance, is the same, whether the medicine in a suitable form is taken into the stomach, or is absorbed by friction on the skin. In either case, the medicine doubtless circulates with the blood, and is in effect applied directly to the organs, on whose actions its efficacy depends. Hence it is a considerable length of time after its reception into the system, before its peculiar action begins; and this time is of nearly equal duration, whether the medicine is applied directly to the stomach, or to the skin.

But in the case of emeticks and cathearticks, the medicines are applied directly to the organs whose actions and functions are affected by them.

There seems hardly to be time, especially in regard to emeticks, for the medicine to be absorbed so as to act on the whole system, so speedily as its local action on the stomach often commences. The action of this class of medicines has, therefore, generally been considered as the result of a local excitement in the stomach and intestines, produced by the application of these stimuli to their villous coats.

If this view of their actions were correct, it is not easy to conceive how the same actions can be excited by these substances, when introduced directly into the mass of circulating fluids. It would be difficult to believe that a medicine, whose action is purely local, may be mixed with the blood in the veins, and separated from it again in the living system, so as to excite the same actions in any particular organ, as when it is applied directly to that organ. When an emetick or a cathartick is introduced into the stomach, all its power is at once directed to the part on which it is to act. But if it is injected into a vein, it must be disseminated through the whole mass of blood, and be thus greatly diluted before it can reach its destination; unless indeed it were, by some unaccountable and improbable chance, to be carried directly forward to the stomach, without mixture with the animal fluids.

Every consideration of theory merely seems to discountenance the expectation of obtaining a similar result from introducing these substances directly into the blood, and from receiving them through the digestive organs. But we are not left to mere theory to determine this question; and it is remarkable that most of the results of experiment are widely different from what we should be led to expect, from reasoning on the natural actions of the system.

Most of the cases in which medicinal substances have been introduced by injection, into the veins of living animals, are the experiments made to investigate the action of poisons. Some of these experiments furnish observations which bear upon the point in question; although instituted for a very different purpose. The results of these experiments are indeed very different in different cases, and sometimes give rise to opposite inferences; but the general effect of the whole is to lead to the belief that the effects of medicinal substances are not materially different, in consequence of the modes of their introduction into the system.

Fontana, after proving by repeated experiments that the poison of the viper, though speedily fatal to animals when received into a wound, is innocent when taken into the stomach, found that the opposite was the case, at least to a considerable extent, with the poison of the cherry-laurel. He says "I made a large rabbit swallow two drops of the oil, united with two drops of spirit. It died in a few seconds, slightly convulsed." "I made a land-turtle of a pound weight swallow about two drops of pure oil. Two hours after, it was become very feeble; in somewhat more than six hours it died with all the symptoms of a loss of irritability." "I gave scarcely three drops to a pigeon, which died in two minutes." "I made a large guinea-pig swallow half a tea-spoonful. It remained well for more than half an hour, but afterwards fell into violent convulsions, and died half an hour after."*

"I thrust a bit of wood dipped in this oil, into a pigeon's leg, and observing that after fifteen minutes the creature was not disordered by it, I took it out, and introduced a great deal of the oil into the wound, which was very deep; the pigeon notwithstanding neither died, nor was convulsed." "I made a wound in the body of a small tortoise, towards its tail, and introduced the oil freely; the tortoise was not disordered by it." "I wounded the legs of a pigeon in several places, and rubbed the wound with the oil. It did not suffer sensibly." "The consequences of experiments on two other pigeons, three rabbits, and four guinea-pigs, were the same, notwithstanding

* Fontana on Poisons, vol. ii. p. 170.

that I did not spare the oil, with which I covered the wounds repeatedly, as I had done after wounding the muscles of these animals."

" It appears beyond a doubt that the oil of the cherry-laurel, which is a poison when swallowed, has not this quality when applied to wounds, of the parts at least, on which I made my experiments; this is quite contrary to the nature of the viper's venom and other poisons, which are innocent when swallowed, and destructive when applied to wounds."*

Although in these experiments the poison was not injected into a vein, but applied to a wound, yet there is much reason to suppose that the effect of such an application of it would be the same as when it is thrown into a vein. Indeed Fontana made the trial in that manner with the cherry-laurel water. " With less than two tea-spoonfuls of this water taken internally," he remarks, " I have seen middle-sized rabbits fall into convulsions in less than thirty seconds, and die in a minute. When given in a small quantity, convulsions more or less violent succeed, and the body and extremities of the animal become lifeless."†

" I sat about introducing this water into the jugular vein of a large rabbit, beginning with five

* On Poisons, vol. ii. pp. 173, 174.

† Ibid. vol. ii. p. 144.

or six drops. The animal giving no symptom of pain, I thought I had not succeeded in the attempt, and that the syringe had found its way into the cellular membrane. I repeated this experiment, introducing afresh, perhaps three or four times the quantity of the water, and assuring myself previously that the point of the syringe had entered the vein, and that the liquor could in no way force itself back; the animal still continued to be unaffected by it. I was more surprised than satisfied at what I saw. I could not persuade myself but that the cherry-laurel water would be a poison, and even a very powerful one, when introduced into the blood, since applied to the nerves it was quite innocent. I returned to my experiments, and now introduced a tea-spoonful of the water into the jugular vein, from which the animal felt no ill effects. I repeated this experiment on another rabbit, and introduced the same quantity of the poison; the creature neither suffered at the moment nor afterwards."*

The experiments of Brodie and Magendie, and most of those of Orfila, lead to an opposite result. Mr. Brodie, in his second paper on the action of poisons says, " Mr. Hume informed me that in an experiment made by Mr. Hunter and himself, in which arsenick was applied to a wound in a

* On Poisons, vol. ii. p. 149.

dog, the animal died in twenty-four hours, and the stomach was found to be considerably inflamed." "I repeated this experiment several times, taking the precaution always of applying a bandage to prevent the animal from licking the wound. The result was that the inflammation of the stomach was commonly more violent and more immediate, than when the poison was administered internally, and that it preceded any appearance of inflammation of the wound. Some experiments are already before the publick, which led me to conclude that vegetable poisons, when applied to wounded surfaces, affect the system by passing into the circulation through the divided veins."*

Experiments with muriate of barytes gave a similar result. "It operates as an emetick in animals that are capable of vomiting, but sooner when taken internally, than when applied to a wound."†

Of the tartrite of antimony, Mr. Brodie says, that "when applied to a wound in animals which are capable of vomiting, it usually, but not constantly, operates very speedily as an emetick." "When a solution of emetick tartar was injected into the stomach of a rabbit, the same symptoms took place as when it was applied to a wound."‡

* Philosophical Transactions, for 1812.

† Ibid. ‡ Ibid.

In respect to this substance, however, the experiments of Magendie are much more complete and satisfactory. Indeed they seem fully to establish the fact, that tartrite of antimony equally operates as an emetick, though with different degrees of violence, upon some animals at least, whether it be taken into the stomach, or injected into the veins, or absorbed through the different absorbing surfaces of the body, except the pleura; and where death is produced by it, the appearances on dissection are in either case the same. "J'ai mis," says Magendie, "une quantité connue de dissolution d'émétique en rapport avec les diverses surfaces absorbantes de l'économie, principalement la membrane muqueuse de l'intestin grêle et du gros intestin, les diverses membranes séreuses, le péritoine, la plèvre; j'en ai injecté dans le tissu cellulaire, j'en ai introduit jusque dans le tissu des organes, comme je l'avois déjà fait pour un autre motif il y a quelques années avec M. Delille. Partout j'ai obtenir le même résultat; d'abord vomissement et ensuite déjections alvines; dans certains cas, j'ai vu celles-ci précéder le vomissement.

"Une seule membrane absorbante fait exception à cette régle; c'est la plèvre; quand on y porte une dissolution d'émétique, le vomissement n'est point produit, et très-rarement les évacuations alvines; c'est au moins le résultat que j'ai obtenu dans plus de vingt expériences faites avec l'intention de constater cette singulière anomalie.

"L'injection de l'émétique dans les veines, selon la méthode des médicins qui, peu de temps après la découverte de la circulation du sang, inventèrent la medicine infusoire, amène absolument les mêmes résultats, avec cette différence, que les effets sont beaucoup plus prompts et plus intenses.

"Le suppose qu'on injecte dans les veines d'un chien adulte et de taille moyenne six à huit grains d' métique dissous dans trois onces d'eau, il y a d'abord des vomissemens et des déjections plus ou moins répétées: puis il devient manifeste que l'animal a de la difficulté à respirer ; son pouls acquiert de la fréquence ; ensuite de légers trémblemens, semblables à ceux qui accompagnent les frissons, se montrent ; la réspiration devient de plus en plus difficile, le pouls irregulier et même intermittent ; la sécrétion de la salive devient plus considérable ; l'animal paroît inquiet, ne sait quelle attitude prendre ou conserver. Ces symptômes acquièrent beaucoup plus d'intensité, et la mort arrive dans les deux ou trois premieres heures qui suivent l'absorption ou l'injection de l'émétique."*

* Memoire de l'influence de l'émétique sur l'homme et les animaux. pp. 35. 31.

" Si au lieu de porter," he adds, " par un moyen quelconque, huit grains d'émétique dans le système sanguin, on y introduit douze ou dix-huit grains de cêtte substance, la mort est beaucoup plus prompt ; elle arrive ordinairement unedemiheure après l'introduction de l'émétique.

" Mais si on ne porte que quatre grains de tartre stibié dans le système circulatoire, les accidens de développent avec moins de promptitude et d'intensité. Les animaux ne périssent que beaucoup plus tard ; il en est que ne meurent qu'au bout de vingt-quatre heures."*

" Deux grains d'émétique injectés par les veines ou absorbés, produisent en général les mêmes phénomènes ; mais les animaux ne périssent ordinairement qu'au bout de deux ou trois jours. J'ai même vu des chiens supporter cette dose sans autre accidens qu'un malaise de peu de durée.

" Un grain d'émétique injecté dans les veines ou absorbé produit rarement des accidens ; dans la plupart des cas, il n'éxcite pas même le vomissement ; mais j'ai observé que si le lendemain du jour où l'on a injecté un premier grain d'émétique dans les veines on en injecté un second, l'animal périt constamment : dans cette circonstance le tissu pulmonaire paroit peu altéré, l'estomac et le duodénum sont les parties qui

* Memoire de l'influence de l'émétique sur l'homme et les animaux. pp. 37. 38.

offrent les traces les plus manifestes de l'action de l'émétique.

" Il s'agit maintenant de voir quels sont les effets qu'il produit quand il est introduit dans l'estomac, et qu'on s'oppose au vomissement par une ligature appliquée sor l'œsophage, derriere la glande thyroïde. Ces phénoménes sont absolument ceux que je viens de décrire ; mais ils se dévéloppent avec plus de lenteur."*

The correctness of these observations is still farther confirmed by those of Orfila upon the other poisons. In very many instances Orfila introduced the different poisons into the veins of animals, and in almost all of them the effects were the same as were produced by the same substance when taken into the stomach ; and the same appearances were exhibited on dissection, with the exception already stated, that the action of the poison is much more violent when injected into a vein, than when introduced into the stomach. Orfila's treatise is so well known that it is unnecessary to quote many of his observations in illustration of this fact. A few examples will be sufficient for our purpose.

Of the acetate of copper, he says, " I have had frequent opportunities of administering verdigris and the acetate of copper to dogs of different sizes, and I have constantly observed,

* Memoire de l'influénce de l'émétique, pp. 38, 39, 40.

that when the dose of the acetate of copper introduced into the stomach was stronger than from twelve to fifteen grains, the animal died in less than three quarters of an hour. The symptoms which preceded death were, a copious vomiting of a bluish matter, evidently coloured by a portion of the acetate of copper; fruitless efforts to vomit after the whole of the aliments contained in the stomach were thrown up; plaintive cries; an extreme difficulty of breathing; irregularity and frequency of the pulse; frequently a general insensibility; the animal lay down, and appeared to be dead; convulsive movements almost always took place, and a few moments before death a universal stiffness came on, with some spasmodick twitchings, and a great quantity of foam from the mouth."*

I have copied the whole of this description for the purpose of comparing it with that which follows, of the effects of injecting the same substance into a vein. "The injection of a grain of the acetate of copper in half an ounce of water, into the jugular vein, occasions death commonly in the space of from ten to twelve minutes; the animal instantly makes motions as if masticating and swallowing, which are followed by vomitings attended by painful efforts; the animal becomes agitated with very violent convul-

* Toxicology, Waller's translation, vol. i. p. 214.

sive motions, lies down instantly, becomes insensible, the rattling in the throat comes on, and he dies."*

Many other similar examples might be selected from the elaborate work of this celebrated author. But I shall only quote one, from his observations on the sulphate of zinc; and I choose this chiefly because this substance is so frequently prescribed as a medicine. "A small dog," says Orfila, "was caused to swallow sixty grains of the sulphate of zinc in powder; five minutes afterwards he twice vomited some white matter. At the end of a quarter of an hour he made violent efforts to throw up a small quantity of a frothy substance, and refused to take any food. The next day he was quite recovered."†

"There was injected into the jugular vein of another small dog twenty-four grains of the same salt, dissolved in sixty grains of distilled water. A few seconds after the injection, the animal vomited a very small quantity of yellow liquid matter, stringy, and as it were bilious, and died in the space of three minutes, in such a state of tranquillity that he might have been thought to have been asleep."‡

It is not my intention to attempt to reconcile the opposite results obtained by Fontana, and by

* Toxicology, vol. i. p. 215.
† Ibid. p. 266.
‡ Ibid. p. 267.

the other authors that I have quoted. Whether this difference arose from the different nature of the substances employed, or from the difference in the animals operated upon, or from the manner in which the experiments were performed, I shall not undertake to determine. It is sufficiently evident, however, in regard to many substances, that the mode in which they are introduced into the system does not materially change the nature of their action upon it, except in respect to its violence.

Such are the leading facts, derived from observations already before the publick, which bear upon the question of the practicability of administering medicines by the veins. There are some other considerations, relating more particularly to the safety and propriety of this mode of administering them; but I shall defer speaking of them, until I have related the observations and experiments which I have myself made, for the purpose of investigating this interesting subject.

In giving an account of the few experiments which I have performed, I shall not attempt any particular arrangement with reference to the conclusions to be drawn from them; but shall give them in the order in which they were performed. By this means the reader will be able to perceive the course which I have pursued in the investigation, and to judge of the correctness of the inferences deduced from the facts and observations here presented.

From a very general view of the subject, founded upon the observations of Orfila and others, I was strongly inclined to the opinion that the medicinal substances in question may in some cases be advantageously introduced into the system, by other means than by the stomach. This supposition was strengthened by the following case. A widow lady of about forty-five had been several weeks affected by a chronick rheumatism in her right thigh, about midway between the hip and the knee, which though not constantly very painful, rendered her very lame. After prescribing a variety of remedies, for about a week with some relief, but without effectually removing the disease, I directed friction to be used with an ointment composed of tartrite of antimony half a drachm, and simple ointment one ounce, rubbed intimately together. Half of this quantity was used in the first two days, and the remainder in the course of the week. The friction with this ointment excited a glow over the whole system, and the next day occasioned considerable nausea, which was followed, the third day, by full vomiting three times. The effect on the rheumatism was very great. At the end of the week nothing but a degree of stiffness remained of it; and this was removed in a short time, by continuing the friction with an ointment containing a smaller proportion of the tartrite of antimony. I afterwards directed friction with the tartrite in several cases of disease of different kinds, but always in a less quantity than in the case just mentioned. It in most cases appeared to be of some service as an alterative; but in no other instance did it act as an emetick.

The objects which I proposed to myself in my experiments were to endeavour to ascertain with what degree of facility medicinal substances may be injected into the veins,—how great is the danger of inflammation of the vein,—and to compare, as far as possible, the effects of medicines thus introduced into the system with those received in the ordinary way, both in respect to the nature of their actions, and the quickness and violence of their operation. To what extent I have succeeded in accomplishing these objects, as well as some of the reasons that my success has been no more extensive, will appear in the result of my experiments.

I have already alluded to the uncertainty of inferences drawn from experiments on the lower animals, respecting the action of medicines, in consequence of the difference of their characters and habits, and of the different action of medicinal substances on the different species of animals.

Some vegetables which are poisonous to certain classes of animals are freely used as articles of diet by others. Where the nature of the action is the same, the quantity required to produce the actions varies constantly in the different animals. Five or six drachms of opium scarcely produce more effect upon a dog, than the same number of grains do upon a man. And it will in general be found that a much larger quantity of medicine of any kind is requisite to operate upon the smaller animals than upon man.

In order therefore to ascertain the effects of introducing medicines into the veins, it is not sufficient that they be injected into the veins of any animal, and the result compared directly with the operation of the same medicine upon our species. The comparison, to lead to a satisfactory conclusion, must be made between animals of the same species, and with the same medicines, administered in the different modes proposed for examination.

But even when this is done, there is another source of fallacy in comparing the effect of injections into the veins of the human species, and of other animals. In man, none but the superficial veins would of course be operated upon, because they are abundantly sufficient for the purpose. But in the smaller animals, the superficial veins are so small as to render it quite impracticable to inject any considerable quantity of any fluid into them. I have made repeated attempts to introduce a small tube into these veins, for the purpose of injecting, but without success. It is necessary in all these animals, to open the internal jugular. Consequently the injury done to the animal is, from the very nature of the operation, a much more serious one than that which is done to man. The errour arising from this circumstance, however, is altogether in favour of the prudential side of the question. If no important injury is done to an animal by an injection into the jugular vein, there is much stronger ground for supposing, that it would be equally innocent in our own race, than there would be, if the veins operated upon were in each case equally important.

The apparatus which I made use of for these experiments, besides a case of common dissecting instruments and two or three surgeons' needles, was a slender silver tube, slightly curved, made quite small at one end, and at the other accurately fitted to a syringe, with a projection or ear on each side to hold it by. I had two small syringes of the common kind, but selected with care, which fitted the tube, and the pistons were carefully prepared so as perfectly to exclude the air. The animals were confined down upon a board by strong tapes, in one end of which were loops that passed over the legs of the animal; the tapes were then passed through holes in the board, and made fast by button-holes to knobs in the edge of it, so as to confine the limbs very closely to the board. I always took care in all the experiments to see that no air, or as little as possible, was forced into the vein with the medicine injected.

EXPERIMENT I.

February 9, 1821. I took two rabbits of nearly equal size and of the same colour, a male and a female, the latter of which was used for the injection into the vein. I counted the pulsations of the heart, and found them one hundred and eighty in a minute. I had previously filled a syringe with two drachms, by measure, of fresh cold-pressed castor oil, and immersed it in a bowl of water of the temperature of 100°; but the temperature fell a few degrees, before the oil was injected. I then made an incision in the neck, and laid bare the jugular vein of the right side, and without passing a ligature under it, made an opening into it, and introduced the tube, and slowly injected the oil into the vein. The animal struggled considerably, but I could not perceive that there was any peculiar struggling as the oil entered the vein; nor was there any thing else to lead one to suppose that it caused any very unusual sensation. There was some hæmorrhage from the vein, to the amount, perhaps, of half an ounce. In consequence of some minor difficulties in this part of the operation, a part of the oil was lost, and not more than two thirds of it was introduced into the vein. A part of the time, the end of the tube was forced more than half an inch down the vein. As soon as the tube was withdrawn, the lips of the wound were brought together and closed with two ligatures. The animal was then liberated and placed upon the floor. He seemed rather feeble at first, as might be expected, and somewhat agitated, though not violently so.

The injection of the oil was accomplished at ten minutes past four, in the afternoon. Ten minutes after, there was a copious discharge of fæces, of a nearly or quite natural appearance.

In fourteen minutes, there was another smaller discharge; and in twenty-four minutes, another which was also small. The fæces, though still in distinct portions, as in the natural state, were softer, and the portions were less regular both in respect to size and shape than natural.

In twenty-seven minutes from the injection of the medicine, there was another discharge, which was more copious, and of the same appearance as the last; and in one minute more, there was another considerable discharge. The animal now began to eat a little, and continued to do so occasionally, through the remainder of the afternoon and evening.

In thirty-one minutes, there was another small discharge of fæces, the portions of which were still more irregular, both in size and appearance.

In forty minutes, there was a very small discharge.

In fifty minutes, another, a little larger.

In fifty-eight minutes, another quite small.

At an hour and thirty-eight minutes, another small discharge.

At an hour and forty-four minutes, another, rather more copious than several of the last; the portions of it were smaller and more irregular than any that had been passed before.

At an hour and fifty minutes, another still more copious discharge.

At an hour and fifty-four minutes, another very small discharge. I now counted the pulsations of the heart, and found them the same as at the beginning of the experiment.

Half past seven, P. M. (Three hours and twenty-five minutes from the injection of the oil.) There have been three or four pretty copious discharges of fæces within the last hour; the last portions of which were more regular and more natural in their shape and appearance.

The animal is lively, and still inclines to eat a little.

Fifty minutes past eight. There has been another moderate discharge of fæces. At ten in the evening, no more had been discharged.

As soon as I could attend to it, I caused the other rabbit to swallow two drachms by measure of the same castor oil. This was done by placing the pipe of a syringe in his mouth and injecting the oil into the throat, at the same time occasionally irritating the fauces so as to oblige him to swallow it. This was done at eight minutes before five o'clock. This animal was carefully watched like the other; but at fifty minutes after eight in the evening, he had passed neither fæces nor urine. At ten o'clock, there had been a small discharge of both. The fæces were of a natural appearance.

February 10. 9 A. M. The animal into whose vein the oil was injected, has passed several very copious stools during the night. The fæces are softer than before, and many of the portions adhere together in an irregular mass. The other rabbit has passed no stool since last evening.

February 15. The wound in the neck of the rabbit appears to heal, and does not seem very materially to affect its health. There is however, a tumour of a considerable size in the neck, probably occasioned by an ecchymosis of the blood through the orifice in the vein, which was left open at the time of the operation. This animal was to-day made to take a quantity of an infusion of rhubarb, [see Experiment iii.] and its health afterwards was not so good. For rather more than a week it was very dull and inactive, and scarcely ate any thing. It then grew better for the space of another week, and ate freely. On the second of March, (three weeks after the operation,) at ten o'clock in the morning, it seemed to be nearly or quite as well as before the experiment. But at about two in the afternoon, it was found dead, and a large quantity of fresh blood upon the floor beside it. I was ill at the time, and was not able to examine this animal until several days after its death; in the mean time it had received some injury, which rendered it impracticable to ascertain with precision the state of the wound, and of the parts concerned. There was a pretty large sac, which seemed to have been filled with a white soft caseous substance. There were also still some more distinct remains of extravasated blood; and as the blood-vessels were empty, there is no room to doubt that this cavity had burst, and that the animal died from hæmorrhage in consequence.

EXPERIMENT II.

February 13. I attempted to make an injection into the crural vein of a rabbit, and made an incision for that purpose a little above the knee; but I found the vein too small to admit the tube. While I was trying to introduce it, the animal gave a sudden start, and I wounded the artery. It bled profusely; and I hastily passed a needle and ligature under the artery, including the vein and the nerve, and tied a single knot. This seemed to give great pain, and I immediately cut away the ligature. The hæmorrhage, however, was stopped. I closed the wound with two ligatures; and two days after it was healing rapidly, and in a short time was perfectly well.

EXPERIMENT III.

February 15. The following experiment was made upon the same pair of rabbits as the first, but the operations were reversed, the medicine being injected into the vein of the male, and the female receiving it into the stomach.

An infusion of rhubarb was prepared by pouring four ounces of boiling water to two drachms of the powdered root, and suffering it to stand twenty-four hours in a warm room, when it was carefully filtered. Two drachms, by measure, of this infusion, were drawn into a syringe, and placed in a basin of warm water, care being taken that the pipe of the syringe should be raised a little out of the water, so as to guard with certainty against any accidental dilution of the infusion. I then made an incision into the neck of the rabbit, and carefully dissected the jugular vein of the left side, so as to pass a double ligature under it. After this was done, I counted the pulsations of the heart, and found them to be one hundred and eighty in a minute. In the mean time, the temperature of the water in the basin, and of course that of the infusion, had fallen to 74°. The upper ligature was now tied, and an opening made into the vein immediately below it; the tube was introduced, and the infusion of rhubarb slowly injected into the vein. This was done with some difficulty, in consequence of the smallness of the tube; and a part of the infusion was lost. Rather more than half, probably about a drachm and a half, entered the vein. There was some hæmorrhage, not more, however, than half an ounce of blood was lost. At the time of the injection, the animal seemed much agitated; he took long inspirations, and the heart beat violently. The injection was completed at ten minutes after eleven in the morning. I immediately tied the lower ligature below the opening, and having closed the wound with a single ligature, placed the animal upon the floor. He very soon appeared lively, and began to eat; and in the course of the day he ate quite freely.

At half past eleven, I made the other rabbit swallow two drachms, by measure, of the same infusion of rhubarb. Half an hour after, he passed a small quantity of fæces, consisting of several portions adhering together. Excepting this, neither animal had passed either fæces or urine, at six o'clock in the evening.

February 16, 5 P. M. The male rabbit continues to be quite lively and active, and has eaten quite freely to day. About ten o'clock this morning, he passed a large quantity of urine, and about five this afternoon, he discharged a moderate quantity of fæces of a natural appearance. The other rabbit continues dejected, and eats little or none. Within about an hour it has passed three pretty copious liquid stools. There was also a discharge in the course of the last night, which was nearly, though not quite, natural in its appearance.

The wound in the neck of the male rabbit, healed rapidly, and was well in a short time. It never seemed to give him much trouble, nor in any degree to diminish his activity.

EXPERIMENT IV.

February 17. I laid bare the crural vein of a rabbit near the knee joint, and passed a ligature under it. I then made an opening into it, and attempted to introduce the tube, for the purpose of injecting an infusion of ipecacuanha; but the vein was too small to admit the tube. After several trials, I was obliged to abandon the attempt. In these trials, partly in consequence of the struggles of the animal, the vein was considerably injured, and was split so that the tube passed through it. I closed the external wound with two ligatures. The animal seemed quite lively immediately after the operation, and very soon began to eat. Very little inflammation followed, and he was well in a few days.

This experiment, and the second, except that they are examples of the difficulty of making injections into the superficial veins of small animals, have no other bearing upon the subject, than to show that considerable injuries to the veins are not always followed by inflammation in them; and it is for this purpose that they are here introduced. In several other instances, I have made similar attempts upon the small veins, without being able to introduce the tube; but in none has there any material injury followed. The wound has always healed readily.

EXPERIMENT V.

February 17. Having been disappointed of the effects, which I had expected would be produced by the medicines which, in the previous experiments, had been conveyed into the stomach of rabbits, I thought it best to make some farther observations upon the operation of medicines given to these animals in this manner, before I proceeded to other injections. I prepared an infusion of colocynth, by pouring two ounces of boiling water upon one drachm of powdered colocynth, which was filtered after twenty-four hours. Two drachms by measure of this infusion was given to a rabbit, at twelve o'clock. As no effect was produced by it, at a quarter past three in the afternoon, I gave four drachms more of the same infusion. Still the medicine produced no effect. The animal had no discharge of fæces, all the afternoon and night, and no unusual discharge the next day.

EXPERIMENT VI.

February 17. At ten minutes past twelve, I gave to another rabbit two drachms by measure of an infusion of ipecacuanha, which was prepared from a drachm and a half of the powder in two ounces of water. In half an hour I gave two drachms more of the same infusion. In thirty-five minutes from the beginning of the experiment, there was a small discharge of fæces of a natural appearance; and in an hour and twenty-five minutes, there was another of the same kind.

Half past 3, P. M. Within the last hour, there have been three pretty copious stools, but of a tolerably natural appearance. The animal has shown no disposition to vomit since the infusion was given. No more effect was produced by this medicine.

EXPERIMENT VII.

February 19. I gave to a rabbit four drachms by measure of the infusion of rhubarb before mentioned, [Experiment iii,] but it had no apparent effect whatever.

EXPERIMENT VIII.

February 19. To another rabbit I gave one scruple of ipecacuanha in powder, mixed with three drachms of water. Half an hour after, I gave as much more in the same quantity of water. I could not perceive that it produced the least effect upon the animal.

EXPERIMENT IX.

February 19. At four o'clock in the afternoon, I gave to another rabbit five grains of tartrite of antimony, in three drachms of water. In twenty minutes, there was a very small discharge of fæces, but no vomiting nor apparent nausea. At half past four, I gave five grains more of the tartrite of antimony, in half an ounce of water. The animal seemed to be very little affected by this medicine, in the course of the next three hours after it was given, during which time he was constantly watched. There was no vomiting, and no effort to vomit, although there was two or three times a movement, which it was thought might, perhaps, proceed from nausea. At ten in the evening, he had passed a small quantity of fæces. He did not appear to be ill, nor in any way altered from his usual health. The next morning he was found dead. He was entirely stiff, and appeared to have been dead several hours. I was prevented, by the preparations for the following experiment, from dissecting it at the time; and was afterwards too ill to do it, until it was too late to derive any benefit from the dissection.

EXPERIMENT X.

February 20. Having been persuaded, from my own observations, and those of others, that some of the milder medicines may be injected into the veins with safety, I resolved to make the experiment upon myself. Accordingly I filled a half ounce measure with cold-pressed castor oil, and placed it in a basin of water of the temperature of 100°; one drachm of it, I drew into a syringe, and placed this also in the basin; it being my intention first to inject one drachm, and if I should feel no inconvenience from it, to continue the injection of the remainder of the half ounce. When I had made all the preparations which I thought necessary, I sat down and counted my pulse, and found it to beat eighty times a minute. I was in good health, but could not avoid some little agitation and excitement of feeling at the novelty and uncertainty of an experiment upon myself, which, so far as I knew, had never been attempted upon any human being; and this had a little quickened my pulse.* An assistant (a medical student) placed a ligature round my left arm, as in the common operation for bleeding, and opened the median vein by a pretty large orifice; taking particular care that the opening in the vein and the skin should be exactly opposite. He then attempted to introduce the silver tube, while I held a bowl to receive the blood, which flowed very freely. But being a little agitated, he was not able to get the tube into the orifice in the vein. As there was no time to be lost, I took the tube myself, and after several ineffectual trials, which gave

* At the time that this experiment was performed, I did not know that Fabritius and Smith had, a long time before, injected medicines into the human veins. But from what I have already said, it is obvious, that if I had been aware of this fact, the results of their trials would have done little to teach me what to expect from mine, or to prevent the feeling of apprehension for the consequences.

considerable pain, I succeeded in introducing it. We immediately took off the ligature and began to inject the oil. The hæmorrhage ceased as soon as the ligature was loosened: we estimated that about eight ounces of blood was lost, before that was done.

The injection of the oil was a slow operation. In consequence of the delay arising from the difficulty of introducing the tube, the temperature of the oil was reduced to about 70°. It was consequently less limpid, and less easily forced through the small tube into the vein. But the principal obstruction arose from the difficulty of carrying the oil forward into the circulation, after it had entered the vein. It was constantly disposed to regurgitate, and to pass out by the side of the tube into the cellular membrane, and upon the arm. To prevent this, we injected the oil very slowly, and as often as a little was collected in the vein, it was necessary to press it forward gently by the fingers upon the arm, until it disappeared. While we were injecting the first drachm of oil, I carefully watched my own feelings, and kept my hand upon the region of the heart, that I might be fully sensible of it, if there should be any unusual pulsation; and after this drachm was all thrown up, we waited two or three minutes to watch its effects before we injected any more. But not perceiving any unnatural sensation, we proceeded to inject the remainder of the half ounce. In doing this about a drachm was lost, by the regurgitation of the oil out of the vein; and we poured out another drachm and injected it, so as to make up the full quantity that I had originally intended to use.

It was half past eleven in the morning, when the vein was opened, and we were occupied twenty-five minutes in completing the injection. In order to be certain that the oil actually entered, and to retain as much of it as possible, I carried the tube some distance into the vein; the greatest part of the time, not less than three quarters of an inch of its length was within the vein. There was no hæmorrhage when the tube was withdrawn. There was a tumour, of nearly the size of half a walnut, below the vein on the inside of the arm, occasioned partly, if not wholly, by an effusion of the oil into the cellular membrane, and increased perhaps by an extravasation of blood with it. A compress of lint was placed upon the wound, and the arm was bandaged as in the common operation of venæsection.

I felt very well for a short time after the operation was finished. The first unusual sensation that I perceived, was a peculiar feeling, or taste, of oiliness in the mouth, a little after twelve o'clock. Very soon after, while I was washing the blood from my arm and hands, and was talking in very good spirits, I felt a slight nausea with eructations, and some commotion in the bowels, then a singular indescribable feeling seemed very suddenly to ascend to my head. At the same instant, I felt a slight stiffness of the muscles of the face and jaw, which cut short my speaking in the middle of a word, accompanied by a bewildered feeling in my head, and a slight faintness. I sat down, and in a few moments recovered myself a little.

This part of the experiment had been performed in a room, at a little distance from my lodgings. At a quarter past twelve, I walked home. My countenance was pale, and the oily taste continued in my mouth, with some dryness; but I felt a little better for the air. After sitting ten minutes, at twenty-five minutes past twelve, I counted my pulse and found it seventy-five in a minute.

Thirty-five minutes after twelve. The disturbance in the bowels continues and increases; slight pains moving about in them, with the feeling as if I had taken a cathartick: copious eructations of wind, and slight nausea. There is a strange sensation in my head; it is not a dizziness, though somewhat like it. My arm feels rather stiff, which I attribute to the bandage; it is also a little sore, but is not painful.

Forty-five minutes after twelve. The bowels are still more in commotion, and the nausea is increased. My mouth still feels oily, but is not so dry as before. In five minutes more the disturbance in the bowels was increased so much that I thought there would have been a discharge from them, and made the attempt; but without success. There is a slight pain in my head, and the strange feeling continues.

Twenty minutes past one. The pain in the bowels is increased, with some tenderness on pressure; have a very strong feeling as if a cathartick were about to operate; but an attempt to procure a discharge from the bowels was unsuccessful, as before. The nausea continues.

Two o'clock. My general feelings are better: the nausea is nearly gone. Notwithstanding my previous experience of the deceptive nature of the pain and uneasiness in the bowels, the feeling of inclination to stool was so strong and distinct, that I was induced to make another attempt, but with no more success than before. The same thing occurred again at forty minutes past two, and twice in the course of the evening. The sensation was, in every instance, strong and perfectly distinct; at least it entirely deceived me, although I was on my guard, from the previous unsuccessful attempts. In the course of the afternoon and evening the flatulence and pain in the bowels diminished; but the tenderness remained for several days.

At twenty minutes past two, while I was making a little exertion, my arm began to bleed again, rather freely; and we had some little difficulty in stopping it. I was alone at the time, and before I could call my assistant, and take off my coat and apply the dressings, about six ounces of blood were lost. We were obliged to make the compress and bandage quite tight over the vein; and even then, there was a considerably ecchymosis into the cellular membrane.

At three o'clock I dined upon a piece of pudding, which I ate with some little relish, but felt a slight nausea afterwards. About four o'clock, having a pretty urgent call to visit a patient, I went a little distance in a carriage, but took my assistant with me, to guard against accidents. I did not go out again until the 25th of February, and only twice for a short time till the second of March. Towards evening my arm became quite stiff, and considerably painful, and continued so through the night. It was swollen on the inside, from several inches below the elbow, almost to the axilla; and there was considerable heat and tension, especially about the elbow, and a little above it. At eleven o'clock, my pulse was eighty-four. I passed rather a restless night, but got some sleep.

February 21. In the morning there was a small discharge from the bowels, which was smaller and more costive than I am accustomed to have at that hour. My arm was more swelled, and more painful than before; pulse eighty. I had some pain in the head, and was all day much inclined to chilliness, though without true rigors. My arm was quite painful through the day. I had some fever and loss of appetite, and felt altogether too ill to make any use of my faculties, either of body or mind. This state continued several days; and when I began to recover from it, I found my strength so much diminished, that it required two or three weeks to restore it to its former vigour. In order that I might interfere as little as possible with the effects of the injection, I wished to defer making any application to my arm as long as it could prudently be done; but on the evening of the 21st it became so painful, that I applied pieces of linen dipped in the water of acetated litharge, diluted. This answered the purpose, and in some measure relieved the inflammation and pain; and I passed a rather more quiet night.

February 22. I was costive the whole day; and at night took a mild cathartick pill. The pain in my arm was less severe, and I was in every respect better. From this time I gradually recovered my health, although very slowly. I made use of no more medicine, except to continue the lead-water. The swelling of my arm went off slowly, by the diminution of the inflammation, and the absorption of the effused blood; so that on the 25th I was able for the first time to touch my forehead with the end of my fingers. For some time after I recovered my appetite for food, my powers of digestion seemed to be enfeebled, and I was frequently annoyed by indigestion.

It is now four weeks since the experiment was begun by injecting the oil, and I have not yet quite recovered my usual strength and vigour. I am more easily fatigued, and am obliged to pay more careful attention to my diet than before. My left arm is weaker than it was previous to the operation, and is still sometimes subject to pains about the elbow. The tumour, which was made by the effusion of the oil and blood into the cellular membrane, is not yet wholly absorbed, although it has been constantly diminishing. It is now to the feeling about the size of one half of an English walnut. It does not however materially impede the motions of the arm.

I have been thus particular in detailing the circumstances attending this disastrous experiment, in order that my readers may judge for themselves, how far the symptoms, which followed the operation upon the vein, were the necessary consequence of the injection, or whether any of them might have arisen from accidental circumstances, or from the manner in which the operation was performed. I am aware of the difficulty of observing correctly one's own feelings, and of the liability to mistake the impressions of the imagination for the effects of disease, or the operation of medicine. But I am not conscious that I have done so in this case.

My next object was to ascertain the effect of injecting medicines into the veins of animals, in the form of powders, and of spiritous tinctures. For the first of these objects I preferred to begin with such a substance as should be nearly or quite destitute of active properties, except from its action simply as a powder, intending, if I found this innocent, to try more medicinal powders afterwards; and for the latter I resolved first to try simple alcohol, and if necessary to follow it with other medicines in solution.

EXPERIMENT XI.

March 15. I mixed twenty grains of calcined magnesia, in fine powder, with five drachms of water, and placed the cup which contained it in a bowl of water of the temperature of 100°; but before I was ready to use it the temperature had fallen to 70°. After I had confined a rabbit to the table, I counted the pulsations of the heart, and found them two hundred and ten in a minute, the animal being much frightened and agitated. I laid bare the jugular vein, and having passed a double ligature under it, I opened it and inserted the tube. I then filled a syringe with the mixture of magnesia and water, taking care that they should be intimately mixed, and began to inject it slowly into the vein. I had not thrown in more than half a drachm of the mixture, when the animal was seized with violent convulsions, and died in four minutes from the time that the injection was begun. One minute after, I opened the chest. The contractions of the heart still continued, though they were very unfrequent, about once in three seconds. They grew more and more faint for about five minutes, when they ceased. The right auricle continued to contract feebly, for about five minutes longer. The blood in each side of the heart was of its appropriate colour. The vessels of the mesentery and intestines were beautifully injected with blood; and the peristaltic motion of the intestines was very conspicuous.

EXPERIMENT XII.

March 15. This experiment was also performed upon a rabbit. The pulsations of the heart after the animal was confined were two hundred and ten in a minute. I made the same preparations as before, and injected into the jugular vein, one drachm, by measure, of alcohol diluted with the same quantity of water. For the first two or three minutes there was considerable agitation of the pulse, and the respiration was quick and laborious. In a short time the pulse became more regular; it was full, and again beat two hundred and ten in a minute. Ten minutes after the first injection I injected two drachms of undiluted alcohol. Before it had all entered the vein, the animal had a slight convulsion, the heart suddenly ceased to beat, and within one minute from the beginning of the injection, he was dead. Five minutes after I opened the chest. There was no motion in any part of the heart; nor was any excited by cutting into it. The blood was florid in the left side of the heart, and dark in the right; in the right auricle it was coagulated. The mesenteric vessels were not filled as in the last experiment; but the peristaltic motion of the intestines was about the same as in that.

It is very obvious that substances which possess corrosive or irritating qualities, to any considerable extent, cannot be thrown into the veins without destroying life. If therefore any medicines are to be thus administered, it becomes an object of interest to ascertain those which admit of being injected with safety. As some of the neutral salts are much used as cathartics, and readily admit of a watery solution, my attention was directed to them, to ascertain the effect of introducing them into the veins.

EXPERIMENT XIII.

March 22. I dissolved two drachms of the tartrite of potass and soda in half an ounce of water, and attempted to inject the solution, at the temperature of about 100°, into the jugular vein of a rabbit. While we were doing this, the ligature which had been passed under the vein, cut through it, so that only a part of the salt (about one third) entered the vein. There was some hæmorrhage; to the amount of about half an ounce of blood being lost. The external wound was closed by ligature without tying the vein. At the moment of the injection, the pulsations of the heart suddenly became very quick and feeble, but soon partially recovered. After the animal was tied at the beginning of the experiment, the pulse was two hundred in a minute; ten minutes after the injection it was two hundred and forty. The animal was very dull and not inclined to move, after he was liberated from the table. Within the first half hour there was no effect on the bowels. Four hours and a half after the injection, there had been several tolerably copious discharges of fæces, of a natural appearance. The animal was now lively, and ran about the room so that I had some difficulty to catch him. The next day he was as lively and active as he was before the operation. Two days after he was taken for the next experiment.

EXPERIMENT XIV.

March 24. Two drachms of sulphate of magnesia were dissolved in half an ounce of water. I made an opening into the other jugular vein of the same rabbit, and fixed the tube in it, by passing a ligature round the vein so as to include the tube. I then began to inject the solution, the temperature of which was about 80°. Before one third of the quantity was injected, the pulsations of the heart seemed to sink away, and stopped; the animal was at the same time convulsed, and died within a minute and a half. In five minutes, I opened the chest. There was no action in the heart. In five minutes more, I began to inflate the lungs with a double bellows, producing an artificial respiration; but the heart was not excited to action by it.

It was originally my intention to pursue this investigation much farther. But several causes, the principal of which was the illness occasioned by the experiment upon myself, have concurred to prevent my doing it. I trust, however, that enough has been done to settle the question, at least, for all practical purposes.

From a review of the observations which have been presented, I think it may be regarded as established, that *in general*, the operation of emeticks and cathartics on the stomach and bowels is the same when they are injected into a vein, as it is when they are received into the stomach, with the exception that their action is much more speedy and energetick. In my own case, although no cathartick effect was actually produced by the oil that was thrown into the vein, yet the injection was followed by symptoms which are the natural result of an insufficient dose of a mild cathartick. Even the subsequent costiveness might naturally enough be ascribed to this cause. It was my intention, when I began this experiment, to have provided a perfect standard of comparison for the action of the oil in the blood vessels, by taking the same quantity into the stomach, as soon as the effects of the first had wholly passed away. But those who know the reluctance which we unavoidably feel, to encounter any thing which has been the medium of inflicting a considerable degree of suffering, will not be surprised that I have little inclination to pursue the experiment. As it is, the effect on the bowels, although it was incomplete, was too decided and characteristick, to leave any room to doubt that it was produced by the oil. And this conclusion is entirely in accordance with the results of the numerous experiments of Orfila, Brodie, and Magendie, on the smaller animals.

These facts lead us to a curious and most interesting field of inquiry in regard to the action of medicines in general, or rather in regard to the manner in which their action is excited, and the system of organs in which the action begins. If emeticks act more speedily, as well as more powerfully, when they are mixed with the blood in the veins, than when they are received directly into the organs, in which a specifick action is to be excited by them, we are led to infer that it is by their absorption, and circulation with the blood, and not by their stimulus upon the stomach and intestines themselves, that these medicines produce their appropriate effects upon the animal system.* There are, however, some phenomena

* I trust it will not be supposed that I consider this observation as original with myself, especially as it respects emeticks, or that I am unmindful of the observations of Brodie and Magendie on this subject. I have not thought it necessary to refer particularly to their remarks in this place, because the question of the mode in which the action of these medicines is excited, curious as it is in itself, is only mentioned incidentally, and has no direct bearing upon the main subject of this discussion.

which it is not easy to explain upon this supposition. For if the medicine, in either mode of administering it, actually enters the circulating system, why are the other effects, besides its action on the stomach and bowels, so much more violent and disastrous in one case than in the other. Indeed there is much left for investigation in reference to this subject. But it would lead me too far from my present purpose to attempt the investigation at this time.

Although it be true that emeticks and catharticks excite vomiting and catharsis with equal, or even greater efficacy, when injected into a vein, than when taken into the stomach, it does not of course follow, that this mode of administering them is equally safe and proper. We return therefore to the inquiry as to the safety and propriety of such injections.

The operation necessary to making an injection of any medicine into a vein, is of too serious a nature, in comparison with taking the same medicine by mouth, ever to come into general use in ordinary cases, even if there were no other objection to it. Simple venæsection is submitted to by most patients with much more reluctance, than they feel to taking an emetick or a cathartick; and the injection must necessarily add not a little to the painfulness, as well as terrour, of the operation. We are not therefore to look upon this as a remedy for common every day practice; but to inquire if it may not be adopted as a substitute, in cases where the ordinary mode of administering medicine is impracticable. Cases may easily be supposed to occur, and some such are found on record, in which an emetick or a cathartick is strongly indicated, but cannot be administered. There may also be one other state of things, in which injections might sometimes be resorted to, provided they were known to be perfectly safe. Where the disease is such as to require both bleeding and cathartics, or emeticks, the vein being already opened, a patient might prefer having the medicine thrown into the vein, to taking it in the common way.

In reference to the safety of injecting emeticks, it must not be forgotten, that all the articles commonly used as emeticks, with the exception perhaps of ipecacuanha, are themselves poisons, if they are retained in the system in any considerable quantity. As they are usually administered, this circumstance is of little or no importance; because the excess of quantity, beyond what is required to excite vomiting, is immediately thrown off, by the operation of the medicine. But if the emetick is injected into a vein, and a little too much is given, there is no mode by which the excess can be promptly carried out of the system and death must be the consequence.

The result is the same when the medicine is introduced into the stomach, if vomiting is prevented, and the medicine retained long in that organ. This is fully shown by the experiments of Magendie on the tartrite of antimony. When the œsophagus was tied so as to prevent vomiting, Magendie found that four grains of the tartrite always occasioned death in dogs, in a short time, although when they were permitted to vomit freely, a drachm very rarely produced any evil effects; and he has sometimes given half an ounce at a time.

"La conséquence générale," says this author, "qu'on peut déduire de tout ce que j'ai dit, est telle qu'on pouvoit le prévoir par le simple raisonnement; savoir, qu'un homme ou un animal pourra prendre sans danger une dose très-forte d'émétique, pouvre qu'il vomisse promptement après l'avoir prise, et qu'en vomissant il rejétte à très-peu-près tout le sel qu'il avoit avalé. Dans le cas contraire, c'est-à-dire, si l'homme ou l'animal qui a pris l'émétique en grande quantité, ne vomit point, ou vomit sans rejeter la plus grande partie de l'émétique qu'il a avalé, il pourra en résulter des accidens graves, et la mort; dans ce dernier cas, on auroit encore un semblable résultat, quand bien même la quantité d'émétique ne seroit point très considérable."*

A similar remark is made by Orfila in reference to all the poisons which occasion vomiting, insomuch that he says that the action of the poison upon the animal economy cannot be properly ascertained, unless the œsophagus is tied so as to prevent its being thrown out of the system.

Since then the consequences of injecting too large a quantity of an emetick would be so disastrous, is it possible to ascertain any precise quantity, that is requisite to excite vomiting, in such a manner as to avoid so imminent a danger? In regard to one species of animals, this question is sufficiently answered by some of the observations of Orfila. By referring to his experiments, we find no such relation between the quantity of medicine given, and the effects produced, as will enable us to fix upon any quantity, in proportion to the size of the animal, that will be always efficacious and always safe; or even generally so. In some cases the effects produced by the injection were much more violent, than in others

* Memoire de l'influence de l'émétique, p. 33. It is worthy of remark, that the effects here described as produced by a small dose of the tartrite of antimony in the stomach with the œsophagus tied, are very similar to those of injecting the same quantity into the veins, as they are described in a previous quotation. Vide page 89 of this paper.

where a larger portion of the medicine was introduced. This is precisely what we should expect to be the case, when we consider the great variety of habits, and difference in strength and vigour among animals of the same species.

We have already seen, that in the experiments of Magendie, two grains of the tartrite of antimony injected into the veins, generally destroyed the animal, and that one rarely produced vomiting; and what is still more conclusive, if one grain only was injected, and a second given in the same manner the next day, death was invariably the consequence. There is therefore no hope in regard to these animals, of fixing upon any quantity that will operate as an emetick, without imminent danger to the life of the animal.

In our own species the uncertainty of the effects to be expected from a given quantity of medicine must be much greater than in other animals, from the greater variety of causes, which might modify its operation. But who would have the temerity to make the experiment, in the certainty that an errour of two or three grains, or perhaps of a single grain, must be immediately fatal ?

These objections to the injection of emeticks into the veins appear to me to be altogether insuperable, and to prove beyond the possibility of doubt the extreme danger and impropriety of adopting such a mode of administering them. It is not indeed certain that ipecacuanha would act thus fatally as a poison. But the watery preparations of this medicine possess little or no emetick virtues, and we have seen that alcohol, and the most inert powder, in the veins, operate to destroy life almost instantly; so that this article, although on somewhat different grounds, must be ranked with the other emeticks, and its use as an injection absolutely prohibited.

Most of the cathartick are liable to the same objections that we have pointed out in emeticks. Almost, or quite, all the more active catharticks, are either poisons in excessive doses, or can only be administered in substance, or in solution in alcohol. It is not necessary to repeat what I have said respecting emeticks, to show that these cannot be injected into the veins with safety. Many others are evidently too acrid in their nature to admit of this use being made of them; and we have seen, from experiment, that little is to be expected from the neutral salts. Of those that remain, it is impossible to determine how many, or which of them admit of being thus administered, except by actual experiment on each individual article.

In the experiments of which I have here given an account, in most of the cases in which the medicine in the veins did not destroy life, there was a considerable agitation, at the time the injection was made ; as if some unnatural shock were given to the system. Whether that peculiar, strange sensation which I have mentioned in my own case, and which was felt soon after the injection of the castor oil, arose from the same cause, or was only the effect of an incipient faintness, I leave to others to determine, although the former supposition appears to me the most probable.

It seems to be true, however, that there are a few catharticks, which may be injected into the veins without very imminent danger ; or rather, that the injection of them is not *certainly* destructive to life. But are the advantages to be expected from the injection of these medicines (being it is to be remembered among the least powerful of the catharticks) at all commensurate with the pain and inconvenience *necessarily* the consequence of such an operation ? I have not spoken of this objection before, because in reference to the other medicines proposed to be injected, there are other considerations sufficiently decisive. And yet the operation is not a slight one, nor the consequences trifling.

It is possible that some of the inconveniences to which I was subjected, might be avoided by a more perfect manner of performing the experiment. But some of them at least are necessarily incident to the very nature of the operation. Unless this operation is rendered still more formidable by prolonging the external incision and passing a ligature under the vein, there must always be some difficulty in inserting a tube into the vein. Without doing this, it must also be impracticable to prevent some of the medicine from returning, and becoming more or less injected into the cellular membrane. The current of blood from below the orifice being intercepted, there is nothing to carry the medicine forward immediately after it is thrown into the vein. External pressure may assist, but is not sufficient to prevent some of it from returning: and the presence of a foreign substance, however mild in its nature, in the cellular membrane will always occasion a greater or less degree of inflammation.

On a review of the whole subject, we find that the evacuations occasioned by the operation of emeticks and catharticks might be procured quite as effectually, and even more so, by injecting them into the veins, as by introducing them into the stomach; but that it would be dangerous in the extreme to administer in this manner any of the emeticks, or of the more powerful catharticks; and that the injection even of the milder catharticks is attended by much more pain and inconvenience, than can be counterbalanced by any advantages that it seems to promise. We must therefore regard this mode of administering medicines as too full of dangers to be entitled to confidence.

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