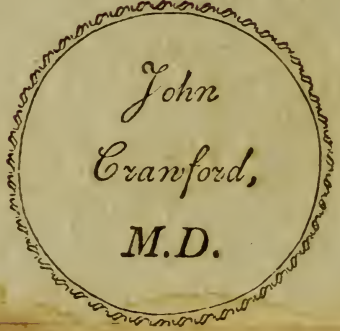
Surgical
and
Physiological
Essays
by
Mrs Anne Ray



SURGICAL

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ESSAYS.

PART II.

BY

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Crawford

AN

ESSAY

ON THE

NATURE OF THE MATTER PERSPIRED AND ABSORBED FROM THE SKIN.

DOCTOR Prieſtley after having well explained the effects, reciprocally produced on the air and the blood, by the proceſs of reſpiration; proceeds to inveſtigate thoſe alterations of the atmoſphere, which, animal perſpiration cauſes. He however only ſlightly purſued this ſubject, nor is it a wonder, that he failed to diſcover that information, which he did not attentively ſeek. He has aſſerted that animal perſpiration does not injure the purity of the air, in the man-

ner effected by the proceſs of reſpiration. But the experiments here related, clearly contradict this aſſertion, and eſtabliſh a ſimilarity in the nature of the matter exhaled from the lungs, and the ſkin of the human body.

Dr. Ingenhouze obſerved, that air, was perſpired from the bodies of animals, as well as of vegetables, but as this was not the immediate object of his enquiry, he did not very attentively examine its nature. He ſays, "it "ſeemed to be partly fixed air, as it was ſomewhat abſorbed by water;" and the remainder was far from being good reſpirable air.

Mr. Cruikſhank entertained the opinion, that the matter of perſpiration, and that expelled from the lungs in breathing, were ſimilar in their qualities. In his experiments, he collected the aqueous exhalation from the ſkin, but only ſlightly examined the aeriform matter: he agitated lime water in the air with which his hand had been ſurrounded, when

when the precipitation of the lime ſhewed the exiſtence of fixed air; he has alſo obſerved that a candle burned dimly in this air. Such I believe was the extent of the information, which we heretofore poſſeſſed on this ſubject.

In the ſummer of 1791 I had made many experiments to aſcertain the nature of the matter, perſpired and abſorbed from the ſkin: but the winters cold obliged me to deſiſt, before I had compleated my deſign. In the ſpring of 1792, the Reviewers announced to the public, that M. Lavoiſier and M. Seguin had delivered a paper on this ſubject, to the royal Academy of Sciences at Paris. Much time may elapſe before the contents of that paper will be made public, and even then, it may not meet with common peruſal in this country.—As I know not the nature or extent of theſe experiments, and as thoſe which I have made, appear to me ſatisfactory, and meriting attention; I have therefore reſolved to ſubmit them to public notice.

It never appeared to me very difficult, to aſcertain with tolerable preciſion, the nature of the matter, which the ſkin emits or imbibes. If the hand be introduced into an inverted jar filled with quickſilver; the matter detached from it will aſcend to the top of the mercury, and remain unaltered a ſubject of chemical examination. A trough containing a ſufficient quantity of quickſilver to allow ſuch introduction, was all that ſeemed requiſite; this I procured, and proſecuted the experiments in the manner, which I ſhall relate: but it will be previouſly neceſſary, briefly to deſcribe the veſſels, and other apparatus, which were employed.

The jars which I filled, and inverted in the quickſilver, were made of glaſs, they were nearly of a cylindrical form, and of ſufficient dimenſions, to contain my hand, and wriſt. I contrived to fix them in the quickſilver, inclined in ſuch a manner, as to admit the introduction of my hand: this was done by confining them in an hollow cylinder of copper, which was faſtened to the ſide of the

trough. I meaſured the ſurface of my hand, and wriſt, as accurately as I could with paper, and computed their extent to be ſeventy ſquare inches. Theſe experiments will probably afford but an imperfect eſtimate of the quantity of the matter of perſpiration; for the propenſity of the hand to riſe in quickſilver, occaſioned a conſiderable preſſure of the wriſt againſt the edge of the bottle, which often impeded the circulation, and benumbed the ſenſibility of the hand, and probably interupted the functions of the ſkin. The coldneſs and weight of the quickſilver would alſo tend to impede perſpiration.

It is right to mention, that more experiments were performed than are related: I ſelected thoſe, the events of which, were moſt common, and which appeared to have been made with moſt accuracy.

Experiment A. *Thermometer about* 60°.

I filled a glaſs jar with quickſilver, which being inverted, and confined in the way I have

deſcribed: I held my hand ten minutes in the trough, beneath the ſurface of the quickſilver, and frequently moved it in that ſituation, in order to detach any atmoſpheric air which might accidentally adhere to it, and afterwards introduced it into the inverted jar. The quickſilver ſoon acquired a degree of warmth, which rendered it not unpleaſant; minute air bubbles aſcended to the top of the quickſilver, more ſpeedily in the beginning of the experiment, more tardily towards the concluſion. After an hour had elapſed, I withdrew my hand; the bubble of air which now appeared on the top of the quickſilver, was, I ſuppoſe in bulk, equal to one ſcruple of water. In ſixteen hours I collected an half ounce meaſure of air, which makes fifteen grains the averaged product of an hour. No kind of moiſture appeared on the ſurface of the quickſilver, ſome ſucking paper was put up, which was withdrawn unmoiſtened. My hand was always damp when taken out of the quickſilver: whatever aqueous perſpiration was produced, adhered to its ſurface, whilſt the aeriform, from its levity, aſcended to the top

of

of the mercury. To the air, which I had thus collected. I threw up lime water, when about two thirds of it were rapidly abſorbed: to the remainder I added a bubble of nitrous gas, but could diſcern neither any red fume nor diminution of quantity. I repeated this experiment ſix times, with ſimiliar, tho' not uniform events. I believe it will be found that the air perſpired, conſiſts of carbonic gas, or fixed air, a little more than two thirds—of nitrogenous or phlogiſticated gas, a little leſs than one third. In one experiment, the nitrogene made only a fourth part of the air collected, and in another I thought it exceeded its uſual proportion of one third.

I believe it will not be doubted, that the air which remains after the ſeparation of the carbonic gas, is entirely nitrogenous. It would be very ſtrange, ſhould atmoſpherical air, which is a compound, be perſpired. If nitrogene be ſuperabundant, and injurious to the body, and therefore thrown out from its ſurface; it is not probable, that oxygene which is ſalutary, and requiſite to the animal, ſhould in the ſame

manner be diſſipated. On the repetition of theſe experiments, much variety in the quantity of air perſpired, was remarked. At one time the quantity collected in nine hours, occupied the ſpace of only thirty-two grains of diſtilled water: at another time, in three hours, it equalled the bulk of two ſcruples of water.

As I concluded, that perſpiration was obſtructed by the weight of the quickſilver, and alſo by the preſſure of the hand, againſt the edge of the bottle; I thought this proceſs would go on more naturally, beneath water: I ſuppoſed if I made an allowance of two-thirds for the quantity of carbonic gas abſorbed by the water, I ſhould thus be able more nearly to compute the uſual quantity of air emitted from a certain extent of the ſkin. In this conjecture, the event proved that I was miſtaken.

Experiment B. *Thermometer* 60°.

Having filled, and inverted a jar in water, I held my hand and forearm for ten minutes beneath

beneath the ſurface of the water, frequently moving it, to detach any adhering air, and then introduced it into the inverted jar. The ſurface of the ſkin, from whence air could ariſe into the bottle, was as nearly as I could compute, one hundred and twelve ſquare inches. My arm ſoon exhibited a curious appearance; it was magnified by the convexity of the glaſs, and every pore ſeemed covered by a little ſpherule of air, which on agitating the water was detached, and quickly aſcended to the top. After an hour had elapſed, a bubble of air was collected, which as nearly as I could gueſs equalled in bulk half a dram of water. In the proſecution of this experiment great variations in the quantity of perſpiration were remarked. I once held my hand an hour in the water without any apparent addition being made to the quantity of air collected. When I had thus, in nine hours obtained about three drams of air, I diſcontinued the experiment. The air remained unattended to, nearly a week, when the quantity was diminiſhed to about one dram

and

and a half, which being examined by nitrous gas, was found to be entirely nitrogenous.

I repeated this experiment, and each day changed the water in the trough, when a much greater quantity of air was collected. I procured, on an average half a dram each day, which was entirely nitrogenous. I attempted a similar experiment, in moderately warm water, but procured scarcely any air; the small quantity which was collected, was quickly absorbed by the water.

I heated myself by exercise, till the aqueous perspiration was moderately copious, but at that time less air was procured.

The conclusions which I have drawn in my own mind, from these and other experiments are, 1st, That the remainder of the perspired air, after the separation of the carbonic gas, is nitrogene. 2dly, That the quantity cannot be well estimated, because the water absorbs some portion of it. Water deprived of its air by boiling, evidently did so in one experiment,

riment, and whenever the water remained unchanged leſs air was produced. I think it probable that the ſkin abſorbs the air diſſolved in the water, and thus the diſpoſition of the water to imbibe air is augmented. 3dly, I have obſerved that, when by exerciſe, aqueous perſpiration was increaſed, leſs air was then produced; if the ſame veſſels ſecrete both theſe fluids, this obſervation would naturally be expected. When the circulation is moderately carried on, inſenſible or aeriform perſpiration is chiefly continued; but when the determination of blood to the ſurface is rapid and powerful, water is poured forth from the exhalents, and the perſpiration becomes ſenſible.

The related experiments clearly prove the quality of the *matter exhaled from the ſkin*, but they do not well ſhow the quantity; it was neceſſary to proſecute them further, and I could only depend on the reſult of thoſe, which were conducted beneath quickſilver. I could however prevent the ſurface of my hand ſuſtaining the preſſure of the quickſilver, by filling the jar into which it was introduced, with air.

air. The examination of this air would inform me, not only of what was added to it, but alſo of what was taken from it by the ſkin, in the ſame ſpace of time. I therefore expoſed my hand in ſucceſſion to different kinds of air, beginning with that of the atmoſphere. It is right to remark, that theſe experiments do not admit of great accuracy. I eſtimated the relative quantity of airs, found on examination, by marking on the bottle, the elevation and deſcent of the quickſilver, and afterwards filled the glaſs with water to theſe marks, which being weighed, afforded me the information that I wanted.

Experiment C. *Thermometer between* 50° *and* 60°.

I filled and inverted a jar in quickſilver, and threw up into it, one meaſure of atmoſpheric air, which could contain ſeven ounces of water. The quickſilver was depreſſed two inches and a half from the top of the jar. After moving my hand ten minutes beneath the ſurface of the quickſilver, to detach any common air which might adhere to it, I put it

it up into the air in the jar, and there retained it for the ſpace of an hour. Before I withdrew my hand, I depreſſed it beneath the ſurface of the quickſilver, ſtill keeping it within the glaſs, and agitated it in this ſituation, for ten minutes; this was done that I might not remove any of the air, which was the ſubject of the experiment. The ſame conduct was purſued in all the ſubſequent experiments. After five hours expoſure of the hand to this air, the quantity in the glaſs was diminiſhed about half an ounce. It might have been xpected that the perſpiration would have increaſed the bulk of the air, but in this experiment, the abſorption ſeemed to ſurpaſs in quantity the ſecretion.

I now threw up into the jar, lime water, by which nearly an ounce of air was rapidly abſorbed, and the lime was precipitated; the remaining air being examined by the addition of nitrous gas, was found to contain nearly one-ſixth leſs of oxygenous gas, than it did before the experiment.

In

In another ſimilar experiment, after the hand had continued nine hours in the air, I found more than one ounce meaſure of carbonic gas had been produced, and the remaining air being examined in the eudiometer, contained one-fourth leſs of oxygene than before the experiment.

The quantity of carbonic gas produced in theſe experiments, much exceeds that, exhaled from the hand when ſurrounded by quickſilver; in that ſituation, I thought half a dram in an hour, a copious product; but in air more than twice that quantity was conſtantly obtained.

The whole of my hand was not in theſe experiments ſurrounded by air; ſome part of it was ſtill preſſed upon by the quickſilver.

It might be enquired, does the oxygenous gas of the atmoſphere contribute to the formation of the carbonic gas? Both reaſon and experiment reply that it does not, for if oxygenous

oxygenous gas combined with carbone on the ſurface of the ſkin, much heat ſhould be produced at the time of their combination; but this production of heat is not found to take place.

Experiment alſo ſhows that carbonic gas is perſpired from the veſſels, for into whatever air the hand be immerſed, the quantity of carbonic gas given out, will be nearly the ſame. This is the firſt point which I wiſh by experiment to eſtabliſh.

Experimont D. *Thermometor between* 60° *and* 70°.

Having filled and inverted a jar in quick-ſilver, I put up into it a ſeven ounce meaſure of nitrogenous gas. I purſued the plan related in the former experiment, to avoid adding to, or abſtracting from, this air. After two hours expoſure of the hand, I thought the bulk of the air had increaſed, but after five hours had elapſed, I could perceive no difference in the quantity. On throwing up lime water, a rapid and conſiderable diminu-

tion

tion of air followed; rather more than an ounce of carbonic gas was produced, when no oxygene was preſent. The increaſe of the quantity of carbonic gas is accounted for, by the heat of the atmoſphere being greater, which diſpoſed the ſkin to more copious perſpiration. Nitrous gas had no effect on the remaining air.

I made ſimilar experiments with the hydrogenous and nitrous gaſes; in theſe an equal quantity of carbonic gas was produced, and when the hand was ſurrounded by oxygene, the quantity of carbonic gas was not greater.

The reader will perceive that in all theſe experiments much abſorption had taken place, the quantity of air contained in the jar was ſcarcely perceptibly increaſed in any experiment, and when atmoſpherical air or oxygenous gas was employed, it was generally much diminiſhed. I forbear however particularly to remark on this circumſtance, as I think the quantity abſorbed will be much better determined by other experiments.

The

The whole of the hand was not in contact with the enclosed air, and though I endeavoured to prevent it, some addition or abstraction, might be made in the introduction, or removal of my hand.

I next wished to discover what effect the action of the hand, would produce on carbonic gas.

Experiment E. *Thermometer about* 50°.

Into a glass jar filled with, and inverted in quicksilver, I introduced six ounces of carbonic gas, and exposed my handto it, for the space of nine hours, in the manner, and with the precautions, before related. In that time, the air was reduced in quantity, to less than three ounces. A portion of the carbonic gas was examined, by the addition of lime water, before the experiment, when it was almost wholly absorbed; an unexaminable bubble only remained. When the remaining gas was examined by lime water, after the experiment, a considerable quantity of nitrogene,

which doubtleſs exhaled from the hand, was found mixed with it.

I twice repeated this experiment, with ſimilar events, though with rather leſs diminution, in the quantity of carbonic gas: it was however ſufficiently evident, that the abſorption of this gas, by the ſkin, was very copious and rapid.

The abſorption of carbonic gas, makes it difficult to aſcertain preciſely, the quantity perſpired, ſince that gas which is thrown out from the body by ſecretion, will probably be readmitted by abſorption. I therefore wiſhed to diſcover the quantity of carbonic gas, perſpired in one hour.

Experiments F. *Thermometer* 80°.

The hand being retained one hour in five ounces of nitrous gas, no aſcent nor depreſſion of the quickſilver was remarked. On the introduction of Lime water into the glaſs, ſix drams of carbonic gas were abſorbed.

In a ſimilar experiment with atmoſpheric air, after the expiration of an hour, the quick-ſilver had rather riſen, and three drams of carbonic gas, were diſcovered by lime water. In another experiment, in which hydrogenous gas was employed, four drams of carbonic gas, were found at the termination of an hour.

All the laſt related experiments, were performed in very hot weather. If two drams of carbonic gas were emitted in an hour, as the quantity uſually obtained in five hours, was but one ounce, it would be a ſufficient demonſtration, of the abſorption of a part of the air perſpired. Neither are theſe experiments concluſive, as to the preciſe quantity of air emitted, for even in an hour, part of that which is exhaled, will be again imbibed. When I firſt attempted the experiments with carbonic gas, I ſuppoſed that the abſorbents would receive it reluctantly, for I thought that matter, which was thrown out from the ſkin in ſuch quantities, could neither be requiſite, nor ſalutary to the body. The expe-

riment proved that I was miſtaken, and there are reaſons, which tend to ſhew the ſalubrity of this gas. When it is admitted into the ſtomach, it is generally found beneficial. When employed as a local application, its ſtimulus is uſeful, and when in combination with the blood, it probably produces, equally ſerviceable effects. The large quantity, in which it is generated, is in my opinion, an argument in proof of its utility. Its production, at firſt ſight, may appear an act of neceſſity. In the decompoſition of oxygenous gas, the carbonic, it might be ſuppoſed, muſt be generated, yet with ſuch wiſdom is the body conſtructed, that the benefit of any operation therein performed, ſeldom terminates in the production, of a ſingle effect; but this reſult of a prior cauſe, becomes itſelf an agent, in the production of other uſeful conſequences. Thus the decompoſition of oxygenous gas, may be neceſſary, but the production of abundance of carbonic gas, is probably very ſalutary.

The experiments that have been related, indiſtinctly

indiſtinctly ſhew, that a ſmall quantity of one kind of air, when mixed with a larger proportion of another, can be abſtracted from it, by the action of the animal body. This circumſtance will be hereafter fully proved. I will now relate an experiment, that was made in ſupport of this opinion, as it was performed beneath quickſilver, and in the ſame manner with thoſe, which immediately precede it.

Experiment G. *Thermometer between* 60° *and* 70°.

Into a jar filled with, and inverted in quickſilver, three meaſures of nitrogenous gas, and three of carbonic were introduced; the two airs depreſſed the quickſilver, two inches and a half, and occupied the ſpace of ſeven ounces of water. After five hours expoſure of the hand, the air contained in the jar, filled the ſpace of only five ounces and a half of water; on putting up lime water to this air, it was diminiſhed to three ounces. In this experiment, one ounce and a half of carbonic gas appears to be removed, and half an ounce of nitrogenous; but if you admit that one ounce

of carbonic gas, was perſpired during this experiment, and one-third of an ounce of nitrogenous, the quantity of air eſtimated to be abſorbed, is increaſed, but the proportions, remain unaltered.

In the experiments with common air, I have mentioned, that it contained leſs oxygene after it had undergone the operation of the hand, than before it became the ſubject of experiment. A queſtion here occurs; does this variation in proportion, ariſe from the addition of the one gas, or the removal of the other? That it is owing to abſorption, will, I believe, be evident, from the following experiments. Although the addition made to any kind of air, cannot be accurately aſcertained, when water is employed, yet if the hand removes any portion of air, that removal, will be aſcertained by examination, neither does the experiment appear liable to deception. In the experiments next related, the air was confined by water; this gave me an opportunity of uſing larger veſſels, and expoſing a greater extent

extent of ſurface of the ſkin to the contact of the air. I forbore particularly to remark the quantity of air abſorbed in the foregoing experiments, for though it correſponded to thoſe, which I ſhall next relate, yet the correſpondence was not uniform, and the degree of abſorption was leſs evident.

Experiment H. *Thermometer* 60°.

I filled and inverted a jar in water, and put up into it twenty-four ounces by meaſure, of atmoſpheric air, to this the hand was expoſed for twelve hours; the ſame precautions were uſed to avoid adding to, or taking from the air contained in the jar. The water had riſen in the veſſel, and about two ounces and a half of the air, were removed; that which remained was examined in the eudiometer, when two meaſures of it, and one of nitrous gas, filled the ſpace of nearly two meaſures, and one-third of another; it therefore follows, that about one-half of the uſual quantity of oxygenous gas, was removed from the other part of the atmoſphere.

That there could be no addition of nitrogenous gas capable of ſo greatly altering the proportions of theſe gaſes muſt, I think, be too evident, to need argument for its proof. Similar experiments, were afterwards made with correſpondent events. In the experiments made under quickſilver, the abſtraction of oxygene was equally evident, and conſiderable; it therefore appears, that the animal body is capable of taking away the oxygene, when in intimate mixture, with a much greater quantity of nitrogene. The avidity with which oxygene is abſorbed, will be made ſtill more conſpicuouſly evident, by the following comparative experiment.

Experiment.

I filled and inverted two jars in water, into one I put twenty-four ounces by meaſure of nitrogenous gas, into the other, the like quantity of oxygenous. The hand was put into theſe airs alternately, and retained there for an hour each time: after it had been expoſed to each, for eight hours, the water roſe one-

eighth

eighth of an inch in the bottle, containing the nitrogenous gas, and nearly a whole inch in that containing the oxygene. On eſtimating the quantity removed, by weighing the water which filled the bottles to the different marks, it appeared that one-twentieth part only of the nitrogenous gas was removed, but one-third of the oxygenous gas, was gone. The remaining oxygenous gas was found to to contain one-eighth more of nitrogenous gas, than before the experiment. I next examined the degree of celerity with which other gaſes would be imbibed.

Experiment.

Having filled and inverted a jar in water, and put into it thirteen ounces of nitrous gas, I retained my hand in this air, at different times, five hours, in which time three ounces were abſorbed. My hand being retained, for as many hours in a like quantity of hydrogenous gas, not more than one ounce and a half was removed.

As

The removal of a quantity of oxygenous gas, from common air, is ſurely a curious circumſtance; if this be the effect of an action, in the abſorbing veſſels, it muſt much exalt our ideas of their ſubtility, and their aptitude, or diſpoſition, to admit one ſpecies of matter, and to reject another. That the abſtraction of one air, in preference to another, depends upon this cauſe, I believe will not, on reflection be doubted; it might indeed be ſuſpected, that oxygenous gas was ſeparated from the atmoſphere, by the ſkin, as it is in the lungs, by chemical attraction: but it has been proved that carbonic gas is removed with equal celerity: and experiments on animal ſubſtances, ſhew in them a diſpoſition rather to part with than to imbibe carbonic gas. The removal of this air, is therefore not likely to be the effect of chemical affinity. The different degrees of celerity with which other gaſes are admitted, ſeem to eſtabliſh the opinion, that the removal of one kind of air in preference to another, is the effect of an active power, in the abſorbing veſſels.

The

The experiments which have been related, ſatisfactorily prove, the quality of the aeriform perſpiration; perhaps the proportions may occaſionally vary, but as nearly as I can determine, it conſiſts of rather more than two parts of carbonic, with the remainder of nitrogenous gas. The quantity of the matter perſpired is leſs certainly aſcertained; in one hour I obtained four drams of carbonic gas: but it ſhould be remembered, that theſe experiments were made in very hot weather; and it alſo deſerves notice that the quantity of the cutaneous perſpiration is ſubject to great variety. In every experiment, abſorption was found to be equal to perſpiration, in many it was much more copious; eſpecially, when the air to which the ſkin was expoſed, was ſalutary to the conſtitution. The oxygenous and carbonic gaſes are very readily imbided; whilſt the nitrous, hydrogenous, and nitrogenous gaſes, tardily gain admittance into the abſorbing veſſels. In the experiment marked G. from about half of the ſurface of the hand, two ounces and a half of carbonic gas were abſorbed in five hours: in thoſe experiments marked H, from the hand

hand and wriſt, there was imbibed,

In eight hours	8 ounces of oxygenous gas.
In five hours	3 do. - - nitrous gas.
In five hours	1½ do. - - hydrogenous gas.
In eight hours	1 do. - - nitrogenous gas.

I next endeavoured to aſcertain the quantity, and quality, of the aqueous perſpiration.

Experiment I *Thermometer* 65°.

I introduced my hand, and forearm, into a glaſs jar, covered with bladder; an aperture was left in the bladder, to admit my arm, round which the bladder was tied; ſo that the aſcent of any vapour was prevented. In ſix hours, I procured nearly three drams of limpid taſteleſs water. The quantity collected, correſponds with the reſult of Mr. Cruikſhank's experiments, who obtained the water of perſpiration in the ſame manner. Half of this liquid was evaporated by a gentle heat, there remained a ſmall reſidue on the glaſs, which had a very ſlight taſte of ſalt. The other half was ſuffered to ſtand many days, in which time, no change

change appeared: it did not then alter the colour of the vegetable blue. Into one portion of this watry liquor, marine acid was drop'd, which cauſed no coagulation, or precipitation, of animal matter: into the other, ſome cauſtic alkali was poured, which produced no viſible effect. I therefore conclude that the water of perſpiration, in a ſtate of health contains little of any thing, except a very ſmall portion of ſalt. The matter of perſpiration has ſometimes a ſour odour; whether it really contains an acid, I have had no opportunity of determining. It is a common obſervation, that the ſkin frequently has a ſalt taſte; doubtleſs if ſaline matter abounds in the fluids, it will be ſecreted in greater quantity.

Perſpiration is generally ſaid to be ſenſible, or inſenſible, perhaps it may be better diſtinguiſhed as aeriform, or watery. It may be expected, that a general eſtimate of the quantity of this ſecretion, ſhould be attempted; but the difficulties which oppoſe any accuracy of ſtatement are conſiderable. In theſe experiments

periments, the proceſs was not continued under its uſual circumſtances; the arm was ſurrounded by water, or quickſilver; and when in the latter fluid, the circulation was in ſome degree interupted by its aſcenſion, and preſſure, againſt the edge of the jar.—For the uncertainty, which theſe circumſtances occaſion, allowance can be made, but before an eſtimate of the quantity of perſpiration be attempted, the extent of the ſurface of the body, ſhould be known. Mr. Cruikſhank ſuppoſes the extent of the hand, to be to that of the body, as one to ſixty: it is much more, according to my computation.

After ineffectually endeavouring in different ways to meaſure the ſurface of the body; I concluded that I ſhould approach neareſt to its true extent, by meaſuring the circumference of the trunk, and limbs, at different parts, and having thus obtained the mean circumference; I could then calculate the extent of their ſurface, as if they were cylinders; the dimenſions of which, were aſcertained. The ſurface of the head, hand, and foot, I computed

puted, by applying paper cut as the occaſion required, over theſe parts: afterwards placing the ſeparate pieces of paper, ſo as to form an extended plain, I meaſured its extent. I ſhall mention theſe meaſurements, that the reader may correct them, if he thinks them erroneous. If a man be five feet ſix inches high, I will ſuppoſe the mean circumference of the trunk of his body, to be thirty-three inches, and its length from the top of the ſternum, to the os pubis, twenty-two inches.

	Square Inches.
The extent of ſurface of the trunk will therefore be	726
The circumference of the neck 13 inches, its length from the ſternum to the chin 3 inches	39
The ſurface of the head, and back of the neck	286
The mean circumference of the arm 10 inches, its length 12. Surface of both arms	240
The mean circumference of the forearm 8 inches, its length 10. Surface of both forearms	160
The ſurface of the hands and wriſts meaſuring to the extremities of the bones of the forearm	140
The mean circumference of the thigh 17 inches, its length 16. Surface of both thighs	544
The mean circumfererce of the leg 11 inches, its length 14. Surface of both legs	308
Surface of both feet	182
Allow for folds of the ſkin, inequalities of ſurface, &c.	175
The extent of the ſurface of the body will be	2700

The

The ſuperficial extent of the hand, and wriſt, according to this calculation, is to that of the body, as one to about thirty-eight and a half.

In three experiments, marked F, the leaſt quantity of carbonic gas emitted from the hand, in one hour, was three drams by meaſure; it may be ſuppoſed that the heat of the weather increaſed the ſecretion from the ſkin, let us therefore conſider two drams as the ordinary quantity. If then the perſpiration of all parts were equal, ſeventy-ſeven dram meaſures of carbonic gas and one third of that quantity of nitrogenous gas, would be emitted from the body, in the ſpace of one hour. If we alſo ſuppoſe perſpiration to be at all times equal, nearly three gallons of air, would be thrown out from the body in the courſe of one day.—Altho' the quantity of air perſpired is ſo large, yet the weight of the body will not be much altered by its loſs, it is the aqueous perſpiration, by which this will be principally diminiſhed. When the thermometer was between 60° and 70°, I obtained about thirty grains of the fluid from my hand and

part

part of the forearm in an hour; the ſurface from which, this ſecretion was made, I compute to be one twenty fifth part of the extent of the body. The ſuppoſitions being allowed, that perſpiration is at all times, and in every part, equal: about two pounds and a half is, the loſs of water which the body would in one hot day ſuſtain. In moſt of the experiments which I have made, the abſorption of air, was equal to the perſpiration; in many, it was much greater, eſpecially if the air was ſalubrious, to which, the ſkin was expoſed. The experiments marked H, make it appear probable, that if the naked body was expoſed to freſh currents of the atmoſphere, that only the oxygenous part would be abſorbed; the decompoſition of which, in the body, would produce an increaſe of animal heat; which might in ſome, degree counterballance the loſs ſuſtained by the expoſure. Our cloathing probably prevents, in ſome degree, this effect, and perhaps makes it leſs neceſſary. If the perſpired carbonic gas be confined by our garments, it ſeems likely, that it will be taken up again by the abſorbents. Whether the body

does uſually imbibe water from the atmoſphere, adequate to the loſs ſuſtained by aqueous perſpiration, is uncertain. I am inclined to ſuppoſe, that the abſorption of air from the ſkin, is greater than the ſecretion. The great quantity of water, which the ſkin can occaſionally imbibe from the atmoſphere, is evinced by people in a dropſy, and has been of late well ſhewn, by Mr. Ford, in the ſecond volume of the medical communications.

I next examined briefly, and in a comparative manner, the effects produced by the proceſs of reſpiration; becauſe I perceived in writers, ſome diſagreement of opinion on this ſubject. I firſt collected the water exhaled from the lungs, by breathing into a deep bottle covered with bladder, in the middle of which, was inſerted a tube, through which I breathed. From the bottom of the bottle another tube proceeded, which aſcended by its ſide and terminated on a level with its top. The air which I impelled into the bottle, found an exit by this tube, but the water being condenſed, became too heavy for aſcenſion, and remained in

in the bottle; thus the uneaſineſs which Mr. Cruikſhank experienced, in performing a ſimilar experiment, was avoided. In an hour, I collected nearly three drams of inſipid, but not perfectly limpid water; half of this liquor was evaporated: a ſlight cruſt remained on the glaſs, which was not ſalt, but had a burnt ſmell. I ſuffered the remainder to ſtand many days, but could perceive little odour from putrefaction, it changed however the vegetable blue to a faint green. I added to one portion of this liquor ſome cauſtic alkali, which had no viſible effect; I drop'd marine acid into the other, it became cloudy, and ſoon a filamentary depoſit was made, which reſembled mucus when precipitated: this precipitate diſſolved with difficulty in cauſtic alkali. In the water exhaled from the lungs, I diſcovered no ſalt, but it ſeemed mixed with a mucous like matter, which gave it ſome viſcidity.

I proceeded to enquire into the changes, which reſpiration produces in the air, that is breathed. Doctor Goodwyn in his experimental

enquiry, has ſtated, that the quantity of nitrogenous gas, is neither augmented, nor diminiſhed, by reſpiration. That nitrogenous gas is emitted from the lungs, I believe is the general opinion, which I think altho' not exactly aſcertained, is yet rendered probable by the following experiments.

Experiments.

Whilſt reſpiration was performed, as nearly as poſſible, in the ordinary manner; I drew in twelve cubic inches of atmoſpheric air, and expelled the ſame quantity into a glaſs veſſel, filled and inverted in quickſilver. One eighth part of this air was abſorbed by lime water; the remainder was examined in the eudiometer, and it was found that only one twelfth part of the oxygenous gas, was taken away.

Having filled and inverted a jar in quickſilver, into which, was introduced a bent tube alſo filled with quickſilver; I inſpired a moderate quantity of air, and retained it untill more than a ſlight uneaſineſs was felt. I then expelled the air from the lungs into the jar. Of this air, one eighth part was abſorbed by lime water; the remainder being examined

in

in the eudiometer, was found to be deprived of leſs than one-ſixth part of its oxygenous gas.

I have related theſe two experiments out of many which were made, as they ſeem to ſhew the greateſt, and leaſt degree, in which the atmoſphere is deprived of oxygene by the lungs.

The change produced in the air inſpired, muſt doubtleſs vary according to its quantity, and the time of its continuance in the lungs: I purpoſely retained the air in the laſt experiment, longer than uſual, that the deprivation of oxygene might be remarkable. I think the quantity abſtracted, varies between one-twelfth and one ſixth part. Doctor Goodwyn makes the diminution of oxygene, much more conſiderable, and has eſtimated the rate of its abſtraction, by ſucceſſive reſpirations. In the firſt reſpirations four parts out of eighteen, were removed: after the fifth, eleven parts of the oxygene had been imbibed from the lungs. In every experiment, which I made, the removal of oxygene, by the firſt reſpiration was leſs, and by the fifth frequently more, than Doctor Goodwyn has related.

Many eminent experimentaliſts have aſſerted, that the bulk of the air inſpired, is diminiſhed in the lungs; and that leſs air can be returned, than was admitted. To me the contrary appeared to be true in experiment, and I believe reflection will convince any one, that it is ſo in reality. For, if between twelve and thirteen parts of carbonic gas, be given out from the lungs, and if only between two and three parts of oxygenous gas, be taken in, whilſt the air remains in thoſe bodies: there muſt be, either an addition made to the bulk of the air; or a conſiderable quantity of nitrogenous gas, muſt be diſſipated in an inexplicable manner. To ſet this in a clear point of view, I ſhall ſtate theſe circumſtances after the manner of Doctor Goodwyn. I ſhall take that experiment, in which one-ſixth part of the oxygene, was abſtracted.

If the air inſpired contains of

nitrogenous gas.	Parts	80
oxygenous do.	do.	18
carbonic do.	do.	2
		100

And

And the ſame quantity of air be expired,
it contains, of nitrogenous gas. Parts 72½
oxygenous do. do. 15
carbonic do. do. 12½
100

But the expulſion of the ſame quantity of air, is the act of the perſon who makes the experiment, and not the natural effect of the proceſs of reſpiration. Were this ſtatement, a correct repreſentation of the change produced; it would excite ſurprize to find ſo large a quantity of nitrogene removed. It could not like the oxygene be imbibed by the blood: and the continuance of the air in the lungs, is too ſhort to admit of its removal by the abſorbing veſſels. The abſorbents of the ſkin, reluctantly admit nitrogenous gas; and analogy induces us to ſuppoſe, that thoſe veſſels in the lungs, would reject it equally. The removal of nitrogenous gas by the lungs, in any evident quantity, is ſo contrary to analogy and reaſon, that the truth of any experiment indicating ſuch abſtraction, ought to be ſuſpected.

That the carbonic gas, diſcoverable in the air expelled from the lungs, is exhaled from the pulmonary veſſels, and increaſes the bulk of the expired air, appears to me ſufficiently evident.

At one time, phyſiologiſts believed that the inſpired oxygenous gas, contributed to the production of the carbonic gas, found in the air expired. This opinion perhaps ſtill prevails in the minds of ſome people. The quantity of oxygene imbibed by the lungs is however too ſmall, to contribute to the formation of ſo much carbonic gas, as we find given out from thoſe bodies. The experiments marked A, clearly prove, that the exhaling veſſels of the ſkin, emit carbonic gas, in a ſtate of complete formation. And doubtleſs, thoſe of the lungs perform a ſimilar office.

If then but little nitrogenous gas, be removed from the air inſpired, and if the carbonic gas be emitted from the pulmonary veſſels, and augments the bulk of the air expired;

expired; the change produced by reſpiration, might be thus ſtated.

The air inſpired contains of			
nitrogenous	gas.	Parts	80
oxygenous	do.	do.	18
carbonic	do.	do.	2
			100

The air expired, contains of			
nitrogenous	gas.	Parts.	80
oxygenous	do.	do.	15
carbonic	do.	do.	12½
			107½

To me however it appeared, that the quantity of air expired, was nearly equal to that inſpired, when the experiment was performed beneath water, and when, the carbonic gas could not increaſe the quantity of emitted air. Of the truth, or falſity, of ſuch an experiment, any one may readily form his own opinion; but if it be admitted, that about two parts in a hundred diſappear; then, the effects of reſpiration, might be thus ſtated:

The

The air inſpired contains of		
nitrogenous gas.	Parts.	80
oxygenous do.	do.	18
		98
The air expired contains of		
nitrogenous gas.	Parts	81
oxygenous do.	do.	15
		96
Add alſo carbonic gas		
abſorbed by the water		12½
		108½

And you will thus obtain an account, which in my opinion, is the neareſt repreſentation of the change that reſpiration produces. As a large quantity of air, is always retained in the lungs, it is probable that the abſorbing veſſels, do like thoſe of the ſkin, conſtantly imbibe it: and this perhaps, is the cauſe of the diminution of the quantity of air, in which, an animal has been ſuffered to periſh. It ſeems alſo probable, that the emiſſion of air, from the bodies of dying animals will be much diminiſhed; but the function of abſorption

ſorption will be leſs impeded, by the declining powers of the ſanguiferous ſyſtem. I think it unneceſſary, to attempt to calculate the loſs or acquiſition of air, and water, to which the body is ſubjected, by the proceſs of reſpiration: it appears ſufficient, to remark, that the air and water, which enter into the compoſition of the animal body, undergo from the actions of the ſkin and lungs, conſtant and rapid changes; whilſt the other more eſſentially conſtituent parts, of the animal, are the ſubjects of a much more gradual alteration.

The ſimilarity of the office, performed by the ſkin, and the lungs, explains, in my opinion, many circumſtances, obſervable in the cauſes, and cure of pulmonary conſumption. The people moſt liable to ſuch complaints, are thoſe, whoſe conſtitutions are feeble, and whoſe thorax is of ſcanty dimenſions. If the cutaneous perſpiration of weakly people, be checked by expoſure to cold, it is tardily reſtored to its former ſtate; the conſtriction which exiſts on the ſurface of the body, is with difficulty overcome, owing to

the deficiency of power, in the heart, and arteries. The conſequence will be, a plethoric ſtate of the veſſels; from a retention of that great quantity of matter, which ought to be emitted from the ſurface. For the relief of this plenitude, other diſcharges will be ſubſtituted: common experience informs us of this fact, that weakly people readily (as it is expreſſed) take cold, the conſequence of which is, diſcharges from the noſtrils, the bowels, or the lungs. But if people of vigorous health, accidentally ſuffer any conſtriction of the cutaneous veſſels, the internal powers poſſeſs ſtrength to remove it, and to propel again the fluids through the exhalants of the ſurface.

When perſpiration is diminiſhed, the determination of fluids to the lungs, is particularly to be expected; becauſe the copious ſecretion which is made from thoſe parts, will relieve plethora; and becauſe, that ſecretion is ſimilar to the one which has been ſuppreſſed. The blood will become ſurcharged with air, to which the lungs only can

can afford an outlet. It is as much to be expected, that when perſpiration is ſuppreſſed, the office of the lungs will be increaſed, as that, performed by the kidnies: by means of the latter, the blood is freed from the ſuper-abundant water; by the former, from the retained air.

Thus an accumulation of fluids in the pulmonary veſſels, will enſue, which will be more conſiderable, and more likely to produce diſeaſe, when the thorax is of ſcanty dimenſions; and the tranſmiſſion of blood through the lungs, in conſequence, difficult. This plenitude of the arterial ſyſtem, in the lungs, probably will produce inflammation; the degree of which will vary, as the exciting cauſe, and the conſtitution of the patient determine. I am inclined on reflection to believe, that a deficient performance of the functions of the ſkin, is the principal cauſe of pulmonary conſumption.

This ſuppoſition explains, why the inhabitants of this variable climate, eſpecially thoſe

thoſe of weakly conſtitutions, and malformed cheſts, are ſo peculiarly obnoxious to ſuch complaints. This ſuppoſition alſo ſhews, in what manner, preventing the effects of accidental cold, by flannel garments, or by removal to a warmer climate, is ſo eminently beneficial. The fluids are invited by warmth to the ſurface, and the functions of the ſkin are encouraged. The lungs are relieved from oppreſſion, and left free to the exertion of the reſtorative powers of the conſtitution.

That ſcrofula may be regarded as a cauſe of pulmonary pthiſis, I will readily admit; if it produces diſeaſe of the bronchial glands, the conſequent obſtruction to abſorption from the lungs, will afford a kind of ſtimulus, likely to induce that indolent inflammation, in which, the diſeaſe generally conſiſts. The opinion that ſcrofula is a cauſe of pthisis is confirmed by the appearance of ſome tubercles, which contain a matter reſembling chalk and water, and which is often found in other caſes, to be the product of ſcrofulous inflammation. If the diſeaſe of the lungs is admitted

admitted to be, in many inſtances, of a peculiar nature, it is ſtill probably occaſioned by the ſame exciting cauſe, a diminution of the functions of the ſkin, and a conſequent determination of fluids to the lungs. The debility of the ſanguiferous ſyſtem of veſſels, which is obſervable in ſcrofulous patients, may alſo, for the reaſons before ſtated, render them more liable to this diſorder. But from obſerving the number of patients who die of pthiſis, who have no appearance of ſtruma, I am induced to believe, that this cauſe is leſs frequent than is generally ſuppoſed. At one time I examined the bodies of many people, who died conſumptive: in a ſmall number I found the lungs ſtill capable of admitting a moderate quantity of air; in ſuch caſes, either the cells were larger, appearing as if many cells were laid into one by ulceration, or the air had entered into the cavities of vomicæ, or abceſſes; but in the greater number of caſes, the lungs admitted but of ſlight inflation, inſtead of being of a light and ſpongy texture, and permeable by air, they were made ſolid, being beſtudded by large or ſmall tubercles,

or

or they were to a conſiderable extent thickened, and rendered uniformly denſe. In thoſe caſes, where the lungs remained irregularly hollow, the diſeaſe appears to be the effect of a more active inflammation; ulceration has taken place, abſceſſes have formed, and the patient has ſuffered conſiderably from pain, irritation, and diſcharge.—But the proceſs by which the lungs are made ſolid, ſeems to be a languid, indolent inflammation, unaccompanied by a degree of pain, that demands much attention. It is of the ſame nature with that inflammation, which produces ſarcomatous growths of glandular parts, and induration of the cellular ſubſtance. This indolent inflammation is frequently regarded as proceeding from ſcrophula; that it often exiſts diſtinctly, I am convinced. A ſcrophulous conſtitution may indeed be more obnoxious to languid inflammation; to this, however, it ought not be attributed as a cauſe; ſince its ſeparate exiſtence, proves its independence.

Where the lungs are ſtill capable of admitting

ting air, could the irritation and diſcharge be leſſened, or removed; it is probable, that life might ſtill be ſupported: but if the lungs, by diſeaſe, be made nearly ſolid; their office can be no longer performed, and death muſt be inevitable.

This diſeaſe, which is the effect of chronic inflammation, ſteals inſidiouſly upon the patient, and is often eſtabliſhed, beyond the poſſibility of removal, before its attack is even ſuſpected. Yet I think, by phyſicians, its preſence may be early known, ſince its effects are, to make the lungs leſs capacious. The capacity of the lungs, admits of menſuration: let a perſon inſpire as largely as he is able; let him then make a forcible and complete expiration; and expel the air into a receiving veſſel: thus, the additional quantity of air, that the lungs can receive by the moſt ample inſpiration, is made viſible. A glaſs bottle may be filled and inverted in water, and the patient may expire through a bent tube. Muſcular debility, or ſpaſm, may occaſionally make the reſult of this experiment doubtful,

yet in general, I believe it will afford uſeful information.

In the cure of almoſt every diſeaſe, the removal of the producing cauſe, ſeems to be the primary object. If, as I believe, a deficient performance of the functions of the ſkin, be the principal cauſe of pulmonary conſumption; the excitement of cutaneous perſpiration, requires particular attention. Maintaining an equable warmth, either from climate, or from cloathing, is one way of effecting this purpoſe: giving ſtrength to the vaſcular ſyſtem, is another; and which, in my opinion, has not met with ſufficient attention; at leaſt the principle, on which its utility depends, has not been ſufficiently explained. All parts of the ſkin may be regarded, as the extremities of the body: if the heart and arteries be wanting in power, the circulation, and ſecretion of this remote part, will be languid, and deficient at all times; and will be liable to ſuſpenſion, from trivial cauſes. The maintenance of the ſtrength of the vital powers, appears to me, eſſential to the cure of conſumption.

ſumption. The ſkin cannot perform its office unleſs theſe powers be ſupported; and if this be deficient, pulmonary plethora will be inevitable. The relief which emetics afford in pulmonary complaints, appears to ariſe from the production of cutaneous exhalation, which they occaſion.

But if by promoting cutaneous ſecretion the cauſe of the diſeaſe is removed, ſtill more remains to be effected: the lungs perhaps have already been thickened, or beſet by tubercles; how then, is the diminution of this diſeaſe to attempted. In other parts, we frequently endeavour to accompliſh a ſimilar intention, by means reductive of the patient's ſtrength: by purges, by mercurials, and other medicines: but all means, which debilitate, ſhould be cautiouſly employed. It is however the nature of the diſeaſe, that I have endeavoured to examine; in the practice, I will not preſume to recommend.

I ſhall here preſent the reader with an account of an uncommonly conſtructed heart:

in conſequence of which peculiarity, the circulation of the blood was often irregularly performed; the effects of which, I think tend to illuſtrate the functions of the lungs. As the body is formed with perfect wiſdom, any unuſual conſtruction of a part, is likely to be leſs adapted to the performance of its peculiar functions, and leſs concordant to the actions, and deſign of the whole. Such deviations from uſual ſtructure, do occur in unimportant parts, without eſſential injury; and are frequently obſerved: but in an organ ſo eminently important as the heart, any malformation can hardly be ſuppoſed to exiſt, and life to continue: nor do I know that any caſe ſimilar to that, which I ſhall now relate, is to be found in the records of anatomy. I have annexed to the account, an engraving of the heart, the better to illuſtrate its peculiarities.

Mr. Adams requeſted me to aſſiſt him, in opening the body of a child who died at a little more than two years of age, and of the ſtate of whoſe health, whilſt he lived, Mr. Adams has given me the following account.

" The firſt paroxyſms of irregular reſpi-
" ration, were obſerved about a fortnight after
" birth. They were however neither violent
" in degree, nor long in duration. For ſome
" time after, they were periodically every third
" day, and for the laſt twelve months of the
" child's life uſually once a day. The fol-
" lowing were the ſymptons: the ſkin which
" from his earlieſt infancy had been remarked
" to have a darker tinge than common, be-
" came unuſually blue. A coldneſs which
" was ſo habitual, as to induce his parents
" always to clothe him in flannel, was much
" encreaſed. The child ſeemed to ſuffer much
" uneaſineſs, about his cheſt, and ſhewed a
" wiſh to be laid with his face downward;
" if this were not immediately complied with,
" he contrived to turn himſelf to that poſ-
" ture. He then gradually, and with apparent
" pain and exertion, expelled air from his
" cheſt; remaining without inſpiration for a
" much longer period, than an adult could,
" without ſuffocation. After this, ſuddenly
" inſpiring, he immediately expelled the air as
" at firſt; this exertion was attended with a

" kind of ſcream. In this manner he conti-
" nued reſpiring with immoderately long in-
" tervals, for near a quarter of an hour: after
" which he breathed naturally, but always
" quickly; and thus uſually the paroxyſm
" ſubſided. But ſometimes the ſymptoms
" have been renewed, after a few minutes of
" ordinary reſpiration, and a ſucceſſion of par-
" oxyſms, has continued through the day.

" Though the child was inattentive to ſur-
" rounding objects, from the moment he was
" ſeized, till his recovery, yet this ſeemed
" rather the effect of his ſufferings, than a
" privation of intellect. When long without
" a fit, he was obſerved not to be ſo well ei-
" ther in health, or ſpirits. If the paroxyſm
" was very ſevere, the colour of his ſkin was
" proportionably livid, and after it was over,
" mended in proportion: his lips which at
" all other times were blue, acquiring a reddiſh
" tint which laſted for an hour or two. His
" pulſe was always regular, but extremely feeble
" and

" and quick, was ſometimes ſo obſcure, as to be
" felt with difficulty.

" For the three days preceding the Child's
" death he had no fit, but on the fourth
" morning his reſpiration was again irregular,
" though not exactly in the uſual manner.
" Reſpiration was performed at ſhorter inter-
" vals, and with leſs exertion than was com-
" mon; but the ſkin became pale, and the
" powers by which the circulation of the
" blood is performed, gradually ceaſed to act."

The length of the body, was rather leſs than that of a healthy child of the ſame age; it was ſlender, but very well formed: the muſcles were moderately large and firm: there was no deficiency of fat: neither diſeaſe, nor peculiarity of formation, were obſerved in any other part of the body, except the thorax. The lungs were healthy, but the ſtructure of the heart was uncommon. The right auricle of the heart, being firſt laid open, was found to be much larger than uſual: and the foramen ovale was perfectly open. The cavity

of

of the right ventricle, was next expoſed; it was of uncommon ſize, and its ſides reſembled thoſe of the left ventricle, in bulk, and apparent ſtrength. From this ventricle, a large veſſel proceeded, in the uſual courſe of the pulmonary artery: I laid it open to ſome extent, but it proved to be the aorta. From its origin in the ventricle, it extended in an arched form, towards the left ſide; then paſſed behind the lungs, and purſued its uſual courſe. The aorta was more capacious than is common; it gave off the coronary, carotid and ſubclavian arteries in the uſual manner. The communication between the ventricle and this artery, was large and direct, and it appeared, that it would allow an eaſy paſſage to the blood, on the contraction of the ventricle. The pulmonary artery alſo aroſe from the right ventricle, but the communicating orifice was ſmall, and the artery was one-third leſs than its uſual ſize: its ſides were uncommonly thin, reſembling thoſe of a vein. In the engraving, the aorta is expanded, and inclined to the right ſide: but before its diſplacement, it was turned towards the left, going before the pulmonary artery, and concealing that veſſel.

The

The blood was returned from the lungs, in the uſual manner, by four pulmonary veins, to the left auricle. The dimenſions of the left auricle and ventricle, were ſmaller than common: they were certainly one-third leſs than the correſponding cavities of the right ſide of the heart. No artery proceeded from the left ventricle, but there was an opening in the upper part of the ſeptum ventriculorum, by which the blood could be projected into the aorta. In the engraving, as the aorta is diſplaced, being expanded ,and inclined to the right-ſide, this communication appears more direct than it was in reality.

A heart thus conſtructed, was well calculated for carrying on the fœtal circulation. Since both ventricles could project their blood into the aorta. The pulmonary artery would of courſe receive no more blood, than what in the uninflated ſtate of the lungs, it was capable of tranſmitting. When reſpiration took place, a quantity of blood adequate to its ſize, would be diſtributed by the pulmonary artery.

The

The dimenſions of that veſſel, as well as thoſe, of the left ſide of the heart, ſhew that the pulmonary circulation was at all times, in ſome degree deficient. When the right ventricle contracted, a conſiderable quantity of venal blood muſt have been projected into the aorta; for the paſſage into that veſſel, appeared more patent and direct, than that, which led to the pulmonary artery. From attentive examination of this heart, I am induced to believe, that at each contraction of the ventricles, nearly an equal quantity of venal and arterial blood, was impelled into the aorta, for the ſupply of the body. This occaſioned the body to be, at all times, more cold and livid than uſual, ſince the blood was not ſufficiently oxygenated, to produce the uſual redneſs and heat.

That the ſtructure of the heart, would admit of the circulation being performed, without the continuance of reſpiration, is ſufficiently evident: that during the paroxyſms of irregular reſpiration, the functions of the lungs were much interrupted, the lividneſs and cold-

neſs

neſs of the body ſhew. The pulmonary circulation, though much diminiſhed, was ſtill probably in no inconſiderable degree continued; ſo that we cannot pretend, from this caſe, to determine, what would be the effect of the diſtribution of venal blood throughout the body. It is, however, apparent, that that the deficiency of oxygenation of the blood, did cauſe great lividity and coldneſs of the body; and was attended with a conſiderable diminution of the ſtrength of the vital powers: this was indicated by the debility of the pulſe, and by the degree of languor, which the child always experienced, on its recovery from this peculiar ſtate of impeded reſpiration.

Caſes of a ſimiliar deficiency of pulmonary circulation, from a different cauſe, are related by Doctor Hunter, in the ſixth volume of the Medical Obſervations and Inquiries.

Explanation of the first plate.

AA The right ventricle laid open.

B The aorta expanded, and inclined to the right ſide: before its diſplacement, it went before the pulmonary artery, and concealed that veſſel.

C The pulmonary artery, which appeared one-third leſs than uſual.

D The opening in the ſeptum ventriculorum.

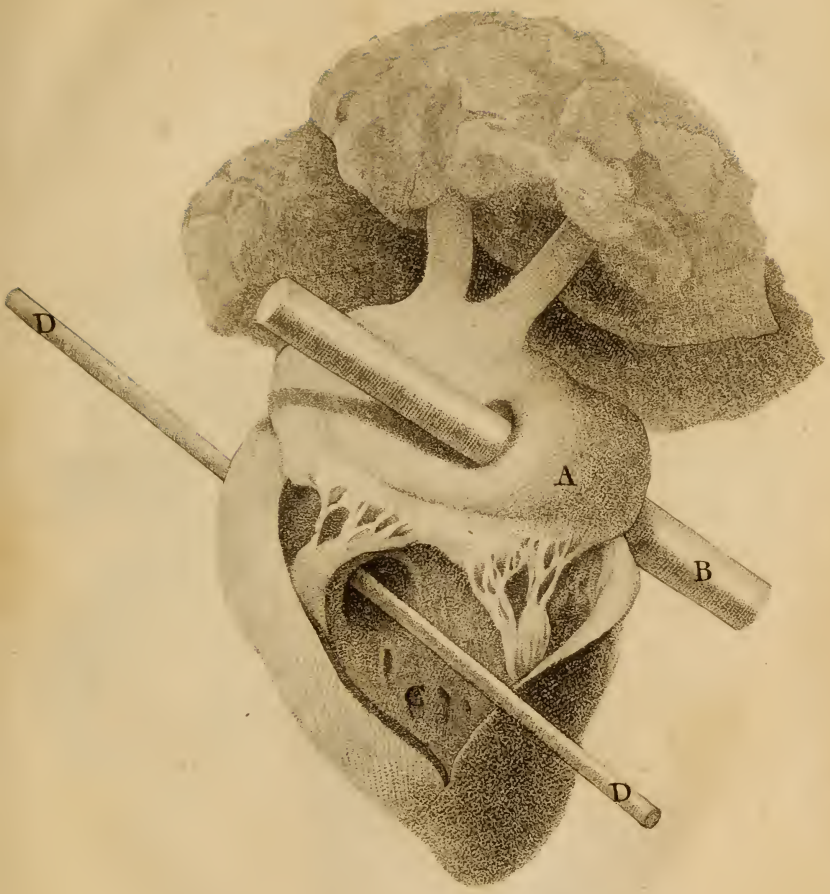
E A probe paſſed through that opening, into the left ventricle.

Explanation of the second plate.

A The left auricle laid open.

B A piece of wood put through the foramen ovale.

C The left ventricle. Both the cavities of the left ſide of the heart, were one-third leſs than the correſponding cavities, of the right ſide.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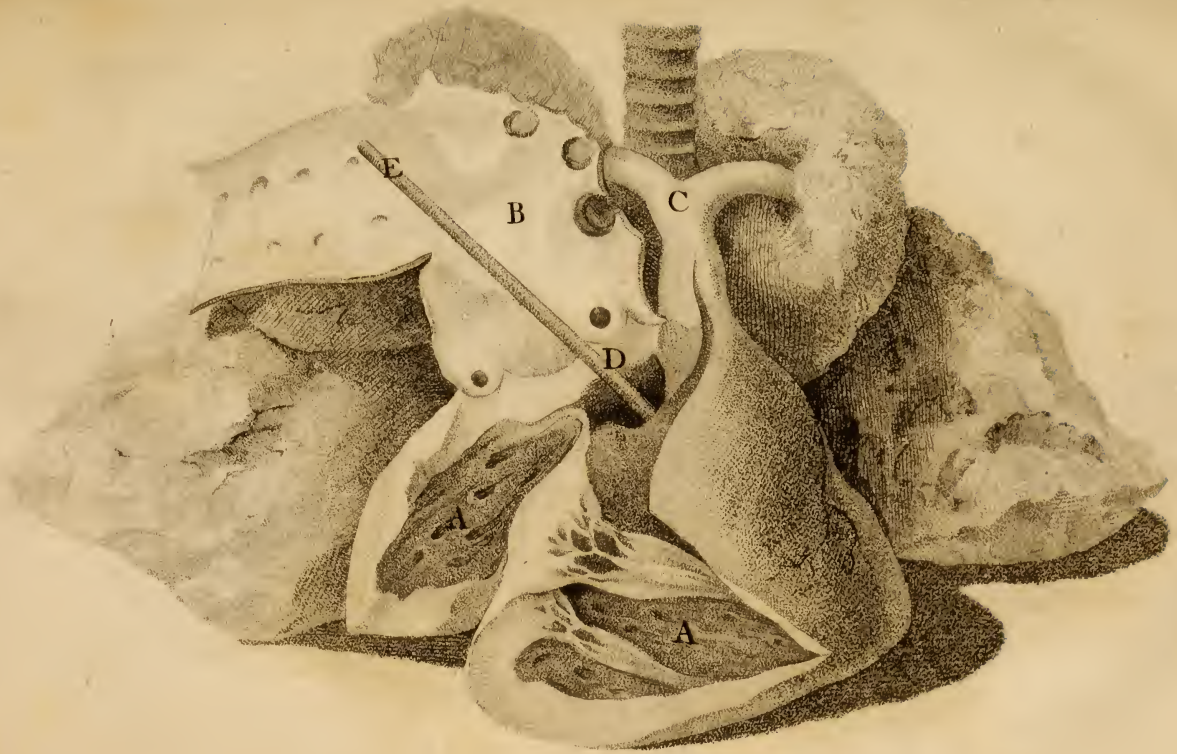
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AN

ESSAY

ON THE

ILL CONSEQUENCES SOMETIMES SUCCEEDING TO VENÆSECTION.

THE public are much indebted to Mr. Hunter, for a judicious account of the appearance, aud effects of the inflammation of the vein, which ſometimes ſucceeds to venæſection. The ill conſequences which occaſionally follow that operation, are numerous and diſſimilar; and they have never I believe been clearly, and collectively ſtated, and explained. The caſes recorded of ſuch complaints, are diſperſed in various periodical publications; and frequently, the nature of the

the diſeaſe, appears to be not underſtood by the perſon, who relates its hiſtory. In proportion as I have ſeen, more varieties of theſe diſeaſes, my own knowledge of them, has become more clear, and ſimple; and as I believe, I can communicate uſeful information, I have ventured to offer to the public, the following obſervations and opinions. I have been alſo incited to this taſk, becauſe the account which Mr. Bell has given of theſe complaints, appears to me confuſed; and the practice recommended, improper. I am hurt to cenſure the works of any author, but this either muſt be done, or injurious error muſt remain uncontradicted.

When from want of attention, or from other cauſes, the wound inflicted in venæſection does not ſpeedily unite; the motions of the arm, occaſion attrition of its ſides, againſt each other, and inflammation of the wounded, or contiguous parts, is likely to enſue. I ſhall give a brief account of theſe different complaints, in the order in which, I believe they moſt frequently happen.

Of

Of inflammation of the integuments, and subjacent cellular substances.

The inflammation, and suppuration of the cellular substance in which the vein lies, is the most frequent occurrence. Of this every surgeon must have seen repeated instances, they may also have remarked, that on the subsidence of this inflammation, the tube of the vein is free from induration: neither does the state of any of the surrounding parts, indicate their previous participation in the disease. The nature of every excited inflammation, will vary as the cause which produced it, and the constitution of the patient determine, it will therefore be unnecessary, to particularly notice the varieties of its appearance. Sometimes the inflammation will be more indolent, and will produce a circumscribed, and slowly suppurating tumour. Sometimes it will be more diffused, partaking more of the nature of erysipelas: and sometimes its violence, and rapid termination, will evidently distinguish it to be a phlegmon.

If

If the lancet with which, the patient was bled, ſhould have been bad, if it lacerated rather than cut the parts, through which it paſſed; if the conſtitution of the patient be irritable, and more particularly, if ſufficient attention be not paid to procure the union of the divided parts, but the motion of the arm be allowed: the irritation, which the friction of the oppoſite edges of the wound muſt occaſion; will moſt probably excite inflammation. The treatment proper to be purſued in this complaint, is manifeſt, and diſtinguiſhed by no peculiarity; I ſhall therefore poſtpone what I have to ſay on that ſubject, until I have noticed the other varieties of theſe diſeaſes.

Of inflammation of the abſorbing veſſels.

The next frequent complaint, which I have ſeen, is inflammation of the abſorbents: it however ſometimes accidentally happens, that one ſurgeon meets with many caſes of a ſimilar nature, ſo that were he to judge merely from his own obſervation; he might conclude, that

that diſeaſe to be common, when the collected experience of others, would determine it to be a rare occurrence. I am inclined to ſuſpect, that my obſervation has been thus partial, ſince Mr. Hunter has not publickly noticed this complaint. I think I cannot give a better hiſtory, of the commencement, appearances, and event of this diſeaſe, than by relating three caſes, of the circumſtances of which, I took an account. It is right however to mention, that I have ſeen two others, of which, I took no minutes; and which, I am unwilling to relate, only from recollection.

CASE.

A lady was bled in the vena mediana baſilica; the wound did not heal, nor was ſufficient attention paid to preſerve the arm quiet. Eight days afterwards, I was conſulted, in conſequence of the patient being alarmed, by the appearance of two ſwellings; one was ſituated about the middle of the arm, over the large veſſels, the other on the forearm, about the mid ſpace between the elbow and wriſt,

in the integuments above the flexor muſcles. The upper ſwelling meaſured rather more in circumference, than an egg, the other, was of ſmaller dimenſions; they were not very painful, they were moderately firm in their texture, and ſo exactly reſembled thoſe tumours, which form round irritated lymphatics; that no doubt could be entertained of their nature. The orifice made by the lancet, was not healed, the integuments for about one-fourth of an inch ſurrounding it, were in a ſlight degree inflamed, and thickened. No induration of the venal tube could be diſtinguiſhed, either at this time, or after the ſubſidence of inflammation.

The account, which I obtained from the patient, of the attack of this complaint; was, that the wound inflamed, became painful, and diſcharged matter; that the gentleman by whom, ſhe was bled, had dreſſed it with ſalve, but did not reſtrain her from uſing her arm; that about five days after the operation, ſhe had felt pains ſhooting from the orifice, in lines,

lines, up and down her arm, and upon preſſing in the courſe of this pain, its degree was increaſed. This account induced me to examine the arm attentively, and I could plainly feel two indurated abſorbents, leading to the ſuperior tumour, but could not perceive any, extending to the lower one. The wounded part, was dreſſed with mild ſalve; a bread and milk poultice was applied to both tumours, and the arm was ſupported by a ſling, without motion, or exertion. The integuments ſurrounding the orifice, loſt their diſpoſition to inflame, and the wound gradually healed; during five days, the tumours underwent no evident alteration; the poultice was changed to one of bread, water, and acetum lythargiritum, cum tinctura opii, under which, they quickly diminiſhed and diſperſed.

CASE.

A man about 35 years of age, was admitted into St. Bartholomew's Hoſpital, under the care of Mr. Pott: he had been bled in the country, about a fortnight before his admiſſion; ſince that time he had been extremely

ill, and was with difficulty conveyed to London. The ſtate in which he was admitted, I ſhall deſcribe: His whole arm was greatly ſwolen, the wound made by the lancet, was not united, the parts immediately ſurrounding it, did not ſeem to be affected, by diſtinct inflammation; but partook of the general tumefaction. Two large abſceſſes had formed, one, ſituated near the inner edge of the biceps muſcle, about the middle of the arm; and the other, on the inſide of the forearm. The patient told us that he had been bled, on account of a pain in his ſide; that the orifice, inſtead of healing had feſtered, that he had for a time, purſued his daily employment, notwithſtanding the pain which he ſuffered; that this, however ſoon became too violent to be endured, the ſwelling, and pain extended towards the armpit, where the glands became enlarged. Inflammation next attacked the forearm, and after ſuffering extreme pain and fever; theſe abſceſſes had formed, and ſince that time, his illneſs and pain had in ſome degree abated. Mr. Pott opened both abſceſſes, and directed his

his whole arm to be covered with a poultice. The patient was kept in bed, and medicines likely to alleviate inflammation, were prescribed. In about four weeks, the arm was reduced nearly to its natural dimensions. The orifice, through which he was bled had united, and the wounds, by which the abscesses had been opened, were nearly healed. The parts surrounding them, however, still remained thickened, and also all the integuments on the inside of the arm. In these thickened integuments, three-chord-like substances, evidently absorbents, were to be distinguished; they extended from the punctured part, to the superior abscess, and again above this, two were continued even to the axilla. Two other indurated absorbents, also were extended from the punctured part, to the inferior abscess. The punctured vein being attentively examined, was found to be a little thickened, both above, and below the orifice; it had however, no connection with these chord-like substances, they were superficial, and their appearance, course, and every other circumstance, clearly shewed

them to be indurated abſorbents. The hardneſs of theſe veſſels, and of the integuments had much diminiſhed, and the patient had regained the ſtrength of his arm, before he was diſcharged from the Hoſpital.

CASE.

A poor man was bled, in one of the bleeding-ſhops of this city. His operator dipped ſome rag in the blood which he had taken, applied it to the orifice, and bound it on the arm, with a tape. The patient felt much pain in the wound, even from the time of the operation, and experienced much difficulty in moving his arm. As the rag ſtuck cloſely to the orifice, he was unwilling to remove it; however, on the third day, the violence of the pain, induced him to take it off: he then found the parts ſurrounding the puncture, inflamed and hardened. The patient had alſo ſuffered much pain, which extended towards the axilla, and one of the glands there, was ſwolen. He anointed the arm with ſome ointment, but the pain

pain ſo increaſed, that he could ſcarcely bear it to touch his ſide. The integuments about the middle of the arm, were elevated by a tumour, which was painful when preſſed, the baſe of it was not circumſcribed, but was gradually loſt in the ſurrounding parts. In this ſituation he requeſted my advice. I gave him ſome mild ſalve to dreſs the wounded part; I directed him, conſtantly to keep applied to the integuments, covering the inflamed lymphatics, ſome cloths wetted with the cold aqua aceti lythargiriti cum opio, to keep his arm compleatly ſupported by a ſling, and to take ſome gently purgative medicine.

This he did, the inflammation gradually ſubſided; and the wound made by the lancet healed.

It might be ſuſpected, that in the caſes which have been related, the lancet which was employed, was envenomed; and that the abſorption of virulent matter was the producing cauſe of inflammation: the deſcent of the diſeaſe, to the inferior abſorbents, in the two firſt caſes, oppoſes that opinion; and it is further invalidated by the obſervations which I ſhall

proceed to offer. Since the ſtructure and functions of the abſorbing veſſels have become ſo well known, the attention of medical practitioners has been directed to their diſeaſes, and much novel information has been acquired. That which relates to the preſent ſubject, I ſhall endeavour briefly to ſtate. Phyſiology ſhews to us, that the abſorbents poſſeſs much ſenſibility: their rejection of one kind of air, which is not evidently acrid, and their ready abſorption of another, in my opinion, proves them to be endued with no ſlight degree of ſenſation. Practical obſervation further ſtrengthens this opinion: the celerity with which theſe veſſels inflame, when they have imbibed noxious matter, and the pain which is ſuffered in conſequence, ſufficiently prove this circumſtance. Their frequent inflammation, in conſequence of diſturbance of the general conſtitution, may be however regarded as an additional argument. A common cold produces a painful tumefaction of their glands; and in ſome fevers, theſe parts are particularly obnoxious to diſeaſe.

There

There is another circumſtance, which deſerves attention; when the abſorbents become inflamed, they quickly communicate this diſeaſe to the cellular ſubſtance, by which they are ſurrounded. Moſt ſurgeons have remarked, theſe veſſels when indurated, to appear like ſmall chords, perhaps of one eighth of an inch in diameter; this ſubſtance is ſurely not the ſlender ſides of the veſſel thus ſuddenly augmented in bulk, but an induration of the ſurrounding cellular ſubſtance, to which the irritated veſſel has communicated inflammation. The formation of a common bubo, is another inſtance of the power, which theſe veſſels poſſeſs, of involving the ſurrounding parts in their diſeaſe; at firſt one or two glands, are found to be inflamed; but they ſoon become undiſtinguiſhable, in the general inflammation of the ſurrounding ſubſtance. This inflammation either is diſperſed, or it terminates in ſuppuration: and on the ſubſidence of the general tumour, the originally diſeaſed glands, again become noticeable. Thoſe frequently enormous tumours, which form by the ſide of the neck, further confirm the obſerva-

ſervation : enquire into the origin, and progreſs of the diſeaſe ; and it will be found that one or two glands were at firſt affected, and that the diſeaſe extended itſelf to the ſurrounding ſubſtance, of which the greateſt part of the ſwelling is compoſed. This remark muſt be taken with ſome limitation, for the glands of the neck, do frequently enlarge to a conſiderable degree, without the ſurrounding cellular ſubſtance, partaking of the diſeaſe ; yet in ſuch caſes, their growth is very gradual, and unaccompanied with active inflammation. Such are the reaſons, which induce me to ſuppoſe, theſe veſſels to be very ſenſible and irritable ; and to poſſeſs a power of readily communicating diſeaſe to the cellular ſubſtance in which they lie.

I now wiſh to ſhew, that their inflammation, in conſequence of local injury, is deducible from two cauſes : one, the abſorption of acrid matter ; and the other, the effect of irritation of the divided tube. Of the inflammation ariſing from the abſorption of morbiferous matter, every one is apprized, but that, which is the effect of irritation, has been leſs remarked.

When

When virulent matter is taken up by the abſorbents, it is generally conveyed to the next abſorbent gland; where, its progreſs being retarded, its ſtimulating properties induce inflammation; and frequently, no evident diſeaſe of the veſſel through which it has paſſed, can be diſtinguiſhed. The abſorption of ſyphillitic, and cancerous matter, affords frequent proofs of this aſſertion. There are, indeed, ſome poiſons ſo acrid, that the veſſel which admits them inflames, throughout its whole extent; yet ſtill the glands are principally affected. When inflammation of the abſorbents, happens in conſequence of irritation, that part of the veſſel, neareſt the irritating, cauſe generally ſuffers moſt: whilſt the glands, being remotely ſituated, partake leſs of the inflammation. The inflammation is alſo of a different kind, and, I think, can be diſcriminated: when it ariſes from poiſon reſident in the part, the gland is firſt indurated, and a phlegmonoid inflammation follows; but if irritation be the cauſe of its enlargement, the tumefaction more ſpeedily takes place,

place, the gland is more painful in its early ſtate, but has leſs tendency to ſuppurate; the enlargement more reſembles that of the lymphatic glands of the neck, which is the conſequence of taking cold.

When the inflammation ariſes from irritation, it will be expected, and I believe it will be found, that the continuity of the veſſel will be apparent: but it does not follow, that the greateſt diſeaſe, will be immediately adjoining that part, which has ſuſtained the injury. The caſes which have been related, ſhew that inflammatory tumours often form in the middle of the arm, and forearm; when the wound of the abſorbent is at the bend of the elbow. Were it neceſſary, I could relate ſeveral caſes, where ſuch tumours formed from injuries done to the fingers, or in conſequence of fretting ulcers of the leg. When they ariſe from the latter cauſe, it might be ſuppoſed, that ſome acrid matter had been imbibed, yet, I think, in that caſe, we ſhould find the glands the principal ſeat of the diſeaſe. It has been proved,

proved, that the abſorbents frequently inflame far below the part where the veſſel has ſuſtained an injury, and where the inflammation could not be occaſioned by abſorption. Theſe obſervations I thought it right to inſert, to illuſtrate the caſes, which have been related; and alſo, to excite more general attention to the diſeaſes of theſe important veſſels.

Of Inflammation of the Vein.

After the account, which Mr. Hunter has given of the inflammation of the vein, (in the Medical and Chirurgical Tranſactions) no additional information from me will be expected, nor is it perhaps required. If the wound of the vein, does not unite, an inflammation of that veſſel will probably follow; which will vary in its degree, in its extent, and in the courſe which it purſues. One degree of inflammation, may occaſion only a ſlight thickening of the venal tube, and an adheſion of its ſides; more violent inflammation may be attended with the formation of more limited, or more

more extenſive abceſſes; the matter of which, may ſometimes mix itſelf with the circulating fluids, and produce dangerous conſequences: or it may be circumſcribed, by the thickening and adheſion of the ſurrounding parts, and then like a common abceſs make its way to the ſurface. When the inflammation of the venal tube is extenſive, it is, indeed, very probable, that much ſympathetic fever will enſue; not merely from the excitement which inflammation uſually produces; but alſo, becauſe irritation will be continued along the membranous lining of the vein to the heart. If, however, the effect of the excited inflammation, has luckily been to produce adheſion of the ſides of the vein, at ſome little diſtance from the wounded part; the inflammation will here ceaſe: its further tranſmiſſion will by the adheſion be prevented. The effect of adheſion of membranes, in preventing the extenſion of inflammation along their ſurfaces; is frequently apparent, and has been well explained by Mr. Hunter on another occaſion. In one caſe, Mr. Hunter applied a compreſs on the inflamed

flamed vein, above the wounded part, and he thought that he ſucceeded in producing adheſion; for the inflammation extended no further. In thoſe caſes, where the inflammation does not continue, equally in both directions, but deſcends along the courſe of the vein, it is probable, that its extenſion in the other direction is prevented, by adheſion.

I have thus briefly and imperfectly tranſcribed Mr. Hunter's opinion, that the preſent Eſſay might not be altogether deficient in information, relative to this ſubject. I have ſeen but three caſes, where an inflammation of the vein ſucceeded to Venæſection; they, however, confirm the foregoing obſervations. The vein did not in either caſe evidently ſuppurate. In the firſt, about three inches of the venal tube inflamed both above and below the orifice; it was accompanied with much tumour, redneſs and pain of the covering integuments, and much fever, the pulſe was rapid, and the tongue furr'd. After the inflammation had terminated, and all tumour had ſubſided, the vein did not ſwell, when compreſſion was made

made above the diſeaſed part. The ſecond caſe was of a ſimilar nature, but leſs in degree. In the third caſe, the inflammation was not continued in the courſe of the vein towards the heart, but extended as low as the wriſt. I have no doubt, but that adheſion of the ſides of the vein, was the cauſe, which prevented the extenſion of the diſeaſe, equally in both directions. The nature of a diſeaſe being known, the treatment is commonly evident. The diminution of inflammation in a vein, is to be attempted by the ſame general means as in other parts. As the membranous lining of the vein is continued to the heart; and as inflammation is very ſpeedily extended along ſuch ſurfaces, unleſs prevented by adheſion: the application of a compreſs at ſome diſtance from the punctured part, in order to unite the inflamed ſides of the vein, appears to be particularly judicious.

I am induced to ſuppoſe, a caſe may occur in which the vein may ſuppurate, and in which, a total diviſion of the tube may be proper

proper practice; not merely to obviate the extenſion of the local diſeaſe, but to prevent the mixture of collected pus with the circulating fluids.

Inflammation of the Faſcia of the Forearm.

As far as my obſervation hath extended, the next frequent ill conſequence, which ſucceeds to Venæſection, performed in the arm; is an inflammation of the ſubjacent Faſcia. When this complaint occurs, it perhaps ariſes not merely from the contiguity of the faſcia to the punctured and irritated parts, but it is probable, that it was wounded by the lancet in the operation. I hope that the caſes, which I ſhall relate, and thoſe, to which, I can refer the reader, will convey ſufficient information of the ſymptoms, and effects of this diſeaſe.

CASE.

A man, aged 40, was admitted into St. Bartholomew's Hoſpital, under the care of Mr. Pott: he had much pain and difficulty

of moving, his arm, in consequence of inflammation succeeding to phlebotomy. The wound inflicted in that operation, was not healed; the surrounding integuments were not much inflamed, but he could neither extend his forearm, nor his fingers without great pain. The integuments of the forearm were affected with a kind of erysipelas; when slightly touched, they were not very painful, but when more forcibly compressed, so as to affect the inferior parts, much pain was suffered. The patient complained of pain, extending towards the axilla, and also towards the acromion; but no tumour of the arm, in either direction, was perceptible. A poultice was applied to the arm, opium was given at night, and aperient medicines were occasionally prescribed. The pain in the arm increased, and it was attended by much fever. After a week had elapsed, a small and superficial collection of matter took place, a little below the internal condyle; this being opened, but little pus was discharged: and scarcely any decrease of tumour or pain followed. About ten days afterwards,

terwards, a fluctuation of matter was diſtinguiſhed below the external condyle; an inciciſion was here alſo made, which penetrated the faſcia of the forearm. Much matter immediately guſhed from the wound, the ſwelling greatly ſubſided, and the future ſufferings of the patient, were comparatively, of little conſequence. This opening, was however, inadequate to the complete diſcharge of the matter; which, had probably been originally formed beneath the faſcia, in the courſe of the ulna: its pointing at the upper part of the arm, depended on the tenuity, and comparative non-reſiſtance of the faſcia, at that part. The collected pus deſcended to the lower part of the detached faſcia, a dependent opening for its diſcharge became neceſſary, after which, the patient recovered, without any circumſtance being obſerved, worthy relation. The caſe which I have juſt related, and that, in which, two large abſceſſes had formed, attended with indurated abſorbents; occurred nearly at the ſame time, at the Hoſpital: and they both fell under the care of Mr. Pott. In the lec-

tures of that eminent ſurgeon, I had heard, dangerous and fatal conſequences attributed to the injury of a nerve, in Venæſection, but I learned no other diſtinction of caſes. Theſe caſes, firſt excited my attention to this ſubject; and as far as I know, ſuch diſcrimination as that, which I now offer to the public, hath not been attempted.

I ſaw one other caſe of inflamed faſcia, but I neglected to take notes of the ſymptoms; I therefore can only ſay, that at the time, they appeared ſo clearly to characterize it, that I entertained no doubt of its nature. No inflammation of the vein or abſorbents appeared, the integuments were not much affected; but the patient complained, that his arm felt as if bound, or compreſſed; and that he ſuffered much pain if he attempted to extend it. The inflammation ſubſided without the formation of matter; and after much time had elapſed, the pliability of the arm was gradually regained. I the leſs regret my deficiency of experience, on this ſubject, as I can refer the reader

to

to the ſecond volume of the Medical Communications; he will there, meet with two caſes, which I believe he will acknowledge to be inflammations of the faſcia: attended however, with ſome peculiarity of ſymptoms.

The firſt caſe is related, by Mr. Colby, of Torrington in Devonſhire; the other, by Mr. Watſon. The inflammation of the faſcia, in the latter caſe, was followed by a permanent contraction of the forearm. From this caſe, I think we have acquired uſeful knowledge: ſhould a ſimilar contraction of the forearm, from a tenſe ſtate of the faſcia, in future occur, it ſeems reaſonable to ſuppoſe, that it may be completely relieved, by detaching the faſcia, from the tendon of the biceps; to which, it is naturally connected. This, I conclude, was the cauſe of the perfect reſtoration of free motion, in the caſe firſt related by Mr. Watſon. On this ſubject I will not enlarge, but ſubmit the opinion to the judgment of the reader.

The treatment of an inflamed faſcia, the conſequence of Venæſection, has in it, no peculiarity. Doubtleſs, thoſe general means which are reductive of inflammation, ſhould be employed. Of local treatment, quietude of the limb, and a ſtate of relaxation of the inflamed part, will tend to leſſen diſeaſe; but as ſoon as ſome abatement of inflammation is procured, the extenſion of the forearm and fingers, ought to be attempted, and daily performed: to obviate that contraction, which might otherwiſe enſue.

Of the ill Conſequences, ſucceeding to a wounded Nerve.

In order to compleat in ſome degree, this Eſſay, I have attempted to diſcuſs the preſent ſubject; tho' I acknowledge, I have no practical information to communicate. I believe, theſe accidents, to be of rare occurrence, ſince thoſe of my medical friends, to whom I applied for information, had never ſeen a caſe, the ſymptoms which they could deciſively, pronounce to ariſe merely from an injured nerve. Mr. Pott, in his lectures, uſed to ſay, that he had

had ſeen two caſes, in which, the patients had ſuffered diſtracting pain, which was followed by convulſions, and other ſymptoms, which could only be aſcribed to nervous irritation. He attributed theſe effects, to a partial diviſion of the nerve, and recommended its total diviſion, as a probable remedy. Doctor Monro, I am informed, relates ſimilar caſes, in which, ſuch treatment has proved ſucceſsful. I rely on the diſcrimination of theſe eminent men; yet I feel convinced, that the greater number of ſurgeons, have been deficient in diſtinguiſhing theſe diſeaſes. A wounded nerve, acting as a cauſe, muſt always produce ſpecific, and characteriſtic ſymptoms and effects. I need not inſiſt on the neceſſity of diſcrimination, in theſe complaints: thoſe, who have deſcribed the ſymptoms, reſulting from an injured nerve, have repreſented them, as at all times imminently hazardous, and frequently fatal. An operation is here demanded; from it, we have reaſon to expect immediate mitigation of the patient's ſufferings; and his future perfect reſtoration. Yet this operation, in any other

of the complaints, before treated of, would be unneceſſary, and perhaps detrimental.

I ſhall arrange, what I have to ſay on this ſubject, in the following manner: Firſt, I ſhall explain, what nerves are expoſed to injury. Secondly, I ſhall inveſtigate, what are the effects likely to be produced by ſuch an accident; and thirdly, I ſhall enquire, what means, are moſt likely to afford relief.

Firſt, the two cutaneous nerves are thoſe, which are expoſed to injury, I diſſected them, in ſeveral ſubjects, with attention; and found ſome irregularity in their diſtribution: moſt frequently, all their branches paſs beneath the veins, at the bend of the arm; but ſometimes, altho' the principal rami ſtill go beneath theſe veſſels, many ſmall filaments are detached before them, which it is impoſſible to avoid wounding, in phlebotomy. As I believe, many ſurgeons retain but an indiſtinct remembrance of theſe nerves, and as I have never ſeen them accurately depicted, in any anatomical book; I thought I ſhould do an

accepta-

acceptable ſervice, by giving an engraving of them. I therefore, made two drawings of them, one, exhibiting their moſt ſimple courſe; the other, their moſt complicated diſtribution. Theſe, I conclude, are the only nerves liable to injury: it may be ſuſpected, that the median nerve might occaſionally be wounded; but its ſituation, I think, makes this opinion improbable. If, however, a doubt ſhould be entertained on this ſubject; an attention to ſymptoms, will ſoon diſpel it; when a nerve is irritated, at any part, between its origin and termination; a ſenſation is felt, as if ſome injury were done to the parts, which it ſupplies. If therefore, the cutaneous nerves were injured, the integuments of the forearm would ſeem to ſuffer pain; but if the median nerve was wounded, the thumb, and two next fingers would be painfully affected.

By referring to the plate, it will be ſeen, that if the patient be bled in the vena mediana baſilica; the branches of the internal cutaneous nerve, are expoſed to injury: or, if the vena mediana cephalica be opened, the branches of the external cutaneous nerves, may be wounded.

Secondly,

Secondly, I wiſh to enquire, what are the ills, likely to ariſe from a wounded nerve.—Whoever reflects on the wonderful minuteneſs of the nervous fibrils, and conſiders their perfect diſtinctneſs, from each other, although connected by a common covering of cellular ſubſtance; will ſcarcely imagine, a partial diviſion of a nervous fibril. If I ſought, to expreſs myſelf ſtrictly on this ſubject, I ſhould ſpeak of a partial diviſion of a pacquet of nerves. But I ſhall uſe the commonly adopted language, and call thoſe chords, nerves; which are really compoſed of multitudes of ſeparate nerves. I firſt beg leave to examine the opinion, which has prevailed, of a nerve being partially divided. Admitting that a nerve be partially divided, would it not, like a tendon, or any other ſubſtance unite? I think, there can be no doubt, but that it would: I am induced to this opinion, by conſidering, that nerves of equal ſize, with the cutaneous nerves of the arm, are diſtributed in conſiderable numbers, throughout the body. In the many operations performed, and in the wounds daily

daily occurring, I think it would be ſtrange, if a partial diviſion of a nerve, ſhould not happen; yet, no peculiar ſymptoms, are obſerved uſually to enſue. The pain which ſome people ſuffer from bleeding, in my opinion, indicates an injury done to a nerve. If the reader refers to the plate, he will perceive, that in ſome caſes, it is impoſſible to avoid dividing branches of nerves, in phlebotomy: as ſometimes they paſs before the vein. I believe theſe to be branches ſo frequently wounded, that I ſhould be ſurpriſed, if they did not, many times, ſuffer a partial diviſion. Surely, however, a half divided nerve would unite without cauſing a general derangement of the nervous ſyſtem. Yet it is poſſible, that an inflammation of the nerve may accidentally enſue; which, would be aggravated, if it were kept tenſe, in conſequence of imperfect diviſion. In the caſes, related by Mr. Pott, and Doctor Monro, I believe, that ſome days elapſed after the receipt of the injury; before any alarming derangement of the nervous ſyſtem, enſued. Inflammation of the ſurrounding parts, alſo appeared. Theſe obſervations, make it to me evident, that the diſeaſe conſiſts

ſiſts in inflammation of the injured nerve, in common with the other wounded parts: and this inflammation, I can conceive to happen, with or without, a total diviſion of the nervous chord. I ſhould conſider, a caſe of inflamed nerve, as an object of great curioſity; every one, I think, will admit, that it is likely to communicate dreadful irritation to the ſenſorium: and every one will perceive, that a cure will probably ariſe from intercepting its communication, with that important part.

Thirdly, I proceed to enquire, what is the moſt probable method of relieving the effects, ariſing from an inflamed nerve. The general opinion is, that the nerve is only partially divided; and that, a total diviſion would free the patient from the continuance of his ſufferings. Mr. Pott ſuppoſed, that the wounded nerve, was ſituated at one or the other extremity, of the wound, which had been made in the vein: he therefore propoſed, to divide it totally, by enlarging a little the original orifice. It is however poſſible, that the point of the lancet might

might injure a nerve, lying beneath the vein. This will be eaſily underſtood by referring to the plate. Mr. Bell directs, an extenſive tranſverſe inciſion, to be made through the original wound: but if the injured nerve, be ſituated, at the upper, or lower extremity of the orifice, it will remain unaffected by this operation. Mr. Bell alſo adviſes the inciſion to be continued to the bone; but this appears to me, dangerous and unneceſſary.

If the injured nerve be inflamed, I think it doubtful, whether, even a total diviſion of it, at the inflamed part, would effectually relieve the general nervous irritation, which the diſaſe has occaſioned. To intercept the communication of the inflamed nerve, with the ſenſorium, does however promiſe perfect relief. This intention, can only be accompliſhed, by making a tranſverſe inciſion above the orifice in the vein. The inciſion, need not be very extenſive, for the injured nerve, muſt lie within the limits of the original orifice; and it need only deſcend as low as the faſcia of the

forearm; for all the filaments of the cutaneous nerves, lie above this faſcia. The vein which had been opened, and ſome filaments of the cutaneous nerves are all the parts of conſequence, which will be divided in this operation. The proximity of the diviſion of the nerve, to the vein, muſt be regulated, by the ſuppoſed extent of the diſeaſe. However, as the extent of the inflammation of the nerve, is uncertain, I ſubmit it, to the conſideration of ſurgeons; whether, it may not be adviſeable, in ſome caſes, to divide either of the cutaneous nerves, ſtill more remotely from the injured nerve.

I find little difficulty, in detecting the trunk of theſe nerves, in the dead ſubject; and I ſhould ſuppoſe, but little would occur in the living ſtate, for the compreſſion of the tourniquet, would prevent any obſcurity, which hæmorrhage might cauſe.

Explana-


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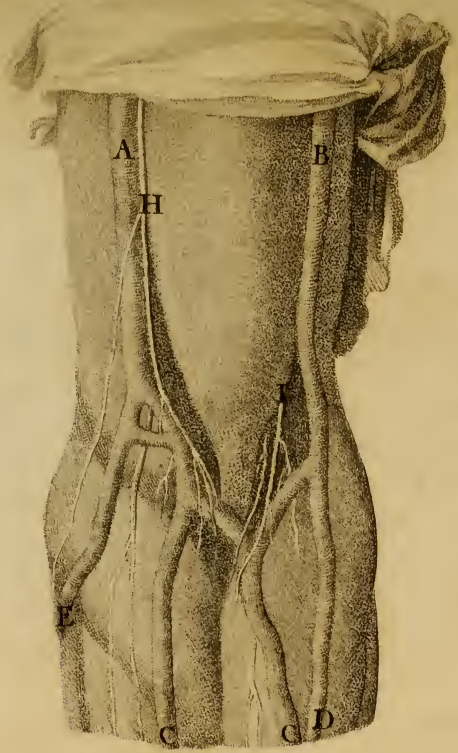
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Explanation of the third Plate.

A Vena basilica.
B Vena cephalica.
C Vena mediana.
D Vena radialis.
E Vena cubitalis.
F Vena mediana basilica.
G Vena mediana cephalica.
H Nervus cutaneus internus.
I Nervus cutaneus externus.

General Observations on the ill Consequences, sometimes succeeding to Venæsection.

I think it very probable, that these diseases would less frequently happen, did not the situation of the veins, usually opened, contribute to their occurrence. The common offices of life, so constantly demand the employment of the arm, that its motion becomes almost inevitable. Unless, the orifice made by the lancet, has been attentively closed; the effect of this motion, will be, to separate the edges of the wound from each other, and to prevent their union by the first intention. Some slight degree of inflammation will ensue; the continuance of motion of the arm, causes a friction of the inflamed surfaces against each other; and thus, the disease is increased. Under these circumstances, if the constitution of the patient be irritable, the inflammation will extend itself; altho' it may still be confined to the cellular substance, and integuments: or perhaps, it may be transmitted to that part, which has sustained most injury

injury in the operation. The vein, the abſorbents, the faſcia, or the nerve may in that caſe, ſuffer peculiar derangement. Although the injury done by a bad lancet, may contribute to the production of diſeaſe, yet I think it probable, that a patient improperly bled, would ſuſtain no injury, if the treatment of the wound was judicious. Whilſt another, on whom, the operation had been dexterouſly, and well performed, would be liable to theſe ill conſequences; if the proper attention to unite the wound, was neglected.

In the account given of theſe diſeaſes, they have been repreſented, as they occurred, ſeparately; doubtleſs, in ſome caſes, they may be combined.

The principal curative indications, appear to be, to mitigate the inflammation about the orifice, and to preſerve the arm, ſupported in a motionleſs ſtate. I need not enlarge this account, by deſcribing the modes of appeaſing inflammation, and irritation; as they are well known, to every ſurgeon.

I cannot conclude theſe Eſſays, without offering to the public, an apology for their defects: whoever, by diligent attention, has acquired a competent knowledge of his profeſſion; if he continues thoſe exertions, by which, he has attained his preſent information; will extend his purſuits, beyond the former boundaries of knowledge. New modes of treatment, which promiſe ſucceſs, he will be induced to practice: whatever appears doubtful, or obſcure, he will endeavour to aſcertain, and illuſtrate. His labour will be profitable to himſelf, for he will acquire the information, which he has ſought. A perſon engaged in the purſuit of uſeful ſcience, ought not here to ſtop, but ſhould communicate the information, which he has obtained. If indolence, or diffidence of ſucceſs, or a wiſh to attain perfection, induce him to poſtpone the communication of his newly acquired knowledge; his attention will probably be attracted by new objects of inveſtigation: the remembrance of his former engagements, will fade in his mind, and the effects of his exertions,

exertions, will be leſs profitable to himſelf, and of little ſervice to others.

Such reflections, have induced me to offer, that, which I knew to be deficient; rather than to ſuppreſs information, which I believed might be, in ſome degree, uſeful. The imperfections, and defects of theſe Eſſays, will, I truſt, be forgiven; if it be found, that they have added any thing, to the general ſtock of knowledge: and I ſhall feel my ambition gratified, in having contributed my mite, to the advancement of the eminently uſeful art of healing.