Surgical and physiological essays. [Pt. I] / by John Abernethy.

SURGICAL

AND

PHYSIOLOGICAL

ESSAYS,

BY

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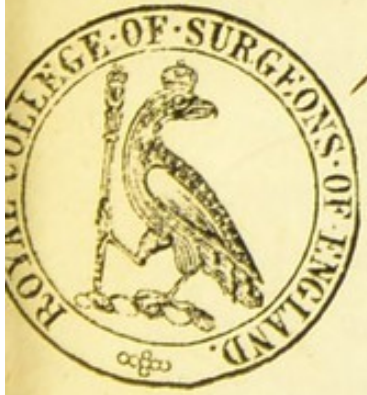

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DEDICATION.

TO

MR. CHARLES BLICKE,

SURGEON OF ST. BARTHOLOMEW'S HOSPITAL.

SIR,

TO your inſtructions I am indebted for a conſiderable portion of that ſurgical knowledge which I poſſeſs: to your friendly exertions, I in a great meaſure, owe the ſituation of life in which I am placed. I ſhould think myſelf equally ungrateful, and unjuſt, not to acknowledge theſe obligations, when an opportunity is afforded me by the preſentation of theſe writings to your notice. I forbear to attempt any eulogium either on the good qualities of your mind, or on your profeſſional talents; for to your friends the account were ſuperfluous, to yourſelf it might not

not be pleaſing, whilſt by thoſe who know you not, it might be regarded merely as the accuſtomed language of dedication.

My chief deſign in this brief addreſs therefore is, publicly to avow my obligations to you, and publicly to teſtify *my great reſpect* for your abilities as a ſurgeon, and *my great eſteem* for your character as a man.

I am, SIR,

Your obliged and obedient

friend and ſervant,

JOHN ABERNETHY.

PRE-

PREFACE.

I SHOULD think myſelf deficient in the reſpect due to the public opinion, were I to permit theſe Eſſays to appear, without offering an apology for their imperfections, and defects. The reader will perceive the reaſons which led me to wiſh the ſpeedy publication of the firſt Eſſay; they have induced me to print it, at a time when my other occupations, prevented me from paying the requiſite attention to correctneſs. I was prompted to undertake the experiments contained in the ſecond Eſſay, becauſe it appears neceſſary to introduce a courſe of anatomical lectures, with a philoſophical account of the nature of the matter, which compoſes an animal body.

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I deſign to ſubmit two other papers to the public conſideration; but I ſhall defer their publication until the ſummer, when I ſhall have more leiſure to perfect them.

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the practitioner of the neceſſity of attending to theſe minutiæ, and alſo to lay before the public the information which I have derived from my preſent degree of experience in theſe diſeaſes.

Whilſt the condenſed cellular ſubſtance which forms the cyſt of an abſceſs remains entire, it continues free from inflammation, and the contained pus ſuffers no putrefaction nor evident alteration of quality. Some lumbar Abſceſſes contain two quarts or more of matter. The ſurface of the containing cyſt muſt in ſuch caſes be very extenſive: whenever the abſceſs is opened either by ulceration, or by the hand of the ſurgeon, a ſudden and generally conſiderable inflammation extends itſelf over the whole cyſt; this is followed by a copious diſcharge of frequently fætid pus. Now this immediate inflammation and conſequent diſcharge cannot but greatly derange and exhauſt the conſtitution of the patient, which is generally irritable, and already much enfeebled by the efforts attending the formation of the diſeaſe.—It is well known that when we evacuate fluids from the cavities of the body, if we immediately cloſe the

aperture through which they were diſcharged we prevent the inflammation which would otherwiſe enſue. The evacuation of water from the abdomen, and Tunica vaginalis Teſtis may be adduced as inſtances of the truth of this remark. It is alſo well known, that if the matter of an abſceſs be diſcharged, its cavity becomes much diminiſhed by the contraction of its cyſt. It will hereafter be ſhewn that this contraction will be greater in chronic lumbar abſceſſes than in thoſe of a more phlegmonoid nature, ſince in the former the cyſt having ſuſtained leſs inflammation and undergone leſs alteration of ſtructure, will be more likely to poſſeſs and exert its natural elaſticity, and thus greatly diminiſh the cavity of the abſceſs.

On theſe two obſervations the practice hereafter related is founded; it occurred to me that if after the evacuation of a lumbar Abſceſs, the aperture were directly cloſed and its immediate union procured, that no inflammation of the cyſt would follow, which being now relieved from preſſure would by its contraction and rarefaction greatly diminiſh the cavity:

the pus, doubtleſs, would ſpeedily reaccumulate, yet I thought by repeatedly evacuating this fluid before diſtention of the cyſt could happen, the cavity would be ſo much reduced, and the cyſt be made ſo much leſs extenſive, that the future admiſſion of air would be productive, comparatively, but of little conſequence. Such were the ſentiments excited in my mind by ſome of the following caſes, and ſuch were the motives which induced me to purſue the practice hereafter related.—I ſhall firſt give an account of the caſes as they occurred, and afterwards offer ſome general obſervations on theſe diſeaſes. To the account of each caſe I will annex thoſe remarks which it ſuggeſted, and which I am unwilling to poſtpone to the concluſion, as they would leſs forcibly ſtrike the mind of the reader if the circumſtances which gave riſe to them were held but imperfectly in remembrance.

CASE.

A young man, about twenty-ſeven years of age, of a muſcular form, and healthy conſtitution, came from the country to the

hoſpital, to obtain relief from a collection of matter which preſented itſelf in the upper and fore-part of the thigh, beneath the Faſcia, and immediately below Poupart's Ligament. The pain which he had previouſly ſuffered in his loins, and the impulſe of matter into the tumour upon his coughing, left little doubt of the original ſeat of the diſeaſe. The Faſcia of the thigh had yielded conſiderably to the collected pus, ſo that it did not deſcend ſo low as is common, but appeared very prominent. Although he had endured conſiderable pain, he had not ſuffered much from fever on the firſt formation of the abſceſs.

A cauſtic was applied on the tumour to give diſcharge to the matter, and three days afterwards the eſchar was divided.—Eight ounces of very perfectly formed, moderately conſiſtent, and inodorous pus iſſued from the inciſion.—The ſides of the eſchar now cloſed up the wound and prevented any further evacuation of matter. This the ſurgeon did not attempt to produce, thinking the delay would be uſeful.

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For three days no more fluid was evacuated, during which time the young man remained perfectly well, and his thigh free from inflammation.—On the fourth day the eſchar became ſo much looſened in its circumference that part of it gave way, and eight ounces more of ſimilar and perfectly inodorous pus was diſcharged. In twelve hours after this detachment of the eſchar, he ſuffered much from fever and pain in the part, and the diſcharge became putrid. In two days the fever, which was of the hectic kind, ſeemed to be eſtabliſhed, and from the ſore there flowed a copious and increaſing quantity of fœtid pus. His ſkin was now hot, his face fluſhed, he ſweated profuſely in the night, his appetite failed him, his pulſe beat 120 in a minute, his tongue was but little altered from its natural appearance, he had no ſleep, and was diſtreſſingly reſtleſs.—Theſe ſymptoms continued about a week without ceſſation, they then appeared ſlightly to remit, and proceeded for three weeks in the ſame manner, with ſome little diminution in their ſeverity; his ſtrength was now greatly exhauſted, the diſcharge from the abſceſs very profuſe, and in this ſtate it

was thought right to have him conveyed into the country, where I am informed he gradually declined, and in about ſix weeks more he died.

This caſe firſt ſuggeſted to me the propriety of cloſing the opening after the diſcharge of the pus; for until the continuity of the cyſt of the abſceſs was deſtroyed by the ſeparation of the eſchar, the part was uninflamed and the ſtate of the circulation unaltered. The ſudden inflammation and fever which followed the ſeparation of the eſchar, in my opinion, prove the ingreſs of air into the cavities of abſceſſes to be injurious. Whether this be admitted or denied, it is however ſufficiently evident from this caſe that a large opening and much irritation of the cyſt of ſuch an abſceſs, is productive of the moſt detrimental conſequences to the conſtitution of the patient.

That mode of treatment which cauſes leaſt irritation I believe has ever been found the moſt ſucceſsful. But whenever a permanent opening has been left in a large abſceſs,

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generally conſiderable and frequently fatal, irritation hath enſued.

It will, however, be evident in the following caſes that the reduction of the abſceſs to an inconſiderable extent has been accompliſhed, without the occurrence of any local inflammation, or general fever.

CASE.

July 1790. John Tucker was admitted into St. Bartholomew's Hoſpital on account of a Pſoas Abſceſs. His health had been declining for more than three years. He had for a conſiderable time been an out-patient under the care of Dr. Auſtin, who had unavailingly endeavoured to prevent the formation of this abſceſs by iſſues made in his back, and by the adminiſtration of various medicines. He had ſuffered greatly from pain in his loins and fever: the abſceſs was very large and had deſcended very low on the inſide of the thigh, the integuments covering it were natural, the impulſe of matter into the tumour upon coughing very conſiderable.

His pulſe was feeble and beat eighty-ſix in a minute, previous illneſs had exhauſted his conſtitution; he had a conſtant cough, and undoubtedly much diſeaſed lungs.—He had little appetite and was of a coſtive habit—he was of fair complexion, light hair, and blue eyes, and his countenance frequently fluſhed:—He was on all theſe accounts as unfit a ſubject, as can well be ſuppoſed, to encounter the derangement of conſtitution, which muſt ſucceed to the ordinary evacuation of the abſceſs.

On Wedneſday the 28th of July, I tapped the abſceſs with a ſmall hydrocele trochar and diſcharged three pints of pus of good quality, although in a ſmall degree more fluid than common. I dreſſed the part with conſiderable caution. I moiſtened the lint which I applied to the orifice with tinctura benzoës compoſita, over this I applied ſome ſticking plaiſter, which was retained by croſs ſlips, and afterwards varniſhed over with gum; ſome compreſſes of linen were applied over the abſceſs, and gently bound on by a flannel roller.

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On Thurſday, there was no very perceptible difference in his health—he had ſlept and eat as uſual, his tongue was moiſt and natural, his pulſe a few ſtrokes quicker.

On Friday, he ſaid, that he found his loins relieved by the evacuation, that he could perceive no difference in his health, and his pulſe was the ſame as before the operation. For many days his health remained unchanged, he became he thought a little weaker, and the frequency of his pulſe had encreaſed about four ſtrokes in a minute. For this little alteration we could readily account, knowing that ſome fluids were drained from the circulation into the cavity of the abſceſs, and that ſome little exertion of the ſyſtem would neceſſarily enſue.—The abſceſs remained without pain, or inflammation, and his conſtitution free from fever; his ſkin continued in its natural ſtate, his appetite was good, his ſleep ſound, and his countenance unaltered. Three days after the operation I removed the dreſſings from the punctured part, it appeared healed, I however carefully renewed the dreſſings every third day.

Friday,

Friday, the 13th of Auguſt, ſixteen days after the firſt diſcharge, the tumour having become prominent, I again punctured it and evacuated its contents. I knew the diſcharge would encreaſe his weakneſs, yet, if the collection were ſuffered to remain it would ſhortly diſtend the cyſt to its former dimenſions, and my original plan of treatment would be fruſtrated.

The quantity of the diſcharged fluid was nine ounces; in appearance and chemical properties it much reſembled blood. This bloody effuſion was probably the conſequence of laxity of the exhaling veſſels, as there had not been the leaſt expreſſion of inflammation in the abſceſs. Before I diſcharged the matter the ſecond time he complained of ſome pain in his loins; but the following day he ſaid he was much relieved, and found himſelf remarkably well. This ſecond puncture was dreſſed like the former and quickly healed.

During the time which had elapſed between the firſt and ſecond diſcharge, he had not been confined even to the ward, but often went

went from the hoſpital to ſee his friends. This his cough, the weak ſtate of his health, his diſinclination to live in the hoſpital, and the obvious impunity with which it was done, induced me to permit. After the ſecond evacuation he altogether lived with his friends, promiſing to come every week to let me ſee the ſtate of his complaint; however, the ſecond week when the matter ought the third time to have been evacuated, he failed in his promiſe. I was now obliged to leave London for ſome time, ſo that I did not ſee the patient again until September the 8th, which was four weeks and five days from the former evacuation; he had refuſed to have the matter let out during my abſence.. I now diſcharged in like manner ten ounces of lymphatic exhalation, rather dark coloured and turbid, as if mixed with true pus. The man, during the laſt week, had complained of pain in his loins and in his knee, both of which were relieved as uſual by the operation.

Before the abſceſs was firſt opened the impulſe of matter from the loins, on coughing,

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was extremely forcible, but was now not at all perceptible. It appears to me that a very conſiderable advantage is derived from this mode of treating theſe complaints. Whatever ſecretion is made in the abſceſs of the loins, will, by its gravity, deſcend into the ſpace left by the ſeceded faſcia of the thigh. The abſceſs of the loins being left perfectly free from diſtention will moſt probably contract to very little dimenſions, if it be not perfectly aboliſhed. Hence in the ſubſequent treatment of theſe complaints you have only to attend to the diſunited faſcia; whilſt the cavity in the loins ſcarcely deſerves notice.

September 22d, a fortnight after the former evacuation, I diſcharged four ounces of ſimilar ſerous fluid mixed with pus. During its evacuation, which was very ſpeedy, I had applied my fingers beneath Poupart's ligament, as if to obſtruct the deſcent of any matter from the loins. I then deſired the man to cough, but no matter deſcended, and the collection appeared to me entirely confined to the thigh.

I found

I found ſome difficulty in introducing a trochar, when the abſceſs contained ſo little fluid. This was remedied by firſt introducing a lancet through the faſcia, and then conveying the trochar through the aperture made by the lancet.

Thus after diſcharging the matter four times, the complaint was reduced from a lumbar abſceſs, containing three pints, to a ſmall collection of matter beneath the faſcia, containing four ounces.—What communication this had with the loins, and what was the ſtate of parts there, cannot be determined. To appearance there was no collection. If I had now immediately opened the abſceſs, the containing cyſt being ſmall, the inflammation probably would not have been conſiderable. But the ſtate of the man's health induced me for a ſhort time to defer this final attempt, this radical cure, as I may expreſs it, and be contented with only evacuating the matter when collected, without ſuffering the collection to increaſe the ſize of the cyſt. It might be expected, by repeating the evacuation, that the cavity would diminiſh to

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its total abolition. This would probably happen were the abſceſs in the cellular ſubſtance; but the inelaſtic faſcia cannot contract, and the ſubjacent muſcles cannot be elevated, ſo that the effuſed matter, though very ſmall in quantity, would ſtill preſerve them diſunited.

I had let out four ounces of matter once in October, and on the 5th of November I opened the abſceſs by an inciſion about an inch and a half in length at the lower part. I introduced my finger beneath the faſcia as high as Poupart's ligament, I deſired the patient to cough, but no matter deſcended from the loins, neither could I aſcertain any communication. The extent of the detached faſcia was about four inches and a half in length, and nearly four in breadth. The cyſt inflamed after opening. The hardneſs and quantity of the diſcharge encreaſed for four days and then gradually ſubſided. His thigh was ſtiff and ſore, ſo that he could not eaſily move it, but he had no particular pain in his loins—his pulſe did not vary—his tongue was not furred—his ſleep was not interrupted—nor could any derangement of his health be perceived. Gra-

Granulations grew from the edge of the inciſion, and the opening nearly cloſed and afforded ſcarcely any diſcharge.—Yet, on introducing a probe through the orifice, I found that the faſcia remained diſunited. With a view to produce an union, by exciting inflammation, I introduced a ſeton from this lower orifice to the upper part of the cyſt. The faſcia again inflamed, indurated, and united, only the track of the ſeton was uncloſed; and this by the injection of ſome ſpirit and water, was alſo ſoon induced to fill up. In diſcourſing with the patient, after opening the abſceſs, reſpecting his health, he ſaid, he was ten times better than before it was opened; that until this time he had always been ſubject to fits of pain, and to a ſtate of weakneſs and faintneſs which he could not deſcribe.

After the perfect cloſure of the abſceſs, he could extend and bend his thigh with freedom and eaſe; he could alſo readily put his foot upon a chair ſet before him. This it would have been impoſſible for him to accompliſh during the formation or continuance of the abſceſs. This freedom of action in the pſoas

muſcle indicated conſiderable ſoundneſs of it, and of the contiguous parts. He ſtill, however, complained of much rheumatic pain in his hips and ſometimes in his loins; and as I ſuppoſed his conſtitution might be affected by the ſuppreſſion of a long-continued purulent diſcharge, and might attempt for its relief the formation of a new abſceſs, I inſerted two ſetons in the integuments of the loins, with a view of preventing inflammation of the internal parts.

They did not, however, relieve his pains; he complained much of their inconvenience, and as he deſigned to go into the country, they were diſcontinued. I ſaw him about a year afterwards—no alteration had taken place in the thigh, nor no fixed pain had attacked the loins, but he was ſtill much teized with unſettled rheumatic pains.

The preceding caſe was very unfavourable both from the patient's conſtitution and from the degree of the diſeaſe. Yet, by four times diſcharging the matter, which was not attended with much more pain than bleeding,

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it was reduced from a lumbar abſceſs, containing three pints, to a ſmall collection beneath the faſcia of the thigh, containing four ounces, and without any evident communication with the loins. Each time, inſtead of ſuffering inconvenience, he experienced relief; he had no fever, neither was he reſtrained from his uſual occupations.

The final opening might have been ſooner made, but as this was the firſt caſe in which I had purſued this practice, I was uncertain of the event and irreſolutely protracted it for two months, hoping the amendment of his health. When it was opened no perceptible fever followed, and it ſhortly got well by the treatment which I have related.

CASE.

Iſaac Dean, thirty-ſeven years of age, had come from Hampſhire to London, to obtain advice for a Pſoas Abſceſs. He was admitted into the hoſpital under the care of the late Mr. Pitts. The account which he gave of himſelf was, that his buſineſs had obliged him to be much on horſeback; that he

had formerly, when riding, bruiſed his left teſtis, which accident had occaſioned an incurable diſeaſe of that gland; he therefore had ſuffered its removal about two years ſince in ſome county hoſpital. Since that time he had frequently ſuffered much pain in his loins; about eight months before his admiſſion into the hoſpital he had caught cold: the pain in his loins then became more violent and conſtant, and much impeded the motions of his left thigh. About three months after this attack of ſevere pain, he perceived a tumour in the upper part of his thigh, which had gradually encreaſed until the time of his admiſſion into the hoſpital. Since the appearance of the tumour, the pain in the loins had much abated. The matter now deſcended about four inches beneath Poupart's ligament; and it received a forcible impulſe when the man coughed. The faſcia, containing the deſcended pus, was very prominent, and the ſkin covering it was more red than the reſt of the integuments.

The patient's health was not unfavourable; his pulſe was rather ſtrong, beating ſeventy-

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ſix in a minute, his tongue rather pale, his hair and eyes dark.

Monday, 3d of October, 1790, by Mr. Pitt's deſire I introduced a trochar into the lower part of the tumour, and gave diſcharge to twenty-four ounces of pus, moderately tenacious, and containing ſome flakes of firmer matter: I cautiouſly cloſed the orifice, as in the former caſe, applied a compreſs, and bound it moderately tight with a roller.

I could not in this caſe perceive any alteration in the man's health deſerving to be recorded, except that the pulſe was a little quickened: he eat and ſlept as uſual.

I carefully took off the ſticking-plaiſter at the end of three days, and renewed a ſimilar dreſſing. On Thurſday, 13th of October, the abſceſs was now again prominent, and the puncture made by the trochar ſeemed ſlightly inflamed. As I concluded the diſtention of the faſcia cauſed this inflammation, and ſuppoſing that if the preſſure of the matter from beneath was ſuffered to continue, it might

occaſion it to ulcerate, I determined to prevent this effect by again evacuating the matter. This I accompliſhed by paſſing a trochar into the lower part of the abſceſs, at ſome diſtance from the former opening; and by this means diſcharged between eight and nine ounces of pus, thinner and rather darker coloured than the former, but not tinged with blood as in the preceding caſe. I now carefully dreſſed both orifices, and again applied a bandage.

I cautiouſly removed the dreſſings, at the end of three days; the ſecond puncture had healed, and the firſt had loſt its diſpoſition to inflame. After having dreſſed the punctured parts, and applied the bandage; I deſired him to moiſten it with aq. ſaturn. which I thought by keeping the ſkin cool, would prevent its diſpoſition to inflame. The man ſuffered no alteration in his health from this ſecond evacuation. On the 25th, at the end of a fortnight, the tumour being again prominent, I introduced a lancet into the faſcia, and by the orifice thus made the troachar, and diſcharged ſix ouches of turbid ſerous fluid, and pur-

ſued the ſame ſubſequent mode of treatment.

After another fortnight had elapſed the tumour was much leſs prominent than before, and there appeared a degree of irritation in the ſkin. The punctures ſhewed a diſpoſition to inflame. I now deſired the man to cough, but could diſcover no impulſe of matter from the loins. This I had not before done, leaſt the exertion ſhould affect the punctures, which were not ſo firmly healed as in the former caſe. As the patient had not ſuffered much from diſcharge, as his health ſeemed fully capable of ſuſtaining the effects ariſing from opening the abſceſs, as it was not probable that its dimenſions could ſuffer further diminution by delay, on Friday, the 23d of November, I opened the cavity by an inciſion of abont an inch in length, at the lower part, and immediately paſſed a ſeton through to the upper part, with a view to inſure the union of the faſcia.

An uſual degree of inflammation of the faſcia and ſtiffneſs of the affected limb fol-

lowed, but he complained of no particular pain in his loins further than general ſtiffneſs. The ſlight fever which accompanied ſeemed rather inflammatory than hectical, his pulſe became a little quicker and harder, and his tongue ſlightly furred. Theſe ſymptoms gradually abated, and at the expiration of three weeks the faſcia appeared to have adhered firmly to the ſubjacent parts: I therefore withdrew the ſeton.

As he now found his health tolerably good, and being, as he thought, recovered from what he conſidered as a dangerous complaint, and imagining that he was made weaker by ſtaying in the hoſpital, he went into the country, promiſing to inform me if any change happened; but I have not ſince heard of him. Two caſes of ſuch remarkable ſucceſs, eſtabliſhed, in my opinion, the excellence of this mode of treatment; my expectations of future ſucceſs were ſanguine; and although they have not been completely fulfilled, yet every ſucceeding caſe has tended to demonſtrate the utility of the method.

CASE.

CASE.

July, 1791. A poor woman, fifty-three years of age, had in the beginning of March, a ſevere pain in her loins, which gradually abated on the appearance of a tumour on her back; this continued to enlarge until her admiſſion into the hoſpital, which was in July. A ſmall tumour was alſo perceived beneath Poupart's ligament. When the patient coughed, matter was forcibly impelled into both ſwellings; ſo that there was little doubt that there a lumbar abſceſs; the matter of which had made its way through the muſcles of the back and formed the principal tumour beneath the integuments.

Friday, July 18, Mr. Earle punctured the abſceſs in the back with a lancet, carrying it for ſome diſtance obliquely, between the ſkin and the cyſt of the abſceſs; ſo that the orifice of the integuments did not correſpond with the orifice of the cyſt: thus a kind of valve was made, which it was ſuppoſed would prevent the ingreſs of air into the abſceſs. Seven ounces of good pus was diſcharged, and the

the puĉture was attentively dreſſed. The following day the abſceſs appeared as large as before. This confirmed the opinion that it communicated with ſome internal collection. On Monday it was leſs diſtended; but on Tueſday, being very full, and fearing leaſt the preſſure of the contained matter ſhould cauſe an ulceration of the former orifice, I punctured the abſceſs as before, and diſcharged five ounces of thick pus; ſuch as could not be ſuppoſed to have been formed in ſo ſhort a ſpace of time. She was not in the leaſt diſturbed in her health, and both orifices healed immediately without trouble.

The following week, on Tueſday the 2d of Auguſt, I again punctured the tumour and diſcharged between four and five ounces of pus. And again, on the ſucceeding Tueſday, I diſcharged in like manner three ounces of matter. On the Tueſday afterwards, as there did not appear any reaccumulation, I made an inciſion into the tumour of about one inch in length. About two ounces of fluid iſſued from the inciſion; the cavity of the abſceſs I found larger than I expected, but I could not perceive

perceive any aperture by which it communicated with the loins. On Wedneſday the patient appeared well; her tongue was moiſt, her pulſe a little more than ſeventy in a minute, and had a common degree of fullneſs and of ſtrength.

On Thurſday ſhe was in the ſame ſtate with reſpect to her pulſe and appearance; but ſhe was in very low ſpirits. She wept much, and ſaid her back was painful, but did not otherwiſe complain. On Friday, buſineſs prevented me from going to the hoſpital, ſo that I did not ſee her. The pain in her back and the depreſſion of her ſpirits had much encreaſed; her hands were cold, and her pulſe was much quickened: in this ſtate Mr. Harvey ſaw her, and directed her wine and other cordials; but ſhe ſoon died.

I opened the body, and it appeared, that the abſceſs had been originally formed on the poſterior ſurface of the pſoas muſcle. This had now contracted into a kind of tubular cavity, the diameter of which was about one-ſixth of an inch; and extended from nearly

oppoſite

oppoſite to the laſt dorſal vertebra to beneath Poupart's ligament, at which place the abſceſs had preſented itſelf. From the upper part of this cavity a paſſage was ſeen on the inner edge of the quadratus lumborum muſcle, by which the matter had eſcaped from the loins through the muſcles of the back, and had afterwards elevated the integuments, cauſing the tumour formerly deſcribed. I could not obſerve any appearance of inflammation on the inſide of this cavity; indeed, as only two days had intervened between the opening of the abſceſs and the patient's death, the time ſeems inſufficient for the eſtabliſhment of a diſeaſe of this nature.

Almoſt every ſurgeon has met with caſes in which the nervous ſyſtem has been ſo circumſtanced, as to be incapable of ſuſtaining the ſhock of an operation, or of attempting to remedy a diſeaſe; and in ſuch caſes the patient has ſuddenly periſhed without any evident cauſe. In the preſent caſe there was nothing indicating a peculiarity of the nervous ſyſtem; the pulſe was ſeventy, and in other reſpects equally natural: ſhe was rather

ther a weakly woman, but ſhe eat and ſlept as uſual; every thing ſucceeded till the final opening of the abſceſs: there was every reaſon to ſuppoſe it was greatly reduced in ſize, and diſſection confirmed the opinion. I know not that the event of this caſe could poſſibly be foreſeen. One inference only can I draw from it, which is, that whenever any debility of conſtitution is perceived, one ought as long as poſſible to delay the final opening, or until that debility be removed.

CASE.

February, 1791. James Leaver is in the 21ſt year of his age, has light brown hair, blue eyes, dilated pupils, pale countenance, frequently fluſhed, and is apparently of an irritable conſtitution. About nine months ago he was affected with a pain in his loins when he moved, which ſoon became very ſevere, even when he was at reſt. This pain was accompanied with fever. Four months afterwards he perceived a ſmall ſwelling in the upper part of his right thigh, which has ſince gradually encreaſed, and has now deſcended

ſcended nearly to the middle of the thigh: he remarked, that he never had the leaſt pain in the part where the tumour was formed. After the appearance of this ſwelling, he no longer experienced the ſame degree of uneaſineſs in his loins; and ſhortly after, he acquired the power of lifting up his right thigh, which he had for ſome time loſt.

For four months previouſly to his admiſſion into the hoſpital, he had regularly profuſe night ſweats, which began about twelve o'clock, but did not prevent his ſleeping; when he awoke he found his cloaths very wet, and himſelf very chilly; he had, however, an appetite for his breakfaſt.

On the 5th of February, Mr. Earle introduced a trochar into the moſt prominent part of the tumour: between two and three pints of healthy matter was evacuated, the wound was immediately cloſed, and lint and adheſive plaiſter were applied. The night ſucceeding the operation he ſlept little, but was free from perſpiration. On each ſucceeding night he ſlept as uſual, but had not in the leaſt degree

thoſe ſweats which had been conſtant until the diſcharge of the matter.

On the 8th of February, he ſaid he found himſelf no worſe for the operation, he was free from night ſweats and ſlept ſoundly. His appetite was perfectly good, his bowels unaffected, and his tongue moiſt and florid. His pulſe, before the operation, was ninety, and for fifteen days afterwards it varied between that and a hundred. February 15th, ten days after the evacuation, his night ſweats returned, although in a leſs degree than formerly.

February 26th, three weeks after the firſt diſcharge, the tumour had now become nearly of its original ſize; the integuments were much diſtended; the part punctured by the trochar had for three days appeared inflamed; and on the tumour being now compreſſed, the cicatrix gave way, and the contained matter oozed from the orifice. The trochar was again introduced through the former orifice, and eight ounces of browniſh matter diſcharged. The wound was carefully

fully dreſſed, in hopes that as the diſtention was taken off, it might cloſe. After the ſecond evacuation, the night ſweats were again diſcontinued; he ſaid, he was rather weaker, but no other alteration in his health was perceived.

On the 2d of March, while in the act of coughing, the imperfectly healed wound made by the trochar, gave way. Very little pus was diſcharged, but as it was impoſſible to heal this ulcerated opening, and as the continuity of the cyſt was now deſtroyed, the mode of treatment hitherto purſued was fruſtrated. Much inflammation of the cyſt immediately took place, and the conſtitution became greatly affected. The next day, if the finger ſlightly compreſſed the abſceſs, it gave him great pain; but before the cavity of the abſceſs became expoſed, the part was perfectly indolent. When preſſure was employed, a fœtid, frothy matter iſſued from the ulcerated orifice. The cyſt, however, was emptied, and, except when preſſed, there was no diſcharge. Such were the appearances of the part. The general diſturbance

of

of the conſtitution was alſo very great; his countenance exhibited ſtrong expreſſions of alarm; if any one approached him he ſtarted, and when any one touched him he trembled. His pulſe beat from 130 to 140 in a minute—for two days his bowels were diſordered—however, the inflammation of the cyſt gradually abated, and in like manner the conſtitutional derangement ſubſided. At the end of about eight days, he was much amended, and in about ſix weeks the abſceſs appeared nearly well, and his conſtitution relieved from febrile indiſpoſition.

In this caſe it is clear, that the ſecond diſcharge of matter was too long delayed, and to me it appears equally evident that the patient derived much advantage from the mode of treatment which had been purſued; for by it the complaint was reduced from a large abſceſs, containing nearly three pints, to one which held leſs than eight ounces. Yet, even in this diminiſhed ſtate, great derangement of the conſtitution followed the expoſure of the cavity of the abſceſs: indeed, I have little doubt, if the abſceſs had been

opened whilſt it retained its original dimenſions, but that the patient would have fallen a victim to the more extenſive inflammation, and more violent fever, which would then have taken place.

CASE.

Elizabeth Ridley, aged fifty-five, had for one year and a half before her admiſſion into the hoſpital, ſuffered much from bad health, ſhe then had a ſevere cough, accompanied with much fever. About ten months ago, ſhe had a very acute pain in her loins, which abated, in ſome degree, ten weeks after its firſt attack; at that time ſhe obſerved a tumour in her groin, which had gradually encreaſed in ſize. The pain had been continued, though at intervals it ſuffered conſiderable abatement: the veins on the fore part of the thigh had become varicous and the leg œdematous. The tumour was of a circular form, about four inches in diameter.—It had much protruded the faſcia, and matter was violently impelled into it on coughing. She now complained of occaſional pain of her

her ſtomach, of failure of appetite, and a coſtive ſtate of her bowels; her pulſe was ſlow and feeble, her tongue pale, and her health conſiderably beneath the natural ſtandard.

On the 8th of November, I punctured the lower part of the tumour with a lancet, carrying it obliquely about half an inch between the ſkin and the faſcia, and diſcharged eleven ounces of good pus, but did not empty the abſceſs. The orifice of the ſkin and cyſt did not then correſpond, and on coughing there was ſtill perceived a conſiderable impulſe of matter from the cavity in the loins.—I was unwilling to irritate the cyſt by the introduction of any inſtrument to ſeparate the lips of the wound, therefore I cloſed the orifice with ſticking plaiſter, and every thing remained quiet till the third day, when, by a fit of coughing, the orifice was burſt open and mattet oozed from beneath the plaiſter. If I ſuffered it to remain open, my original plan of treatment would be fruſtrated. I therefore reſolved to let out the collected matter, leaſt diſtention of the faſcia and integuments

ſhould prevent the wound from healing. I again introduced the lancet through the ſame orifice, and wounded it ſo as to make it bleed and gave a diſcharge to five ounces of pus; the abſceſs though, did not even now appear to be completely emptied. I preferred to introduce the lancet through the ſame orifice rather than make another opening, that this new injury might excite in the divided parts a new diſpoſition to unite. If I had not again made the ſeparated parts bleed, they probably would have united by granulations; their ſurfaces would have been for ſome time kept ſeparate by a purulent ſecretion, and air would have been admitted into the cavity of the abſceſs: but the effuſed blood glued together theſe edges, and thus obſtructed the aperture till its organization made the reunion perfect.

The woman ſuffered no evident alteration in her health, but became much eaſier with reſpect to her loins. The varicoſe veins and the oedema of the leg now no longer appeared. Theſe ſymptoms, doubtleſs, originated from the preſſure of her loins, occaſioned

ſioned by matter, of which it was very evident there was a large collection.

On the 18th, the tumour was again punctured and eight ounces of fluid evacuated. The matter before had been incompletely diſcharged; now I believe the tumour was entirely emptied. This laſt diſcharged matter was perfectly inodorous and the thigh uninflamed. I made this aperture at the ſide of the tumour with the edges of the lancet held upwards and downwards, and not tranſverſely as the former openings had been made. This I did that the efforts employed in coughing might have leſs effect in impelling the matter through the orifice, which ſoon healed.

In the following week ſhe complained that ſhe was reſtleſs and could not ſleep, neither had ſhe her uſual degree of appetite; her pulſe, however, was not quickened, nor did any other ſigns of conſtitutional indiſpoſition appear. No matter was now collected beneath the faſcia, and after waiting another week without any apparent collection being made, on the 25th of November I intro-

duced a lancet through the faſcia of the thigh, with a deſign to admit the air into the cavity of the abſceſs that remained. I did not perceive any matter iſſue from the opening. As the integuments covering the faſcia were thickened and ſhewed ſome diſpoſition to inflame, I directed the aqua aceti lithargyrita to be applied to them. On the following day ſome matter flowed through the orifice. The patient ſuppoſed, if collected it might be a table ſpoonful; nearly the ſame quantity continued to diſcharge for about a fortnight, and afterwards it gradually diminiſhed, and the wound healed. She was not affected by fever in conſequence of this laſt opening, and ſeemed to ſuffer very little inconvenience with reſpect to her health. She, however, complained much of pains reſembling thoſe of the rheumatiſm, which affected principally her hips, though ſometimes they attacked her loins; for theſe pains ſhe was placed under the care of the phyſician, and as her conſtitution was languid, ſhe was recommended to continue the medicines preſcribed for her as an out-patient.

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In this caſe one circumſtance appeared to me curious; after I had twice diſcharged the contents of the abſceſs, no further collection of matter took place. Yet not becauſe the cavity of the abſceſs was aboliſhed, but becauſe from ſome little indiſpoſition of the conſtitution the ſecretion into that cavity was for a time ſuſpended. This, however, was rather an advantageous circumſtance, for as the cyſt was without repletion, the contraction of the ſides was unoppoſed.

There were alſo three other patients in the hoſpital, from whom the matter had been twice diſcharged, and the dimenſions of the abſceſs conſiderably diminiſhed; yet when the abſceſſes were punctured a third time, the wounds either ulcerated, or did not unite; ſo that the complete reduction of the abſceſs was prevented. As I had not theſe caſes under my own management, and as the plan of treatment was not perfectly executed, I ſhall not engroſs the reader's time by the relation of circumſtances which do not, in my opinion, contain any uſeful information. The account which I have given, compriſes my preſent experience in theſe complaints: in

every new undertaking unexpected circumſtances will occur, which will often baffle and ſometimes defeat its intention. The difficulty of uniting the wounded parts has, in ſome caſes, fruſtrated the intended treatment. Particular attention is required to prevent the rupture or ulceration of the punctured parts. In one caſe the final opening ought to have been longer delayed: when theſe circumſtances ſhall be attended to in future, there is, I think, reaſon to believe that our ſucceſs will be more complete.

I ſhall now offer ſome general remarks on the lumbar abſceſs, not, however, deſiring to be ſyſtematic, or to comprehend every particular; but to notice principally thoſe circumſtances, which have a reference to the practice purſued in the foregoing caſes. The remarks which I deſign to offer will be arranged under the following heads:

1ſt. I will deſcribe the appearances of theſe abſceſſes, as I have obſerved them in diſſection, with a view to eſtabliſh my opinion of their nature.

2dly.

2dly. I will endeavour to explain the difference between theſe and plegmonoid abſceſſes.

3dly. The treatment appropriated to their different ſtages will be conſidered. The reaſon why theſe abſceſſes ſo rarely diſperſe, and ſo frequently enlarge to ſuch extenſive dimenſions will be inveſtigated.

4thly. The cauſe of the local and conſtitutional derangement which ſucceeds to the ordinary evacuation of the abſceſs will be enquired into; and it will be attempted to be demonſtrated, that the lumbar abſceſs is dangerous only from its magnitude.

5thly. It will be ſhewn, that the propoſed mode of treatment reduces its dimenſions to a inconſiderable extent, and proportionably detracts from the hazard of the patient.

And laſtly, the principal circumſtances conducing to render the treatment ſucceſsful, will be ſtated.

It has been, I think, too prevalent an idea,

that the pſoas abſceſs frequently is connected with and often cauſes a caries of the bodies of the vertebræ. This opinion requires examination. I have formerly ſeen many bodies opened in which theſe abſceſſes havebeen found. I will deſcribe the appearances of their formation in the loins, and their progreſs to other parts of the body, from my own obſervation.

The cellular ſubſtance, interpoſed between the peritoneum and the loins, is the common ſeat of theſe abſceſſes; this ſubſtance is in greater quantity at the ſides, where it connects that membrane to the pſoas, and quadratus lumborum muſcles, than in the middle when it attaches it to the ſpine. Where this ſubſtance is moſt abundant there moſt frequently are abſceſſes formed; and this probably is the reaſon why we generally find theſe ſuppurations limited to either ſide of the vertebræ, and ſeldom extending acroſs them. If matter was formed in the middle, oppoſite to the bodies of the vertebræ, its gravity and the want of reſiſtance would determine its deſcent to either ſide. As the peritoneum would readily yield to the protuſion of matter collected

collected behind it; as the cellular ſubſtance connecting it to the vertebræ would be eaſily ſeparated; ſo the preſſure which the collected pus would make againſt thoſe bones would be quite inſufficient for the production of diſeaſe. The matter of ſuch an abſceſs is alſo perfectly mild, and could not ſtimulate except by its mechanical properties. I therefore think it improbable that a caries of the bones ſhould be the conſequence of an ordinary lumbar abſceſs.

Sometimes the abſceſs is formed in the cellular ſubſtance behind the pſoas muſcle, and then perhaps it moſt frequently makes a paſſage towards the integuments of the back. This happened in the fourth caſe which I have related.

But the moſt frequent ſituation of the abſceſs is before or by the ſide of the pſoas muſcles, from whence the fluid collected ſometimes extends itſelf laterally, and making its way between the three ſtrata of abdominal muſcles preſents itſelf beneath the integuments of the abdomen. Theſe abſceſſes are gene-

rally regarded as leſs dangerous; perhaps the complicated courſe which they purſue prevents the ready propagation of inflammation to the original cavity after they have ruptured or are opened.

But the gravity of the matter, and the yielding ſtate of the cellular ſubſtance generally determine it to deſcend with the pſoas muſcle beneath Poupart's ligament, in which ſituation it elevates the faſcia of the thigh. This is the moſt common progreſs and preſentation of the lumbar abſceſs.

Some caſes are on record where the matter has deſcended in the cavity of the pelvis, and through the upper part of the obturator foramen, or through the great ſacro iſchiatic foramen, or has protruded the integuments by the ſide of the rectum. Such is the original ſituation, and ſuch the various preſentations of the matter of a lumbar abſceſs, which I believe very rarely cauſes any diſeaſe of the adjacent bones.

That pus may be collected in the loins, in con-

conſequence of carious vertebræ, that it may accumulate, extend, and preſent itſelf like a lumbar obſceſs, is, I believe well known; but here the diſeaſe is complicated. When ſuch an abſceſs burſts, the patient's life is in imminent danger; his conſtitution is now obliged to encounter all the irritation attending an inflammation of a very extenſive cyſt, and to ſupport a copious, and conſtant purulent diſcharge, at a time when it has been enfeebled by a prior and very conſiderable diſeaſe. I have, however, ſeen patients eſcape the immediate hazard ariſing from the rupture of ſuch an abſceſs, I have ſeen them with openings in the thigh diſcharging matter, when the vertebræ have been carious, and the limbs in conſequence palſied.

To theſe caſes the practice which I have propoſed ſeems peculiarly adapted. The accumulated matter would probably make a ſpontaneous outlet, and the deſtruction of the patient would be the almoſt inevitable conſequence. But the temporary evacuation of matter which I have recommended, obviates diſtention and prevents this effect. Whilſt

the

the art of the ſurgeon is employed in remedying the cauſe which produced the matter; in detracting from the diſeaſe of the corpora vertebrarum by external derivation by iſſues made in the back, of the utility of which means we are aſſured by long continued experience.

I believe I cannot give a more uſeful or intelligible account of theſe diſeaſes than by tracing them through their various ſtages, and by remarking the ſymptoms, and treatment appropriate to each. Theſe complaints preſent us with a ſpecimen of languid chronic inflammation, terminating in an abſceſs. Sometimes this is effected with little pain, and though the patients uſually complain much, during the continuance of the inflammation, yet their ſufferings are by no means ſo ſevere, as thoſe, which attend on a phlegmon. Both the rapid and violent inflammation, which is called phlegmon, and this more languid and chronic inflammation may terminate in ſuppuration, and produce an abſceſs; but the circumſtances of the ſurrounding parts are very different, and to theſe I wiſh for a ſhort time to direct the attention of the reader.

In

In phlegmonoid tumours the violent inflammation which cauſes ſuppuration in the center gradually abating in the circumference, occaſions there, adheſion and thickening of the cellular ſubſtance; ſo that there are two cauſes of the confinement of pus in a plegmonoid aſceſs, viz. condenſation of the ſurrounding cellular ſubſtance, which is the conſequence of the preſſure of the collected matter; and a thickened unnatural ſtate of that ſubſtance which the effect of the degree of inflammation, that it has endured. When, therefore, a phlegmonoid abſceſs is opened, although, as is well known, the ſides will greatly approximate to each other, and the cavity will be much diminiſhed; yet, the contraction will be leſs complete on account of the diſeaſed ſtate of the ſides of the abſceſs. But if the abſceſs be the conſequence of chronic inflammation, the ſurrounding partake very little of the diſeaſe; theſe, therefore, are but little altered from their natural ſtate, and the collected pus is confined almoſt ſolely, by the condenſation of the ſurrounding ſubſtance into a cyſt. It therefore appears that the recommended practice is particularly adapted to theſe caſes;

caſes; as when the evacuation of the matter diminiſhes the condenſing cauſe, the ſurrounding cellular ſubſtance is likely, in ſome degree, to regain its original rarity and looſeneſs of texture, and thus greatly to diminiſh the cavity of the abſceſs.

In abſceſſes, formed in conſequence of acute inflammation, as the ſurrounding parts partake of the affection, thoſe which intervene between the matter and the ſurface of the body, ſoon inflame and ulcerate, and thus the collected pus quickly obtains a ſpontaneous outlet. But in the chronic abſceſs the ſurrounding parts have little diſpoſition to inflame, and the approach of matter to the ſurface is therefore much more tardy. When it deſcends, as uſual, beneath the faſcia of the thigh, there is an additional cauſe, why the ſkin is not readily affected; the matter confined beneath the reſiſting faſcia cannot irritate it by its preſſure.

I will relate the caſe of a woman who was late in the hoſpital, as it well illuſtrates all the circumſtances of the formation and progreſs of theſe chronic abſceſſes. An accummulation of

of twelve ounces of well-formed pus happened beneath the integuments covering the upper part of the pectoral muſcle; it had elevated the ſkin, and had formed a globular kind of tumour. This ſuppuration had been attended with ſcarcely any pain, and the integuments, although diſtended, were indolent, and appeared perfectly healthy and natural. I punctured the abſceſs with a lancet conveyed obliquely between the integuments and the cyſt, evacuated the contained pus, and cloſed the aperture with ſticking plaiſter: but on the re-accumulation of matter it was no longer confined in a cyſt, but had diffuſed itſelf through the cellular ſubſtance leading to the axilla, in which inflammation was produced. I was, therefore, obliged to make a new orifice, and leave it open, that the ſecreted matter might have an outlet, and not extend diſeaſe, by thus pervading the cellular ſubſtance.

The lumbar abſceſſes generally occur in unhealthy ſubjects, but they ſometimes are found in people who poſſeſs conſiderable ſtrength. Any thing which can induce inflammation in the cellular ſubſtance may give riſe to them. Their origin is often to be

traced to accidental colds, to ſtrains, and to febrile diſeaſes inductive of inflammation. Doubtleſs the inflammation is ſometimes more acute, ſometimes more languid, as the producing cauſe, and the conſtitution of the patient determine. If matter be once formed its preſence produces an increaſe of diſeaſe, it preſſes on and gently irritates the ſurrounding parts in a manner likely to perpetuate this chronic inflammation. In conſequence of ſuch irritation more matter is depoſited; and the greater the accumulation of pus the greater is the ſtimulus to the ſurrounding parts, and thus the diſeaſe is augmented.

Pain in the loins is the common attendant on fever, and on weakneſs, when no inflammation is preſent. This circumſtance, perhaps, makes us too little attentive to ſuch complaints, from our patients. An inflammation may at a certain period have begun, it may now be checked, and many a lumbar abſceſs might probably be prevented by timely attention. The means by which this purpoſe is to be accompliſhed, either operate on the conſtitution, or on the affected part. The

former

former comprehends ſuch evacuations from the circulating ſyſtem as the ſtrength of the patient will permit, and other means which it would be unneceſſary to mention ſince they are familiarly known to every ſurgeon. Of local treatments, ſcarifications, bliſters, and ſetons in the loins, promiſe advantage by deriving inflammation to the ſurface.

Confinement to bed and perfect quietude ſhould be enjoined. Whenever the patient ſits up, or puts his thigh in action, the inflamed parts muſt be injured by motion. An impediment to the motions of the thigh does in ſome degree indicate the preſence of inflammation in the loins, and a conſequent affection of the pſoas muſcle. This ſymptom often goes off when ſuppuration is formed, but frequently the inability to bend and to turn the thigh outwards remains during the greater part of the continuance of the complaint. As motion is painful and muſt aggravate inflammation, quietude of the thigh is evidently very proper in every ſtage of this complaint. M. David in France, and Mr. Juſtamond in England, have particularly extolled its utility.

Such are the uſual ſymptoms which denote the formation of a lumbar abſceſs. I have already noticed the means by which we ought to aim at its diſperſion, and I have attempted to ſhew that the preſence of matter is the cauſe of its accumulation.

Let us now ſuppoſe that no efforts on the part of the ſurgeon could have prevented the formation of an abſceſs. Let us ſuppoſe that the collected matter has deſcended in the courſe of the pſoas muſcle beneath Poupart's ligament, and now preſents itſelf beneath the faſcia of the thigh. The ſymptoms which then characterize the complaint are, the diſcovery of a fluctuation in the tumour;—the abſence of pain in the part, which ſhews that the fluid was not formed therein;—the intelligence that the patient affords of the pain which he had previouſly ſuffered in his loins; and the forcible impulſe of matter from the loins into the tumour, which is perceived whenever the patient coughs.

4. Before I proceed to relate the conduct which I ſhould then purſue, I ſhall endeavour to in-

veſtigate the cauſe of that derangement of conſtitution, which ſucceeds to the ordinary mode of evacuating the matter from a lumbar abſceſs; for could this be determined, the moſt rational means of preventing it would be immediately indicated. This, therefore, is the firſt point which I ſhall endeavour to aſcertain. The fever, which is excited, appears to be ſubſequent to the local inflammation, and correſponds, in degree, to the diſeaſe exiſting in the part. It has been generally obſerved, that where the abſceſs has ſpontaneouſly burſt, or has been opened with the leaſt poſſible degree of irritation, if fortunately but little topical inflammation was produced, the derangement of the conſtitution was ſmall in proportion.

Our firſt enquiry will therefore be, to what cauſe we ought to attribute this local inflammation. Surgeons formerly were accuſtomed to aſcribe it almoſt entirely to the admitted air, which they ſuppoſed to act by powerfully ſtimulating the cyſt of the abſceſs; and alſo by producing putrefaction of the contained pus. This putrifying matter was alſo ſup-

poſed to act in a twofold manner; firſt, by irritating and aggravating the inflammation of the contiguous parts; ſecondly, by being abſorbed and conveyed into the circulating veſſels, where by its ſtimulus it occaſioned the fever concomitant to the complaint.

Theſe, I believe, are the principal opinions that have been maintained: I wiſh now to enquire into their truth or fallacy. Firſt, then, is the admitted air capable of ſo greatly ſtimulating the cyſt of an abſceſs? and here our enquiry becomes extended: the queſtion may be ſtated—Does the air admitted into the different cavities of the body cauſe that inflammation which enſues when they are laid open? or ought we rather to attribute it to the irritation produced by the inflicted wound? Surgeons were formerly inclined to impute very miſchievous effects to the entrance of air into cavities: they ſeem to have imagined it poſſeſſed of very deleterious powers. This opinion appears ſtrange, ſince it is very little ſtimulating to the animal fibre; and that it does not particularly irritate the membranes of the body, common obſervation and expe-

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mental enquiry have evinced. Air is admitted into the cellular ſubſtance in Ephyſema, in which, however, it produces no inflammation. Mr. Aſhley Cooper permits me to mention the reſult of experiments which he made, in order to determine how far the air was ſtimulating.—He inflated the abdomen, thorax, and cellular ſubſtance of dogs, and immediately cloſed the aperture through which the air was impelled; the wounds healed by the firſt intention, the air was abſorbed from the cavities, but no inflammation was excited.

The circumſtances, however, are different when the opening is permanent; a conſtant renewal of air is permitted; and the application of a matter ſo unuſual to theſe ſurfaces I am inclined to believe does harm. Whenever the integrity of the cyſt is deſtroyed, though by ſpontaneous ulceration, or by means productive of the leaſt poſſible irritation, ſtill much inflammation frequently enſues; for where ulceration of the cyſt takes place, little, if any, inflammation is perceptible until the diſcharge of the pus has hap-

pened :—and when a cauſtic has been applied to the tunica vaginalis teſtis, for the cure of the hydrocele, though that membrane has ſuffered all that it can do from irritation, yet, the ſeverity of the ſymptoms is always greatly aggravated when the ſloughy tunic has ruptured. Whether the unſupported and collapſed ſtate of the cyſt is the cauſe exciting inflammation,—whether this action is occaſioned by the ſenſation of imperfection in the part,—or whether it is owing to the irritation of the admitted air, may be left as matter of opinion ; I am only ſolicitous to ſtate, that an inflammation appears to me to take place, independent of the local ſtimulas of the wound.

The next opinion that I have to notice is, whether the admitted air may not do injury by inducing putrefaction of the pus ? If the matter had only an incomplete diſcharge, it was confined in a ſtate of putrefaction, and thus applied to the ſurface of the abſceſs, it ſurely would be, in ſome degree, injurious ; but as the outlet in general is ſufficient, and as the former matter is waſhed away by that which is newly ſecreted, this is not likely to

be

be a common occurrence. I here beg leave to remark, that ſome confuſion appears to me to have ariſen from the word putrid being uſed occaſionally to ſignify both fœtid matter, and matter in a ſtate of putrefaction:—but putrefaction is owing to chemical decompoſition of the animal ſubſtance, and matter thus circumſtanced cannot fail to irritate the animal fibres.

On what the odour of newly ſecreted matter depends is not well known, nor does it follow of conſequence, that becauſe it is diſguſtful to our ſenſes, it is ſtimulating to the ſurface which ſecretes it. The fœtor, ſurely, denotes a deviation from its uſual properties; and, therefore, probably, it may be capable of irritating the ſurrounding veſſels.—But the degree of irration we are induced to believe is not great, from obſerving that ulcers continue for a long time to diſcharge putrid pus, whilſt their ſurface appears little affected by the qualities of the ſecretion.

It has been generally remarked, that where there is little irritation conſequent to the opening

opening of the abſceſs, in general, little fætor of the diſcharge is perceived; it appears probable, therefore, that the unnatural qualities of the ſecretion depend on the local excitement of the veſſels.

The third point for enquiry is, whether the abſorbtion of this putrid matter produces hectic fever? Before this queſtion can be diſcuſſed it ought to be ſtated, whether putrifying or fœtid matter be meant? We daily ſee inſtances of people having copious diſcharge of fœtid matter from ulcers, who do not ſuffer hectic fever. If matter which is injurious be abſorbed, the abſorbents are firſt ſtimulated, and inflame; and when that matter enters the circulating ſyſtem, it excites fever by its ſtimulating properties:—but this is not hectical; it is more violent, more approaching to the nature of inflammatory fever. Now, as there are no ſymptoms indicating inflamed lymphatics,—as the fever is of a different nature,—and as we have daily inſtances of large ſurfaces ſecreting putrid pus, without the production of fever ſimilar to that attendant on lumbar abſceſs, I conclude

that

that the abſorption of matter is not, generally, the cauſe of the fever which ſucceeds the ordinary evacuation of the lumbar abſceſs. To what origin, then, are we to impute this local and general irritation?

There are two cauſes which ſeem to induce the topical inflammation. 1ſt, An irritation extended over the whole cyſt from the part wounded or ulcerated.—This is, generally, proportionate by the injury done to the cyſt: a knowledge of this circumſtance ſhews the great impropriety of much injuring the cyſt, by extenſive inciſion, or the application of cauſtic. 2dly, Great inflammation ſometimes occurs where the local irritation is not conſiderable. The cauſe of this I have endeavoured to inveſtigate; and in our treatment of theſe complaints we ſhould endeavour to avoid thoſe circumſtances likely to induce it. There is alſo a third circumſtance very injurious to the health of the patient, which is, a copious and exhauſting diſcharge from an extenſive cyſt.

The fever appears to be a ſympathetic affection

affection of the circulating ſyſtem with the local diſcharge. It is more violent during the firſt ſtage of local inflammation; and is continued, though moderately, from the exertion and debility attending the production and continuance of the diſcharge from the cavity of the abſceſs.

The method which I have purſued appears to me well calculated to obviate all the injurious conſequences ſubſequent to the evacuation of a lumbar abſceſs. The matter is by this method diſcharged; and, as the wound made in the cyſt immediately unites, inflammation is prevented:—neither does that inflammation enſue, the cauſe of which, it is difficult to aſſign, but which appears to me independent of the irritation of the wound inflicted in the cyſt. The general circulation alſo remains undiſturbed. It is natural to ſuppoſe that the ſecretion into the abſceſs would, in ſome degree, increaſe the patient's weakneſs; yet, ſcarcely any alteration is perceptible. When one of theſe abſceſſes held three pints of pus, the loſs of fluid from the ſyſtem was generally in the firſt fortnight twelve

twelve ounces; in the ſecond, between ſix and ſeven ounces; in each ſucceeding fortnight, about four ounces. In about ſix weeks, as no matter is impelled by the diſunited faſcia when the patient coughs, I conclude that the original abſceſs of the loins is nearly aboliſhed, in conſequence of the diſpoſition of its ſides to contract, when they are not kept aſunder by collected fluids. And that no repletion of the abſceſs is likely to happen, evidently appears, for whatever ſecretion is made into it, will, by its gravity, deſcend into the cavity left by the ſeparated faſcia of the thigh. Thus is the reduction of the abſceſs, to a very conſiderable extent, accompliſhed, without irritation or debilitating exertion of the vaſcular ſyſtem. Neither is the loſs of fluids which I have ſtated, to be compared to the profuſe quantity drained off from the circulating ſyſtem, when inflammation is produced and ſecretion greatly increaſed, in conſequence of the abſceſs being immediately laid open. I alſo think it probable that the violent inflammation which is thus induced, thickens the cyſt, leſſens its elaſticity, and prevents that ſpeedy contraction

which

which I believe happens when its inflammation is obviated by the means I have related.

A knowledge of the dreadful conſequences attending the ordinary mode of diſcharging the matter made ſurgeons delay, as much as poſſible, to open theſe abſceſſes. The quantity of pus was therefore ſuffered to encreaſe till it had very extenſively detached the faſcia, and until the dimenſions of the cyſt in the loins was much enlarged. This practice is evidently injurious; the longer the opening is delayed, the greater does the accumulation of matter become, and the more extenſive muſt be the containing cyſt,—in proportion to its extent, ſo will be the ſucceeding inflammation and diſcharge. I have little doubt but if every pſoas abſceſs had been opened on its firſt appearance fewer people would have died of this complaint. When ſuch an abſceſs firſt preſents, perhaps, it may contain eight ounces of pus; yet I have ſeen a caſe where the faſcia was tenſe and did not readily admit of protuſion; in which one-third of it was detached from the ſubjacent parts, and the abſceſs contained forty ounces of fluid before the

the tumour became ſo prominent as to demand of the ſurgeon to give diſcharge to its contents. The ſkin will retain its natural appearance, becauſe it is not diſtended by the matter,—the faſcia ſuſtains all the preſſure. If the pus be not evacuated, the quantity collected beneath the faſcia muſt increaſe. The ſame obſervations apply to matter originally formed beneath the faſcia, which muſt be diſcharged early, if we wiſh to prevent the increaſe of diſeaſe. The fears of exciting dangerous fever has been the cauſe of delay; but by the method here recommended the matter may be diſcharged and the dimenſions of the cavity reduced without ſuch apprehenſions. After having two or three times punctured the faſcia and diſcharged the matter, no fear I think, in ordinary caſes, need be entertained of leaving a permanent opening in the cyſt of the abſceſs, and thus inducing that inflammation which is neceſſary for its final cloſure. I ſcarcely need obſerve, that where the tumour is ſmall, care is required in the introduction of a lancet beneath the faſcia, leaſt the large veſſels be injured, doubtleſs the

opening

opening ſhould be on the one ſide, and not oppoſite to theſe veſſels.—

5. I now wiſh to repreſent to the reader the manner in which I propoſe to treat ordinary caſes.—I at firſt tapped them with an hydrocele trochar; but I found on the rediſtention of the faſcia the preſſure againſt the orifice induced it to inflame and ulcerate. Although this never happened but in one caſe, as I have related, yet, I was fearful of it in others. In that caſe, the ſecond diſcharge of matter was too long delayed, ſo that the diſtention of the faſcia became very conſiderable.

I prefer diſcharging the pus by introducing a lancet through the integuments, then paſſing it obliquely for a ſmall diſtance between the ſkin and faſcia, and then by depreſſing the point of the lancet to puncture the cyſt.

When a trochar is introduced, the orifice of the cyſt is oppoſed to that of the integuments; the opening through which the trochar

trochar has paſſed is filled up by newly formed vaſcular ſubſtance: when diſtention takes place, and this ſubſtance is preſſed upon, we know that it will more readily ulcerate than parts originally formed, and, therefore, there is danger of its giving way from preſſure; but when the cyſt is punctured in the manner which I have deſcribed, the preſſure is exerted againſt the original cyſt, and not againſt a newly formed ſubſtance.

If the lancet be introduced too obliquely, the orifice in the integuments will not correſpond with that of the cyſt, and the diſcharge of the pus will be difficult, particularly if there be mixed with it flakes of firmer matter. In that caſe I think a flat trochar ſhould be introduced through the wound. The canula ſhould be of a ſufficient ſize to allow the paſſage of any flakes of firm ſubſtance which may be contained in the matter; for the introduction of a probe through the canula, to remove ſuch obſtructions, is obviouſly injurious; it may ſtimulate the cyſt, and admit air into the cavity of the abſceſs. The mat-

ter, if poſſible, ſhould be drawn off in an uninterrupted current; and the aperture ſhould be immediately, and exactly, cloſed. It might be ſaid that the canula could be introduced without the ſtilette; but this, perhaps, cannot be effected without difficulty: every degree of force ſhould be avoided, and the utmoſt attention paid, that the cyſt ſhould not be irritated. I ſhould object much to the introduction of probes or directors through the orifice, in order to expedite the flow of the matter; if the opening made by the point of the lancet be too ſmall, I would rather enlarge it, than riſk the production of inflammation of the cyſt by ſuch irritation.

The complete evacuation of the abſceſs ſeems indiſpenſible in the proſecution of this plan of conduct; for if the matter be but partially diſcharged, re-diſtention of the cyſt will ſoon happen, and ulceration of the punctured part will be inevitable, if an outlet be not given to the accumulated fluid. I have uſually dreſſed the orifice made by the lancet with lint, whilſt its edges are brought into cloſe

cloſe contact with ſticking plaiſter, as we commonly treat the wound made in venæ-ſection. I think it better to make this inci-ſion in a longitudinal direction with regard to the thigh; for it appears leſs likely to be burſt open by the exertion of coughing. Much attention to procure the immediate union of theſe punctures is requiſite, as upon this cir-cumſtance depends the ſucceſs of this mode of treatment. I have generally poſtponed the ſecond diſcharge of pus for a fortnight; but if the cavity ſoon fills again, and the newly-healed punctures are irritated by the preſſure of the contained fluid, there is an abſolute neceſſity for evacuating the contents of the abſceſs at an earlier period.

The advantages of the mode of treatment which I have purſued and recommended in theſe complaints appear to me ſtrikingly evident; yet, I do not expect that in all caſes our in-tentions can be accompliſhed. The exertion uſed in coughing will ſometimes impel the matter through the puncture, and prevent its immediate union; ſometimes inflammation of

the cyſt, and ulceration of theſe orifices, may happen. But if the original plan of treatment has been fruſtrated, the conduct which has been purſued is, ſtill, the beſt poſſible to be practiſed:—the matter has been evacuated by a ſmall wound, and with the leaſt poſſible degree of irritation to the cyſt of the abſceſs; conſequently, the ſucceeding inflammation and fever will probably be ſmall in proportion. Since the former part of this Eſſay was printed, a caſe has occurred which proves the truth of theſe opinions.

CASE.

A man was admitted into the hoſpital with a large lumbar abſceſs. I punctured it obliquely with a lancet; but the matter not diſcharging freely, I introduced a trochar through the orifice, and evacuated thirty ounces of pus. In conſequence of violent exertion in coughing, ſome matter was the ſucceeding day impelled through the orifice; the puncture was again carefully dreſſed, in hopes that it might ſtill heal, and for ſome days the

event was doubtful; but the man, in getting up, had twice diſplaced the plaiſter. On the fifth day, an inflammation of the integuments and induration and tenderneſs of the faſcia, were evident: the patient alſo complained of pain when the tumour was ſlightly compreſſed.

The preſent object of ſurgery was I thought, to leſſen as much as poſſible the irritation of the punctured parts, that the ſtimulus imparted to the cyſt from that cauſe might be proportionably diminiſhed. I directed a poultice to be applied to the part, from which he obtained much abatement of pain: on the following day he did not complain, the tumour was, comparatively, little ſenſible, and ſoon after became indolent. However, much irritation, both locally and generally, ſucceeded; the diſcharge was copious and fœtid; and the fever hectical,—it prevented his ſleeping, and induced great debility. After about ten days, the ſeverity of the fever diminiſhed, and his ſtrength began to return. I forbear to detain the reader with a recital of every circum-

ſtance—it will be ſufficient to ſay, that the ſevere illneſs which he has endured, the extreme debility occaſioned by the fever and diſcharge, and the length of time to which his cure will probably be protracted, yet more forcibly impreſs my mind with the conviction of the advantages derived from this mode of treatment; by which the cure of ſuch an abſceſs has been accompliſhed in leſs time, and without the production of inflammation or evident fever.

I have thus taken the liberty of offering to the public my obſervations and ſentiments reſpecting theſe abſceſſes: I have not interrupted the account by remarking in what they vary from the opinions of others; but I now beg leave, briefly, to point out theſe differences.

1. The prevailing opinion has been that the matter originated from a diſeaſe of the ſpine. This is directly contrary to the information which I have derived from diſſection; and were the idea true, it would ſurely be

ſtrange, that in nine ſucceeding caſes, to which I was particularly attentive, there was not the leaſt reaſon to ſuppoſe that any ſuch diſeaſe exiſted. That diſeaſe of the vertebræ, when it exiſts, may irritate the adjacent cellular ſubſtance, and induce ſuppuration, is very probable; and, therefore, it cannot be ſurpriſing occaſionally to find the two diſeaſes concomitant. Where there is the greateſt quantity of looſe cellular ſubſtance theſe abſceſſes, which, perhaps, are more the product of conſtitutional indiſpoſition than local irritation, generally form. Of this nature are the abſceſſes which take place in the cellular ſubſtance ſurrounding the rectum, and which afterwards contract into fiſtulous tubes. Theſe caſes are, in my opinion, ſomewhat analogous; yet, in the latter no one ever ſuſpected a caries of the adjacent bones.

2. Perhaps, the idea which I have formed of the nature of the cyſt of theſe abſceſſes may not accord with the general opinion. The caſe which I have related, and diffuſion of the pus after the evacuation of ſuch an abſceſs,

which is no uncommon occurrence, are the arguments on which my opinion is founded; and they appear to me ſufficient for its ſupport.

3. The propriety of ſpeedily opening moſt chronic abſceſſes, though contrary to general practice, appears to me obviouſly proper. In phlegmonoid abſceſſes a ſpontaneous outlet to the collected matter is ſpeedily afforded by the extenſion of diſeaſe; but in chronic ſuppurations this natural opening is very long protracted, and the diſeaſe, in conſequence, augmented.

Perhaps, this mode of diminiſhing cavities, by occaſionally evacuating their contents, may be applied with advantage to other caſes in ſurgery. In the only caſe of chronic abſceſs in which I tried this practice, inflammation enſued in the ſurrounding cellular ſubſtance, when the matter became diffuſed: but I do not think it is likely to happen in other caſes.

As the deſign of this treatment is merely to diminiſh

diminiſh the extent of an abſceſs, I think it will not be found applicable to collections of matter beneath the faſcia of the thigh; for the extent of the detached faſcia cannot be much diminiſhed. I ſhould propoſe in ſuch caſes early to diſcharge the collected matter, by means as little irritating as poſſible to the ſurrounding parts: for the effect of much inflammation is a profuſe diſcharge, which often greatly exhauſts the conſtitution. Having thus avoided immediate inflammation, we ſhould next endeavour to procure an union of the ſeparated parts, and to effect ſuch a deſign I think, together with the uſe of bandages, we are then vindicated in employing ſtimulating applications.

I ſubmit to the reader whether a mode of treatment ſimilar to that which I have related may not be ſometimes proper in caſes of ſpina bifida. The reaſon of the accumulation of fluid in theſe diſeaſes beneath the Dura Mater is not very apparent; nor does the cauſe producing the ſecretion appear to be powerful or conſtant; for the water collects very ſlowly at firſt, and in ſome caſes none has

ever

ever been effuſed, and the child has grown up without experiencing any inconvenience. When once the collection has begun, the cauſe of its continuance and increaſe is evident; the collected fluid irritates and diſtends the membrane which ſecretes it, and thus augments the diſeaſe. I do not know that any attempts have been made to remedy theſe complaints; but I ſee no reaſon why we ſhould forego all endeavours. I think it very probable that a gentle degree of preſſure made on the tumour from birth, or at its commencement, might produce the abſorption of any depoſited fluid, and thus prevent the diſtention of the unſupported Dura Mater.

It has been an opinion, too generally adopted and inculcated, that the imperfect formation of a part ſo eſſential to the animal, implies a deficiency of power in the conſtitution. This reaſoning appears to me fallacious. Want of vigour of conſtitution might cauſe debility in any part, but could not cauſe an error of formation. I have ſeen very healthy infants who have been thus imperfectly formed, and

whoſe

whoſe health has ſuſtained but little derangement till the tumour has burſt, when they have periſhed from the inflammation of the Medulla Spinalis which inevitably enſues.

Surely, ſome attempt ought to be made to repreſs this collection of fluid: but even if it ſhould have begun, I would ſtill think it right to endeavour to produce its diſperſion, by the application of gentle preſſure. If this ſhould have no effect, and ſhould the accumulation of fluid continue to increaſe, as the death of the patient would be inevitable on the ſpontaneous rupture, I think it would be vindicable to diſcharge the fluid by a puncture with a finely cutting inſtrument, and endeavour to heal the wound immediately; and ſhould this be accompliſhed, to attempt to repreſs a future collection, by bandage, or by thoſe topical applications which appear beſt adapted to this purpoſe.

AN

ESSAY

ON THE

COMPOSITION AND ANALYSIS

OF

ANIMAL MATTER.

MR. Boyle hath ſhewn by experiment, that vegetables will grow, and will produce a woody vegetable ſubſtance when in contact only with air and water: now, as pure water is a compound of two airs, the ſolid fabric of vegetables muſt be a modification of the ſame particles of matter, which might previouſly have exiſted in an aeriform ſtate.

Some obſervations have made it appear probable, that even animal matter may, under certain circumſtances, be formed of ſimilar in-

ingredients. The ſtationary life and growth of marine zoophites induce us to this opinion, their ſuſtenance appearing chiefly, if not entirely, to be derived from the ſurrounding air and water. At certain ſeaſons of the year, immenſe ſhoals of fiſh tranſport themſelves from one part of the ocean to another; the foremoſt would ſurely devour all the food which they might accidentally meet with; yet thoſe which follow do not ſeem to ſuffer from deficiency. However, it may be remarked that animals of this nature do not need that conſtant renovation of ſubſtance which thoſe of warmer blood require: yet ſtill the long abſtinence they undergo, without adequate diminution of bulk, appears to confirm the opinion.

Dr. Fordyce, in his Treatiſe on Digeſtion, ſays, that he has put this idea to the teſt of experiment: he kept gold fiſh for ſix months in diſtilled water, during which time they grew.

Believing that, if this ſuppoſition of the formation of animal matter could be proved

to

to be fact, it would render our knowledge of its compoſition and analyſis clear and ſimple; and foreſeeing that ſome curious deductions in chemiſtry would follow its eſtabliſhment, I ſought to aſcertain or refute the opinion by the following experiments. I firſt reſolved to repeat, in different ways, the experiments of Doctor Fordyce; yet I ſoon perceived that experiments thus conducted could not be concluſive. I ſhall only preſent the reader with a general ſketch of what was done on this plan, as it were unneceſſary and improper to relate with accuracy every circumſtance of indeciſive experiments.

EXPERIMENT.

I placed ſome frogs ſpawn in a large earthen pan, with plenty of clear river water; and when the tadpoles had extricated themſelves from the viſcous matter of their ova, I removed them into diſtilled water; they were about two hundred in number.

At firſt the diſtilled water was renewed once a week, but afterwards it was merely filtered: the

the pan was placed in a garden and covered with moderately fine linen; however, any covering, which did not exclude the air, was inſufficient to prevent the admittance of the ovulæ of minute vegetables and animals; each week, when I filtered the water, I ſeparated from it abundance of dark-green vegetable matter, and great numbers of animalcules were left ſtruggling on the filter. The preſence of theſe were not ſo defeative of the intention of the experiment, as at firſt ſight, might be imagined; for, excepting only the imperceptibly ſmall quantity of matter contained in the ova, theſe inſects and vegetables grew from pure air and water alone; ſo that admitting the tadpoles fed upon theſe, they ſtill were ſupported by a modification of air and water. Many of the tadpoles died, and the others, I believe, fed on them: thoſe that died were however ſeparated from the others, weekly, by filtration.

I obſerved that the tadpoles were not, as in common, reſtleſsly ſeeking for food, but lay quietly at the bottom of the veſſel, unleſs diſturbed;

diſturbed; yet, when the veſſel was ſhaken, they ſwam with much vivacity. This experiment began on the 19th of April, and on the 21ſt of June I counted forty tadpoles, many of which were in a ſtate of transformation, and four perfect frogs. Their number now quickly diminiſhed, for, after having undergone this transformation, they either died or eſcaped from the pan; had they died their remains would have been viſible, yet none were diſcovered. I did not ſuppoſe they could have climbed up three inches heighth of a glazed pan, yet this they doubtleſs accompliſhed.

EXPERIMENTS.

The following year I took twelve leeches, which weighed ten ſcruples, and put them into two gallons of diſtilled water, in a glaſs jar. I covered the jar with two layers of paper, and pricked holes in the cover with a pin, to give admiſſion to air. I never filtered this water, but ſuffered the leeches to remain in the ſame ſtate for three months. Much vegetable matter had grown in the jar during

that time. When I then examined them, eight only were living, but theſe weighed twelve ſcruples; ſo that the weight of the leeches had encreaſed, although their number was diminiſhed.

I alſo caught ſome ſmall tadpoles, which weighed two drams and one ſcruple. I put them in a ſimilar bottle and covered them in the ſame manner as the leeches; they lived, but did not appear to grow; at the end of a month three were found dead, and the remainder weighed one dram and one ſcruple. Theſe experiments were made in London.

The impoſſibility of excluding the ovulæ of animals and vegetables from water, muſt, I think, always make theſe kind of experiments indeciſive. The concluſion which I have drawn in my own mind, from theſe and from future experiments, is, that the leſs perfect animalcules are capable, like vegetables, of converting mere air and water into their own nature, and that animals of a ſtill higher order may derive occaſional ſuſtenance from the ſame materials;

materials; yet, as nature probably deſigned them to be ſupported by already prepared animal and vegetable matter, the ſuſtenance which they derive from mere air and water is imperfect, and inadequate to their continued ſupport.

The experiments which I made upon this plan were all executed during the ſummer ſeaſon, the prevention of vegetation was then impoſſible; for, when I attempted to obviate this circumſtance by cloſing the bottle, even the leech, the moſt vivacious of theſe animalcules, quickly periſhed. In the winter, perhaps, vegetation might ceaſe; but then, the torpid and little varying ſtate of the animal body would, I think, render the experiment indeciſive.

It has been long well known, that vegetables will grow when in contact only with pure water and air. I do not, however, know that their analyſis has been attended to. Mr. Boyle indeed ſays, that they yielded a ſpirit and a caput mortuum; but the extent of chemical

knowledge in his time, did not enable him to inveſtigate this ſubject, as he would have wiſhed. I was deſirous to know if vegetables thus procured would yield the ſame ſubſtances as are afforded by the decompoſition of common vegetables; for if from theſe we can obtain vegetable alkali, lime, iron, and charcoal, it follows, that theſe ſubſtances are only varieties of arrangement of the ſame particles of matter, which previouſly exiſted in the ſtate of air and water. To aſcertain this circumſtance, I made the following experiments.

EXPERIMENT.

I ſtrewed the ſeeds of cabbages on thin clean flannel, ſpread on glazed earthen plates, and each day ſprinkled them with diſtilled water; the ſeeds ſoon began to vegetate, and the young plants grew as ſpeedily and vigorouſly as uſual. After five weeks I mowed off the young plants from the ſeeds and roots. I took nearly two ounces of theſe young plants and reduced them to aſhes in a crucible; they yielded eight grains of whitiſh aſhes; this was a ſmaller proportion of fixed reſidue than what I ob-

obtained from common vegetables, for ſix drams of common cabbage-leaf yielded when burned, five grains of aſhes. To the eight grains of aſhes half an ounce of diſtilled water was added, and gently heated; upon dipping ſome paper, tinged with the vegetable blue into this liquor, it inſtantly became of a bright green. The water being poured off, one dram of marine acid diluted was added, which produced a gradual long-continued effervescence; this being filtered was divided into two equal parts, to one portion the pruſſicated alkali was added, and a very copious precipitate of pruſſian blue was produced; the other portion was ſuperſaturated with cauſtic vegetable alkali, and a very plentiful floculent precipitate of lime was cauſed: ſtill ſome fixed reſidue remained, which doubtleſs was charcoal.

To this experiment it may be objected, that the vegetables derived the ſubſtances which were diſcovered on their analyſis, from their ſeeds, or from the flannel on which they grew. A quantity of ſeed equal to that which

produced theſe plants, being burned in the ſame fire, left too ſmall a quantity of aſhes for examination: neither is it likely that the flannel ſhould be decompoſed by the growth of vegetables: however, ſhould any experiment of this kind be in future attempted, I think it would be better to grow the vegetables, if it be poſſible, on ſand, which had been previouſly well waſhed with marine acid, by which it would be freed from accidental admixtures.

EXPERIMENT.

I took ſix very ſmall ſprigs of mint, which weighed thirty grains, and put them in phials filled with diſtilled water in a hot-houſe. Roots ſhot from them in plenty, but the ſtem and leaves grew very ſcantily: it was in the autumn when probably their power of vegetation was diminiſhed. When the roots had filled the bottles I removed them and burned them in a crucible and obtained eight grains of aſhes. Thirty grains of ſimilar ſprigs of mint had been previouſly conſumed, and had left too ſmall a reſidue to admit of examination. To

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the eight grains of aſhes thus obtained, ſome diſtilled water was added, and gently heated. This liquor inſtantly changed the vegetable blue to a deep green colour; thirty drops of marine acid, diluted with diſtilled water, was now added, which being filtered, was divided into two equal portions: one part being ſaturated with cauſtic vegetable alkali, a floculent precipitate of lime was depoſited; and the pruſſicated alkali, being added to the other, a conſiderable quantity of pruſſian blue was produced. Some aſhes ſtill remained undiſſolved by the marine acid: theſe I conclude are charcoal.

This experiment is not liable to the ſame objections as the preceding one, here the vegetable matter could only be produced from air and water, yet, when analyſed, it yielded the ſame products as other vegetables. It may be queſtioned if the marine acid which I uſed was pure, and the teſts which I employed were good; it is therefore right to mention, that I mixed the ſame proportions of marine acid and pruſſicated alkali as were uſed in the experi-

ment, and though a ſlight blue colour was exhibited, the appearance was very different to that copious blue precipitate which was thrown down from the acid which had ſtood in the vegetable aſhes. Neither was any precipitate cauſed when the marine acid was ſuperſaturated by the vegetable alkali.

Every one knows how readily mint grows in common water, and few people, I believe, will doubt, that this mint when analyſed, will yield vegetable alkali, lime, iron, and charcoal; yet, ſurely the water does not contain theſe ſubſtances in ſuch quantities as to impart them in a ready-formed ſtate to the vegetable. Some authors have queſtioned whether the alkali formally exiſts in vegetables, or whether it is produced by their decompoſition; to inveſtigate this ſubject I made the next related experiments.

I took ſix drams of cabbage-leaf, and added one ounce of ſtrong marine acid; after it had ſtood two days I ſtrained off the acid and burned the remaining leaf, and obtained four

grains

grains and a quarter of whitiſh aſhes. To theſe three drams of diſtilled water was added, the vegetable blue was not changed by this liquor; the water became, however, ſalt to the taſte, and, on evaporation, left about half a grain of ſaline chryſtals; twenty drops of marine acid diluted was added to the remaining aſhes, half of which being ſaturated with cauſtic vegetable alkali, an evident precipitate of lime followed: and ten drops of pruſſicated alkali being added to the remainder, a deep blue was produced: ſtill ſome charry matter remained undiſſolved by the acid.

Equal quantities of ſtrong marine and nitrous acids were ſuffered to ſtand ſome days on freſh parſley, but they took up neither lime, iron, nor alkali; ſome of the ſame parſley reduced to aſhes by heat yielded abundance of theſe materials. There is ſurely nothing in the mild juices, and delicate texture of vegetables, which could protect theſe ſubſtances if already formed from the attraction of ſuch potent menſtrua. If theſe ſubſtances are formed by the decompoſition of vegetable matter,

matter, it is clear, that they are not elementary or unchanging, but only varieties of arrangement and motion of the ſame atoms, which differently combined formed a vegetable, and which might previouſly have exiſted in the ſtate of air, or water.

EXPERIMENT.

I was deſirous to know if an animal would live and grow when fed only with vegetables, which had been produced from air and water. I therefore procured two rabbits, ſix weeks old, the produce of the ſame mother, one weighed twenty-three ounces and a half; this I killed and analyſed: the other weighed twenty-three ounces, which I fed in the following manner. Having obtained a large quantity of young cabbages and lettuces, which grew on flannel and were only ſprinkled with diſtilled water, in the manner before related; I mowed off the tops and gave them for food to the rabbit. On the third day after he had fed on them, he appeared very ill; he breathed very quick, and his hair was ruffled; he was alſo purged: fearing that he would die, I gave him

him a few ſhelled oats, ſtill leaving a plate with theſe vegetables before him. Next day he appeared much better and had eat both the oats and greens. For four ſucceeding days he eat a plateful of the freſh vegetables, and a ſmall quantity of oats, he appeared thin but was very lively: he only eat two ounces and a half of oats in the week, a quantity, I think, very inſufficient to ſupport him in the ſtate he appeared to be, had not the vegetables contributed to his nouriſhment. I was now obliged to go into the country for a few days, the rabbit was therefore fed for a week with common cabbage-leaf and parſley. After this I fed him for a fortnight in the former manner, allowing him ſcarcely any oats; he eat as much cabbage as grew upon ſix plates each day; he was lively and ſtrong, and though he looked thin, yet he encreaſed two ounces in weight during this fortnight. Deficiency of vegetables obliged me to diſcontinue the experiment: I did not analyze the rabbit, as I had originally intended, becauſe I thought the experiment incomplete, and I deſigned to repeat it more extenſively the following year. I did

did procure every thing for that purpoſe, but the garden where I attempted to grow theſe vegetables was too near to the ſmoke of London, which prevented their vegetation.

If the vegetables which are thus produced contain the ſame matter as common vegetables, though in a leſs degree, there appears no reaſon why an animal ſhould not be ſupported by them. Let it be then remembered that ſuch an animal is nouriſhed, only by a modification of air and water, from which his organs are capable of forming the mineral alkali, phoſphorus, and every ſubſtance which formally exiſts in the animal body. I have no doubt but that an animal can be thus nouriſhed; caution is, however, required in the experiment; for it is probable the vegetables will at firſt diſagree with his ſtomach. Some other food ſhould then be allowed him, and if the quantity and analyſis of that food be eſtimated, the object of the experiment will not be defeated. If a young rabbit, living on theſe vegetables, encreaſes one pound in weight, and during the time of this growth eats

eats only one pound of oats; if the oats on analyſis will yield only a certain quantity of lime, iron, and charcoal, and the animal, when analyſed, yields a larger proportion of lime, and iron, together with mineral alkali, and phoſphorus, than what is produced from this food, or than could have been obtained from that animal before the commencement of this experiment; it is then ſufficiently evident, that the animal body has a power of preparing theſe ſubſtances, or in forming an animal matter, which yields them on its decompoſition. Whoever alſo reflects on an animal being perfectly ſupported by vegetables, and remembers that his circulating fluids contain abundance of mineral alkali, which the art of chemiſtry cannot extract from the vegetable; muſt perceive, that the animal powers are certainly capable of forming this ſubſtance.

The chicken contained in an egg, after incubation, has lime depoſited in its bones, has red particles and conſequently much iron in its blood, and probably a proportionate quantity of mineral alkali. I wiſhed to know if theſe

ſubſtances

ſubſtances exiſted in ſimilar quantity, in the egg before incubation, and to diſcover this circumſtance made many experiments, the reſults of which were not exactly ſimilar. I ſhould needleſsly engroſs the time of the reader if I were to detail ſeparately the circumſtances of each experiment: it will be ſufficient that I relate the general reſults. I ought in this place to mention, that the eggs, and chickens, on which theſe experiments were made, were all produced by the ſame hens. A chicken contained in the egg ſhell, after complete incubation, weighs in general ſix drams leſs than the contents of the ſhell before that proceſs. There is, therefore, more matter emitted, than imbibed, through the ſhell during incubation.

I reduced an equal number of chickens and eggs to aſhes, ſometimes in retorts, ſometimes in crucibles. On the aſhes I digeſted firſt ſome of diſtilled water and obtained the ſalt contained in them. In ſome experiments the quantity of ſalt found in the aſhes of chickens greatly exceeded that found in the

aſhes of the eggs; in others it ſtill exceeded, but in a leſs proportion, and in other experiments the quantities were equal. I next digeſted marine acid a little diluted on the aſhes, which took up the lime and iron contained in them. I precipitated the lime by ſaturating the acid with the vegetable alkali.

That the precipitate was lime, was proved by pouring on it diluted vitriolic acid, with which it often formed a ſtone like ſelenite. The average proportion of lime to a chicken was five grains; but the egg yielded in general ſcarcely one grain.

On adding to another portion of this marine acid the pruſſicated alkali a copious blue precipitate enſued, and in quantity ſo nearly equal from the eggs, and from the chickens, that I could not decide which had the ſuperiority. After this I boiled vitriolic acid on the aſhes, expecting by this means to take up any lime which might be combined with phoſphoric acid. Upon ſaturating the vitriolic acid with vegetable alkali a cloud like precipitate

pitate appeared, and, I think, generally in equal quantities, whether it was obtained from the egg or the chicken; but the precipitate was too ſmall in quantity to be weighed. I afterwards heated the remaining aſhes with charcoal. Yet ſtill there was a good deal of fixed reſidue, ſuch I believe has ever been found to remain after chemiſts have proſecuted to the utmoſt, the analyſis of animal matter.

I alſo inveſtigated the difference of the ſubſtances found in the egg and chicken, by digeſting on them immediately ſtrong marine acid; this would certainly take up whatever alkali, lime, or iron exiſted formally in thoſe bodies. Marine acid alſo combines with vegetable and animal gluten. The precipitates obtained by ſaturating the acid were, therefore, not pure and might deceive. I found, however, an exceſs of lime in the chicken, when thus examined, proportionate to that which had appeared in the former experiments; and alſo a larger quantity of ſalt was obtained, but I could not decide which body poſſeſſed the ſuperior proportion of iron. After I had fil-

tered the marine acid from the eggs and chickens, on which it had been digeſted, and waſhed the reſidues with diſtilled water, I conſumed them in crucibles, and ſtill obtained from the aſhes which remained, more lime, iron, and ſalt.

This circumſtance proves to me, that the ſubſtances found in the aſhes of burned animal matter, do not all formally exiſt in the maſs before its deſtruction; but are only new combinations of the ſame ultimate particles, which under their former mode of arrangement made the animal ſubſtance, but which being driven aſunder by the repulſive power of fire, are left at liberty to form other modifications of matter.

It is, however, right to mention, that the ſame reſults did not always follow experiments ſimilarly conducted; for after having macerated ſome lean beef in water, till it had loſt all its colour, I digeſted on it a large quantity of ſtrong marine and nitrous acids. I then waſhed and conſumed the reſidue, but could

not from its aſhes obtain an evident quantity of lime, iron, or ſalt. This circumſtance I account for by obſerving, that theſe acids ſo completely diſſolve the animal gluten, that the reſidue is no longer capable of producing ſuch compounds. Yet the truth of the preceding obſervation remains eſtabliſhed; for if the acids be ſaturated by an alkali, they will precipitate that matter which they had taken from the animal ſubſtance, and it will be found merely gluten, containing neither lime, iron, nor alkali.

Lime, it is evident, exiſts formally in the chicken in much greater quantities than in the yolk or white of an egg; and to me it appears that it is formed in the proceſs of animalization of the chick. Yet it may be contended, on the contrary, that it may be taken from the egg-ſhell. In anſwer to this, I can only ſay, that the inner ſurface of the ſhell appears ſmooth after incubation, and not as if any matter had been taken from it. I have alſo taken the ſhells of eggs, when the time of incubation was nearly expired, and compared them

them with ſhells before incubation, diſſolved their lime in marine acid, and afterwards obtained an equal weight of precipitate from each.

If the ultimate particles of animal matter be the ſame, and if the various products obtained from it, depend on the accidental combination of thoſe particles: it accounts for the want of uniformity which is obſerved in the quantity of the ſubſtances procured by its decompoſition. It alſo ſeems probable, that theſe products will differ as the mode of deſtroying the original matter varies. It occurred to me, that there would be leſs fixed reſidue, when the particles were ſuddenly driven aſunder by the repulſive power of fire, than when they were gradually ſeparated by ſpontaneous putrefaction: in which proceſs, they would for ſome time remain within the ſphere of each other's attraction.

EXPERIMENT.

It being requiſite to bleed two perſons, who had ſuffered accidental injury, but

who in other reſpects were healthy; I caught in a common crucible five ounces of blood from one of them, and four ounces from the other; an equal quantity of blood from the ſame people was collected in another crucible. The blood contained in the one veſſel was immediately decompoſed in a moderately ſtrong fire, and yielded five ſcruples and two grains of aſhes.

The other crucible was carefully covered, and hung up in the corner of a chimney; where it was expoſed to the varying heat of the fire for four months. Whilſt it remained in this ſituation, I added to it, four different times, three ounces of diſtilled water; for had it been ſuffered to dry, the proceſs of putrefaction would have ceaſed. At the expiration of four months I put the crucible in the fire, and expelled all the volatile matter from its contents: the remaining aſhes weighed only ſeventy-eight grains. The aſhes which I had obtained from the immediately decompoſed blood, I had put by in a paper, in a covered gallypot; the ſalts contained in them had deliqueſced. I now added diſtilled water to both parcels of aſhes

aſhes, and having thus diſſolved the ſalt contained in them, I evaporated the water, and obtained from the firſt aſhes only ſix grains of ſalt; from the latter (the reſidue of the putrified blood) I procured fifteen grains. Strong marine acid was now digeſted on each parcel of aſhes, which being examined by ſaturation with the vegetable alkali, and by the addition of the pruſſicated alkali, it was found that thoſe aſhes left by the putrefaction of blood, contained near forty grains of aerated lime; whilſt in the other reſidue, there was not half that quantity: the former aſhes alſo contained more than twice the quantity of iron, than was found in the latter.

Vitriolic acid was now boiled on the aſhes, which took up from each nearly an equal quantity of lime. The reſidue of the blood, which was immediately reduced to aſhes, weighed but a few grains, whilſt the remains of that which was ſuffered to putrify weighed thirty-five grains. The aſhes were now mixed with charcoal and put into a ſtrong fire, in which ſituation they loſt much of the weight; being

now

now again examined, it was found that they contained nothing diſſoluble in ſtrong acids.

In proſecuting the analyſis of animal matter, the nature of the remaining refractory aſhes naturally excite attention; it might be ſuſpected that they might contain lime and iron, ſo combined as to be inſoluble in chemical menſtrua. This ſuppoſition is not probable, for when they are mixed with charcoal, they may be almoſt entirely diſſipated in a ſtrong fire.

Theſe experiments, I think, ſhew that vegetables and animals poſſeſs organs capable of aſſimilating to their own nature, the matter deſtined to their nutrition; that the animal powers can prepare ſalt, lime, and iron, which are found to exiſt formally in the body; however, the greater part of an animal, or vegetable, is without ſuch ſubſtances, yet, when deſtroyed by fire, its component parts do in general recombine, and thus produce theſe ſubſtances. Since then, animal matter is only a peculiar arrangement of common matter,

why

why may not the organs of imperfect animals be capable as well as vegetables, of producing this arrangement? it ſeems probable that they are capable; but animals of more perfect organization, who poſſeſs ſenſation, and were deſigned by nature to live on previouſly prepared animal and vegetable matter; they appear leſs able to accompliſh ſuch converſion, and die ere it is effected, from the derangement of their ſenſitive organs.

When the ſubſtances obtained by the analyſis of animal and vegetable matter are once formed, their particles mutually attract each other with ſuch force, that fire, or any artifice employed by the chemiſt will not decompoſe them. Yet, though their compoſition cannot be ſhewn by analyſis, theſe experiments prove it by ſyntheſis.

I was prompted to undertake theſe experiments, becauſe it was neceſſary to give ſome account of the nature of animal matter, in a courſe of anatomical lectures, previouſly to deſcribing its arrangement, in the ſtructure of the

the human body: and alſo, becauſe I had imbibed the opinions of the great philoſophers of this iſland, who, from reflection and reaſoning, were induced to believe, that the ultimate particles of matter were the ſame, and that the various ſubſtances, with which, this world preſents us, were only differences in the arrangement and motion of ſimilar particles. The teſtimony of experiment, appears to me to be now added to the truth of an opinion, formerly ſupported merely by the ſuggeſtions of reaſon. Whoever alſo reflects on the wonderful diviſibility of matter, will ſcarcely ſuppoſe any eſſential difference in its ultimate parts.

Theſe experiments, it is true, only extend to thoſe ſubſtances found in animals and vegetables; as the vegetable and mineral alkalies, phoſphorus, lime, iron, &c. but if lime and iron can be thus compoſed, why may not clay and gold?

The reader will perceive that the train of experiments, which I have related, are ſimilar

to

to thoſe by which Mr. Boyle ſupported the ſame opinion. But the extent of chemical knowledge, in his time, did not enable him to examine the nature of the aſhes, left after the combuſtion of animal matter: he could therefore only ſuggeſt a probable opinion, he could not offer to the incredulous, convincing proofs.

I much wiſh that theſe experiments were more diverſified and extended: I undertook them only for my own information, and having proſecuted them ſo far as to convince myſelf, I deſiſted from further inveſtigation. An opportunity now preſenting, I offer them to the public notice; becauſe, to me it appears, that the late advancement of chemiſtry, though it has given us great knowledge of the properties of every ſpecies of matter, yet has tended to contract our views; it has made us direct them to partiular objects, and ceaſe to contemplate this beautiful and extenſive proſpect of matter, and its combinations. The beſt chemiſts, M. Lavoiſier, M. Chaptall, and M. Fourcroi, either entirely avoid the conſideration of the elements of matter, or if they do

ſpeak of them, they do not ſeem to advert to, or underſtand, this beautiful theory.

If the related experiments ſhould be conſidered inſufficient to prove theſe doctrines, I heartily hope it may excite others to further inveſtigation: ſo that, if the opinion be true, it may be perfectly aſcertained. For I know not any thought, which, on contemplation, can ſo delight the mind with admiration of the ſimplicity and power evident in the operations of the Creator, as the conſideration, that by different arrangement and motion of ſimilar atoms, he has produced that variety of ſubſtances which are found in the world, and which are ſo conducive to the wants and gratification of the creatures which inhabit it.

ERRATA.

Page 25, line 12, *after* there *add* was.
Page 40, line laſt but one, *for* my *read* any.
Page 47, line 20, *after* ſurrounding *add* parts.
Page 48, laſt line but three, *for* late *read* lately.
Page 56, line 15, *for* ſtimulas *read* ſtimulus.