An inquiry into the remote cause of urinary gravel / by Alexander Philip Wilson.

AN

INQUIRY

INTO THE

REMOTE CAUSE

OF

URINARY GRAVEL.

BY

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TO

DR. JOSEPH BLACK,

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UNIVERSITY OF EDINBURGH,
ETC. ETC.

AND

DR. ALEXANDER MONRO,

PROFESSOR OF ANATOMY AND SURGERY
IN THE UNIVERSITY OF EDINBURGH,
ETC. ETC.

GENTLEMEN,

WHETHER I look up to You as Instructors, or consider You as Friends, there are none, to whom, with equal

equal propriety, I can dedicate this little Work. If it merit your Approbation, that will encourage the Diffidence I feel on laying a firſt Attempt before the Public, and amply repay any pains it has coſt me. But, however little this Paper may deſerve your regard; I hope You will look upon my offering it to You, as a Teſtimony of that Eſteem from an Individual, which the

World

World have for your Characters; and a Public Acknowledgment of the Attention which You have honoured me with, ſince I became your Pupil.

I AM, WITH THE GREATEST RESPECT,

GENTLEMEN,

YOUR MOST OBEDIENT SERVANT,

ALEXANDER PHILIP WILSON.

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Would have for your Character, and a Public Acknowledgment of the Attention which You have honoured me with, since I became your Pupil.

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INTRO-

INTRODUCTION.

THE attempt to remove the Proximate Cauſe of Urinary Gravel by Lithonthriptics, has been attended with ſo little ſucceſs; and, at the ſame time, there is ſo uncertain a proſpect of finding a ſafe and effectual ſolvent for the Urinary Calculus; that the mind is naturally led to an inveſtigation of the *remote cauſe of this diſeaſe;* and to endeavour, by determining this, to lay down certain rules, which might tend

to

to prevent a complaint, which the present knowledge of medicine seems but ill fitted to cure. We attempt this with the greater prospect of success, as the experience of every age has told us, that there are certain circumstances, giving a predisposition to gravel, which no succeeding cure seems capable of counteracting.

But, although Experience, the best of guides, has pointed out many of the predisposing causes of Gravel; yet, unassisted by reason, it cannot investigate the change induced on the body, which constitutes the *remote cause of the disease*, and the hinge on which

which every rational plan of preventing it muſt turn. Ever in queſt of lithonthriptics, and often deluded by a deceitful appearance of what they ſought for, phyſicians ſeem to have paid but little attention in order to determine the cauſe of this diſeaſe. Anxious to relieve the complaints occaſioned by its attack, they have neglected aſcertaining the means of preventing its firſt appearance. But, as this is ſurely the eaſier taſk of the two, let us endeavour to determine the manner in which the prediſpoſing cauſes of gravel act; and ſee if, by doing ſo, we can lay down any rules for preventing this ſevere diſeaſe, which

has,

has, from the infancy of medicine, demanded the particular attention of phyſicians, and defied the moſt aſſiduous application of their art. It is with this view I have made the following experiments; of the imporance of any thing new I am to offer, experienced practitioners muſt judge; my part is to aſſert, that I have been careful in marking appearances, and have drawn thoſe concluſions only, which ſeem ſupported by fact.

The great connection which dyſpepſia has with urinary gravel, both in giving the pathology of the diſeaſe, and in laying down indications for its cure,

cure, has induced me to divide this paper into *two* parts; the latter of which treats ſolely of the morbid affections of the ſtomach, and is ſubdivided into three chapters, the firſt containing a ſhort introduction, the ſecond an inquiry into the proximate cauſe of dyſpepſia, and the laſt, the treatment of that diſeaſe. The former part is ſubdivided into five chapters; the firſt, containing experiments made with a view to determine the remote cauſe of Urinary Gravel; the ſecond, ſome remarks concerning the depoſitions of the Urine, and the manner in which acids act on that fluid; the third, general obſervations

on

on the foregoing Experiments; the fourth, the application of the foregoing experiments to determine the remote cauſe of Gravel; and the laſt, the circumſtances rendering it probable, that the remote cauſe of Gravel is preſent; and the means for removing it.

I proceed, then, to relate the Experiments; making ſuch remarks as may occur, but reſerving the more general obſervations for the third chapter.

PART

PART I.

CHAP. I.

Experiments, made with a view to determine the Remote Cauſe of Urinary Gravel.

BEFORE I proceed to relate theſe experiments, it is neceſſary to take notice of the following circumſtances.——Having been made at different times, they were not all performed with the ſame degree of accuracy. All of them, however, ſeem ſufficiently accurate to enable us to determine with certainty concerning the reſult; and this is all

 that

that is neceſſary. Beſides, every point is proven by the moſt accurate experiments, and their reſult confirmed by others leſs ſo. The chief circumſtance wherein the inaccuracy of theſe laſt experiments conſiſts, is, that I did not take notice of the ſtate of the thermometer, as they were performed ; but as hardly any of them laſted above three days, it is not probable that the temperature could vary much : and, as it muſt be evident, from conſidering the experiments where the ſtate of the thermometer is mentioned, that even conſiderable changes in this did not vary the reſult ; it will appear, that every experiment I am to relate has been performed with all the accuracy requiſite. Where the thermometer was uſed in any experiment, the ſtate of it is generally mentioned every day till the urine was examined ; although, in relating the experiment, the depoſition of the urine is always

men-

mentioned on the day on which it was made, that the reſult may be the more eaſily perceived. With regard to the ſtate of the perſons on whom theſe experiments were made, their particular ſituation at the time is afterwards mentioned. It is enough at preſent to obſerve, that none of them had ever been troubled with any calculous affection.

Experiment I.

In this caſe, as well as in all the other experiments which I am to relate, I kept the diſhes containing the urine, in a place at a diſtance from any fire, and conſequently little ſubject to changes of temperature.—The thermometer, where it is mentioned, ſtood in the ſame place.

The following experiment I made on myſelf when in good health.

Living partly on animal and partly on vegetable food, and at the ſame time taking a certain degree of exerciſe; morning, midday, and evening, I ſet apart a portion of urine in clean veſſels. After theſe had ſtood 48 hours, I found in each of them a ſandy depoſition, which, after Sheele, I ſhall in the ſequel call the *lithic acid*; the ſeveral depoſitions, taken together, weighed gr. i. ß.

On the following day, living in the ſame manner with reſpect to diet and exerciſe, I ſet apart, as formerly, portions of urine, which, after ſtanding the ſame length of time, had alſo depoſited gr. i. ß. of lithic acid; on the evening of this day I eat a lemon. Next morning I ſet apart a portion of urine, eating at this time a ſecond lemon; about dinner-time I eat a third, and

ſet

ſet apart another portion of urine; In the evening I eat another lemon, and ſet apart another portion of urine. All this day I had taken no animal product but milk, which is aceſcent, and had taken nearly the ſame quantity of fluid as on the former day, and the ſame exerciſe. After each of the above portions of urine had ſtood 48 hours, I found in them a depoſition of lithic acid, amounting to gr. iij. ß. Next day I uſed the ſame regimen; and the portions of urine, after ſtanding the ſame time, alſo depoſited gr. iij. ß. of lithic acid; the depoſitions of the two laſt days not differing one fifth of a grain.

The portions of urine ſet apart on the different days of this experiment were exactly equal to one another; as were thoſe in every experiment I am to mention; except where the whole urine made was kept.—

Either of theſe methods evidently gives a reſult ſufficiently deciſive. I alſo uſed, in all theſe experiments, veſſels of a ſimilar ſize, and ſimilarly ſhaped; for the veſſel expoſing a larger or leſs ſurface to the air, evidently affects the depoſition.

One fact, which ſeems curious, I obſerved to take place in this experiment, and as often as I examined my urine, at different times of the day, except in particular circumſtances afterwards to be mentioned; the morning urine was darkeſt coloured, that made at mid-day next, and that in the evening paleſt.

Experiment II.

This was made on a boy about 15, in good health, which he enjoyed during all the experiments which I made on him.

Monday

Monday.

On getting out of bed, at 8 in the morning he made water ; this I did not keep. Having eat nothing this morning before 9, at that time he took for breakfaſt,

of bread,
of milk, *ſing.* ℥ viij.
of honey, ℥ i.

At 11 in the forenoon, he ſet apart of urine ℥ vj. ; (Farenheit's thermometer ſtanding at 29°.)

At 3 in the afternoon, he ſet apart of urine ℥ vij. (therm. 29°) ; at this time he eat of honey ℥ i. About half paſt 3, he took for dinner,

of ſour cream, ℥ xvj.
of bread, ℥ xj.
of ſugar, ʒ vj.

At

At 6, he made of urine ʒ iij. ; this I did not keep.

At 7, he took of bread, ʒ vj.

of milk, ʒ viij.

At half paſt 8, he ſet apart of urine ʒ iij. All his urine this day was ʒ xjx, his drink being ʒ xvj ; beſide ʒ xvj. of ſour cream, which was ſemifluid. All the three portions of this day's urine, after having been kept 48 hours, contained both cryſtals of lithic acid, and cream-coloured ſediment ; the cryſtals of lithic-acid amounted to gr. $\frac{3}{4}$.

Tueſday.

On getting out of bed, at 8 o'clock in the morning, he made water ; this I did not keep.

At 9, he took for breakfaſt,

of bread, ʒ viij.

of milk, ʒ x. ß.

of honey, ʒ i.

At

At 11, he ſet apart of urine ℥ vj. (ther. 32°)
At 3, he alſo ſet apart of urine ℥ vj. (therm. 32°). At this time, he eat of honey ℥ i.
At half paſt 3, he took for dinner,

of ſour cream, ℥ xij.
of bread, ℥ x.
of ſugar, ʒ vj.

At 6, he took of bread ℥ vij.

of milk, ℥ viij.

At half paſt 7, he ſet apart of urine ℥ vij. (ther. 33°). All his drink this day was ℔. i. ℥ ii. beſide the cream, his urine, ℔. i. ℥ v. I kept each portion of this day's urine 48 hours alſo. The appearances at the end of that time were as follow; in the morning urine I found a little cream-coloured ſediment, with ſome lithic acid; in the mid-day urine no cream-coloured ſediment, ſome lithic acid; the evening urine contained little cream-coloured ſediment, and a good

deal

deal of lithic acid. The whole lithic acid of this day's urine was gr. j. ß.

Wedneſday.

At 10 A. M., thermometer 33°; at 3 P. M. thermometer 33°.

Thurſday.

He made water on getting out of bed, as uſual, which I did not keep.

He took for breakfaſt at half paſt 9,

of fiſh, ℥ v. ß.

of water, ℥ vj.

At 11, he ſet apart of urine ℥ vij. (ther. 33°)

At 3, he ſet apart of urine ℥ viij. (ther. 33°)

He took for dinner,

of beef, ℔. ß.

He took nothing after dinner, and at 8 in the evening, he ſet apart of urine ℥ viij.; this

this day his drink was but ℥ iv.; his urine ℔. i. ℥ vij.; ſo great a check had perſpiration ſuffered by that ſickly diſguſt, of which he complained from living ſo much on animal food. After each portion of this day's urine had ſtood about 48 hours, I found in all much cream-coloured ſediment; but in none of them any lithic acid.

Friday.

Getting up as uſual at 8, he made water, which I did not keep. His breath this morning had a very ſour ſmell, and even infected his bed-room with the ſame. He now expreſſed ſo great a diſguſt for animal food, that I could not prevail on him to continue the uſe of it alone. He therefore lived to-day in the following manner.

At

At 9 in the morning, he took for breakfaſt,

of mutton, ℥ iv. ß.

of potatoes, ℥ vj.

of ſmall beer, ℥ viij.

At 11, he made of urine ℥ vij. The ſtate of the thermometer to-day was, by miſtake, not attended to; but that of the weather was not ſenſibly different from what it had been the day before.

At 3, he ſet apart of urine ℥ vj. and took for dinner,

of mutton, ℥ vj.

of potatoes,

of ſmall beer, *ſing.* ℥ viij.

He eat nothing more this night, till the experiment was ended.

At 7, he ſet apart of urine ℥ vij.; his drink to-day amounted to ℔. i. his urine to ℔. i. ℥ iv.; the ſourneſs of his breath went off towards evening. This day's u-

rine

rine was poured off, each portion about 48 hours after it had been made. The appearances were theſe : In the morning urine I found ſome lithic acid, no cream-coloured ſediment. This was evidently the effect of the great ſourneſs preſent on Friday morning ; indicated, as I have already mentioned, by the breath. In the ſecond portion of Friday's urine, I found ſome cream-coloured ſediment, and ſome lithic acid ; in the laſt portion there was no lithic acid, but a good deal of the cream-coloured ſediment. It is remarkable how quickly the urine is affected by ſourneſs in the alimentary canal. The urine, which was made in the morning when the breath was very ſour, depoſited chryſtals of lithic acid, without any cream-coloured ſediment ; the mid-day urine, made when the ſourneſs was nearly gone off, depoſited both lithic acid, and cream-coloured ſediment ; while the

even-

evening urine, made after every ſymptom of ſourneſs had been gone for ſome time, depoſited the cream-coloured ſediment, but no lithic acid. The ſandy depoſition of the two firſt portions of this day's urine amounted to gr. i. ß. On each day of the above experiment, the exerciſe was equal, and taken at the ſame time of the day.

Saturday.

Thermometer at 11, A. M., 33°; at 3, P. M. 33°; at night, 35°.

Sunday.

In the morning, thermometer 36°; in the evening, 37°.

Ex-

Experiment III.

I made this experiment on the ſame boy.

Thurſday.

He got out of bed at 8, and made water, which I did not keep.

At 9, he took for breakfaſt,

of beef, ℥ ij. ʒ ij.

of bread, ℥ i.

At 11, he ſet apart of urine ℥ iv. (thermometer 38°.)

At 3, he ſet apart of urine ℥ vj. (ther. 38°.)

At half paſt 3, he took for dinner,

of mutton, ℥ vij.

of potatoes, ℥ vj.

of ſmall beer, ℥ iv.

At

At half paſt 8, he made of urine ℥ ii. and took nothing to-day after dinner-time; at night he complained of ſickneſs, from having lived ſo much on animal food. After each portion of this day's urine had ſtood about 48 hours, in all of them there was cream-coloured ſediment, by no cryſtals of lithic acid.

Friday.

He got up at 8, and made water, which I did not keep; his breath to-day had a ſour ſmell, and infected his bed-room with the ſame.

At 9, he took for breakfaſt,

of mutton,

of bread,

of water, *ſing.* ℥ iv.

At 11, he ſet apart of urine ℥ iv. (therm. 37°)

At 3, he ſet apart of urine ℥ v. (therm. 37°)

At

At half paſt 3, he took for dinner,

of mutton, ℥ iv.

of potatoes, ℥ vj.

of water, ℥ iv.

At half paſt 7, he ſet apart of urine ℥ vj; the ſourneſs of his breath went off towards the evening. After each portion of this day's urine had ſtood about 48 hours, I found in that made in the morning about gr. $\frac{3}{4}$ of lithic acid, but no cream-coloured ſediment; in that made at 3 o'clock, P. M. gr. j. of lithic acid, no cream-coloured ſediment; in the laſt, no lithic acid, but ſome cream-coloured ſediment. The exerciſe taken on each day of this experiment was equal.

I found conſtantly in this boy, that after he had lived a day chiefly or entirely on animal food, although there were no ſymptoms of ſourneſs that night; yet by next

day theſe were always very evident; this acidity producing the ſame effects on the urine, as acid *ingeſta* do: This effect went off toward the evening, the acidity of the breath alſo going off. It appears, therefore, that the acidity was was ſome way or other produced in the night time, when the boy uſed a diet chiefly compoſed of animal food. The great check given perſpiration by the ſickly ſtate which was induced by the uſe of ſuch food, in a perſon accuſtomed to a very different manner of life, ſeems to have had a great ſhare in producing theſe effects on the urine, as will afterwards appear.

Experiment IV.

Was made on the ſame boy mentioned in the laſt.

Firſt

Firſt Day.

He got out of bed at 8 in the morning, as uſual, and made water, which I did not keep. At 9, he took for breakfaſt,

of boiled beef, ʒ iij. ß.

of water, ʒ iv. (ther. 44°).

At 11, he ſet apart of urine, ʒ viij.

At 3, he ſet apart of urine ʒ viij.

At half paſt 3, he took for dinner,

of boiled beef, ʒ v.

of water, ʒ iv.

At half paſt 8 in the evening, he ſet apart of urine ʒ viij. (thermometer 46°). He eat nothing after dinner, complaining of ſickneſs and a great diſguſt for animal food. His drink this day was only ʒ viij., while his urine was ℔ i. ʒ viij.; ſo much in this caſe alſo did his manner of living check per-

ſpiration. I examined each portion of this day's urine about 48 hours after it was made. In all there was cream-coloured ſediment, but no cryſtals of lithic acid.

Second Day.

As uſual, he got out of bed at 8, and made water, which I did not keep; he ſtill complained of a great diſguſt for animal food, and ſome degree of nauſea; his breath felt ſour. I prevailed on him, however, to live for this day as he had done yeſterday; he therefore took for breakfaſt,

of cold boiled beef, ʒ iij. ß.
of water ʒ iv.

At 11, he ſet apart of urine ʒ vj. (ther. 46°)
At 3, he ſet apart of urine ʒ vj. (ther. 46°)
At half paſt 3, he took for dinner,

of cold boiled beef, ʒ ij.
of water, ʒ iv.

He

He took nothing after dinner, till paſt 8 in the evening, at which time he again ſet apart of urine ℥ vj. (therm. 46°). All his drink this day amounted to ℥ viij.; his urine to ℔ j. ℥ ij. I poured off the three portions of this day's urine, each 48 hours after it had been made; in the morning urine I found ſome cryſtals of lithic acid; in the ſecond portion I alſo found cryſtals of lithic acid; and in the third a very few; in none was there any cream-coloured ſediment. The reſult of this part of the experiment is very ſtriking. On the firſt day, when there was no acidity preſent, all the three portions of urine depoſited the cream-coloured ſediment, but not the leaſt particle of lithic acid: on the ſecond day, when there was much acidity preſent, the urine exhibited juſt the contrary appearances; every portion of it containing cryſtals of lithic acid, but no cream-coloured ſediment. The cryſtals of

lithic acid found in the three portions of this day's urine amounted to gr. j.

Third Day.

The thermometer ſtood this day at 47°.

Fourth Day.

Having got out of bed, and made water at 8 in the morning;

At half paſt 8, he took

of lemon juice, ℥ j.

of ſugar, ℥ ß.

At 9, he took for breakfaſt,

of milk, ℥ iij.

of bread, ℥ viij.

At half paſt 11, he ſet apart of urine ℥ iij. (therm. 46°), and took

of lemon-juice,

of ſugar, *ſing.* ℥ j.

At

At 3, he ſet apart of urine ℥ iij. (ther. 46°.)
As his urine had been ſo ſcanty, I made him drink between 11 and 3 o'clock,

of water, ℥ viij.

At half paſt 3, he took for dinner,

of apple-dumpling, ℥ xij.
of ſugar, ʒ vj.

Immediately after dinner he took,

of lemon-juice,
of ſugar, *ſing.* ℥ j.

At half paſt 6, he took,

of bread, ℥ vj. ß.
of milk, ℥ viij.

At half paſt 8, he ſet apart of urine ℥ iij. (thermometer 44°).

All the urine of this day amounted to ℥ ix. his drink, including lemon-juice, to ℔ i. ℥ xjv. So much had the lemon-juice, and vegetable diet increaſed the excretion by the ſkin, for he had no ſtool this day. If we compare the proportion the drink bears to the urine

on

on this day, to what it bore to that excretion on the two former days, we ſhall perceive a very ſtriking difference indeed. I poured off each portion of this day's urine, 48 hours after it was made, and found in each a little lithic acid mixed with much cream-coloured ſediment.

Fifth Day.

He lived exactly as yeſterday, each meal conſiſting of exactly the ſame food; he alſo took the lemon-juice and ſugar, as yeſterday, and in the ſame quantity; at 11 o'clock A. M. he ſet apart of urine ℥ iij.; at 3, P. M. of urine ℥ iv.; and at half paſt 8, of urine ℥ v. His belly was ſtill rather bound; (therm. in the morning 44°, in the evening 45°), his urine this day amounted to ℥ xij.; his drink, which was always water, (except where the contrary is mentioned), being

being ℔ j. ℥ iij. ; his exercise was equal on each day of this experiment. I poured off each portion of this day's urine 48 hours after it was made, and found in all of them much cream coloured-sediment, but no crystals of lithic acid.

Sixth Day.

Thermometer during this day 45°.

The result of this experiment may appear at first sight singular ; but if we consider that the lemon-juice and vegetable diet acted here as powerful diaphoretics, and consequently that the acid passed by the skin (for it will afterwards be shown that this organ secretes an acid from the blood, even by insensible perspiration) we shall not find it contradict the result of any of the other experiments. I also found that a quantity

of

of apples produced the ſame effects on this boy, whoſe perſpiration was naturally very vigorous. And on making him eat a conſiderable quantity of honey, (two ounces twice a-day) and at the ſame time live on vegetable food, he complained of acid eructations; and it was evident that this acid alſo acted in the ſame manner as the fruit had done; his urine being very ſcanty, when compared to the quantity of drink he took; containing much cream-coloured ſediment, and few or no cryſtals of lithic acid.

This experiment clearly ſhows how little aceſcent *ingeſta* prediſpoſe to gravel, where the action of the ſkin is vigorous. And from this, as well as the two preceding experiments, and one ſtill more deciſive, afterwards to be mentioned, we learn, that no abſtinence from ſuch food takes off the diſpoſition to that diſeaſe, when the action of the

the ſkin is much diminiſhed. Theſe facts have been overlooked by phyſicians; I ſhall therefore ſoon endeavour to confirm them by repeated experiments, and ſhow, in the ſequel, of how much importance they are in giving the pathology of gravel. For a little, however, I return to the preſent ſubject, *h. e.* by further experiments to prove, that, *cæteris paribus*, aceſcent *ingeſta* increaſe the depoſition of lithic acid from the urine.

Experiment V.

This was alſo made on the ſame boy.

Monday.

He got up as uſual at 8 in the morning, and made water, which I did not keep; At 9, he took for breakfaſt,

of

of beef,
of potatoes,
of ſmall beer, *ſing.* ℥ iv.

At 11, he ſet apart of urine, ℥ iij. (ther. 39°).

At 3, he took for dinner,
of ſalt fiſh,
of potatoes,
of ſmall beer, *ſing.* ℥ iv.

At 6, he ſet apart of urine ℥ ix. (therm. 38°), having made none ſince 11, about 48 hours after they were made, I examined each portion of this day's urine, and found in both ſome cream-coloured ſediment, but in neither any cryſtals of lithic acid.

Tueſday.

He lived this day in his ordinary manner, (*h. e.* eating animal food once a-day) that the effects of his diet on the former might go off.

Ther-

Thermometer this day 35°.

Wedneſday.

He got up at 8, and made water, which I did not keep.

At 9, he took for breakfaſt,

of bread,

of milk, *ſing.* ℥ viij.

At 11, he ſet apart of urine ℥ iv. (ther. 33°)

He took for dinner,

of apple dumpling, ℥ xvj.

of ſugar, ℥ j.

At 6, he ſet apart of urine ℥ viij. having made none ſince 11, as on Monday; (ther. 32°) his exerciſe on each day of this experiment was equal. After each portion of this day's urine had ſtood about 48 hours, I found in both ſome cream-coloured ſediment, and a depoſition of lithic acid, which amounted, on the whole, to nearly gr. j.

Thurſday.

Thurſday.

Thermometer 32°.

Friday.

Thermometer 29°.

Experiment VI.

This experiment was made on a young man about 20 years of age, and in good health.

He breakfaſted, dined, and ſupped entirely on vegetable matters and milk; at breakfaſt time he eat a lemon; at dinner-time another, and a third in the evening. At 6 o'clock in the evening, he ſet apart a certain portion of urine, and at 10, another; after each

each had ſtood 24 hours, there was depoſited from them of lithic acid gr. ij.

Next day he eat no lemons, and dined chiefly on animal food ; at the ſame times of the day he ſet apart the ſame quantities of urine. After each of theſe had ſtood 24 hours, neither had depoſited any ſand at all, ſo much had his manner of living on the firſt day increaſed the diſpoſition of his urine to depoſite the lithic acid.

His exerciſe was about equal on each day of this experiment.

Experiment VII.

This was made on the ſame perſon.

Firſt

Firſt Day.

He breakfaſted on beef and bread. For dinner he eat of the fleſh of a pig, beef, and bread. For ſupper, beef and bread. He ſet apart no urine this day.

Second Day.

He did not complain of his manner of living, nor was there any ſourneſs produced, as had taken place in the boy, from a ſimilar diet.

He took for breakfaſt, beef and bread; for dinner, of the fleſh of a rabbit, beef, and bread.

Morning, mid-day, and evening he ſet apart of urine ℥ iv. (thermometer this day 39°). After each had ſtood about 48 hours there was in all, ſome cream-coloured ſediment,

ment, in none of them any cryſtals of lithic acid.

Third Day.

Thermometer 39°.

Fourth Day.

Thermometer 35°.

Fifth Day.

Having eat a lemon laſt night, he lived this day entirely on vegetable matters; except that at dinner-time he took ſome broth, in which fleſh had been boiled: he alſo eat two lemons. He ſet apart no urine this day.

Sixth Day.

This day he lived as yeſterday, eating 3 lemons, one in the morning, a ſecond at mid-day, and a third in the evening; at which times alſo he ſet apart portions of urine, each as formerly, ℥ iv. (thermometer this day 39°). Having examined theſe portions of urine about 48 hours after they were made, I found in all of them cryſtals of lithic acid; which, put together, amounted to about gr. j. ß. In none of them was there any cream-coloured ſediment.

Seventh Day.

Thermometer 39°.

Eighth

Eighth Day.

Thermometer 39°.

His exercise was equal on each day of this experiment.

Experiment VIII.

First Day.

This experiment was made on myself, when in good health. Having made water at 8 o'clock on getting up, which I did not keep,

At 9, I took for breakfast,

of beef,

of water, *sing.* ℔. ß.

At 12, set apart of urine ʒ vj. (therm. 37°).

Took about this time of water ʒ iv.

At 3, took for dinner,

of fiſh, ℥ vij.
of beef, ℥ ij.
of water, ℥ vj.

At 5, ſet apart of urine ℥ iij. (therm. 37°).

This day my drink and urine were nearly equal; the depoſitions of this day's urine are mentioned with thoſe of the next.

Second Day.

Having got up about 8 in the morning, I made water, which I did not keep.

At 9, I took for breakfaſt,
of milk and water, ℥ x.
of bread, ℔. j.

At 11, I eat a lemon; and at 12 another.

At 1, I ſet apart of urine ℥ vj. (therm. 35°)

Took for dinner,
of milk,
of bread, *ſing.* ℔. j.

At

At 5, ſet apart of urine ℥ iij.

The urine of this day was nearly equal to the quantity of liquid taken. Exerciſe on both days equal.

Two days afterwards, I examined all theſe portions of urine at the ſame time; in thoſe of the firſt day, although they had ſtood three days, I found only 5 cryſtals of lithic acid; in the ſecond day's urine which had ſtood two days, I found about 120 ſuch cryſtals; in neither day's urine was there any cream-coloured ſediment.

The experiments which have now been related, are ſufficient to ſhow, that conſiderable changes in the manner of living, produce very evident changes in the ſtate of the urine; but theſe appear from more trifling changes of diet; having repeatedly ob-

ſerved, that a ſingle meal or two, more or leſs aceſcent than uſual, (provided it were ſo to a conſiderable degree) affects very ſenſibly the ſtate of the urine.

It is ſufficient to relate the following inſtances, which I have ſeen confirmed by a great number of others.

Experiment IX.

This was made on a young man aged 19 years, and in good health; living partly on vegetable, and partly on animal food; he ſet apart about 2 o'clock in the afternoon, a portion of urine; next day he breakfaſted as on the foregoing, and took about the ſame degree of exerciſe; after breakfaſt he eat about ℔. j. ß. of apples; and at 2, as on the

the preceding day, ſet apart the ſame quantity of urine. After each had ſtood about 72 hours, I found in the latter of lithic acid gr. ij., in the former of the ſame, gr. j.

Experiment X.

This was made on myſelf, when in good health.

I ſupped chiefly on bread and milk. Next morning I breakfaſted on the ſame; and after breakfaſt eat ſome aceſcent fruit. About 2 o'clock in the afternoon, I ſet apart a portion of urine, which, after ſtanding 24 hours, had depoſited of lithic acid gr. j.

Next night I ſupped entirely on beef and bread; the following morning I breakfaſted on the ſame; and at 2 o'clock P. M. ſet apart a por-

a portion of urine equal to what I had done the day before. After ſtanding 24 hours, it had depoſited no cryſtals of lithic acid.

Having related a ſufficient number of experiments to prove that acidity of the *primæ viæ (cæteris paribus)* increaſes the tendency of the urine to depoſite the lithic acid; I now proceed to ſhow, that, by whatever means we increaſe the excretion by the ſkin, the quantity of lithic acid found in the urine is diminiſhed, and ſometimes totally abſtracted. Before I proceed to this, however, I ſhall relate the following experiment, as it renders the reſult of thoſe I am afterwards to mention more concluſive.

Ex-

Experiment XI.

This I have not made either to convince myſelf or others of a fact every one is ready to grant, viz. that diluents, *cæteris paribus*, will diminiſh the quantity of lithic acid found in any given portion of urine.

But, as it is neceſſary to take nothing on this ſubject for granted; I made the following experiment, which I have accidentally had frequent occaſion to repeat with a ſimilar reſult.

On the night before this experiment, I drank ℔. j. of milk, and another of water; next morning, on getting out of bed I ſet apart a certain portion of urine; from this time till between 2 and 3 o'clock in the afternoon,

I drank ℔. j. of milk, and iij. of water, and at this time ſet apart a portion of urine. After each of theſe had ſtood 48 hours, the ſediment of lithic acid found in them amounted to gr. $j\frac{1}{4}$. A night or two afterwards I drank ℔. j. of milk, and next morning ſet apart a portion of urine equal to that ſet apart on the firſt day. From this time till between 2 and 3 o'clock in the afternoon, I drank ℔. j. of milk, and ℔. ß. of water, and at this time again ſet apart a portion of urine equal to that of the former day. After each had ſtood 48 hours, they contained a ſediment of lithic acid, amounting to gr. $ij_{\frac{1}{4}}$.

My exerciſe and diet were ſimilar on each day of this experiment.

I meant now to have tried the effect of exciting the action of the kidney by diuretics,

tics, and for this purpoſe took a quantity of cream of tartar. But I ſoon found that I could draw no concluſion from ſuch an experiment: for if the urine were not much increaſed, we could not be certain of the diuretic having taken effect; if it were, there would be too much watery liquor ſeparated, to leave it poſſible to judge with any degree of certainty concerning the quantity of lithic acid it contained. But I am inclined to think, for reaſons which will afterwards appear, that increaſing the action of the kidney by diuretics, is a more probable method of preventing the formation of gravely concretions, than the moſt careful employment of diluents.

I now proceed to ſhow, that diaphoretics leſſen the depoſition of lithic acid from the urine; and ſhall begin with *exerciſe*, which

is

is by all allowed to be a moſt powerful diaphoretic.

Experiment XII.

The following experiment not only ſhows how much the depoſition of lithic acid from the urine is increaſed by indolence; but alſo that this matter may ſeparate from the urine in conſiderable quantity, independent of all acid *ingeſta*.

About a year and a half ago, I was attacked by rheumatiſm, chiefly confined to the right ſide of my head, and right ſhoulder, unaccompanied with any degree of pyrexia. This affection was ſo ſevere, and continued for ſo long a time, that it confined me to bed for near five weeks, during which time I lived on beef-tea, and calf-foot gelly; any

other

other food aggravating the pain of the head to a great degree. Yet, during this confinement, my urine depoſited much more lithic acid than when I had taken my uſual exerciſe, and lived on much more aceſcent diet. Having kept about half a pound of it 24 hours, I found that it had depoſited about gr. ij. of lithic acid. This I repeated at leaſt half a dozen of times with a ſimilar reſult. After I had recovered from my indiſpoſition, and renewed my uſual exerciſe; I found that the above quantity of urine, kept the ſame time, depoſited about gr. $\frac{3}{4}$ of lithic acid, often leſs, and hardly ever more. This I alſo often repeated; nor indeed have ever found my urine depoſite ſo great a quantity of lithic acid as gr. ij. from half a pound in 24 hours; while I was taking exerciſe, however aceſcent my diet was. This fact might perhaps be partly attributed to the kidney, during indolence, ſeparating leſs watery

watery liquor, and hence more lithic acid in a given portion of urine. But that the appearance is not explained by ſuch a ſuppoſition, is clear from this, that, with a view of determining the point, having taken much diluent liquor, I ſtill found my urine depoſite more lithic acid than when I was taking exerciſe and leſs diluent liquor. That we muſt attribute the appearance juſt now mentioned to the check given perſpiration by indolence, will appear evident, from what is related in Experiment xiv.

Theſe obſervations account for the following remark of Scheele's : (it is made in the laſt number of his treatiſe on the calculus veſicæ) " It is remarkable, (ſays he,) that " the urine of the ſick is more acid, and " contains more animal earth than that of " healthy perſons."

The

The reſult of the experiment now related, is confirmed by the following.

Experiment XIII.

When in good health, I repeated the laſt experiment, as follows. I purpoſely remained at home two days without exerciſe, and found that half a pound of urine made on the ſecond day, and kept 24 hours, depoſited near gr. ij. of lithic acid, *h. e.* above double the quantity it did, when I was taking exerciſe, and uſing a ſimilar diet. Having had often occaſion to be confined ſince I began theſe experiments, either by buſineſs or indiſpoſition, I have ſeen the reſult of the two laſt confirmed a great number of times; ſo that I muſt look upon it as well aſcertained, that, *cæteris paribus*, the quantity of lithic acid depoſited by the urine, is inverſely as the exerciſe taken.

Nor

Nor is this all; for I have conſtantly obſerved that, continuing in indolence, my urine not only depoſited more lithic acid than uſual in the mean while, but continued to do ſo for ſome time after I had returned to exerciſe. This I particularly attended to, in the two caſes mentioned; in the latter of which, for two days after returning to exerciſe, my urine depoſited more than its uſual quantity of lithic acid, and in the former, for no leſs a time than upwards of two weeks. Theſe appearances are much connected with the ſtate of the ſtomach; hence probably they are more remarkable in thoſe whoſe ſtomachs are moſt readily affected by indolence: but there are alſo other cauſes acting here, which tend to produce the above mentioned change on the urine, and which will appear, I hope, fully explained in the ſequel of this diſſertation. I ſhall only at preſent remark, that this effect of indolence

dolence cannot be accounted for, by ſuppoſing that the weakneſs of the ſtomach produces much acid in the *primæ viæ*, (which has already been ſhown to increaſe the depoſition of lithic acid from the urine), and hence a greater than ordinary quantity of lithic acid in the urine; for in Experiment xii. we have ſeen the ſame effect take place where little aliment was uſed, and that entirely animal, and where there was not the leaſt ſymptom of acidity preſent.

Experiment XIV.

In this, and the three following experiments, it is ſhown, that ſudorifics or medicines promoting ſenſible perſpiration, diminiſh the quantity of lithic acid found in the urine.

In the illneſs I have already mentioned, (experiment xii.) I repeatedly found that half a pound of urine, when kept 24 hours, depoſited about gr. ij$\frac{1}{4}$. of lithic acid, although, as was formerly obſerved, my diet was not in any degree aceſcent. With a view of removing my complaint, I took ʒ j. of Dover's powder. After I had ſubmitted myſelf to the briſk operation of this ſudorific for 12 hours, while the ſweat ſtill flowed copiouſly, I ſet apart ℔. j. of urine, all I had made this day, which, after ſtanding 24 hours, had depoſited no lithic acid at all. I again examined my urine after the effects of the ſudorific were over, and found, that it now depoſited as much lithic acid as before I had taken the Dover's powder. The above change, therefore, on the urine, I could attribute only to the increaſed action of the ſkin. I had drunk, indeed, that day more than I uſed to do, but my urine was

not

not more diluted; for its quantity was not greater than uſual, the ſuperfluous moiſture running off by the ſkin.

Experiment XV.

This I had an opportunity of making on a man aged 50, and who had at that time rheumatic pains in the joints of the lower extremities.

In the morning he took ℈ j. and gr. vj. of Dover's powder. In about half an hour the ſweat broke out, and continued to flow freely all day; as he did not complain of thirſt, I allowed him to take only his uſual quantity of drink, which did not much exceed ℔. j. He had now been in a profuſe ſweat from 9 in the morning till 6 in the evening, at which time he ſet apart ℥ vij. of urine, while the ſweat ſtill flowed copi-

ouſly. This I kept for 48 hours, without finding that it had depoſited any lithic acid; I found in it, however, much cream-coloured ſediment; his urine this day was leſs than half the uſual quantity.

It was neceſſary to compare his urine in its natural ſtate with this; next day, therefore, when he was going about, as uſual (for his complaints did not confine him), at the ſame time of the day (6 o'clock, evening) he ſet apart a ſimilar portion of urine; this I alſo kept for 48 hours, and found in it at the end of that time, a copious ſediment of lithic acid, weighing above gr. iij. He had taken about the ſame quantity of liquid this day, as on the former, his urine being much more copious.

Ex-

Experiment XVI.

About 8 months after I recovered from the above mentioned illneſs, I ſubmitted myſelf to the following experiment, when in good health.

On an evening I took gr. ij. of Dover's powder. Next morning I took gr. vj. more; about half an hour after this, the ſweat broke out, and continued as I took a little warm drink, till 7 o'clock in the evening. Between 3 and 4 in the afternoon, during the ſweat, I ſet apart a portion of urine, and between 6 and 7, another, while the diaphoreſis ſtill continued. From morning till this time, I had drunk lb. ij. of milk and water; my urine being only lb. j. Each of theſe quantities of urine I examin-

ed, after it had ſtood 24 hours, and found, that the lithic acid which they contained, amounted only to gr. $\frac{1}{4}$.

Next day, I treated myſelf in the ſame way; except that I underwent no diaphoreſis. I took no exerciſe, and drank about ℔. ij. of milk and water: my urine this day did not exceed ℔. j. during the time of the experiment, viz. from 3 in the morning till 7 in the evening. The ſmall quantity of urine was probably owing to the great abſtraction of moiſture, which had taken place the day before; it was fully equal, however, to what I had made that day; my food alſo this day was ſimilar to what it had been on the former. At the ſame times of the day, I ſet apart ſimilar portions of urine; on examining them, each 24 hours after it had been made, I found in them a depoſition of lithic acid, weighing gr. iij. or 12

times

times the other depoſition. Notwithſtanding the greater depoſition from the urine of the laſt day, that of the firſt was darkeſt coloured.

Experiment XVII.

This was made on the man mentioned in Experiment xv.; after he was recovered from his late complaints, and going about his uſual occupation as a day-labourer; only troubled with two or three biles, a topical affection, and conſequently of no importance in influencing our concluſions from the following experiment.

On Sunday morning, he took ʒ j. of Dover's powder After he had ſweated 5 hours, he ſet apart a certain portion of urine. From morning till this time, he had drank rather more

more than uſual ; but his urine was hardly equal to what he generally made.

On Tueſday, when going about his uſual buſineſs, he ſet apart, at the ſame time of the day, another portion of urine. Both of theſe I examined on Wedneſday morning. Now the whole urine made on Sunday, was hardly equal to that made on Tueſday ; but the quantity ſet apart on Sunday was greater than that ſet apart on Tueſday. He had taken no exerciſe on the former day, on the latter he had employed this liberally. The urine ſet apart on Tueſday had not ſtood 24 hours ; that ſet apart on Sunday, nearly 3 days. Yet in this I found only 21 particles of lithic acid, a quantity hardly ſufficient to turn a nice balance ; while, in the Tueſday's urine, there was a depoſition of lithic acid, weighing gr. j. ß., a dozen or more times the other depoſition.

The caſes now related ſhow, in the moſt unequivocal manner, that, by producing ſweat, we diminiſh the quantity of lithic acid found in the urine. Although theſe are the only caſes of this kind I have notes of; yet I have ſeen the ſame effect produced by ſudorifics at other times, having never examined the urine, during their operations, without obſerving it.

The following experiments were made with *diaphoretics*, or medicines increaſing inſenſible perſpiration only.

Experiment XVIII.

The perſon mentioned in Experiment ix. underwent the following; when in good health.

He

He took in ſmall doſes, from 12 o'clock noon, till 6 in the evening, gr. j. ß. of tartar emetic. At 10, in the ſame evening he ſet apart a portion of urine. This ſtood 24 hours without depoſiting any lithic acid.

Next night, at the ſame time, he ſet apart an equal portion of urine; after it had ſtood only 12 hours, I found in it a ſediment of lithic acid.

He uſed ſimilar diet and exerciſe, on each day of this experiment.

Experiment XIX.

This was made on the boy, whom I have already frequently mentioned, ſtill in good health.

Having

Having examined his urine in its ordinary ſtate; I found too little lithic acid depoſited from it to enable me to draw any certain concluſions, even from its total abſtraction; I therefore performed this experiment on him in the following manner.

When living in his uſual way, except that he took rather leſs exerciſe, I made him eat four apples after breakfaſt; and, about 2 o'clock P. M. ſet apart a portion of urine; after this had ſtood 24 hours, it had depoſited gr. j. of lithic acid.

Next day, he lived exactly as on this, except that he took from morning till noon, in ſmall doſes, gr. j. of tartar emetic; which produced no ſenſible effect. About 2, P. M. he again ſet apart a portion of urine, equal to what he had ſet apart the day before;

after

after this had ſtood 24 hours, I found it had depoſited no lithic acid at all.

Experiment XX.

The following experiment was made on myſelf, while in good health.

Living as uſual, but taking rather leſs exerciſe; I ſet apart, about 1 o'clock, P. M. a portion of urine, which, after ſtanding 24 hours, had depoſited a little more than gr. ij. of lithic acid.

Next day I took, in ſmall doſes, from morning till mid-day, gr. j. of tartar emetic; this occaſioned ſuch a degree of nauſea, that I felt a cold ſweat on the forehead; but there was no ſenſible perſpiration on any other part of

of my body. On this day I took no exercise, that the result of the experiment might be the more striking; living in every other respect as on the former day, the liquid I took being equal, but my urine less. On this day also, I set apart a portion of urine at 1 o'clock P.M. equal to what I had the day before. After this had stood 24 hours, I found that it had deposited only two or three particles of lithic acid, a quantity hardly visible, had it not been collected in one part of the vessel, and not to be measured by the nicest balance.

Experiment XXI.

Living as usual, but taking rather less exercise, I set apart about mid-day a portion of urine, which, after standing 24 hours, had deposited gr. ij. of lithic acid.

Next

Next day I took a ſmaller quantity of tartar emetic than I had done in the laſt experiment, not much above half a grain, which hardly produced any nauſea: and lived in every other reſpect as on the firſt day of this experiment. About mid-day, I ſet apart a portion of urine equal to what I had the day before; which, after ſtanding 24 hours, had depoſited no lithic acid at all.

On conſidering the four laſt experiments, it occurs, that, by increaſing inſenſible perſpiration, we have a more convenient method of preventing the urine from depoſiting its lithic acid, and not only this, but a more efficacious one; for I have always obſerved, that a ſmall doſe of tartar emetic more certainly prevents the depoſition of lithic acid, than a large one of Dover's powder, producing a copious ſweat. This I would account for on the following principles: It is ſhown,

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in the 3d chapter, from repeated experience that the ſecretion of the matter occaſioning the depoſition of lithic acid from the urine, depends not upon the mere relaxation of the kidney, but upon its vigorous action. I ſhould imagine then, that the ſame thing takes place in the ſkin, and that this matter is only ſeparated by it, in proportion to its activity. (For it will afterwards clearly appear, that the matter occaſioning the depoſition of lithic acid from the urine, paſſes alſo by the ſkin; and indeed, from the experiments already related, we can hardly ſuppoſe otherwiſe.) Now Dover's powder, although it may in ſome degree increaſe the action of the ſkin, yet we muſt ſuppoſe that its ſudorific effect is in a great meaſure to be attributed to the relaxation induced on that organ, by the opium it contains; whereas antimony acts only by promoting the activity of the ſkin; and hence ſeems to proceed the pro-

perty

perty it has, of throwing off the noxious matter in ſo remarkable a degree. There is alſo another difference in the manner in which theſe medicines affect the urine. While the Dover's powder produced in general no effect on the urine after the ſweat had ceaſed to flow; the antimony continued, for ſeveral days after it was taken, in a greater or leſs degree, to affect that excretion. There is ſtill another fact with regard to antimony, which I have repeatedly obſerved, viz. that the depoſition of lithic acid from the urine was not ſo effectually prevented by this medicine, when it produced nauſea, as when it produced no ſenſible effect on the body. I would account for this on the principles above mentioned; for although nauſea produces ſweat, this is evidently owing to the relaxation it induces on the ſkin; and a little reflexion will ſhow, from the intimate connexion between the ſkin and ſtomach,

that

that we cannot ſuppoſe the one in a ſtate of vigorous action, while the other is affected in a contrary manner; for nauſea never tends to increaſe the action of the ſtomach, but evidently to diminiſh it; and indeed it affects in the ſame manner every function of the body, vital, natural, and animal.

Of all the medicines phyſicians are acquainted with, there is none which more uniformly and effectually ſupports the excretions than mercury. By proper treatment, we can generally direct its operation to the ſkin; in which circumſtances, it proves a ſafe and powerful diaphoretic. On this account, I wiſhed to try its effects on the urine; which I had an opportunity of doing, in the following manner.

Experiment XXII.

A young gentleman of my acquaintance contracted a ſlight *ſyphilitic* affection, for which he was obliged to have recourſe to mercury. He was a very proper ſubject for my obſerving the changes induced on the urine by this medicine, as I had had occaſion to examine his urine in all ſituations, when he was in health; at leaſt 50 or 60 different times; ſo that I knew perfectly what changes to expect from different manners of life. I found the ſtate of his urine at all times much affected by indolence; half a pound, when he remained at home, depoſiting about gr. ij. of lithic acid in 24 hours, although his diet was not more aceſcent than uſual.

When

When rubbing in ʒ j. of mercury each day, ſtaying at home without exerciſe, and living on vegetables alone, he ſet apart ℥ iv. of urine; this I kept 48 hours, and found in it no lithic acid, but a conſiderable quantity of cream-coloured ſediment. At this time his urine was leſs than uſual. From treating his affection careleſsly, although at firſt a very ſlight one, he found it neceſſary to continue the uſe of mercury for no leſs a time than three months, during the whole of which time I carefully examined the ſtate of his urine; and conſtantly found, that, when it was much leſſened in quantity, it depoſited no lithic acid, but much cream-coloured ſediment. For the firſt week or two, his urine was not above the half of its uſual quantity; as his ſtomach, mouth, and general health, however, became affected by the mercury, the above appearance began to go off: hence it is evident, that the mer-

cury acted at first as a diaphoretic; this effect ceasing as the debility of the system increased; and particularly that of the stomach; the vigorous action of which is ever necessary for promoting the activity of the skin.

These appearances I saw take place a second time. He was persuaded to give up the mercury for a little, and try the effects of a pretty full diet. He became better in his general health; (for the affection under which he laboured was too trifling to affect this,) and, on returning to the use of mercury, the same scanty urine took place, together with the same deficiency of the lithic acid, and increase of the cream-coloured sediment in that fluid. These effects, however, were neither so great nor permanent at this time as formerly; his stomach too, and general health became sooner affected.

After

After he had remained at home for about three weeks, the mercury ſeemed to act as a diuretic. The urine was then of a lighter colour, in a greater than uſual quantity, and depoſited leſs lithic acid; undoubtedly owing to the greater proportion of watery liquor preſent, as more than uſual ran off by the kidneys.

Before I leave this caſe, it is neceſſary to remark, that when the urine was ſo ſcanty, the belly was rather bound than otherwiſe. I alſo found in the perſon now mentioned, that applying mercury in the form of ointment to the ſkin, tended more to promote perſpiration, than mercury taken by the mouth; although this did not produce the leaſt cathartic effect; a circumſtance probably owing to the mercury taken in this way, producing a greater degree of dyſpepſia; which, in this patient, it always did.

Such were the appearances repeatedly obſerved in the caſe now mentioned. The effects of mercury on the urine, when acting as a diaphoretic, are alſo ſeen in the following experiment.

Experiment XXIII.

This was made on the boy whom I have frequently had occaſion to mention, ſtill in good health.

Firſt Day.

On getting out of bed, at 8 o'clock in the morning, he made water, which I did not keep.

At 9, he took for breakfaſt,

of bread,

of

of milk, *ſing.* ℥ viij.

of honey, ℥ j.

At 11 in the forenoon, he ſet apart of urine ℥ vj. (thermometer 39°).

At 3, he ſet apart of urine ℥ vij. (thermometer 39°).

At this time, he eat of honey, ℥ j.

At half paſt 3, he took for dinner,

of ſour cream, ℔. j.

of bread, ℥ xj.

of ſugar, ʒ vj.

At 6, he made of urine ℥ iij.; this I did not keep.

At 7, he took,

of bread, ℥ vj.

of milk, ℥ viij.

He ſet apart at half paſt 8, of urine ℥ iij.

All his urine this day was ℥ xjx. his drink ℔. j., and ℔. j. of ſour cream which was ſemifluid. I poured off each portion of this day's urine, after it had ſtood 48 hours, and found

found in all ſome cream-coloured ſediment, and ſome lithic acid; this laſt amounted to gr. $\frac{3}{4}$.

Second Day.

He made water, on getting out of bed, at 8, as yeſterday; this I did not keep.

At 9, he took for breakfaſt,

of bread, ℥ x. ß.

of milk, ℥ viij.

of honey, ℥ j.

At 11, he ſet apart of urine ℥ vj. (thermometer 32°).

At 3, he ſet apart of urine ℥ vj. (thermometer 33°).

At this time, he took of honey ℥ j.

At half paſt 3, he took for dinner,

of ſour cream, ℥ xij.

of bread, ℥ x.

of ſugar, ℥ j.

At

At 6, he took,

of bread, ʒ vij.

of milk, ʒ viij.

At half paſt 7, he ſet apart of urine ʒ vij. (thermometer 33°).

He took no more to-day of any thing. His urine this day was ʒ xjx.; his drink ℔. j. beſide the cream. After each portion of this day's urine had ſtood 48 hours, I found in all of them a little cream-coloured ſediment, with a conſiderable quantity of lithic acid, amounting to gr. j. ß.

Third Day.

Thermometer this day 33°.

Fourth Day.

On the evening of this day, he took a mercurial pill; which he continued to do morning and evening, for four days.

Fifth

Fifth Day.

This was the morning of the fifth day ſince he began to take the mercury, which he continued to do to the end of the experiment.

He got out of bed, as uſual, and made water at 8 o'clock in the morning, which I did not keep.

At 9, he took for breakfaſt,

of bread,

of milk, *ſing.* ℥ viij.

of honey, ℥ j.

At 11, he ſet apart of urine ℥ v. (thermometer 38°).

At 3, he ſet apart of urine ℥ v. (thermometer 38°).

His mouth to-day was no affected.

At 3, he eat of honey ʒ j. ;

At half paſt 3, he took for dinner,

of apple-dumpling, ℔. j. ʒ v.

of ſugar, ʒ j.

At half paſt 5, he made of urine ʒ iij. ; this I did not keep. At this time he had one natural ſtool.

At 6, he took of bread, ʒ iv.

of milk, ʒ viij.

At 8, he ſet apart of urine ʒ ij. his whole drink this day amounted to ℔. j. not including a conſiderable quantity of moiſture in the dumpling, his urine to ʒ xv. ; he took nothing more to night but ʒ iv. of milk. After each portion of this day's urine had ſtood 48 hours, I found in all of them cream-coloured ſediment ; but in none was there any lithic acid.

Sixth

Sixth Day.

His mouth to-day was a little affected, but there was no ſenſible ſalivation.

He got up this morning as uſual, making water at 8, which I did not keep.

At 9, he took for breakfaſt,

of bread,

of milk, *ſing.* ℥ viij.

of honey, ℥ j.

At 11, he ſet apart of urine ℥ iij.

At 3, he ſet apart of urine ℥ vj. (therm. 36°)

At this time he took of honey ℥ j.

At half paſt 3, he took for dinner,

of apple-dumpling, ℔ j.

of ſugar, ℥ j.

At 6 in the evening, he took

of bread, ℥ vj.

of milk, ℥ viij.

of water, ℥ iv.

At

At 8, he ſet apart of urine ℥ iv. (therm. 35°) He had one natural ſtool to-day.

All his drink this day was ℥ xx. beſide the moiſture in the dumpling; his urine only ℥ xiij. I examined each portion of this day's urine 48 hours after it had been made; in the morning urine there was ſome lithic acid; and a ſmall quantity of cream-coloured ſediment: the lithic acid in this amounted to gr. ß. In the other two portions there was no lithic acid, but ſome cream-coloured ſediment.

Seventh Day.

Thermometer in the morning, 35°; in the evening, 34°.

The experiment now related, would have been even more concluſive on a perſon whoſe perſpiration

perſpiration was not ſo eaſily promoted; for in this boy the vegetable diet alone was ſufficient to produce this effect in a remarkable degree.

It is neceſſary that I ſhould now ſay ſomething of the excretion by the ſkin. M. Bertholet found that ſweat contained an acid; and there are many reaſons which would incline us to believe that an acid alſo paſſes by inſenſible perſpiration; that this ſuppoſition is well founded, appears from the following experiment.

Experiment XXIV.

I TIED a piece of paper ſtained with litmus about my neck. After it had remained there 8 hours, during which time there was no

no ſenſible perſpiration on any part of my body, I found it changed to a red colour. This I again repeated; allowing the paper to remain applied only about 4 hours; and after that time found it alſo changed to a red. In making this experiment, I preſerved a piece of the ſtained paper torn from what was applied, that by comparing the two pieces at the end of the experiment, the reſult might be the more deciſive.

Having now finiſhed the narration of theſe experiments, and the more particular obſervations which I intended making in this chapter, I proceed to what was propoſed as the ſubject of the following.

CHAP.

CHAP. II.

Some Obſervations concerning the Depoſitions of the Urine, and the Manner in which Acids act on that Fluid.

THERE are ſtill ſome circumſtances to be aſcertained, in order to ſet the reſult of the foregoing experiments in a clearer point of view. I mean the manner in which acids act on the urine, after it is out of the body; and the nature of thoſe depoſitions, which I have had ſo frequently occaſion to mention.

On the firſt of theſe heads I learned the following curious fact from an anonymous author.

author, after the treatiſe I am now laying before the public, was nearly completed: This author mentions, that on adding any acid, even the carbonaceous, to urine, he always procured a copious depoſition of what he calls the concreting acid, which is the ſame matter I have mentioned under the name of the lithic acid.

This experiment I have repeated frequently, both with recent urine, and that which had been kept ſome time; uſing the vitriolic, nitrous, muriatic, acetous acids, or the acid of lemons; and in all inſtances found the reſult as the above mentioned author has ſtated it. On this ſubject I can add the following particulars, from repeated experiments.

Urine left to itſelf, depoſites either a whitiſh matter, rendering it muddy, and this often

ten, in an hour or two after the urine is made; or cryſtals of lithic acid; or ſometimes both. From the number of experiments I have had occaſion to make on this ſubject, I have had it in my power to obſerve, in the cleareſt manner, the different circumſtances of life which produced the one or other depoſition. The obſervations I am now to mention, are ſupported by all the experiments I have related, where the cream-coloured ſediment is taken notice of; the reaſon why it is not always ſo is, that, when I firſt began theſe experiments, I neglected it, as an accidental appearance; and it was not till it had very frequently occurred, that I paid particular attention to it, in order to diſcover by what laws its appearance in the urine was regulated.

In the *firſt* place, then, with regard to theſe two depoſitions, I never found both exiſting

exiſting in any conſiderable quantity in the ſame urine, but always obſerved, that where there was much of either, there was little or none of the other; from this, I was led to ſuppoſe, that the preſence of the one was to a certain degree incompatible with that of the other. This opinion is confirmed by what I am ſoon to mention. *Secondly*, While the lithic acid was found in greateſt quantity in the urine of a perſon living on an aceſcent diet; the cream-coloured depoſition was increaſed by food of a contrary tendency. *Thirdly*, Any cauſe encreaſing perſpiration, while it diminiſhes the quantity of lithic acid, tends to produce the cream-coloured ſediment in the urine. *Fourthly*, The cream-coloured ſediment is more ſoluble in the urine than the lithic acid. *Laſtly*, The lithic acid is leſs eaſily acted on by acids than the cream-coloured ſediment.

From theſe circumſtances, as well as other conſiderations, we infer, that it is the lithic acid which is apt to form concretions in the urinary paſſages ; ſince it is leſs ſoluble, and more apt to concrete than the other, and is produced by that manner of life which experience has taught us, is apt to induce calculous complaints. We muſt alſo infer from them, that the ſecretion of any acid matter by the kidneys, tends to produce a depoſition of lithic acid ; and at the ſame time to prevent that which, from its appearance, I have called the cream-coloured ſediment : this I confirmed by experiment ; for, I always found, that the addition of an acid to the urine, while it produced a depoſition of the lithic acid, prevented the appearance of the cream-coloured ſediment ; and that, on adding it to urine which contained the cream-coloured ſediment, but no cryſtals of

lithic

lithic acid, the former, ſooner or later, diſappeared, while the other was depoſited, leaving the urine, formerly turbid with the cream-coloured ſediment, perfectly tranſparent; nor is this an effect which will take place merely by keeping the urine for ſome time at reſt; for, after keeping it for months, without the addition of an acid, it is always found as turbid as at firſt.

Another effect of acids on the urine is, that of changing its colour, which they redden very conſiderably, and render darker: theſe effects appearing more ſuddenly if heat be applied *.

* Vinegar and lemon-juice produce the precipitation of the lithic acid, without altering the colour of the urine. It alſo happens, that if the colour of the urine be much darkened by any acid, that of the cryſtals of lithic acid produced, is ſo likewiſe.

There are particular acids which alſo produce other phenomena, when mixed with the urine. The ſtrong nitrous acid produces a violent efferveſcence with it, whether recent or not. There is, during this, a permanently elaſtic fluid diſengaged, which precipitates the calcareous earth of lime-water, and produces no contraction on the admixture of atmoſpheric air; the diluted nitrous acid hardly produces any efferveſcence. The vitriolic acid produces the ſame effect on the urine, but in a leſs degree. The efferveſcence, in both caſes, is much increaſed on the application of heat.

The muriatic acid excites no efferveſcence with urine, whether applied in its common, or oxigenated ſtate: if urine be expoſed to the vapours ariſing from muriatic acid, and calx of manganeſe, theſe are totally abſorbed, but not the leaſt elaſtic fluid is produced

duced from the urine, nor is any effervescence obſerved; neither do theſe appearances take place on adding to the urine the accetous acid, or the acid of lemons, although a conſiderable heat be applied.

The carbonaceous acid produces the ſame effect on the urine, which acids in general do. If urine be expoſed for ſome time to the elaſtic fluid, produced on mixing chalk and vitriolic acid, its colour appears ſomewhat reddened, and the lithic acid is ſeparated in a greater than uſual quantity; but theſe effects are much leſs perceptible with this than any other acid, except that lemon-juice and vinegar ſeem to change the colour, in a ſtill leſs degree.

I now proceed to ſay ſomething of the depoſitions which ſpontaneouſly take place from the urine. The nature of one of theſe is pretty well underſtood; the other, viz. the cream-

cream-coloured ſediment ſeems to have been entirely overlooked.

From conſidering M. Scheele's* experiments, and what I have now ſaid concerning this ſubſtance, we would be inclined to think it the calcareous phoſphat. It very evidently differs, however, from this ſalt; it never falls to the bottom of the veſſel, leaving the urine limpid; which the calcareous phoſphat always does. This laſt diſappears immediately on the addition of an acid; the other very gradually, and continues to render the urine turbid for ſeveral days, or even weeks, after the acid has been added. The cream-coloured ſediment is eaſily re-diſſolved by the application of heat: the calcareous phoſphat never is. We cannot, therefore, ſuppoſe

* Mr. Scheele ſhews, that by adding volatile alkali to urine, we produce a precipitation of calcareous phoſphat, owing to the neutralization of the ſuperabundant phoſphoric acid, which rendered this ſalt ſoluble in the urine.

ſuppoſe it calcareous phoſphat. The following obſervations ſeem, with ſufficient certainty, to point out its nature.

On adding a certain proportion of acid to urine, containing much cream-coloured ſediment, and an equal quantity of the ſame acid to urine, containing little or none of it: I always found moſt lithic acid precipitated from that which had contained moſt cream-coloured ſediment; and likewiſe, that the more of this it contained, the longer time it required to become limpid; and, for the complete depoſition of the lithic acid to take place. Beſides, where there was a long time required for the depoſition of the lithic acid, which always happened as I have mentioned, when much cream-coloured ſediment was preſent, one could eaſily perceive the gradual change induced on this laſt, which altered its colour, and being at the ſame time

preci-

precipitated from the urine, was, by degrees, totally changed into a dark red-coloured ſandy-looking matter, lying at the bottom of the veſſel.

Theſe obſervations point out, that the cream-coloured ſediment is the neutral ſalt containing the lithic acid; from which it may be precipitated by perhaps every other acid; which forming a new compound more ſoluble than the cream-coloured ſediment; the urine appears tranſparent, while the lithic acid is depoſited in the form of very fine ſand.

Having finiſhed what I propoſed to do in this chapter, I now proceed to make ſuch obſervations, as immediately reſult from the experiments which have been related.

CHAP.

CHAP. III.

Some General Remarks on the foregoing Experiments.

On taking a view of the experiments related in chapter ii. it appears, that a diet compoſed of animal food, or any cauſe promoting perſpiration, leſſens the tendency of the urine to depoſite the lithic acid.

It does not appear difficult to explain this on the principles already laid down; we have ſeen every acid which was mixed with the urine, producing a precipitation of lithic acid: hence we muſt infer, when we ſee more than ordinary of this acid in the urine,

rine, on uſing aceſcent diet, that the acid derived from ſuch diet, acts in a ſimilar manner, thus producing that great quantity of red ſediment we obſerve on ſuch occaſions. But however acid the diet may be, if we artificially increaſe perſpiration, or if this be naturally very vigorous; the acid will paſs by the ſkin, (for it has been ſhown, that an acid paſſes even by inſenſible perſpiration), and hence produce none of its effects on the urine.

Here a very important queſtion occurs; Does the body, by its own powers, generate an acid capable of precipitating the lithic acid from the urine? Or is ſuch an acid always derived from aceſcent diet? Several of the above experiments ſeem to ſhow, that this acid is conſtantly generated in the body, independent of all acid derived from the alimentary canal; and that from circumſtan-

ces

ces already taken notice of, it may pafs in great quantity by the kidney, while the perfon ufes aliment which can produce no acidity. We have feen the urine depofiting much lithic acid, when there was little food taken; and that which was entirely animal, continued not for a day or two, but feveral weeks; and what plainly indicates, that it is an acid which acts in this as in other cafes, is, that the increafe of perfpiration by a fudorific, prevented the depofition of lithic acid from the urine. See experiment xiv.

If we confider the different appearances of the urine, we fhall find three diftinct ftates in which this fluid exifts at different times; indicating different conditions of its fecreting organs.

The firft is, when the veffels of the kidney are conftricted; in this cafe, the urine

flows

flows limpid, and depoſites little ſediment of any kind; we ſee this taking place in the cold ſtage of fevers, from the application of cold to the ſurface of the body, &c. The ſecond is, when the urine is as high coloured as uſual, but depoſites leſs lithic acid; the kidney ſeems now in a ſtate of relaxation, rather than of vigorous action: this I infer, from having always obſerved the urine ſecreted during ſleep, however ſhort a time retained in the bladder, fully as high coloured as that ſecreted during vigilance, when every part of the ſyſtem is in greater activity; this urine ſeems more frequently to contain the cream-coloured ſediment, than that ſecreted when the kidney is moſt active. When the vigorous action of the kidney takes place, it forms the third ſtate; here the colour of the urine is not higher than where mere relaxation takes place; it, however, depoſites more of the lithic acid. This ſtate

of

of the kidney is always induced by the following circumſtances.

In the *firſt* place, by vigilance, as I have already mentioned; *Secondly*, By any cauſe obſtructing perſpiration.

The ſkin and kidneys ſeparate the ſame acid matters from the blood; hence, when the action of the one is diminiſhed, that of the other muſt be increaſed, in order to prevent an accumulation of acid in the ſyſtem: hence it is, that the proper action of the ſkin being prevented, more of this acid paſſes by the kidney, and conſequently there is produced in the urine a greater depoſition of lithic acid. Whether this action of the kidneys may be produced by diuretics, and the ſyſtem freed from any over proportion of that noxious matter, is a queſtion I cannot poſitively anſwer, for the reaſons given in

Experi-

Experiment xi. But, if we conſider what has juſt been ſaid, and for a moment reflect on the general laws of the animal œconomy, we muſt ſuppoſe, that, increaſing the action of the kidneys by diuretics, is a manner better calculated for freeing the ſyſtem of this acid matter, than the uſe of fluids acting merely as diluents, and which ſeem to be of little ſervice, but as they waſh out any particles of ſand adhering to the kidney; and as by increaſing the proportion of fluid, they render the lithic acid rather leſs apt to be depoſited: for Scheele and Bergman have ſhown that this matter, though difficultly, is ſoluble in watery liquors.

The three ſtates in which the kidney exiſts at different times, have now been deſcribed; and from what I have obſerved in myſelf, as well as from other conſiderations, I cannot help thinking, that every perſon experiences

periences theſe three ſtates once a-day, in a greater or leſs degree, according as their body is more or leſs irritable.

At night there is certainly formed a febrile ſtate, (as Dr. Cullen mentions) even in the moſt healthy; in myſelf I can very eaſily perceive it; and to this I would attribute my uniformly obſerving my urine paler in the evening than at any other time of the day, except where a diaphoretic had been uſed, evidently preventing the febrile ſtate. This then is the firſt ſtate of the urine mentioned above.

The ſecond ſeems to take place in the night time, eſpecially towards morning. During ſleep, there is a relaxation of that febrile ſtate formed in the evening; and hence one reaſon of the morning urine being higher coloured than that made at any other time of

the

the day; this urine likewiſe more generally depoſited the cream-coloured ſediment. When I firſt began theſe experiments on the urine, I expected to find, according to the general opinion, that the morning urine, as being higheſt coloured, would alſo depoſite moſt lithic acid; but the reſult of repeated experiments convinced me, that this was not the caſe; ſo much the contrary ſometimes happened, that having kept the morning and mid-day urine of the ſame day, each 48 hours, I found not above a few particles depoſited from the former; while in the latter there was a copious ſediment of lithic acid and this notwithſtanding that the morning urine was both higher coloured, and in greater quantity.

The mid-day urine forms the third ſtate; this I generally obſerved of a colour not ſo dark as the morning urine, nor ſo light as

that

that of the evening; but depoſiting a greater quantity of lithic acid than either. Theſe appearances of the mid-day urine are perfectly explicable on the principles already laid down; the application is ſo evident, that it is unneceſſary to ſpend time in ſhowing it.

We muſt ſuppoſe the ſame diurnal revolution to take place in the ſkin. In the evening during the febrile ſtate, it will be conſtricted; during ſleep relaxed; and in vigorous action during the day-time. I have already mentioned my reaſons (Experiment xxi.) for ſuppoſing that the acid occaſioning the precipitation of lithic acid is only thrown off by this organ, as by the kidney, in proportion to its vigorous action; hence there will be conſtantly an accumulation of acid during the night time, to be thrown off, the

the following day by the renewed vigour of the ſkin and kidneys.

Theſe obſervations tend to eſtabliſh a fact of conſiderable importance, with regard to the pathology of gravel, viz. That it is by the vigorous action of the ſkin and kidney, that any dangerous accumulation of acid muſt be guarded againſt; former obſervations pointing out, that no abſtinence from aceſcent *ingeſta* whatever, is ſufficient for this purpoſe.

Upon the whole, from the foregoing experiments and obſervations, I would conclude, in the 1*ſt* place, That any cauſe obſtructing perſpiration produces a greater than ordinary precipitation of lithic acid from the urine. 2*dly*, That the ſame precipitation is, *cæteris paribus*, increaſed by aceſcent diet, and much diminiſhed by uſing a large proportion

tion of animal food. *3dly*, That by the inactivity of the ſkin and kidneys, an accumulation of acid may take place in the ſyſtem, only to be thrown off, by reſtoring their proper action. *4thly*, That by the proper uſe of diaphoretics, we can often entirely prevent the depoſition of lithic acid from the urine. *5thly*, That the quantity of lithic acid depoſited is by no means in proportion to the height of the colour of the urine. *Laſtly*, We muſt conclude from Meſſ. Scheele's and Bergman's experiments, as well as from the above obſervations, that it is the lithic acid which is apt to form inſoluble concretions in the urine; hence the danger of all thoſe circumſtances of life, tending to occaſion its precipitation, as will be fully ſeen in the following chapter.

Having now ſhown what the circumſtances are, which tend to produce a depoſition

of the lithic acid, I proceed to ſhow, that all the prediſpoſing cauſes of gravel act, by precipitating the ſame acid; and to point out, that all theſe cauſes produce the ſame change on the body; which change we muſt conſider as *the remote cauſe of the diſeaſe.*

CHAP.

CHAP. IV.

The Application of the foregoing Experiments to determine the Remote Cauſe of Urinary Gravel.

I COME now to make an application of the foregoing experiments and obſervations, in order to determine the change induced on the body by the prediſpoſing cauſes of gravel; or what is properly called *the remote cauſe of this diſeaſe.* From what has been ſaid in the three preceding chapters, I hope I ſhall be able to explain the action of the different circumſtances in life prediſpoſing to gravel; and ſhow that all theſe produce the ſame change on the body.

What

What I would look upon, then, as the chief prediſpoſing cauſes of gravel, are, 1*ſt*, Too great a proportion of ſolid from the particular formation of the body. 2*dly*, Old age. 3*dly*, Exceſſive labour. 4*thly*, High living, and the liberal uſe of fermented liquors. 5*thly*, Indolence; and, 6*thly*, Too much heat applied to the body in general, or particularly to the kidneys.

In the *firſt* place, I am to determine how too great a proportion of earthy matter from the particular temperament of the body, ſhould give a prediſpoſition to gravel.

That this really takes place, we ſee from men being more ſubject to this diſeaſe than women; whoſe bodies are more lax, and conſequently contain a leſs proportion of earthy matter;—and likewiſe from this, that wherever there is a greater proportion of

earthy

earthy matter in the ſyſtem, the watery part of every ſecretion muſt be proportionably leſs; hence in robuſt men we find the urine higher coloured than in thoſe, *cæteris paribus*, of a more lax habit of body.

At firſt ſight, we might be apt to attribute entirely to this cauſe, the fact juſt now mentioned; but on conſidering what I have related, it will appear, that there is another cauſe prediſpoſing to gravel, and one perhaps of greater conſequence, which people of this temperament are particularly ſubject to.

It has already been ſhown, that any cauſe obſtructing perſpiration, throws a greater quantity of acid on the kidneys than ought to paſs by that outlet. If we conſider, then, for a moment, we ſhall find ſeveral cauſes rendering people of the above temperament ſubject to failure in this neceſſary diſcharge.

In

In the *firſt* place, in them the fluids move ſlower, and conſequently will be leſs diſpoſed to ſupport an excretion, which ſeems particularly to depend on the general activity of the ſanguiferous ſyſtem. *2dly*, It is well known, that as the human body advances in life, it gradually acquires greater firmneſs, and that an excretion of the ſmaller veſſels is continually going on; now people who have naturally a greater firmneſs of body, will experience this the ſooner, and in the greater degree. As the ſkin then is chiefly compoſed of theſe ſmall veſſels, perſpiration muſt ſuffer a conſiderable check by this cauſe; and thus a great quantity of the acid matter which ought to have paſſed by the ſkin, will be thrown on the kidneys. *Laſtly*, The ſame cauſes affecting the ſkin alſo tend to deſtroy the action of the kidney, thus ſhutting up the only natural vents given this matter, and unavoidably occaſioning an

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accumulation of it in the ſyſtem, which at laſt will be forced off by the kidney; and there precipitating the lithic acid in a greater quantity than is conſiſtent with health; this will be depoſited, and thus lay the foundation for a fit of the gravel, and ſometimes for a worſe diſeaſe.

From theſe conſiderations, it appears in what manner an over proportion of earthy matter from the original conformation of the body prediſpoſes to gravel. I am now therefore to conſider in what manner old age induces the ſame diſpoſition.

That old age prediſpoſes to gravel, none will deny; every day's experience has told us, how ſubject that period of life is to calculous complaints; a circumſtance which ſeems in a great meaſure to depend on what I have already hinted; for in all temperaments,

ments, as old age advances, perſpiration is gradually checked, partly owing to the increaſing proportion of the ſolids, partly to the gradual debility induced on the powers ſupporting perſpiration, and partly perhaps to the long expoſure to the air the ſkin has undergone, which, by its gentle, but long continued friction, muſt tend to obſtruct the minute pores ſpread over the ſurface of our bodies.

The ſame cauſes affecting the kidney in the former caſe, act alſo here; occaſioning a greater accumulation of acid in the ſyſtem; and when we conſider that the ſeparation of this depends ſo much on the vigorous action of the ſkin and kidneys, (as it ſeems to do from what has been obſerved), we ſhall find in that weakneſs, induced by old age, not perhaps the leaſt of the cauſes, which

which prevent the proper expulſion of this matter by urine and perſpiration.

There yet remains a circumſtance often occurring in old age, which prediſpoſes to gravel, in ſeveral ways; I mean dyſpepſia: but of this I particularly ſpeak afterwards.

The next cauſe mentioned as prediſpoſing to gravel, is exceſſive labour, which ſeems to act by inducing a ſtate ſimilar to old age, before that period has arrived. Van Swieten mentions a perſon who died of old age between 30 and 40, from the exceſſive labour he had all his life been engaged in.

The prediſpoſing cauſe of gravel I am now to conſider, is high-living, and the too liberal uſe of fermented liquors. That this ſtrongly prediſpoſes to gravel, appears evident from the experience of every age; which

has

has ſhown us the indulgence in ſuch gratifications, ſeverely compenſated by the acceſſion of this diſeaſe, in the decline of life.

From what has already been ſaid, will appear the manner in which I would explain the action of this cauſe. In the *firſt* place, by the repeated application of ſtimuli, it finally debilitates the ſyſtem, thus tending to diminiſh the vigour of circulation, and conſequently the action of the ſkin and kidneys. But it has a farther, and perhaps a more important action; it particularly debilitates the ſtomach. The changes induced on the ſyſtem by the debility of this important part, I am now to conſider.

The firſt change induced on the ſyſtem by dyſpepſia, to be taken notice of, is an increaſe of that general debility, which I have already mentioned, as ariſing from the repeated

peated ſtimuli, applied by high-living, and the liberal uſe of fermented liquors. The ſecond, that want of action in the ſkin, which always attends the debility of the ſtomach. It is needleſs, after what has been ſo often ſaid by authors, and particularly Dr. Cullen, to attempt, by any arguments, to prove that cloſe connection, which takes place between the ſkin and ſtomach; and from which, muſt proceed the inactivity of the former in every dyſpeptic patient. Thus far then dyſpepſia prediſpoſes to gravel, by throwing on the kidney, a great part of that acid which paſſes by the ſkin, in a healthy perſon.

But there is another manner in which dyſpepſia acts: I mean by producing acidity in the *primæ viæ;* for, from the obſervations of others, as well as from the experiments above related, it appears, that by increaſing the acid in the *primæ viæ*, we, *ceteris pari-*

bus,

bus, increaſe the quantity of the lithic acid depoſited by the urine.

The circumſtance which now falls under conſideration is, perhaps, the moſt powerful of all the prediſpoſing cauſes of gravel: I mean indolence. The inſtances of gravel induced by this, are numerous; and many are the different ways in which it prediſpoſes to that diſeaſe.

Its action on the body is, in ſome reſpects, ſimilar to that of fermented liquors; it weakens the circulation, and induces dyſpepſia; by theſe means, prediſpoſing to gravel, in the way I have already endeavoured to explain. But the manner in which this cauſe acts, is, perhaps, ſtill more pernicious, while high-living and fermented liquors finally induce debility, by applying repeated ſtimuli to the ſyſtem; it induces a more direct

rect debility, particularly of worſe conſequence on this account; that while the former cauſes act, they ſupport perſpiration; but this from the firſt tends to deſtroy it; for, by continuing in indolence the cuticular pores muſt gradually cloſe, ſince they are never properly excited to action; and thus, produce a more inveterate malady than could ariſe from the mere debility of the ſkin.

I come now to conſider the laſt of the prediſpoſing cauſes of gravel, which has been mentioned. The effects of much heat unaſſiſted by any of the other prediſpoſing cauſes, might probably be inſufficient to give any conſiderable prediſpoſition to the gravel; or might even, by ſupporting perſpiration, tend to prevent it. We ſee the gravel more rare in warm climates. It is, when it accompanies ſome of the more powerful, and particularly the laſt mention-

ed prediſpoſing cauſe of gravel, eſpecially in the colder climates, where perſpiration is more apt to fail; that it ſeems by weakening the action of the ſkin and kidney, to conduce to the accumulation of the acid matter in the body.

Heat applied to the kidneys, has ever been looked upon as prejudicial, and if we conſider how much the expulſion of this acid depends on the vigorous action of the kidneys, we ſhall not be ſurpriſed, if any cauſe tending to debilitate this, prove pernicious to the ſyſtem.

There ſtill remains one circumſtance looked upon by all, as prediſpoſing to gravel, viz. the preſence of the gout. I have not mentioned it, however, as ſuch, conſidering this effect, produced by the gout, inducing

ſome

ſome of the prediſpoſing cauſes already mentioned.

There is no reaſon for ſuppoſing theſe two diſeaſes eſſentially connected. *1ſt*, Becauſe we do not ſee the gravel accompanied by the gout, when the former is the primary diſeaſe. *2dly*, We may often account otherwiſe for their concurrence, ſince the ſame cauſes applied, through life, tend to induce both diſeaſes. *3dly*, We often ſee the gravel induced by other diſeaſes and accidents; when they are accompanied with the circumſtances I am ſoon to conſider as occuring in the gout; for, with equal reaſon, might we reckon a fracture eſſentially connected with the gravel, ſince it has often induced that diſeaſe. *4thly*, We do not ſee the gravel induced by the gout, till it has been preſent ſome time; and the prediſpoſ-

ing cauſes of gravel I am to mention, have been remarkably applied.

The firſt of theſe is indolence, which ever accompanies ſevere caſes of gout, and predispoſes to gravel, as has been ſhewn; the ſecond is, that great degree of dyſpepſia, which at all times, by diminiſhing the action of the ſkin, and producing acidity in the *primæ viæ*, aggravates the effects of indolence, and ſtrongly prediſpoſes to gravel. The heat alſo, often applied to the region of the kidneys, from the patient lying on his back, and the ſituation of the kidney lower than the bladder; and hence, the urine perhaps more than it ought to be retained in its pelvis, muſt tend to weaken the action of that organ; and thus to produce an accumulation of acid matter in the ſyſtem.

Now

Now, if we conſider what I have already mentioned, that the manner of life inducing the gout, alſo prediſpoſes to gravel; we ſhall not be ſurpriſed, that the above cauſes often induce this diſeaſe in gouty patients, although there be no eſſential connection between the two complaints. Not that I would deny that the gravel and gout proceed from the ſame noxious matter, exiſting in an over-proportion in the ſyſtem; but that I think we cannot be certain of this, till we know more of the nature of the gout. Be this however as it will, it ſtill appears that the diſeaſes are connected in the manner which has been explained.

Hence we ſee, that the gravel will alternate with the paroxiſms of the gout; for, during theſe, the perſpiration is better ſupported; the ſtomach leſs affected with dyſpepſia, and every part of the ſyſtem in great-

er activity, than in the atonic ſtate which intervenes between the paroxiſms, where the want of vigour in the ſkin and kidney, (which laſt is ſurely much hurt in the preceeding paroxiſm of the gout, by the cauſes which have been mentioned), together with the acidity of the *primæ viæ*, muſt occaſion an accumulation of acid in the ſyſtem, ſoon to be forced off by the kidney.

It has now been ſhewn, that all the predisposing cauſes of gravel induce the ſame change on the body, viz. the inactivity of the ſkin and kidneys, hence an over-proportion of acid matter in the ſyſtem; and a depoſition of lithic acid from the urine, ſo ſoon as it paſſes by the kidneys. This inactivity of the ſkin and kidneys then, we muſt reckon, *the remote cauſe of gravel.*

It

It only remains now, to mention certain circumſtances rendering it probable, that an accumulation of acid has taken place, and the means for clearing the ſyſtem of it, before it be forced by the kidney.

CHAP. V.

Of the Circumſtances rendering it probable, that the remote Cauſe of Gravel *is preſent, and the Means for removing it.*

In the *firſt* place then, as an over-proportion of this matter is gradually accumulating in the ſyſtem, by the inactivity of the ſkin and kidneys; the gravel muſt be a periodical diſeaſe; and as the inactivity of theſe organs is gradually increaſing, the fits of the gravel muſt by degrees become more frequent. This is confirmed by experience; hence people ſubject to the gravel, know when to expect its return; and conſequently, when there is an accumulation of acid in the ſyſtem. 2*dly*, When the gout has

continued

continued for ſome time, and the paroxiſms have been frequent, experience as well as the above conſiderations aſſure us, that an accumulation of acid has taken place. *3dly*, When Hypochondriaſis occurs in old age, after the prediſpoſing cauſes of gravel have been applied, there ſeems good reaſon for ſuppoſing an over-proportion of acid in the ſyſtem. *Laſtly*, every old perſon of the temperament prediſpoſing to gravel, particularly, if a male, and more ſo, if this diſeaſe has been hereditary in the family, may dread an accumulation of that noxious matter, ſooner or later, about to make its appearance by inducing a fit of the gravel.

When it appears, that the remote cauſe of gravel has taken place, we muſt uſe every means in our power for correcting this morbid condition of the body, and preventing it from relapſing into the ſame ſtate.

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In attempting this, there are four indications 1*ſt*, Strengthening and aſſiſting the digeſtive organs. 2*dly*, Avoiding ſuch *ingeſtæ* as increaſe the quantity of the matter we endeavour to expel. 3*dly*, Uſing ſuch as have an oppoſite tendency. 4*thly*, Throwing out this matter by every means in our power.

With regard to the firſt indication, it forms a chief part of the treatment in gravel, and is fully conſidered in the laſt part of this paper: I am therefore to ſpeak here of the other three.

It has already been ſhown, that aceſcent diet tends to precipitate the lithic acid from the urine in great abundance; and on the contrary, that alkaleſcent food diminiſhes its quantity. The patient ought certainly then to live as much on this as poſſible, of which fiſh of all kinds ſeem the beſt, as, in general,

general, eaſily digeſted, and producing a more alkaleſcent ſtate of the body than any other diet. To this it would certainly be a good addition, as far as the ſtate of the body will permit; to take, for a certain time, ſuch alkaline matters, as have been found beſt fitted for the purpoſe, ſuch as lime-water, aerated alkaline-water, &c.

I come now to the laſt indication which I muſt conſider at greater length, viz. throwing the acid matter out of the body.

Diluents ſeem to have been employed with this view; but from what was formerly ſaid, deduced from repeated experience. Diluents ſeem to be of leſs ſervice, than at firſt ſight we might be inclined to ſuppoſe.

Diuretics appear medicines well ſuited to this indication, and have been accidentally employed

employed in the diſeaſe; but a diuretic which has proved ſucceſsful, has rather been regarded as a ſpecific, than a medicine, the manner of whoſe action was underſtood; and conſequently, pointing out a claſs, in which different individuals might ſuit different conſtitutions.

But all diuretics often fail of exciting the action of the kidney; and it may prove fortunate, that nature has given another outlet to this matter, and one whoſe action we have it more in our power to command: I mean the ſkin. The experiments which have been made with diaphoretics, and ſudorifics, ſhew, that the noxious acid, whether derived from acid *ingeſta*, or generated in the body, is perſpirable; and that, by increaſing the action of the ſkin, we can ſo entirely free the ſyſtem of it, that little or none is ſecreted by the kidney.

If

If this were only a part of the groſſer perſpiration, it would be attended with much inconvenience to throw it out of the body in this way. But the experiments made with diaphoretics ſhew, that this is by no means the caſe, and that the ſyſtem may be moſt effectually relieved from its acid matter, merely by increaſing inſenſible perſpiration, without the leaſt inconvenience to the patient; for, as I have already mentioned, I have always found tartar emetic moſt efficacious, when it produced no nauſea; and obſerved, that this medicine produced a greater effect on the urine, and a more laſting one, than Dover's powder, although inducing a copious ſweat.

Hence it appears probable, that we may find in the diſeaſe I now treat of, another uſe for this valuable medicine, no leſs im-

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portant than thoſe to which it has been already applied.

There is another remedy, which, from what has been ſaid, (Experiments xxii. and xxiii.) promiſes to be of much advantage in this diſeaſe. I mean mercury; this medicine is apt to promote the action of both the ſkin and kidney, ſo that it is admirably ſuited for the laſt of the indications above mentioned. And the more ſo, as it as certainly produces its effects, as almoſt any medicine we are acquainted with.

The only objection which could be urged againſt giving mercury in this caſe is, that being a medicine, the uſe of which tends to debilitate the ſyſtem conſiderably, it might be improper in a diſeaſe which generally depends much on debility, and in circumſtan-

ces

ces where it would be neceſſary to continue its uſe for a conſiderable time.

To the firſt part, however, of the objection, it might be anſwered, that mercury given in ſmall quantity, is not apt to debilitate the body in any great degree. Dr. Gregory, profeſſor of the Practice of Medicine in the Univerſity of Edinburgh, obſerves, that he has often ſeen people under a gentle and well conducted courſe of mercury, recover both fleſh, and a healthy appearance. When given to a moderate extent, it generally produces the effects here deſired, in the fulleſt manner. Hence the good effects of mercury in dropſy, even where this proceeds from debility. It is only given in ſuch a manner, that it could be uſed; and has all the appearance of proving uſeful in gravel. The form of ointment applied to the ſkin, as being that in which mercury is leaſt apt to af-

fect

fect the ſtomach, would probably prove the moſt convenient manner of giving it. (See Experiment xxii.)

With regard to the latter part of the objection, were its exhibition well timed, it would not be neceſſary to continue it very long. The time moſt proper for this is very evident, viz. a little before the paroxiſm is expected; and to be continued till the ordinary time of its appearance is paſt, in order gradually to expel by the ſkin and kidneys that dangerous accumulation of acid, which ſeems at this time to take place, and to occaſion the periodical return of the diſeaſe.

As exerciſe excites both the action of the ſkin and kidney, a proper degree of this will always be a good addition to what has been propoſed; but in no caſe, perhaps, can we truſt entirely to it, or expect, that the body,

already

already weakened, will, by its own powers alone, reſtore the vigour of organs, ſo long habituated to inactivity.

Upon the whole, we anſwer the laſt indication by exciting the action of the ſkin and kidneys. The certainty with which we can increaſe that of the former, allows me to hope, that, by the means here propoſed, we may ſometimes prevent a diſeaſe, one of the moſt diſtreſſing the human body is ſubject to.

PART II.

Of Dyspepsia.

CHAP. I.

Introduction.

The diſeaſe I am now to treat of, is more neglected perhaps by phyſicians than it deſerves to be. Conſidered in itſelf, indeed, it is not attended with any immediate danger, but often with extreme diſtreſs to the ſufferer; conſidered in its conſequences and connexions, we ſhall find it of no ſmall importance. It is ſurely worth while, then, attempting to inquire more particularly into the proximate cauſe of this diſeaſe, than

has been done; and to ſee, if, by doing ſo, we can throw any light on the treatment of itſelf, or of thoſe more important diſeaſes with which it is connected. This, then, I ſhall do, dividing what I have to ſay on dyſpepſia, into An Inquiry concerning the proximate Cauſe of it, and Some Remarks on its Cure.

Before I proceed to this, however, it is neceſſary to determine, from the numerous experiments and obſervations, which have been made by Spallanzani and others, on the ſubject, what we are to eſteem the efficient cauſe of digeſtion in the human body.

We ſee, in many animals, ſuch a ſtructure as ſeems fitted to excite an incipient fermentation, previous to the digeſtion of their food. Spallanzani himſelf found, that food underwent fermentation in the crop of gallinaceous

ous fowls. In ruminant animals, the food lies long before it can be digeſted; which only takes place in the laſt ſtomach. Many animals, particularly inſects, chooſe food far gone in fermentation.

Thus, there can be no doubt of the food entering the ſtomach of all theſe different animals, in a ſtate of greater or leſs fermentation. To this I may add, that Spallanzani conſtantly found an acid preſent at the beginning of digeſtion, when the animal had taken vegetable food: and this he candidly owns, although ſeemingly contrary to his own theory of digeſtion.

If this fermentation, then, be not favourable to digeſtion, it muſt retard it; for the ſame perſon has ſhown, that all the ſenſible qualities given food by fermentation, muſt be deſtroyed by the gaſtric liquor, before it

can be digeſted. Was nature then obliged needleſsly to multiply her work? Could ſhe not have given the liquor of the crop of gallinaceous fowls an antiſeptic power, as ſhe has the gaſtric liquor? Or why do ſo many animals chooſe fermenting food, if a certain degree of fermentation be not favourable to digeſtion?

Theſe are arguments drawn from other animals; but there are alſo arguments drawn from man himſelf in ſupport of this opinion.

When we conſider the food reduced to a pulp, by the action of the teeth, and mixed with the ſaliva, (a fluid found, by Spallanzani, Macbride and Pringle, to promote fermentation), ſent down in mouthfuls, and preſſed by the act of deglutition; ſo that the gaſtric liquor cannot immediately pervade it; when we conſider the warmth of the human ſtomach; and, laſtly, that a mixture of food is

equally

equally favourable to fermentation and digeſtion in a ſound ſtomach; we cannot do otherwiſe than conclude, that ſome degree of fermentation muſt take place previous to digeſtion; and that it promotes this operation.

Thus, there ſeem many arguments for an incipient fermentation, previous to the digeſtion of our aliment; and the experiments of Spallanzani do not ſeem to deny it; as he always judged of its preſence by inteſtine motion, and extrication of air; appearances which only become ſenſible in more vigorous fermentation.

Yet Spallanzani's experiments prove beyond reply, that fermentation is not the efficient cauſe of digeſtion; we muſt therefore ſuppoſe it ſubſervient to it, and that the truth lies between the two oppoſite ſides of this diſputed queſtion.

With

With regard to the uſe fermentation ſerves, I would offer the following conjecture, viz. That an incipient fermentation, in a manner ſimilar to maſtication, by minutely ſeparating the more ſolid parts of our food, renders it more pervadeable by the gaſtric liquor; and conſequently more eaſily digeſted. Hence one uſe of the crop in gallinaceous birds; where there is no maſtication, and the food of very difficult digeſtion.

That a certain degree of fermentation renders our food more digeſtible, appears from many obſervations. Dr Withers, in his diſſertation on chronic weakneſs, ſays, "animal "food ſhould be kept ſome time before it is "uſed, that, by having undergone a ſlight de-"gree of fermentation, it may be ſufficiently "tender, and eaſy of digeſtion."

Induced by ſuch conſiderations, we cannot help concluding, that a ſlight degree of

fermen-

fermentation, previous to digeſtion, is favourable to that proceſs. But we muſt ſtill look upon the gaſtric liquor as the inſtrument of digeſtion, while maſtication, trituration, and fermentation are beneficial only by facilitating its action.

Having ſhown, in a few words, what is to be conſidered as the efficient cauſe of digeſtion, we are now better prepared for ſpeaking of the circumſtances which impede it.

CHAP.

CHAP. II.

Of the Proximate Cauſe of Dyſpepſia.

It is ſurpriſing, while ſo many experiments have been made, in order to determine the efficient cauſe of digeſtion, that no perſon has endeavoured to inveſtigate the proximate cauſe of its failure.

It has, however, been the opinion of authors, that dyſpepſia is owing to one of three cauſes; either to a diminution of the muſcular action of the ſtomach, or to a vitiated ſtate of the gaſtric liquor, ſo that inſtead of digeſting the aliment properly, it ſeems to promote a morbid fermentation, tending to obſtruct

obſtruct the neceſſary change of our food; which, in its healthy ſtate, it is the chief means of effecting; or, laſtly, to a deficiency of the ſame liquor.

Dr. Cullen ſeems to have been of the firſt opinion, as he mentions in paragraph 1193; his words are: "That there is a diſtinct diſ-"eaſe, attended always with the greater part "of the above ſymptoms, is rendered very "probable by this, that all theſe ſeveral "ſymptoms may ariſe from one and the "ſame cauſe, that is, from an imbecillity, "loſs of tone, and weaker action of the muſ-"cular fibres of the ſtomach; and I con-"clude, therefore, that this imbecillity may "be conſidered as the proximate cauſe of "the diſeaſe I am to treat of under the name "of Dyſpepſia."

Dr.

Dr. Cullen, however, mentions in paragraphs 1195 and 1196, that a change in the quantity or quality of the gaſtric liquor, may conſtitute another proximate cauſe of dyſpepſia; and ſurely, ſince the experiments of Spallanzani and Dr Stevens, we cannot ſuppoſe that a weakneſs in the muſcular action of the ſtomach induces dyſpepſia in any manner, but by changing the gaſtric liquor; and therefore that it never can be reckoned the proximate cauſe of this diſeaſe. It remains, then, to conſider in what manner the gaſtric liquor is changed.

With regard to the ſecond hypotheſis mentioned, many circumſtances render it probable, that this is not the proximate cauſe of dyſpepſia. But as my reaſons for forming this opinion, will ſufficiently appear, from what I ſhall ſay of the laſt mentioned hypotheſis; to avoid needleſs repetition, I ſhall

enter

enter immediately upon the conſideration of this; where I ſhall, in the 1*ſt* place, ſhow, in what manner it explains the operation of the occaſional cauſes of dyſpepſia. 2*dly*, The ſymptoms occurring in dyſpepſia; and, *laſtly*, the methods practiſed for alleviating or curing this diſeaſe; and, as I go along, ſhall point out, that all of theſe are leſs explicable by the ſecond ſuppoſition, than by that I have undertaken to defend.

With regard to the occaſional cauſes of dyſpepſia, I ſhall be very ſhort; it is needleſs to enter into an explanation of each of them. They are very numerous, as related by Dr. Cullen in his firſt lines, and Dr. Withers, in his treatiſe on chronic weakneſs; they are all ſuch as tend to weaken the ſtomach directly, or the ſyſtem in general; and it is ſurely more conformable to what we know of the animal œconomy, to ſuppoſe,

that

that cauſes acting in this way, ſhould rather produce a deficiency of the gaſtric liquor, than change its properties in ſo eminent a degree. But, independent of this conſideration, there are ſome of the occaſional cauſes of dyſpepſia particularly explicable on the hypotheſis I defend; for example, narcotics and indolence, which are well known to leſſen every ſecretion.

I am now to ſhow in what manner the ſame hypotheſis accounts for the ſymptoms occurring in this diſeaſe.

With reſpect to the production of acidity, and its conſequences, as they are eaſily explicable on either ſuppoſition, I paſs them over. There is one thing on this head, however, I have always obſerved, which tends to confirm my opinion concerning the proximate cauſe of dyſpepſia. It is, that although a

dyſpeptic

dyſpeptic cannot digeſt an ordinary quantity of food, without morbid ſymptoms, yet will he digeſt a ſmaller perfectly (I do not talk of the extreme caſe of dyſpepſia, where there is certainly no digeſtion at all). I have alſo frequently obſerved in myſelf, (for I have been much troubled with dyſpepſia), that if I faſted ſeveral hours longer than uſual, the fermentation in my ſtomach was corrected, and the food perfectly digeſted; muſt we not ſuppoſe, then, that this was owing to the gaſtric liquor which had flowed in during this time; and that, had this quantity of liquor been ſupplied ſoon enough, the food would have been digeſted without any dyſpeptic ſymptoms; and conſequently that theſe were owing to the failure, and not to any depravation of the gaſtric liquor.

Before I proceed farther in explaining, by this hypotheſis, the ſymptoms of dyſpepſia,

I ſhall

I ſhall relate a ſevere fit of it I ſuffered ſome time ago; as the inferences we muſt draw from this, tend to confirm the opinion I have undertaken to ſupport.

I was ſeized above a year ago, after a long confinement, with great anxiety, weakneſs, and complete anorexia, which laſted near four days, accompanied with conſiderable thirſt, and failure of ſaliva. During the ſecond night of the above, having ſucked an orange, in order to remove the diſagreeable dryneſs occaſioned by the want of the ſaliva; next day I felt nauſea, and oppreſſion referred to the ſtomach; which induced me to evacuate it, by irritating the fauces, about eight hours after I had taken the orange juice, I was much ſurpriſed to find it, after that ſtay in the ſtomach, unaltered and unmixed with any other ſubſtance.

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The inferences I would draw from this, are the following.

1*ſt*, That my dyſpepſia was owing to a deficiency of gaſtric liquor; for, it was evident, that there was nothing in my ſtomach at that time; but the orange juice which had undergone no change, becauſe there was no gaſtric liquor preſent to digeſt it.

2*dly*, That nauſea, in ſuch circumſtances, ariſes from the moſt grateful food.

3*dly*, That anorexia ſeems to proceed from the complete failure of the gaſtric liquor; and that this ſenſation is a wiſe proviſion of nature, which prevents us from eating at a time when no digeſtion could go on; by which we ſhould produce repeated vomitings, without receiving any nouriſhment.

Although theſe inferences appear well founded, yet, in order to confirm them, it was neceſſary, as far as poſſible, to reduce them to experiment.

From the above it would appear, at firſt ſight, that, by emptying the ſtomach of its gaſtric liquor, we might, at will, produce anorexia; from what has been ſaid, as well as from what I am going to mention, I believe we might; but it is a very difficult matter to empty the ſtomach entirely of its gaſtric liquor. *1ſt*, Becauſe it is difficult to empty it entirely of any of its contents. *2dly*, Becauſe the very act of vomiting, by the ſtrong ſtimulus applied to the ſtomach, excites it to pour out a freſh quantity of its liquor; but that anorexia can be nearly produced, and the ſenſation of hunger almoſt entirely taken away, by freeing the ſtomach

of

of the gaſtric juice, appears from the following experiment.

Eating nothing after dinner, nor drinking any thing but pure water; next morning I ſti l increaſed my appetite by walking. On returning home, I felt exceedingly hungry, having eat nothing for above 17 hours; inſtead of taking breakfaſt, I got a quantity of luke-warm water, and by means of this, repeatedly excited vomiting.

The water came up quite clear, and only mixed with a ropy tranſparent fluid, ſuch as the gaſtric liquor is deſcribed by Spallanzani, or as I have myſelf procured from the ſtomach of a crow.

This plainly indicated, that there had been nothing in my ſtomach but the gaſtric

 liquor;

liquor; which was mixed with the water, and in that ſtate, without ſenſible taſte, ſmell, or colour.

After I had undergone this operation, I found every ſenſation of hunger removed, and rather a diſguſt for food produced, which I ſenſibly felt on ſeeing others eat. I ſat down, however, to breakfaſt, but found myſelf ſatisfied, even to ſickneſs, after eating half my uſual quantity; this continued for ſeveral hours, accompanied with oppreſſion referred to the ſtomach, and anorexia.

Although I could not perhaps, by theſe means, produce complete anorexia, for the reaſons already given, yet any perſon, I am perſuaded, can produce it ſo far, as to require a very ſmall quantity of food indeed, to ſupply every craving for it.

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From theſe two caſes, then, I cannot help thinking, that the ſenſation of hunger depends on the preſence of the gaſtric liquor, unmixed with any alimentary matter which alone can divert the powerful action of this fluid from the ſtomach itſelf; and that the deficiency of the ſame fluid is the proximate cauſe of anorexia.

This hint might probably be of ſervice to thoſe who are in ſuch ſituations, that they cannot procure enough of food otherwiſe to allay their hunger: but it is never to be attempted except in extremities; for by repeated vomiting the ſtomach muſt be materially hurt, and by the privation of the principal inſtrument of digeſtion, the body ſoon weakened to ſuch a degree, that death would certainly be the conſequence of continuing the practice.

Might it not be uſeful in ſome caſes of dangerous, and very urgent plethora, where it is often impracticable to perſuade the patient to reſiſt the calls of hunger?

From what has been ſaid, it would appear, that the preſence of the gaſtric liquor in the ſtomach, without ſuch ſubſtances as are fit for combining with it, and rendering it inert, is a principal cauſe of that death which is occaſioned by hunger; for, we have ſeen anorexia produced by the evacuation of the gaſtric liquor; and every one knows, how long a perſon, labouring under anorexia, will live without aliment.

The ſpeedy acidity which took place in this experiment, is very remarkable. Although my ſtomach was perfectly free from every fermenting ſubſtance, when I went to breakfaſt, as was evident from the ſtate

of

of what I threw up; yet, the food I had taken, (bread and milk), acquired acidity in a quarter of an hour, as I found by acid eructations. Was not this artificially producing dyſpepſia, by leſſening the quantity of the gaſtric liquor? For, after this meal, although a very ſpare one, I felt oppreſſion referred to the ſtomach, anorexia accompanied with nauſea, and acid eructations.

The application of the hypotheſis I defend, to explain all the circumſtances related, is evident; if we attempt to do this by the other, we ſhall find it impoſſible.

Finally, the thirſt, bound belly, and failure of ſaliva, ſo often occurring in dyſpepſia, ſupport the ſame opinion.

Having ſhewn the manner in which I would explain the occaſional cauſes and

ſymptoms

ſymptoms of dyſpepſia: I am now to attempt an explanation of the manner in which thoſe remedies act, which are found to alleviate, or cure this diſeaſe.

All ſtimulants relieve the ſymptoms of dyſpepſia. Are we then to believe, that theſe have indiſcriminately a property of correcting the vitiated ſtate of the gaſtric liquor? Do not they act in this, as in other caſes, by ſtimulating to a more copious ſecretion? And how can we ſuppoſe the dyſpeptic ſymptoms relieved by an increaſed ſecretion of the gaſtric liquor in a vitiated ſtate? But ſtimulants have another action when too often repeated; they, by degrees, increaſe the tendency to dyſpepſia. This part of their action, likewiſe, is explicable by the ſame hypotheſis; for the debility induced on the ſtomach by the too frequent application of ſtimulants muſt ever tend to leſſen its ſecretion.

It is alſo evident, in what manner, by the ſame ſuppoſition, we may explain the operation of the other remedies uſed in this diſeaſe. Internal tonics ſtrengthen for a time, the ſyſtem, enabling the ſtomach to ſupply a quantity of gaſtric liquor ſufficient for the purpoſes of digeſtion; but theſe, like the former, either by applying an unnatural ſtimulus to the body, or by a narcotic power, at length prove hurtful, on the principles already explained.

There ſtill remain to be conſidered, two great remedies, I mean Exerciſe and Cold-bathing; of their action, however, I ſay nothing, as this is ſo well known, and ſo eaſily expiicable on the preſent hypotheſis.

From conſidering what has been ſaid, it will be ſufficiently evident, I hope, that the

opinion

opinion I defend is well founded, and capable of giving the pathology of dyſpepſia.

Having finiſhed what I had to ſay on the proximate cauſe of this diſeaſe, I proceed to ſpeak of its cure.

CHAP.

CHAP. III.

Of the Treatment of Dyſpepſia.

I SHALL not enter fully into this part of the ſubject; as that would be only repeating what has ſo often been ſaid already; but confine myſelf to ſome remarks which have occurred to me, either from my own obſervation, or that of others, in addition to what authors have ſaid on the cure of this diſeaſe.

From what experience has often taught me, as well as from the hypotheſis which I have

have conſidered as the proximate cauſe of dyſpepſia, I am perſuaded, that vomiting, which is generally the firſt remedy here employed, is for the moſt part hurtful. I have frequently uſed it, and always found the ſame neceſſity for it next day; unleſs I had taken other means for preventing the return of my complaints.

The reaſon of this is evident; for, while we vomit, we evacuate the ſtomach, indeed, of its morbid contents, but at the ſame time, along with theſe, of its gaſtric liquor; ſo that, by throwing in food after vomiting, the ſourneſs and troubleſome ſymptoms ſoon return, becauſe there is no gaſtric liquor in in the ſtomach to check fermentation, and digeſt the food. But if the patient, after vomiting, ſhould faſt ſome time, till the gaſtric liquor flow into the ſtomach in ſufficient quantity; and only take at once a ſmall quan-

quantity of food; the dyſpeptic ſymptoms will not be ſo apt to recur.

In Dr Gregory's *Conſpectus Medicinæ* he ſays, vol. i. p. 344. "Neque profecto aut "qualitas aut mixtura ciborum, (quamvis "hæc ſane haud parum aliquando nocent "ſicubi totam maſſam nimis cito fermen- "teſcere facit) helluoni tantum nocent "quantum ingens copia quam ingurgi- "tat."

As every author is of opinion, that, by frequent vomiting, the ſtomach is conſiderably weakened, this ought to be avoided as much as poſſible; and in moſt caſes, it can be ſo entirely: for I have often ſeen dyſpepſia removed by faſting, without any vomiting at all: and almoſt every dyſpeptic patient will find, that, although he cannot digeſt his uſual quantity of food, yet he

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can a leſs, without any morbid ſymptom. Hence he ought to uſe the moſt nouriſhing, and at the ſame time the moſt digeſtible food, in ſuch quantity only, as experience teaches him, his ſtomach can eaſily bear: by which means he will be enabled to avoid the uſe of many medicines, which, although affording preſent relief, tend to confirm this diſeaſe; in which, we may almoſt lay it down as a rule, that the more internal remedies we employ, the more obſtinate the complaint becomes.

In young people labouring under dyſpepſia, this diſeaſe is generally accompanied with a morbid irritability, which ſeems often to give riſe to it. In ſuch indiſpenſibly, and indeed in all dyſpeptics, thoſe remedies muſt always be employed, which act on the ſyſtem in general, and which are by nature ſo adapted to the human body, that they give

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it the proper ſtimulus without any ſubſequent injury; with this caution, however, that they be not applied when the body is too weak, or at any time in too great a degree; for by ſuch an error, they always exhauſt the ſtrength, and increaſe every complaint, which they are intended to alleviate or cure.

Theſe remedies are, in the *firſt* place, Exerciſe. *2dly*, Cold-bathing; of the former I ſhall ſpeak afterwards; of the latter I ſay nothing; every one is acquainted with the propriety and the methods of employing it.

There is one valuable remedy of this kind, however, which has been leſs attended to: it is to be regretted, that it can ſo ſeldom be uſed with propriety; and that when it is, it is often carried to ſuch exceſs, that it proves

more

more frequently the cauſe, than the cure of dyſpepſia.

It is certainly on this account, that it has not been openly recommended. I have however ſeen the moderate uſe of it, when it had not formerly been employed, attended with ſuch ſudden, and remarkably good effects, where almoſt every other remedy had failed, not excepting the proper uſe of exerciſe and cold-bathing, that I cannot help recommending it to the attention of thoſe, who are qualified to know when it may be proper, in circumſtances where it can be made uſe of, to employ it. The remedy I allude to is the *uſus modicus Veneris*. Many have remarked its good effects in chloroſis, a diſeaſe much akin to the one I treat of; which is often cauſed by, or attended with debility of the genitals, even in the male. We may ſay, I believe, of this remedy,

medy, what we can hardly do of any other recommended by phyſicians, that it only requires to be leſs agreeable to the patient, to anſwer every intention for which it is deſigned.

There is ſtill one thing to be conſidered, and which is always of the utmoſt conſequence; I mean the proper regulation of ſleep.

Every dyſpeptic patient will ſoon diſcover, that too much ſleep is equally bad with too little. For, as the proper degree of exerciſe is that which applies the neceſſary ſtimulus to the ſyſtem, while a greater degree overpowers it; ſo there is a proper proportion of ſleep, which refreſhes the body after its daily exertions; while a greater, by diminiſhing the application of thoſe ſtimuli, neceſſary for preſerving the body in a healthy ſtate, con-

ſtantly tends to relax it. This is not all; for the time of the day ſpent in ſleep ſeems to be of equal conſequence to weakly people with its duration.

Nor is this fact inexplicable, if we conſider the diurnal revolution to which the human body is ſubjected; and which becomes the more ſenſible, the more we are weakened by diſeaſe. In the evening, there is ſurely formed, more or leſs of a febrile ſtate, even in the moſt healthy; in thoſe of an irritable habit of body, and particularly in the young, it is very diſcernible. As the continuance of ſuch a ſtate tends in a great degree to debilitate the body, we muſt avoid it as much as poſſible; there is but one effectual method of fulfilling this indication, and which is univerſally pointed out by nature; I mean repoſe. Hence, dyſpeptic patients find much advantage in going early to bed; for

for whatever tends to exhauſt the ſtrength, increaſes, in a remarkable degree, the ſymptoms of this diſeaſe.

The ſame conſiderations explain the fact formerly mentioned, viz. the harm done by indulging in ſleep for too long a time; for after the relaxation of the febrile ſtate, and after the body is ſufficiently refreſhed, any longer continuance of ſleep tends unavoidably to weaken and relax it.

If a perſon then has a mind to protract the time allotted by nature for repoſe, he ought to do ſo, rather by going early to bed than lying long in the morning.

It is difficult to point out exactly any rule with regard to the duration of ſleep and the time of the day moſt proper for indulging it, which might be univerſally applicable.

The reſult of ſeveral years' obſervation, during which time I was much troubled with dyſpepſia, has convinced me, that I always enjoyed beſt health, when I ſlept about 7 hours and a half, and when I went to bed between ten and eleven in the evening, having made a full trial of going to bed earlier and later, and of ſleeping a longer or ſhorter time.

Of ſuch conſequence is the regulation of ſleep in dyſpepſia, that I have known a perſon labouring under this diſeaſe, uſe every remedy in vain, while he indulged too much in it, and by properly regulating this alone, having intermitted all other remedies, become free of every morbid ſymptom. I have alſo obſerved, that the weaker I was, and of conſequence, the more evidently the febrile ſtate was formed, the more I ſuffered by any want or exceſs of ſleep; a circumſtance

ſtance which ſufficiently ſupports the explanation I have offered.

From what has been ſaid it reſults, that there is alſo a certain time of the day moſt proper for exerciſe. Hence dyſpeptics find walking in the morning among the beſt remedies for their complaints. Much of the good effects of this, however, muſt be attributed to their getting out of bed at a proper time, though that they are not entirely to be accounted for in this way, is taught us by experience.

It may appear to ſome, that I have been too prolix on this part of my ſubject. But the great advantage derived from a proper regulation of ſleep and exerciſe, not only in dyſpepſia, but in every chronic diſeaſe accompanied with weakneſs, affords the beſt

apology; and as I would ſtrike out as far as poſſible all internal remedies, uſed in this complaint I can never dwell too long on that, which, with a proper attention to diet, already taken notice of, forms the plan that, in moſt caſes, I would propoſe in their ſtead; ſince experience, on which alone we muſt depend, has proved it to be effectual.

I have ſaid nothing of the ſtate of the mind in this diſeaſe; concerning which, many uſeful regulations are laid down; as theſe have often been taken notice of elſewhere. The great rule is, to keep the mind conſtantly engaged, but never to fatigue or overſtretch it.

The general view in this treatment, however, is more adapted to the young than the old: In whom, and indeed, in all labouring under ſevere dyſpepſia, and hence much debility

bility, digeſtion is ſo weakened, that the body can hardly be ſupported, far leſs ſtrengthened to ſuch a degree, as is neceſſary for employing the remedies mentioned above; and which we may conſider as the treatment for a radical cure. On this account, we muſt uſe ſome artificial means of ſupporting, for the preſent, the digeſtive powers; and it remains to be determined, in what manner this may be moſt ſafely and effectually done.

Stimulants and internal tonics, have been univerſally uſed with this intention. Phyſicians, however, have always complained, that while, by theſe, they relieved the urgent ſymptoms of the diſeaſe, they unavoidably increaſed the tendency to it. It would, therefore, be of much conſequence to dyſpeptic patients, if a remedy could be found, which would relieve the ſymptoms of their

diſeaſe

diſeaſe, without tending ſtill farther to increaſe the debility of the ſtomach. Such a remedy, I cannot help thinking, is pointed out by what is ſaid in the laſt chapter concerning the proximate cauſe.

We have ſeen all the occaſional cauſes of this diſeaſe, leſſening the quantity of the gaſtric liquor: and every remedy found of ſervice in it, increaſing the ſecretion of that fluid; hence, inſtead of preternaturally ſtimulating the ſtomach, and thus finally increaſing its morbid affections, were we to introduce into it, the gaſtric liquor of other animals, it appears probable, that the dyſpeptic ſymptoms might, in this manner, be relieved, and the body, by a greater ſupply of nouriſhment ſo ſtrengthened, that by the method formerly taken notice of, a radical cure might be accompliſhed; which would the more readily happen on this account

account, that the patient had not been in the cuſtom of uſing ſtimulants, and internal tonics.

There are alſo other uſes to which the gaſtric liquor of brute animals might be applied; particularly in ſuch caſes of *dyſphagia*, or vomiting, where it is neceſſary to nouriſh *per anum*. The little nouriſhment which is ſupplied in this way, is moſt probably owing to the want of that change which the gaſtric liquor produces on our aliment, before it reaches the inteſtines; if the food however, were digeſted with a gentle heat, for ſome time, previous to uſing it, with a proper quantity of the gaſtric liquor of ſome brute animal, living on food ſimilar to that of man; it is probable, that the chief circumſtance would be removed, which prevents us at preſent from nouriſhing patients for any conſiderable time in this way.

Of

Of the medicines calculated to relieve particular ſymptoms in dyſpepſia, ſuch as abſorbents, antiſpaſmodics, demulcents, I ſay nothing; their effects are tranſitory, and of little conſequence, except that many of them may certainly prove hurtful if uſed too frequently. The uſe of theſe would always be much, and in many caſes entirely ſuperſeded, by the plan of treatment which I have, from repeated experience, ventured to propoſe.

I have now finiſhed what I intended doing in this treatiſe: I have been full on dyſpepſia becauſe it appears, that our treatment of this, will ever prove a principal part of the practice in urinary gravel. Whether, by promoting the action of the ſkin and kidney, in the manner above directed, we ſhall be enabled to relieve a gravely diſpoſition, experience muſt determine: but every age has

has told us, that living in ſuch a manner, as ſupports their neceſſary excretions, and at the ſame time tends to preſerve the vigour of the digeſtive organs, is a method of avoiding a diſeaſe which ſo often makes the decline of life almoſt intolerable. This can only be done, by ſteadily obſerving ſobriety, and uſing that degree of exerciſe for which nature has deſigned us.

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FINIS.

ERRATA.

PAGE 14.	LINE 8.	*read* care *for* cure.
—— 4.	—— 9.	—— Scheele *for* Sheele.
—— 10.	—— 11.	—— iv. *for* vj.
—— 45.	—— 18.	—— , *for* ;
—— 67.	—— 1.	—— mercurial ointment *for* mercury.
—— 74. laſt	——	—— not *for* no.
—— 78.	—— 7.	—— Berthollet *for* Bertholet.
—— 87.	—— 4.	—— acetous *for* accetous.
—— 106.	—— 8.	—— accretion *for* excretion.
—— 118.	—— 5.	—— paroxyſm *for* paroxiſm.
—— 123.	—— 5.	—— , *for* ; & *l.* 14. *r.* , *for* .
—— 145.	—— 6.	—— , *for* ;

Note omitted in page 88.

If we allow the cream-coloured ſediment to grow dry by ſpontaneous evaporation, it exhibits ſmall white ſemipellucid cryſtals; a circumſtance firſt ſhown me by Mr. Ruſſel, a gentleman who has paid ſome attention to this ſubject, and which I have ſince frequently obſerved.