stages of chronic renal disease. I have already told you that the vomited matters are usually very offensive; and, bearing this fact in mind, if you direct your patient to take copious draughts of tepid water, he will often obtain relief from his nausea by the speedy expulsion of the foul secretions and the thorough washing out of the stomach.

The relief afforded by this simple cleansing process is analogous to that which is experienced by a patient whose bladder has been washed free from foul mucus and fetid ammoniacal urine. In both cases, the cleansing has to be repeated from time to time.

The food must be of the lightest and most digestible kind. The process of digestion may be aided by a dose of fifteen or twenty drops of dilute hydrochloric acid with each meal, and to this may sometimes be added with advantage the thirtieth part of a grain of strychnia. The mineral acid neutralises the ammonia which is often thrown off abundantly, and which tends to render the secretions of the stomach alkaline or only feebly acid, and strychnia is certainly a most valuable gastric tonic. Other vegetable bitters may be substituted for this; but they are less efficacious. A glass of champagne is sometimes a grateful and wholesome stimulant with the food.

In some cases, vomiting may be best relieved by a purgative enema, or by stimulating the lower bowel by the compound colocynth pill if the stomach will retain it. In not a few of these cases, the irritability of the stomach is excessive, and vomiting occurs with far greater frequency than is required for the expulsion of its morbid contents. In such cases, the excessive irritability may sometimes be allayed by constantly sucking lumps of ice. In other cases, I have seen the occasional inhalation of a small quantity of chloroform vapour afford great relief, or a few drops of chloroform may be swallowed from time to time with mucilage. A mustard poultice on the epigastrium sometimes has a good effect. In this class of cases, such remedies for vomiting as creasote, hydrocyanic acid, etc., are, according to my experience, quite useless. When the vomiting is incessant, nutritive and stimulant enemata often afford great relief and comfort; and, indeed, they are absolutely essential to prevent fatal exhaustion when the normal functions of the stomach are so entirely suspended as they commonly are in these painful cases.

As the irritability of the stomach may be excessive, so may be, in some cases, the irritation of the bowels; and we must sometimes endeavour to allay tenesmus by the very guarded use of opium. With this view, ten or fifteen drops of laudanum may be given in an enema, or half a grain of opium with a grain of ipecacuanha in a pill, the effect being carefully watched, and the dose repeated or not according to circumstances. I have already warned you of the great danger which attends the incautious employment of opium in these cases. Bear in mind that the object of the opiate is to soothe; to allay irritation, and not close a safety valve; and, remembering this, you are not likely to err.

The gastro-intestinal symptoms will be mitigated if, by any means, we can increase the secretory action of the kidney. Amongst the means which may be usefully employed with this end in view are, counterirritation over the loins, either by dry cupping or by mustard and linseed poultices; hot-air baths to stimulate the functions of the skin, and thus to lessen the work and the congestion of the kidney; and, as a diuretic, the imperial drink, made with cream of tartar and lemon, may be taken liberally, and rendered more diuretic by the addition of a small quantity of gin.

When there is much anasarca, an incision into

each leg is often followed by great relief; the fluid is rapidly drained away; then, the vessels being partially unloaded, the circulation through the kidney, as through every other organ, becomes more free; there is, consequently, a more copious secretion of urine; and thus the gastro-intestinal symptoms which resulted from uræmic contamination are indirectly relieved. The result of my experience is that, in cases of anasarca, inflammation rarely follows puncturing the legs when the dropsy is simply renal and not complicated with obstruction of the circulation by valvular disease of the heart. The relief which follows the operation is often very great.

ON
THE MANNER OF THE INHERITANCE
OF CANCER,
AND ITS RELATION TO QUESTIONS CONCERNING
THE LOCAL OR CONSTITUTIONAL ORIGIN
OF THIS DISEASE.

By W. MORRANT BAKER, F.R.C.S.,
Demonstrator of Anatomy and Operative Surgery at St. Bartholomew's Hospital, etc.

THE theory that cancer is, at the first, a local and not a constitutional disease, has recently been strongly upheld by an eminent writer on this subject (*The Antecedents of Cancer*, 1865, by C. H. Moore, F.R.C.S.; *A Brief Report on Cases of Cancer*, by C. H. Moore, F.R.C.S., *Brit. Med. Journal*). At the same time, he has shown how necessary for the establishment or destruction of any such theory are large statistics of cases of the disease; and it therefore seems incumbent on all to contribute, so far as they can, to the settlement of this vexed and oft-mooted question.

It is a matter of common observation, that no subject illustrates better than cancer how much may be said on both sides of an argument. There are, indeed, but few facts which have been imported into discussions on this subject that will not serve as weapons as well for one side as the other—as evidence as much of the local as of the constitutional origin of the disease.

There is, however, one point in the history of cancer which has a real bearing on the question at issue, and has more weight, perhaps, than any other; namely, the manner of the inheritance of the disease. And I propose to consider this point alone, not because it is by any means the only one that should be taken into consideration, but because it seems to me sufficient by itself to settle the particular question in dispute, so far as a settlement is at present possible.

Before bringing forward any statistics of inheritance, it may be well to direct attention to the fact that two very different notions concerning that which is inherited appear to exist in the minds of different writers on this subject. For instance, the question is argued sometimes on the apparent supposition that cancer, as such, is handed down from parent to offspring; and that, therefore, the laws by which its transmission is regulated ought to be the same with those which govern, say, the appearance of syphilis in the offspring of a syphilitic parent. Thus it has been said that, had inheritance much to do with the appearance of cancer, this disease should be more frequently congenital, or, at least, a more common malady in infants and young children than, from the examination of statistics, it appears to be (*The Antecedents of Cancer*, by C. H. Moore, 1865).

But this argument, which at first sight seems a strong one, becomes fainter when it is examined from another side. That cancer, as cancer, is handed down from one generation to another, is not, I think, generally accepted as the usual manner of its inheritance, although it may so happen sometimes. It is not necessary to suppose, in every case in which inheritance has had to do with the matter, that there has been direct transmission of the disease; else a belief in the inheritance of cancer from a grandparent, when the intervening generation escapes, would be an absurdity; for we cannot believe that, in this instance, cancer could actually exist in the second generation, and even be transmitted to offspring, and yet remain undeveloped. Mr. Paget has put this fact very plainly. "That which is transmitted," he observes, "from parent to offspring is not cancer itself, but a tendency to the production of cancer at some time far future from the birth. We have no reason to believe that a cancerous material passes with the germ. To suppose such a thing, where the cancerous parent is the male, would be almost absurd. Moreover, no reason to believe that cancerous material passes from either parent is furnished by any frequency of congenital cancer." (*Lectures on Surgical Pathology*, 2nd ed., p. 774.)

So, then, in speaking of the propagation of cancer by inheritance, it is necessary to have a definite idea of what is supposed to be inherited, or, at least, to be not inherited; for, on the ordinary supposition that it is a tendency to the disease, and not the disease itself, that is transmitted from parent to child, we must be contented, necessarily, with a very vague notion, or, indeed, none at all, concerning that which is passed down from one generation to another. But the impossibility of saying how a tendency to disease, and not the actual disease, can be transmitted from parent to offspring, need not make us discard the notion of such being the manner in which cancer is inherited. At least, if we do so, we must fly in the face of all statistical records of the inheritance of cancer, and be prepared to deny altogether the possibility of arguing on this subject from the analogy afforded by many other diseases. For certainly a tendency to many others than cancer is inherited, as, for instance, gout, phthisis, brain-disease, etc.; and yet the disease itself may never show itself at all, or may skip over a generation; and, even in the event of its appearance, it is frequently or commonly not observed until the subject of it has reached about the same age as that at which the disease showed itself in the parent. Indeed, there is no more impossibility in the transmission of a tendency to disease than there is of a tendency to the development of likeness in feature at about corresponding ages in parent and child. Both are results of the descent of a material something or other by inheritance; but just as cancer, which is not inherited, is the result of certain material conditions which become potent for its production only at a certain period of life—it may be at an advanced age—so there is no improbability in the supposition that, in the case of inherited cancer, the inheritance is of the conditions which may or may not develop the disease, rather than of the disease itself. So far as the fact can be proved by statistical records, this is certainly the usual way in which it is handed down from generation to generation; and, if argument from analogy be admitted, this is the manner in which, from observation of the inheritance of other diseases, we should expect it to be transmitted.

In the next place, it must be observed, that the mere fact of cancer being a heritable disease has, by itself, nothing to do with the question, whether it is local in its nature or constitutional. The inheritance of this disease is sometimes mentioned as

if to allow that such an occurrence is frequent, is almost to grant that the malady is a general and not a local one. Surely, this is an error. Epidermal cysts, crooked fingers or toes, hernia, etc., which may be taken as instances of what are called local diseases, are as undoubtedly heritable as cancer. And we may therefore regard the question, whether cancer is, at its beginning, local or constitutional, is distinct from the inquiry, whether, and to what extent, it is inherited.

As to the fact, that inheritance has a frequent influence in the production of cancer, there can be little doubt. The last published *résumé* of Mr. Paget's statistics (*Medico-Chirurg. Trans.*, 1862) gives 24.2 per cent., or nearly one in four, as the proportion of cancerous patients who were aware of the occurrence of the disease in other members of their families.* And it must be remembered, that although some of these cases may have been only coincidences of disease; yet the deduction that should be made on this account is probably more than balanced by the increase that would have to be made, if the inheritances from patients, who die with unknown internal cancers, could be reckoned also.

It may be fairly said, too, that in a certain number of cases, a tendency to cancer must be transmitted by inheritance; but the fact is never known on account of the death of the parent from some other cause before the disease has manifested itself. The frequent delay in the appearance of cancer until advanced age makes it the more probable that, from this cause, all statistics of the disease must give too low an estimate of the frequency of its inheritance.

It is not, however, the extent to which the inheritance is influential in the production of cancer that can settle the doubt whether the disease is, at first, only local. There are local diseases which are inherited as frequently as many which are constitutional, and, in some instances, even more so. The mode, however, in which the disease is passed from one generation to another, is that which should afford a crucial test of its beginning, as a local disease or a general one.

Mr. Moore has, indeed, put the case very plainly. He remarks (*Antecedents of Cancer*, 1865, p. 20), "If it be usual for dissimilar cancers to prevail in direct inheritance,—for the children of cancerous parents to have primary cancer of various organs,—then cancer passes from parent to offspring as a general and not a local disease. It belongs indifferently to all the body. Its constitutional nature is established." And he then proceeds to say that such is not the result of his experience, although he is not in a position to state the fact numerically. In his later paper (*BRITISH MEDICAL JOURNAL*, Dec. 1, 1866), he supplies this want by a relation of twenty-four cases of cancer, which occurred in the relatives of cancerous patients who were under the care of various members of the British Medical Association. "One-half of these multiple cancers, in related persons, occupied similar, and one-half dissimilar, organs."

By the kindness of Mr. Paget, I have also lately been enabled to publish some statistics of cases of multiple cancers in families, and of the proportion of similar to dissimilar cancers among relatives (*St.*

* Mr. Moore quotes, on Mr. Paget's authority, apparently from his earlier statistics, one in six. The later statistics, however, consisting only of cases observed by Mr. Paget himself (*l.c.*), give the number quoted in the text—the difference being probably due to a larger number of these having been gathered among patients in private practice; the family history of whom would be much better known than that of the hospital cases. To the same fact is probably to be attributed the difference between the proportion of inheritances in Mr. Paget's cases and in those tabulated a short time before by Mr. Sibley.

Bartholomew's Hospital Reports, vol. ii, 1866). They have been collected from those of his cases in which any information of the kind had been recorded, and have been arranged in the order in which they occurred, without reference to any particular result. It is unnecessary to quote the whole table, which may be found in the place to which a reference has been given. It will suffice to say, that altogether I have been enabled to tabulate eighty-three cases in which the occurrence of cancer, in more than one member of a family, had been recorded, together with the situation of the cancer in the relatives affected. Examination of the cases gave the following result:—

"There were altogether forty-five instances (in forty-one families) of direct inheritance from father or mother; and of these, the disease was in the same organ in both parent and child in nineteen, and in different organs, in twenty-six instances.

"It is a curious fact that in all the cases but one, in which the disease occupied the same site in both generations, the breast was the organ affected; the exception was the uterus. As might be expected from this fact, almost all these patients were females, only one case occurring of cancer of the breast inherited by a male.

"Among the cases of direct inheritance from a parent, but in which the disease was transmitted to a different organ in the child, nine were instances of inheritance from the father, and seventeen from the mother.

"There were sixteen instances (in fourteen families) of inheritance from a grandparent or great-grandparent, or both; of these, eight were cases of cancer in the same, and eight in a different, organ in the two generations. As before, the cases of disease of the same site in both generations, were cases of cancer of the breast, with a doubtful exception.

"There were forty-nine families—some of them have been included in the former statement—two or more members of which had cancer, the relationship between whom, however, was not that of parent, or grandparent, and child. Of these, twenty-eight were families in which the disease was seated in the same organ in all the relatives affected; twenty-one in which different organs were attacked.

"There were twenty-five instances of the disease in brothers or sisters, or in brother and sister. In fourteen of these the same organ suffered in both the relatives affected; in eleven, a different one.

"The whole of the twenty-eight instances of cancer of the same organ, in all the affected relatives, were cancer of the female breast; and fourteen of them were in sisters" (*St. Barthol. Hosp. Rep.*, vol. ii, p. 136).

Taking Mr. Moore's statistics (twenty-four cases), therefore, and Mr. Paget's (eighty-three cases), we have altogether one hundred and seven cases wherewith to decide the question whether cancer is, at its beginning, local or constitutional, so far, that is to say, as it can be determined by observation of the manner in which it is inherited.

In the sentence before quoted from Mr. Moore's paper, it is rightly said, that the occurrence of inherited cancer in other organs than those affected in the parent, would be a sufficient reason for considering the disease a constitutional one and not a local. How then does the case stand, now that a fair number of cases, having a special reference to this point, has been collected?

Taking the whole of the cases together, in which cancer occurred in relatives, one-half, almost exactly, occurred in similar, and one-half in dissimilar, organs in the affected members of the same family.

Taking only the cases of direct inheritance from a parent,—and these are, of course, the more conclusive—the number of instances of unlike cancer in

parent and child is found to preponderate over those in which they were alike.

Now, it is difficult to see how any evidence, so far as it goes, can be more conclusive. It is equally difficult to see how this evidence of cancer being not merely a local disease, can be resisted. The only way in which it can be resisted, or shown to be not conclusive, must surely be by doubting whether inheritance has anything whatever to do with the matter; or, and this is a less reasonable alternative, by assuming that the instances of cancer, similar in the offspring to that in the parent, are cases of true inheritance; and that those of cancer, unlike in parent and child, are simply coincidences, and nothing more.

As the latter alternative, indeed, is almost a *reductio ad absurdum* in the presence of the histories of cases of multiple cancers in families, such as may be found recorded in treatises on this subject, it will be unnecessary to consider it. It may be well, however, to inquire whether there is any reason for thinking that all supposed cases of inheritance of cancer may be only coincidences of disease in the same family.

The percentage, before quoted, of cancerous patients who were aware of the occurrence of the disease in other members of their family, seems by itself almost conclusive; but the supposition of coincidence is rendered still more untenable by the observation made some time ago by Mr. Paget (*Med. Times and Gazette*, Aug. 22, 1857), that among a large number of patients with innocent tumours, neither cancerous nor recurring, only 6·8 per cent. were aware of any any relative having cancer; while 22·4 per cent. of the cancerous had one or more relatives with the same disease.

If, then, we may believe on the present evidence, that cancer is really inherited in a large number of cases, and that, in the transmission of the disease in this way, it happens, as often as not, that a different organ suffers in the offspring from that affected in the parent, how can the theory be maintained that cancer is a local malady?

The fact that, among inherited cases, cancer is so frequently in the same organ in both parent and offspring is no evidence for its merely local nature. The occurrence of tuberculous disease in the lung, and of gout in the great toe in successive generations may be fairly taken to prove for what parts these diseases have a special liking, but is scarcely a sign either of their local nature, or of their transmission by inheritance as local diseases. And it is not easy to understand why, under analogous circumstances, cancer, and not gout or phthisis, should be considered a local disease.

Moreover, on reference to the statistics, it will be seen that it is only in instances of cancer in those organs, especially the breast and uterus, for which it has a very great liking, that we see any special tendency to a repetition of disease in the same part in two members of successive generations. And that cancer of the breast or uterus, when transmitted to offspring, should be repeated in the breast or uterus respectively is surely not, on any theory, to be wondered at, when it is remembered how likely it is that, under any circumstances, cancer in the female will be found in one or other of these organs.

But again, the proportion of cases in which cancer occurs in a different organ in parent and offspring is of comparatively little moment. If it be allowed that cancer is ever transmitted to a different organ in the child from that which was affected in the parent, then it is practically allowed that cancer is not merely a local disease. For such an admission in any individual case, must of necessity be the same thing as granting that in this special instance the disease was not merely a local one. And before

those which occur in the same organs in parent and offspring can be supposed to be merely local, surely some difference must be shown plainly to exist between the cancer which occurs in the two groups of cases (those in the same and those in different organs). For, while there is every reason to believe that a constitutional disease will always be found to prefer certain organs as its site before others, there is no reason for believing that a disease, not traceable to external influences, can spring up at one time as a local and at another as a constitutional disease.

If the number of cases in which there occurred unlike cases in successive generations were very small in proportion to the number of those in which the disease was, under the same circumstances, alike, there would be, of course, an increased chance that the exceptions might be only coincidences. But, if it be granted or proved that such cases as those referred to really occur, then one such case would weigh more in the decision than a hundred or a thousand on the other side, and for the reasons just given. But the case is not reduced to such straits. Under all circumstances, the occurrence in question seems to be as frequent as the reverse; and, if we except the cases of cancer in those organs towards which it always has a very strong tendency, there really seems to be comparatively little chance of an inherited cancer attacking the same organ in the child with that which it affected in the parent. At least, if we except cancer of the breast and uterus, such an event happened in Mr. Paget's forty-five cases of direct inheritance (from parent to child) not once, and in Mr. Moore's eight cases only once.

CONTRIBUTIONS TO PRACTICAL MIDWIFERY.

By ARTHUR B. STEELE, Esq.,

Lecturer on Midwifery, Liverpool Royal Infirmary School of Medicine; Surgeon-Accoucheur to the Ladies' Charity; etc.

I.—THE USE OF THE FORCEPS.*

IN the following observations, I lay no claim to originality—the subject having occupied the attention of obstetricians ever since the art of midwifery has been cultivated as a branch of medical study. Nevertheless, as it is one of much interest and importance to all who are engaged in obstetric practice, and as on certain points much difference of opinion exists, I have thought that a critical commentary upon the conflicting opinions of high authorities, with such practical hints as my own experience have suggested, might be worthy the attention of the Society, and lead to a discussion at once interesting and instructive.

The particular points to which I propose to direct your attention are, first, that the tendency of the teaching of the British school of midwifery, as laid down in the text-books, is, for the most part, to discourage the use of the forceps in a wide range of cases, in which clinical experience has shown their application to be not only justifiable, but eminently calculated to diminish suffering and lessen the chances of mortality both to mother and child; and, secondly, that there exists among obstetric teachers a want of uniformity in their directions as to the best method of operating and as to the most appropriate form of instrument to be used, arising from a

non-appreciation of certain mechanical principles, which, although clearly understood and taught by some accoucheurs of eminence, are, nevertheless, misunderstood or ignored by others of equal repute. Much of the difficulty and difference of opinion connected with the general question as to the proper use of the forceps has, I believe, arisen from an arbitrary distinction between what are called long and short forceps cases. Dr. Ramsbotham, for example, in common with most obstetric writers, treats of the long forceps in a distinct chapter; and, while fully recognising their value in special cases, to which he considers them exclusively appropriate, at the same time describes them, as more capable of inflicting injury than the short forceps, and limits their use to those cases in which the principal bulk of the head remains above the brim, or in which it has not descended sufficiently low to admit of the application of the short forceps. Dr. Robert Lee altogether discards the use of the long forceps, substituting craniotomy, as, in his opinion, a less hazardous alternative. Dr. Tyler Smith describes the long forceps as “not merely a variation of form, but that it is applied to an order of cases altogether different from those in which the short forceps is useful.” (*Manual of Obstetrics*, p. 578.)

A general review of the opinions of British obstetricians, on this question, leads to the conclusion that they may be classed under two divisions; namely, those who either oppose the use of the long forceps altogether or resort to them only in certain extreme cases of exceptional difficulty; and, secondly, those who more freely admit their utility, and who sanction their use in a wider range of appropriate cases, but who, nevertheless, follow Dr. Ramsbotham in limiting their use to those cases in which the short forceps are inadmissible. As opposed, however, to the views of both these classes of teachers, we have the more advanced, and, as I hope presently to show, the sounder doctrines enunciated by the Edinburgh school, represented by Sir James Simpson, and also by a certain number of obstetricians in this country, of whom Mr. Roberton, of Manchester, may be considered the representative. These authorities are not only favourable to the use of the long forceps in the abstract, but consider them to be equally applicable to all cases with the short forceps, and in some respects even better adapted. Mr. Roberton in his work, published in 1851, thus expresses his opinion: “For reasons the readers will gather, I discard the term ‘short,’ and approve as little of the other term ‘long,’ persuaded that only one kind of forceps is required.” (P. 263.) Having described the construction of his own instrument, which closely resembles that known as Ramsbotham's long forceps, he adds, “they are fitted for seizing the head either in the brim, or when it is in the cavity of the pelvis, or when it is at the external orifice; moreover, it is in the latter description of cases, hitherto left to the short forceps, that, that my forceps are especially useful, and superior to the short.”

The mechanical advantages of Mr. Roberton's forceps are as follows, transcribed pretty closely in his own words: “When the head is at the brim, and the bulging part more or less engaged, or when the base of the head is arrested in the brim; the length of the blades facilitates their introduction, and enables them to lay hold of the head in the proper manner—the parallel shanks in advance of the lock prevents that distention of the orifice laterally, which is unavoidably produced by forceps, the blades of which spring directly the lock, and which is a frequent cause of laceration.”

This lengthening of the shanks in advance of the lock, I consider a *sine quâ non* in the construction of

* Read before the Liverpool Medical Institution, Oct. 18, 1866.