Gulstonian Lectures

ON

THE USES OF BLOOD-LETTING IN DISEASE.

Delivered at the Royal College of Physicians.

BY

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ANOTHER argument, though of a like nature, adduced in support of this theory, is one derived from the personal observation of medical men, and of medical men of high repute and of large experience, whose opinions rightly claim our respect and full consideration. These gentlemen do not rely solely upon tradition—upon the records of the past—as proofs of the position they assume. They tell us that they have seen with their eyes the facts on which their belief is founded. They have witnessed the effects of blood-letting in other long-gone-past days; and they have also seen and noted its effects in these more recent times, and are therefore able, of their personal knowledge, to contrast the past with the present effects, and so to deduce the—to them—legitimate conclusion, that diseases have passed during the interval from a sthenic into an asthenic phase.

To meet this argument, which bears, on the face of it, strong apparent testimony in favour of the theory, I am necessarily driven to the somewhat presumptuous position of questioning the correctness of the observations referred to. Happily, however, the high authorities who have given their sanction to the theory are the very men who have always taught the profession, above all things, never *jurare in verba magistri* in matters medical; but to try all things by the light of their own reasoning and observation. They, therefore, I may confidently feel, are the last men who will object to the freest criticism of their opinions.

Let me here again remind you that the change in type theory rests solely upon the assertion that blood-letting was found by practitioners of medicine to be well borne and of high service in inflammations some thirty, forty, fifty, or seventy years ago; and that it is not found to be so borne now; and that the assertion is based solely upon a comparison of the effects of blood-letting in past days with its effects at this present day. I have already attempted to show you how impossible it is for us, practising medicine in the middle of the nineteenth century, to compare the results of our treatment with the results of the treatment of the past generation of practitioners. I shall now attempt to show, with the greatest deference to those high authorities who have sanctioned the theory disputed, that neither can any safe conclusions be drawn from a comparison of the practice of men of the same generation, made at distant intervals of time.

In the first place, I would ask, Where is the scientific man of medicine, who at sixty years of age sees diseases and treats diseases as he saw and treated them when under the influence of the ideas which held possession of his mind at twenty-five? His be-

lief in his curative powers, his enthusiastic faith in the energetic power of his art to cure diseases, will probably have gradually abated, and *pari passu* with his advance in years. With the development of his reasoning powers, his questioning of the efficacy of his remedies will probably have become stricter. At all events, and most assuredly, the activity, the heroism of his treatment will be found to have declined with his increased age and better experience. Let any one call to mind the exact state of medical knowledge at the beginning of this century—during the first quarter of it, for example; let him turn to the works from which men of medicine then drew their inspiration in medical knowledge; let him compare that knowledge with the scientific knowledge of this day,—and then say if he can believe that even the calmest judgment and the most gifted observation, so trained and fed, would be able now, at this time of day and under the bright light of all our advanced and accumulated knowledge, to calculate and measure to any effective purpose the analogies and differences between the results of the practice of those past and of these present days? Where is the philosophic and experienced physician of modern days, whose ideal of medicine has not been revolutionised since the day when he first entered upon practice? Is it possible that, with these modernised views of his concerning the powers and virtues of his therapeutical agents, and the nature of diseases, he should be able, out of his general recollection of what the effects of blood-letting were some thirty or forty years ago, to draw trustworthy conclusions as to the effects of bleeding at this present day? With these difficulties before me, and calling to mind the deep meaning and profound truth of the aphoristic words of the wise man, who warned us that, in the practice of medicine, experience was fallacious, and a right conclusion hard to come at—I think I may fairly question the correctness of a theory which rests upon so unstable and fluctuating a basis. Every page of the history of medicine records the painful fact that, in matters medical, it is most difficult to extract the right conclusion from the lessons of experience; and that, in fact, theory has not been a more fertile breeder of errors in medicine than this so highly extolled experience. Men may observe and record the facts which lie before them most correctly; but still ever is it that some one or more essential items of the case may have escaped their observation—so compound are the phenomena touching disease and its treatment, so many and so hidden or dimly seen the disturbing causes which affect its right reading.

I have said that the change-in-type theory of diseases was not adopted until long after the practice of blood-letting in inflammations had been generally given up; and I must adduce what seems to me the proof of this very significant fact.

How comes it, that the authorities who now tell us that this present asthenic phase of disease came upon us soon after the advent of cholera in 1832, did not make the discovery or note it down at that time? How comes it, that the fact of this change in type—the fact that men would not bear bleeding as they formerly did—was not announced until twenty or twenty-five years after the asserted change displayed itself? Surely the asthenic change in type could not have been more clearly seen now out of a distant memory of past practice, than it was then, at the moment when (as we are told) men manifestly

bore bleeding ill—at the moment when they were actually under the lancet, and displayed the signs of the asthenia—at the time when physicians gave up the practice of bleeding, because they saw their patients would no longer bear the bleeding? We are, in fact, asked to give our assent to a theory based upon conclusions drawn from the observation of facts made a quarter of a century ago, the main significance of which facts (so far as records tell us) entirely escaped notice at the very moment when it must have been most prominently marked. We give up blood-letting twenty-five years ago; and it takes us twenty-five years to discover that we gave it up because patients would not bear it, or, in other words, because diseases had changed their type! It seems to me inexplicable, except on the supposition that there must be some fallacy in the argument, that Dr. Alison and other high authorities, who believe in this change of type, should have continued, for twenty years at least after the change is said to have occurred, to eulogise in the strongest terms the practice of blood-letting in inflammations. And, curiously enough, we even find these authorities actually explaining to the student how it is that they, who at that time so highly extolled blood-letting, so rarely resorted to it in their hospital practice. We should have expected that the first reason given would be this change in type; but nothing of the kind. The reason is, as Dr. Alison and others say: When this class of patients come under our hands, the time for blood-letting is past; and, besides, they have often been bled largely before they came into hospital. Everywhere, in fact, do these authorities extol in the highest language the virtues of blood-letting in inflammations; and nowhere do they tell—at least, until a very late date—that the human body is in an asthenic phase, and will not now bear blood-letting. Nay, years after the assumed advent of this asthenic type, do we find authorities actually warning the student against being led away by the fallacious therapeutics of Louis, who was misguiding the minds of the French youth by teaching them that pneumonia could be cured without blood-letting.

One remarkable exception, it is true, is made; but the exception clearly goes to prove the fallacy of the general argument. Fevers, we were long ago told—I mean, told in the recorded words of authorities—have not borne bleeding well since 1832. But how comes it, I would ask, that it has been recorded as a fact, that fevers did not bear bleeding; and yet no kind of allusion made at the same time to the *now* asserted fact, that neither have pneumonia and other inflammations borne bleeding well since 1832? How comes it, that the physician failed to note the asthenic character of pneumonia, as well as the asthenic character of fevers? How is it, that for the first time in 1857 we are told that pneumonia has been for a quarter of a century of an asthenic, non-bleedable type? If the fact could long ago be so clearly predicated of fevers, why was it so undiscernable in pneumonia?

Now, it does happen that we can give a very satisfactory explanation of why blood-letting was said, and no doubt truly said, to be so ill borne in fevers. Firstly, men's theories of fever were completely revolutionised about the year 1830; and, secondly, as Dr. Murchison has so well shown, the prevailing fever in this country at that date was typhus; the inflammatory or relapsing sort, which had previously

been so common, having become then very rare. But it is a well known fact that blood-letting is infinitely better borne in relapsing than in typhus fever—*i. e.*, does infinitely less mischief. Consequently, blood-letting in typhus would naturally produce worse results in practice than blood-letting in inflammatory fever. No doubt, under the benign influence of better theories, men observed this greater injury done by bleeding in typhus, and therefore they wisely abstained from blood-letting; and therefore, I must also add, unreasonably assumed that diseases had changed their type.

It seems to me, therefore, that a consideration of this subject clearly leads us to this conclusion: that the high authorities to whom I have alluded very correctly recorded their views that fevers were injuriously treated by blood-letting, but that they fell into the *ex post facto* error of judging of the use of bloodletting in pneumonia and other inflammations by the light of the ill effects produced by blood-letting in fevers; and that, in truth, the change-of-type theory, as applied to inflammations in general, was in reality based upon what was observed of the result of blood-letting employed in typhus fever?

Sydenham has been called the prince of observers; but who, at this day, can read those beautiful lessons, and not note—I might almost say on every page—proofs of the fact, that his observation was trammelled by the preconceived ideas which possessed his mind when he came to engage in the practice of medicine? Can we believe that Sydenham's observation was based on a true record of what he saw, when he tells of the rapid changes—I suppose he meant what we call change in type—which fevers underwent, according to the particular constitution of the year? And it is interesting to note, that Sydenham appears to have judged, or rather to have learnt the existence, of these changes, just as they have been learnt in modern days; viz., from noting the different effects produced, as he believed, by his remedies at different periods. Thus, he tells us, that he often found a remedy which would cure a fever at the beginning of a year would kill the patient at the end of the year. "This, at least," his words run, "on the strength of a multiplicity of accurate observations, I am convinced of; that diseases of the character alluded to, and more especially continued fevers, differ from one another, north and south; and that the remedy which would cure a patient at the beginning of the year will kill him perhaps at the close." And, again, he says, "When once, by good fortune, I have hit upon the true and proper practice that this or that fever requires, I can, with the assistance of the Almighty, by taking my aim in the same direction, generally succeed in my results, respect being always had to the age and temperament of the patient, etc. This lasts until the first form of epidemic becomes extinct, and until a fresh one sets in. Then I am again in a quandary; and am puzzled to think how I can give relief. And now, unless I use exceeding caution, and unless I exert the full energies of my mind, it is as much as—nay, it is more than—I can do, to avoid risking the lives of one or two of the first who apply to me as patients."

Can we now, with all our modern knowledge of the nature of fevers, be satisfied that these diseases had really changed their constitution in the brief period alluded to? But if Sydenham fell into such error, is it not probable that we also may err in drawing con-

clusions in a like case from a comparison of what we saw twenty-five years ago with what we see now?

And, again, how comes it, that the other equally remarkable changes in the use of certain capital remedies which have occurred during the last quarter of a century, have not been also adduced as proofs of the change in type of diseases? If the conclusion, as drawn from the effects of bleeding at the present day, be good, why should it not be equally valid as drawn from analogous instances—the employment of other remedies? Thus, for example, why should we not argue that diseases have changed their type, because, at the present day, various acute diseases—peritonitis, pneumonia, meningitis, and carditis—are found to undergo resolution without the induction of salivation? Surely the revolution which has banished the use of mercury, or something like it, from our practice in such cases, is as remarkable as that which has discarded venesection. Why may we not with equal justice and reason say, these acute diseases no longer require mercury for their cure, and have, therefore, changed their type, as say that they have changed their type because we cure them without bleeding?

No one pretends that syphilis has changed its type, because doctors cure it now without mercury, or without giving mercury so as to run off three or four pints of saliva daily from the mouth of the mercurialised victim.

Moreover, the conclusion which I am combating, that men do not bear bleeding now as they did formerly, seems to be still further vitiated by the fact, that not only are we, as observers, unfitted to make the comparison, but that the actual elements of a fair comparison are wanting. In those other past days to which we are referred, bleeding was a sort of universal remedy. Whatever were their diseases, men were let blood for the cure of them. Surgeons prepared their patients for operations by a seasonable bleeding; and, in fact, men in perfect health had themselves periodically bled in order to anticipate the possible occurrence of disease. Hence, the medical man of long experience, who has such a vivid recollection of the excellent effects of blood-letting in his youthful days and in the supposed sthenic times of inflammations, derived his esteem of it from what he saw of the effects in cases which he counted by the hundreds: whereas he now condemns its use from what he notes of its effects in a few rare and isolated instances. He forgets that, in most of his ancient cases, the bleeding was never required at all; and that, being frequently practised on persons who were but little injured by disease, it was not likely to produce injurious effects. I would venture to ask this learned assemblage, how many there are of us who have given up the practice of bleeding through a personal witnessing of its pernicious and depressing effects on man at the present day? I think, indeed, I may safely assert, that the profession as a body has accepted the suspicious conclusion solely on the "word of the master"; this word of the master having been accepted without examination, and re-echoed again and again so repeatedly, that at last what was originally suggested hypothetically has been generally accepted as an undeniable and unanswerable fact.

Besides this, I might add that, in those other days, and under the lingering influence of the old humoral theory, men did not note the injuries inflicted on the human constitution by immoderate bleeding, as we,

in our wiser generation, have been enabled to do. And they, therefore, never thought to lay to the charge of their lancet any ill effects of their blood-lettings; but put them all down to the credit of the disease for which the bleeding was practised. The data, therefore, for any true comparison between the effects of bleeding as the physician noted them in his youthful days, and the effects as he notes them now, are completely wanting; leaving out of the case all consideration of personal incapacity for making the comparison on the part of the observer, resulting from the revolution which his ideas must have undergone whilst passing from youth to advanced age.

Then, again, it must not be forgotten, that there are practitioners who to this moment refuse to admit the truth of the proposition, that the human body will not now bear losses of blood well, and who still practise blood-letting. Certainly, arguing from my own limited personal experience of the effects of blood-letting during late years, and from what I can learn on the subject from others, the conclusion—that men will not bear bleeding, and that we are living in an asthenic phase of time—seems to me erroneous. I cannot, indeed, help saying, that the evil effects of blood-letting have been greatly exaggerated at the present day; indeed, beyond all proof and reason. Where are the proofs of the facts asserted? Who can produce them? I fancy there are few of my auditors who could give any satisfactory reply to the question out of his own personal knowledge. Is not the idea, in truth, so generally accepted, of the dangers of blood-letting, founded rather on some vaguely floating notions—received from authority and accepted without inquiry—than on any tangible and real valid facts? On the other hand, have we not strong proofs to show that the human body still bears large loss of blood with as little prejudice as ever it did? Have we not the experience of those who still practise bleeding? Have we not also the constant fact before us that men lose large quantities of blood in various diseases, from the lungs and the stomach, etc., and yet note that they recover rapidly and well from the hæmorrhage? Again, women after parturition, at times, lose enormous quantities of blood now as formerly: but accoucheurs do not complain that women bear these losses less well at this time of day. Patients, in the hands of the surgeon, also, after accidents and under operations, often suffer severe hæmorrhage; but I never heard of a surgeon who invoked this change-of-type theory to account for any evil results of the same.

Again, even in diseases deemed of peculiar asthenic character, we often observe a similar result. In typhoid fever, for example, large quantities of blood are often daily passed by the half unconscious prostrate patient, who yet may make a capital recovery, and apparently none the worse for his loss. How is it, one cannot but ask, that no illustrations of the change-of-type theory have been afforded us from these quarters?

Moreover, I must also call your attention to the remarkable fact, that the idea of the human body bearing bleeding ill is not an idea of the present generation. There, probably, has never been an age, from the days of Galen to our own, in which blood-letting has not been denounced as injurious to the constitution. But, to keep closely to the case in hand, I will content myself with showing you that, for the last hundred years at all events, men of high authority

have never ceased to affirm, that man's constitution was growing weaker; the proof and test of their assertion being founded on the very fact which forms the base of the modern argument: viz., that men did not bear bleeding as formerly. The opinion of those physicians was, in truth, just the opinion which is now held; only, instead of saying that diseases had changed their type, they say that the human body is less tolerant of loss of blood than it was formerly. I will give you, rapidly, a few proofs of this.

I have already given you the words of those who affirm a present existing asthenic type of disease. I will, therefore, take you back about thirty years, and give you the high authority of Mr. Lawrence at that time, who refutes the theory, let it be observed, while referring to its existence. "A notion," he says, "has prevailed, that people who live in London and other large towns, do not bear depletion well. I consider that this opinion is supported neither by experience nor argument. The dread of depletion has been transmitted from one writer to another without examination or inquiry, and has led to an inert practice." Dr. Southwood Smith and Dr. Tweedie strongly recommended bleeding in fever in 1830; and at that time—the asthenic period, remember, of modern authors—Dr. Tweedie tells us, "No remedy in the treatment of fevers has been more abused than wine." Writing in 1816, Dr. Armstrong deprecates the stimulating practice of erysipelas "still followed in some metropolitan hospitals; for the sake of science and humanity, he earnestly hoped that it would be soon abandoned:" thus rebuking the dread of asthenia which at that time existed amongst certain physicians. Dr. Bateman, again, writing in 1818, thus speaks of blood-letting: "The other active remedy which I have mentioned as capable of abridging the course of fever, if employed early, is blood-letting. I believe there are few physicians, who, like myself, commenced their professional career impressed with the doctrines that prevailed in the schools at the close of the past century, in which the term of debility was certainly predominant, who will not acknowledge that their subsequent practice has been a continued struggle between the prejudices of education and the staring conviction of opposing facts, which were continually forcing themselves upon their observation; and that they have more especially been compelled to a gradual, but material, change in their views respecting the use of the lancet, not only in fever, but in other diseases. I am fully convinced of the extent to which my own practice has been cramped by this prejudice." Why, here, at the very time when, as we are told by some authorities, the sthenic phase of disease was in full operation, we have the learned Dr. Bateman assuring us that blood-letting is the remedy, and rebuking the ignorance of those who, in his youth, had such a dread of debility!

Still further back, we have the testimony of John Hunter that the physicians of his day, just like physicians of our own day, laboured under the idea that inflammations were more asthenic than formerly—i.e., would not bear bleeding as well; the two phrases being then, as now, exactly convertible. "It is observed," he says, "by some of the ablest physicians of this day, that the fever called inflammatory is not now so common in this country as it was formerly represented to have been; that it is now seldom that in fevers they are obliged to have re-

course to the lancet, at least to that excess which is described by authors in former times. They are now more obliged to have recourse to cordials than evacuations; and, indeed, the disease called the putrid fever and putrid sore-throat are but of late date. I remember when the last was called Fothergill's sore-throat, because he first published upon it and altered the mode of practice. I remember when practitioners uniformly bled in putrid fevers; but signs of debility and want of success made them alter their practice. I believe we have much less occasion for evacuations in inflammations than there was formerly. The lancet, therefore, in inflammations, and also purgatives, are much more laid aside."

Why! are not these the very words which are met with in our text-books at the present day? What could be said, or is said, more against the practice of bleeding, than what John Hunter said? And yet men actually base their theory of the change of type on the assumed fact of the greater tolerance of bleeding in men of Hunter's day, and even of days long subsequent to his time.

Another quotation from Hunter I must give, because, curiously enough, he finds in the fast-living of men of his time a reason why they bear bleeding so badly in inflammations; whilst, on the other hand, we find writers of the present day suggesting that we don't bear bleeding as our forefathers did, because we don't live as freely as they did! Never was a more remarkable illustration of the bending of facts to make them fit theory. Hunter says: "We certainly live now more fully than they did formerly. We may be said to live above par. At the full stretch of living, therefore, when disease attacks us, our powers cannot be excited further, and we sink, so as to require being supported and kept up to that mode of life to which we have been accustomed." Hence, then, we see that for a hundred years past, at all events, men of authority have continually, and at all periods of the century, maintained the same doctrine; viz., that men's constitutions were weaker than formerly; and that, therefore, they would not bear bleeding; that this theory of change of type, which now exercises so powerful an influence over our practice, is the very same theory which has existed for at least a hundred past years; and that John Hunter said the very same things about the incapacity of men to bear bleeding in his day, as authorities of this day say.

Surely, then, I may conclude that the practice of physicians at different periods tells little either in favour of or against bleeding. The remarks of Bordeu—the most sagacious Bordeu—delivered about a hundred years ago, express, as I believe, the actual facts of the case. They are golden words. "It has always been thus with physicians," he says. "Past ages ever exhibit them, sometimes storming against bleeding, and desirous of banishing it altogether from medicine; and sometimes using it as a cure for all diseases—counting their victories by the number of their bleedings. The public, too, has always taken a part in these quarrels. We have even known whole towns divided into sections—the friends of the bleeding doctor, and the friends of the doctor opposed to bleeding. Truth to say, bleeding has ever been a subject of dispute, and of most surprising dissensions. But let it be said, to the honour of medicine and of those who cultivate it carefully, that there have always existed physicians of judgment—*médecins*

d'élite—who have joined neither party, rejecting the extreme ideas alike of the ultra worshippers of bleeding and of its enemies. There have always been, and always will be, patricians of this high class in medicine." Elsewhere he sums up, and still in golden words, the actual value of bleeding. "It may be said that, as a rule, the partisans of bleeding over-estimate the value of its effects and the necessity for its use; and that, on the other hand, those who rarely resort to bleeding have an unreasonable dread of its consequences. It is not true that numbers die from bleeding, as some would have us believe; and still less is it true that those who rarely ever bleed lose as many patients as the partisans of bleeding say they do. Daily observation in hospitals shows the fallacy of both these conclusions; and the truth is, that it is not easy to distinguish between bleedings which are useful and necessary, and bleedings which are hurtful and injurious."

[To be continued.]

Clinical Records.

BY

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XIII.—UNUSUALLY SEVERE AND PROTRACTED CASE OF SYPHILITIC DISEASE CURED BY CALOMEL FUMIGATION: WITH OBSERVATIONS.

IN 1856, Mr. — contracted syphilis in India, and went under a course of mercury. In June of the same year, secondary symptoms made their appearance. These consisted chiefly of copper coloured patches, appearing first on the palms of the hands, subsequently on the arms and legs, and on the body. Numerous excoriations, very tender to the touch, appeared on the scalp. The nails of the toes and fingers became affected, and in two or three instances they fell off. Iodide of potassium and sarsaparilla were the remedies chiefly employed at this time. The eruption gradually disappeared, and the remedies were discontinued. In about a fortnight afterwards, however, all the symptoms appeared again. The same remedies were given, and the symptoms again disappeared. In about a month after he left off these medicines, the eruption appeared for the third time, now accompanied by sores on the tongue and lips. The tongue was also fissured, and presented some warty elevations. This third attack of symptoms did not yield readily to treatment. The eruption, however, gradually subsided and disappeared, while the patient was taking nitric acid; but reappeared a fourth time, soon after that medicine was left off. Various medicines were now tried, including the liquor potassæ.

In June 1857, the circular spots of discoloration were still apparent on the body; and the affection of the nails had reappeared. The tongue and lips were also at this time affected. A great variety of local applications were made to the nails, and also to fissures on the tongue, without benefit. The whole of the soft palate gradually participated in the disease, and, together with the sides of the tongue, were excessively tender. Nocturnal pain in the limbs and in the head now made their appearance; and Mr. — became generally out of health, and was subject to much dyspepsia, accompanied occasionally by vomiting and colic.

One of the testicles now became enlarged, and resisted all the ordinary modes of treatment. As there

appeared no probability of Mr. —'s being restored to health, he was recommended to return to Europe, which he did in October 1857.

Mr. — remained in England, being under treatment the whole time, until August 1859, when he returned to Bombay. He continued tolerably well until June 1860, when he contracted fever. This was followed by the appearance of ulcers on the legs. These ulcers healed under the internal use of iodine; but were followed by others of a much more intractable character. The latter were treated by nitrate of silver, zinc ointment, and iodine internally. The ulcerations ultimately healed; but shortly broke out again, and he was now treated by nitro-muriatic acid internally.

Mr. — now appeared to recover; but after being again attacked with fever, the ulcers again broke out, and extended much farther than before. Mineral acids, iodine, guaiacum, and a variety of other medicines, were now given without any apparent benefit.

Fearing that Mr. —'s stomach would become impaired if any more medicine were forced upon him, the surgeon under whose care he was recommended him again to proceed to England with all convenient despatch.

Mr. — arrived in England the second time in September 1861, and underwent treatment by a country practitioner for nine months.

In August 1862, I saw Mr. — for the first time. The upper part of the left tibia was much enlarged and extremely tender to the touch. He suffered much from nocturnal pain in the leg and in the head, accompanied by occasional profuse perspiration.

His legs were scarred in every direction with dark brown cicatrices, and there were some remains of the secondary syphilitic eruptions on the skin. He complained particularly of "flushings of heat", and of pains in the limbs, which were, to use his own expression, "excruciating." Feeling his health undermined, and broken in spirit, he contemplated leaving his profession, and had almost made up his mind to do so.

This gentleman commenced the use of the calomel vapour-bath in August 1862, and continued it steadily for nearly three months. He used occasionally other remedies; but the calomel vapour-bath was the only specific treatment that was adopted. From a very short period after commencing his treatment, his improvement was marked. The enlargement of the upper extremity of the tibia disappeared. The pains in the limbs subsided; the flushings of heat and perspirations ceased. He gained flesh; his spirits revived; and at the expiration of eleven months, he left again to join his regiment in India, apparently cured, and having during that period had no fresh symptom of a specific character. I have had the satisfaction of hearing from this patient that he continued in better health than he had enjoyed for years, on the 1st of January last.

The foregoing is one out of a large number of cases in which the use of the calomel bath has appeared to have been eminently successful, after all other ordinary remedies had failed.

It very frequently happens that patients return from India or China, having suffered from syphilis, and with their constitutions more or less impaired by the influences of a tropical climate. Such patients have often already taken large quantities of mercury internally; and the further use of the medicine in that form is, for many reasons, inadmissible.

When given internally, mercury produces a direct effect upon the liver, probably in consequence of its absorption into the portal system; and, in patients