

In describing the operation of lithotrixy as practised by percussion or crushing, and comparing this method with cystotomy, the surgeon of the Middlesex Hospital has fallen into several grave errors, both with respect to the manual part of the operation and its effects. It would require too much space to refute them in detail, I shall therefore content myself with a general notice. In the parallel which I have just published between lithotomy and lithotrixy, may be found the most convincing proofs that Sir Charles Bell has been led into error, partly by certain disappointed intellects, who do not hesitate to alter facts to suit their own purposes, and partly by an illogical interpretation of the small number of cases which he himself has had an opportunity of witnessing. The new facts which extensive practice in a large hospital cannot fail to present him with, will, I have no doubt, suffice to change his opinion on a method of treatment which, in my hands, has already led to the cure of more than 300 individuals, amongst whom I may enumerate twenty-two medical men, with the distinguished names of Dubois, Edholm, Lisfranc, &c. As to the doubts which certain persons have endeavoured to throw on the results of my practice, and which may, for a time, have suspended public opinion in countries removed from France, I possessed an irrefragable means of dissipating them by invoking an unreserved publicity. I therefore published the name of each patient, and the circumstances attending each operation; the documents have been deposited for many years at the institute, where they may be consulted. It is true certain persons thought they had discovered in those documents proofs contradictory of my assertions, but a more exact analysis has demonstrated the error into which they have fallen. As to the arguments drawn from some vague expressions found in a Report made to the Academy of Sciences, and from some errors which had crept into another Report, addressed to the same learned body, it is sufficient to mention that the very commissioners on whose expressions certain persons thought themselves justified in saying I had lost one patient in four, declared in 1835 that out of 244 patients operated on by me up to the year 1833, 236 were cured and 5 died, while three continued to suffer from the original malady. Surgeons who have not been placed in a position, like the members of the Academy of Sciences, to discover these alterations of facts, must have experienced some difficulty in refusing to believe assertions presented to them with all the appearance of good faith and sincerity; perhaps it was under the inspiration of similar feelings that my honourable confrere of the Middlesex Hospital conceived his very unjust access of anger against the art of destroying calculi in the bladder. The whole publica-

tion of Sir C. Bell, however, is not quite so serious as the points to which I have already alluded; as we advance we find Mr. Bell indulging in certain comic sallies of which the following is an example. A French surgeon established in London, refused 100 guineas from a patient whom he had operated on for the stone, and demanded 400. A short time afterwards the patient having experienced fresh pain, addressed himself to Sir C. Bell, who discovered the presence of a calculus and received a guinea for so doing. The author having thus indirectly acquainted us with the price of his visits, expresses his astonishment at the difference between this latter sum, and that thrown into the *enormous throat of the Parisian Cormorant*. Assuredly the surgeon to whom Sir C. Bell alludes has every right to estimate his operations as he may think fit: but how can any reasonable mind find in this circumstance a reproach against lithotrixy? In mentioning this fact, Mr. Bell adds, the French operator had left some fragments in the bladder, which served as so many nuclei for new calculous formations. I shall not now discuss this point with the author, but cannot again forbear repeating that both the instrument and method employed by the surgeon alluded to are insufficient to remove all fragments completely from the bladder, and that consequently the reproach should be addressed, not to lithotrixy in general, but to a process which has been extolled far beyond its merits.

Paris, June, 1836.

#### ON THE

#### USE OF TURPENTINE IN IRITIS.

By HUGH CARMICHAEL, Esq., Dublin.

To the Editor of THE LANCET.

SIR: In THE LANCET for the 4th of June, I was much surprised to observe, that at a meeting of the *Medico-Botanical Society of London*, which took place on the 25th of May, it was stated by Dr. Stoker, of Dublin, on the reading of Dr. Foote's paper upon the efficacy of turpentine in iritis, that the use of that medicine had been now abandoned by me in the above disease, in consequence of its inconvenient effects upon the bowels and urinary organs.

I am quite at a loss to understand how Dr. Stoker could have been led into this mistake, or imagine such to be my present views respecting it, and beg to state, that the one I have already expressed upon it, is not only unaltered, but is the more confirmed the more my opportunities of trying it are increased; and I believe I may now safely assert, that the correctness of

such an opinion is fully borne out by the testimony of almost all the profession upon the subject.

There are cases of iritis occasionally to be met with, as I have elsewhere remarked, which resist the generally controlling powers of turpentine; and in some of these, where I have seen it urged to a very considerable extent, with a view to ascertain if, under such a mode of administering it, the disease would ultimately yield, the constitution appeared to me to have suffered much; in fact it would seem that turpentine, when so used, would be productive of very deleterious effects upon the system, whence I have been led to think, that as these are peculiarities of constitution, some limit should be put on the extent of administering it, and that where the disease has resisted it after a certain trial, it should not be further persisted in.

This opinion I have expressed to some individuals, and among others, probably to my friend Dr. Stoker; but with respect to its use, in the generality of cases, I never had but the one, namely, that of its being, when judiciously given, a safe and efficacious remedy, and which must be considered as adding very much to our means of controlling the disease, and he certainly mistook me much if he imagined I meant otherwise. Indeed were I now to endeavour to decry it as a remedy in iritis, I am inclined to think I should have no small difficulty to encounter.

I am induced, however, to suppose that there must have been some inaccuracy in the mode in which the observations of Dr. S. were reported on the evening alluded to.

He is stated among other things to have said that he witnessed some of Mr. Carmichael's experiments; but as I never had the pleasure of treating a case of iritis in conjunction with him, I imagine he must here have meant Mr. Richard Carmichael of this city, the author of the well-known essay on Syphilitic Diseases; yet from the manner in which this part of his remarks is reported, they would apply to the person who first submitted this power of turpentine over iritis to the profession, and consequently to me instead of that gentleman. However this may be, I beg to state, that my opinion of the utility of turpentine in iritis is quite unaltered, and that I consider its powers such as entitle it to a very considerable share of use in the treatment generally; and that it is the only means we possess of combating iritis, and, consequently, of preserving vision, in those instances where mercury from previous extensive use or otherwise may be inadmissible, or those in which its employment is incapable of arresting the disease.

I beg leave to take this opportunity of observing "that I have always looked upon it as entitled to a very considerable share of

use in the treatment of the disease generally," because it has been stated that I have recommended it in cases *only* where mercury was inadmissible; that I have not however limited its employment to such instances, requires in my mind a perusal of my observations on the subject, with but a very moderate degree of attention indeed, and in proof of its utility in others besides these, allow me shortly to submit the following case:—

A gentleman whose profession and avocations would render a course of mercury the greatest possible inconvenience, because the subject of an iritis of that low character that is sometimes to be met with as one of the sequelæ of syphilis, was accordingly put on the use of turpentine in drachm doses three times a day; in twenty-four hours a decided amendment was evident, and in four days he was well: the cure was effected without any interruption to his usual occupation, there was no strangury nor any other inconvenience. It would be superfluous to comment on this case as an illustration of the propriety of employing turpentine in others than those alluded to; I think it right however to say, that I should fear it might be received with incredulity, were I not satisfied that similar instances of its powers in certain cases, must by this time be known to every person much engaged in the treatment of ophthalmic disease, unless indeed we can conceive an unwillingness to innovation in practice, which, with some, might, and it would seem had led them to decline altogether a trial of it, probably in the hope that by discountenancing it they might blot it out entirely.

While on the subject, allow me to remark that I cannot coincide in the opinion of its *modus operandi* being referable to counter-irritation, or that its beneficial effects are to be attributed to irritation induced in the alimentary canal and urinary passages, a theory I have seen on more occasions than one put forward in the different journals—at least I consider we are not by any means in possession of sufficient data as yet to justify this conclusion.

That turpentine does generally induce these symptoms, when administered in sufficient doses to cure iritis, must be admitted. But I think it is assuming too much at once to say, that such, therefore, is the mode by which the cure is effected. I know it is stated that, in those cases which are found amenable to its influence, strangury and irritation of the bowels are to be met with, and that the cases which resist it are those in which the above symptoms cannot be produced; to this however I cannot assent, for I certainly, in many instances, have witnessed its sanative powers, without any inconvenience whatever of this description arising to the patient. The cases, as far as my observations go, in which its influence

is manifested in the most favourable and expeditious manner, are those which occur in broken-down constitutions, or of the low syphilitic character; for example that just related, and it is rarely the inconveniences we are speaking of are present in them. Moreover, if the fact be that it is by producing counter-irritation in this manner that turpentine effects the cure, how completely at variance with correct views is it to adopt measures for their removal when they do appear (camphor mixture and flaxseed tea)! Yet such are then had recourse to, and I must admit that I have on my part been so injudicious as to recommend them, and still employ them.

But, further, are we to understand that turpentine cures iritis by the counter-irritation here described, and not by virtue of some specific quality in it, the nature of which we probably have not yet arrived at? If so, should not any medicine capable of inducing these symptoms exercise a similar control over the disease; and might we not, therefore, expect cantharides to be successful—at least capable of producing some benefit, inasmuch as it rarely, if ever, fails of inducing strangury and bowel irritation, when used internally to a certain extent? Yet I should be disposed to hazard the opinion, that with the exception of the occasional advantage to be derived from it in the shape of blister, no benefit whatever is likely to result from its use in iritis.

It is, I perceive, generally considered by those who have spoken of my views of this subject, that I have endeavoured to explain them on the supposition that it is by exciting the absorbents that turpentine acts; with regard to this, however, I have been misunderstood. I do not think I have given any opinion on it; at least, I did not intend to do so, and it would seem as if conclusions which I attempted to draw from a fact that I considered I had established, have been mistaken for an explanation of the manner in which that fact was produced.

That turpentine cures iritis, no matter from what stock of morbid causes it may proceed, I believe I may now safely assume as an admitted point. If we contemplate the disease in certain of its stages, and these quite amenable to that medicine, we shall find it a specimen of the adhesive inflammation, with globules of lymph, the result of that inflammation, effused on the tissues of the organ affected. In such cases turpentine not only arrests this inflammation, but dissipates its morbid effects, among others causing the disappearance of these globules of lymph. My intention, then, in the third section of my treatise on the subject, simply was, from these facts to infer—first, that turpentine must be considered an antidote to the adhesive inflammation; and next, reasoning upon the same grounds as others have done, in consequence

of the disappearance of these globules under its influence, I claimed for it the powers ceded to mercury for the same reason, and that was, that we must regard it as a medicine whose capability of exciting the absorbents was by this fully established; but I did not intend by this to imply that the question as to how it operated in curing the disease was thus solved. Indeed, were anything on that score to be deduced from my reasoning on the subject, I think it might rather refer to the question I likewise put, as to whether the species of iritis met with after syphilis, and in company with what are admitted to be its constitutional symptoms, participated with these symptoms or not, and if so, how this must affect the character of turpentine in a therapeutic point of view. I have the honour to be, Sir, your obedient servant,

HUGH CARMICHAEL.

18, Hume-street, Dublin,  
June 16, 1836.

## IMMENSE TUMOUR OF THE HIP AND THIGH.

*To the Editor of THE LANCET.*

SIR: I have waited with some anxiety for several weeks, under the hope and expectation that the case I am about to record may have found its way into some of our medical journals, drawn up by those who may have watched the progress of the disease, and aided by those who are in the habit of defining and explaining those pathological niceties, which nothing short of the constant habit of such morbid dissections can possibly supply. This opportunity, I understand, has been afforded by the death of the patient, being the tumour sent up to London, but where and to whom I have yet to learn. The object, therefore, of this communication is not only to make known the case itself to the profession, but also to draw forth such pathological remarks as the dissection of the morbid parts has afforded to the practical anatomist in whose possession the tumour is, and which it may, and certainly can afford.

The case has been very briefly noticed in Dr. Johnson's *Medico-Chirurgical Review* for July 1st, 1835, No. 145, under the head of "Intelligence, Correspondence, &c.," where the following account is given of this singular mass of disease:—"We have received drawings of a most extraordinary case, of which but a very inadequate idea can be conveyed in words."

The patient is a woman, thirty-one years of age, of a scrofulous diathesis, her health