

nitre produced from it is not likely, therefore, to be uniform in composition or quality." Two positions are here made, the second of which certainly follows from the first. The first, however, I am in a predicament to prove is not correct, so that the condemnation pronounced by Prof. Redwood on the new spirit of nitre is a deduction from a false premiss. I have frequently prepared the nitrite of soda of the British Pharmacopœia, and have determined on three different occasions the amount of actual nitrite which it includes by measuring the volume of nitrogen it evolves when boiled with a solution of sal ammoniac; and the results,\* which differ from each other only in a very slight degree, have conducted to the conclusion that this artificial saline compound contains constantly very nearly one-fourth, or twenty-five per cent., of absolute nitrite, the remaining seventy-five per cent. being a mixture of soda, carbonate of soda, and unaltered nitrate. Although, then, the nitrite of soda is not pure (it was never believed to be so), the amount of nitrite in it is very nearly constant; and we are, therefore, I apprehend, justified in concluding that the sweet spirit of nitre derived from it will include a nearly constant per-centage of nitrite of ethyl. This, I am sure, Prof. Redwood will admit to be a legitimate inference; for, in his published discourse, he says: "The process I have no doubt would be a very good one if we had the means of carrying it out as intended," &c. These means are surely as certainly supplied by a material including a constant proportion of nitrite, as by an absolutely pure salt.

There are some other remarks by Prof. Redwood on this subject which scarcely require to be noticed. "I have (he says) compared it (the new sweet spirit of nitre) with the preparation made by the London Pharmacopœia, and I certainly prefer the latter as a more agreeable spirit;" and, in another passage, he states that the London process, "under the most favourable circumstances, has yielded a sweet spirit of nitre which the public, who are great consumers of it, have generally approved of." The directions of the London process are very precise, and I cannot guess what the *favourable circumstances* are of which mention is made. There is, however, no difficulty in understanding why the ordinary London sweet spirit of nitre should be popular. It is, as already stated, nothing but spirit; and there is reason to believe, and also to regret, that alcoholic medicines are those in greatest favour with invalids, and that the sale of them is greatly on the increase.

Passing over Professor Redwood's remarks on distilled water—in which I am far from entirely concurring—I come to those on pyroxylin, and am ready to admit that in the process for its preparation erroneous directions have been given. The process was originally correctly written, the nitric acid directed being of the specific gravity of 1.42, or that which belongs to nitric acid including four atoms of water. At the conference, however, subsequently held in Edinburgh, of members representing the several sub-committees, the strength of the nitric acid was raised to that of the acid having the specific gravity of 1.5; and the error has undoubtedly occurred of omitting to direct that this stronger acid should be diluted with half its volume of water. In relation to this omission Professor Redwood has what is technically called a *locus standi*, and he certainly seems thoroughly well inclined to make the most of it. But were he in a more dispassionate mood it might have occurred to him that in a work comprehending such a multitude of details, and prepared for the press by the joint labours of different persons living at great distances from each other, errors of this kind could scarcely be entirely wanting. I believe they will be found not numerous, or of a nature to lessen in any appreciable degree the value of the Pharmacopœia; and I am not without hope that the candid and unprejudiced reader will find in it many improvements, and feel little disposition to join in the clamour which some parties,† for reasons best known to themselves, have endeavoured to raise against it and the Medical Council under whose authority it has been published.

I am, Sir, yours &c.,

Dublin, April 9th, 1864.

JAMES APJOHN, M.D.

\* \* Professor Redwood is no doubt competent to defend his observations. We would point out, however, that Dr. Apjohn

\* The details of the first experiment have been mislaid; but the two others have given the following volumes of nitrogen at 60°, and under pressure of 30. 100 grains of the nitrite of soda of the British Pharmacopœia have been used in each experiment:—

Experiment 1.—Gave of nitrogen ...	32.50	cubic inches.
" 2.—Gave of nitrogen ...	34.78	"
Mean ...	33.64	"

The weight of the 33.64 cubic inches of nitrogen = 10.138 grains; and assuming it to proceed from the reaction exhibited in the equation  $\text{NH}_4\text{O}, \text{NO}_3 = 4 \text{HO} + \text{N}_2$ , the corresponding weight of nitrite of soda will be 24.98 grains.

† It is fit to state that there is no intention whatever of grouping Professor Redwood with the parties here alluded to.

admits that the new liquor acetatis ammoniæ does not contain carbonic acid, and is therefore less agreeable than the old, which is mainly what the former gentleman alleged. As to the strength of the nitric acid, there can be no doubt that it is excessive and inconvenient: the Pharmacopœia was compiled for the purposes of pharmacy, and not for those of the laboratory. Dr. Apjohn dismisses the objections to the formulæ for the scaled preparations with the assertion that they are destitute of foundation. We can assure him that he is wrong, and that they are well founded: for example, the temperature ordered is destructively high. As to making spiritus ætheris nitrosi from nitrite of soda, the idea was a good one, provided this substance could be obtained in a state of anything like purity, but this Dr. Apjohn admits is not the case. Under the most favourable circumstances, the nitrite of soda made according to the process of the Pharmacopœia contains at least seventy-five per cent. of impurities, which entirely vitiates the tests that are given. The product is anything but what the Pharmacopœia describes it:  $\text{NaO}, \text{NO}_3$ . As a general rule it will be still more impure; and its composition will always be variable, for the reasons we pointed out in our seventh article. The spirit will certainly contain nitrous ether, derived partly from the undecomposed nitrate, partly from what nitrite happens to be present. The product is therefore subject to variation, and the theory of the process to a great extent breaks down.—ED. L.

## THE HANWELL AND COLNEY HATCH ASYLUMS.

To the Editor of THE LANCET.

SIR,—I am obliged by your good opinion, but I take leave to complain again of your "inaccuracies." In the first place, it is hardly fair to say that by the course which I have taken—that is, by espousing the cause of my much-slandered asylum—I "indirectly advocate the perpetuation of such blunders as Colney Hatch and Hanwell." No one, believe me, can be more sensible than I am of the overgrowth of these two institutions. It is one thing, however, to abuse them because of their overgrowth; it is another to try to improve them to the utmost in spite of it. I have endeavoured to do the latter by one of these asylums, in my small measure; and you are good enough to give me credit for the same. It is clear that we differ as to the mode and method of improvement; it is equally clear, I think, that those who complain so much of the colossal nature of the Middlesex asylums overlook the most obvious cause of an insane growth, which is not likely to be attained elsewhere.

The difficulty of obtaining land in the county of Middlesex and the neighbourhood of London is far greater than in any other county of England. It may be very fine and very philanthropic to write about the desirableness of building asylums for five or six hundred patients only. But there are others to be consulted besides editors of newspapers, medical superintendents, or even commissioners in lunacy. The ratepayers have a right to a voice, and they will say, with much truth, that if Hanwell and Colney Hatch had been distributed into five separate asylums, and one more were now to be added to meet the growing pressure even upon our limited space, they would be much more heavily taxed than they are now, or than they have any right to be. The same advantages and the same opportunities do not present themselves alike in all counties any more than do the same soil or the same salubrity. This is specially true in reference to the possibility of obtaining five or six eligible sites for large lunatic asylums in the small metropolitan county. I do not think this circumstance ought to be overlooked in discussing and censuring the admitted evil of magnitude; nor do I think that the pointing out the fact justifies the imputation of a desire to "perpetuate blunders."

I still maintain that I have a very large, though not an absolute, discretionary power in the selection and discharge of attendants. The solitary case which I mentioned of my advice to the Committee not being followed may be forced into an illustration of your argument. But the case is so exceptional that it ought not to be made use of for the purpose of strengthen-

ing your position. It was but an instance of kindness on the part of the Committee, and it did not take place without an expression on the part of the Chairman of the value which he set upon the opinion of the medical superintendent. You have contrasted our system of hiring and discharging attendants with that which obtains at the principal county asylums, four of which you specially allude to, and one of which—the Essex—is admitted to be about the best asylum in the kingdom. Now it so happens that I was at Brentwood a few weeks ago, spent the afternoon with my friend, Dr. Campbell, and went with him over his admirably managed institution. Of course it is only the opinion of one man against another if I say, in opposition to you, that I think our airing courts are quite as good as his. But what about the hiring of attendants? Dr. Campbell distinctly told me that he declined to take the responsibility of hiring and discharging. He sees beforehand and approves, and upon his recommendation to the Committee, (before whom they appear,) attendants are taken into the service. So also as regards discharging. In this matter, therefore, as superintendents our positions are precisely the same. We have both the power to suspend attendants, and send them off the premises, at any moment, until the next Committee meeting.

Again. You observe: "At Colney Hatch it is, moreover, not uncommon for an attendant to refuse to submit to the stoppage of leave &c. imposed by one of the officers, with the request 'to go before the committee'—a step sometimes ending in the reversal of the proposed punishment, with what effect on the discipline of the house can readily be supposed." Speaking for the male department, for which alone I am responsible, and with the management of which alone I am acquainted, I give this statement the most emphatic denial. Such a circumstance has never occurred, and never will occur, in my time. Regarding the labours of the attendants as very arduous, and of a nature to require our prescribed recreation, I never think of stopping their leave, whatever their conduct may have been. On the score of health alone I decline to do this, and prefer enforcing discipline by safer methods. I have the power, it should be added, of granting extra leave; and this I frequently do to attendants who have conducted themselves satisfactorily. So that what you state to be of not uncommon occurrence, certainly does not take place on the male side of the house.

Again. If I abstained in my last letter from noticing your remarks about our workshops, it was not because I was insensible of their injustice, but because I was anxious to confine myself strictly to my own department. But you force me now into pointing out your unfairness and "inaccuracy." You write: "The shoemaker's work is very deficient in quantity. Only one pair of men's leather shoes made! and no men's leather boots. A well organized asylum ought to make up from the raw material all the clothing required." Now the object of this remark is to bring under notice the small measure of our industry. But surely it is somewhat disingenuous not to make equally public some of the facts recorded in the very table which has furnished you with these truths. Why not have supplemented your telling fact with another one which you now compel me to supply, though it was ready and available for you at the very time when you made your first extract? We made up 528 cloth and canvas boots during the last year; we repaired 889 boots and shoes, soled and welted 4876, and locked and strapped 1598. To suppress this great fact, and give prominence only to the other little one, upon which the acute critical eye could fasten so eagerly, is something very like being "inaccurate"—something very like misrepresenting—something very like conveying a false impression of the value of our labour in the shoemaker's shop. In your table it would stand at 3s.; in mine at £283 4s. 8d. The number of our repairs is so enormous that we have no hands for new work from the artisans furnished from the ranks of the patients. And the same remark applies even in a larger sense to the tailor's shop, where the number of repairs in 1863 ran up to 11863. To put a new piece into an old garment is often a much more troublesome affair to mad fingers than to make a new garment altogether. The measure of our industry is the *quantity* of work done, and not the *newness* of it. Perhaps you will scarcely credit me when I tell you that, during the past year, I only admitted seven tailors and seven shoemakers as patients. This is not a very large number to add to our fluctuating staff of artisans, after deducting (more than half) those who have been unfit or unwilling to work. All that I can send to the shops I do send; and it rests entirely with the medical superintendent to subtract from or to multiply the number of working patients. So far, therefore, I am responsible for the inefficiency of which you complain, though you are good enough to say that if the workshops were under my management things would be very

different. But I cannot admit this. Things would, I assure you, be nothing of the kind; and I have no desire to see an injustice rendered, at my expense, to a long-trying and valued officer of this institution. Such is Mr. Henderson, our steward, who knows his work much better than I do. When we have a sewing-machine (as I hope we soon shall have), we shall, perhaps, be able to bring up the number of newly made articles to the high figure of the Sussex Asylum, where one of these valuable machines is in full operation. Whether more articles of clothing are made up at the Essex Asylum (one of the model ones by which you gauge poor Colney Hatch) than here, I am not prepared to say with certainty, as I have not received one of Dr. Campbell's Reports. But I have an impression that they do not do much there in the way of artisan labour. Nay, I have almost a conviction that they do not make up the patients' clothing; and I know that a London contractor, who supplies us, has supplied them largely with various articles of wearing apparel which might be put together on the premises, if the medical superintendent thought such a course expedient or desirable.

If, in the last place, I refer again to the organization of our night-watching, it is not with a view of depreciating the very excellent and creditable report which you placed side by side, for the sake of contrast, with mine. I maintain that for the class of patients under my care, comprising nearly 100 "general paralytics," upwards of 150 epileptics, more than 300 cases of uncomplicated dementia, together with idiots and imbeciles in the lowest state of physical deterioration, an average of 35 wet and dirty per night is a very moderate one. I do not approve of the indiscriminate getting up of all patients who are insensible to the calls of nature, simply because I have tried it and found that in many ways, and for various reasons, it does not answer. I may be wrong. But, believing I am right, I act consistently and conscientiously.

Thus, Sir, I have endeavoured to point out to you, as succinctly and in as moderate a tone as I am capable of, some of the "inaccuracies" of which I complain, together with some of the opinions which I hold, at variance with your own. I hail your recognition of the improvement at Colney Hatch with the greatest satisfaction, and I am fully sensible of the kind manner in which you speak of myself. But still there are so many old grievances to those who embark in a critical spirit upon the large subject of this institution! Ever since this colossal structure was erected, it has been customary for the medical press to abuse it, and those connected with it a magisterial capacity. No one has taken up the pen to defend it, or repair the literary breaches which have been made in its walls by the sharp arrows drawn from the professional armoury. But while I am partly in charge I shall mount the walls, even if I stand alone, and do what I can to repel the assaults which are made against us. If departments are here divided by reason of our magnitude (an admitted evil, not now to be rectified) our councils should not be divided, and we should learn to work adaptively and harmoniously.

Do regard us, Sir, with as much favour as you can in the future, and we will strive under all circumstances to be worthy of your good opinion.

Your faithful servant,

EDGAR SHEPPARD, M.D.,  
Medical Superintendent Male Department.

Colney Hatch, April, 1864.

\* \* Owing to the absence of our reviewer we have been unable to submit this letter to him, but think it right meantime to print Dr. Sheppard's letter as it stands, without comment.—ED. L.

## THE CASE OF INTESTINAL OBSTRUCTION AT TUNBRIDGE.

To the Editor of THE LANCET.

SIR,—In the discussion at the Royal Medical and Chirurgical Society which took place on the 12th instant on a case of intestinal obstruction, my name is mentioned in connexion with it; and as I was present at the post-mortem examination (and which was only permitted at my urgent request), I feel bound, after the remarks made in your leading article, to state the impression left on my mind by the morbid appearances. I have hitherto steadily resisted all controversy on the subject, and regretted exceedingly that the friends of the patient should have been pained by the premature publication of the case, involving, as it did, much personal feeling.

The account of the post-mortem examination, published by Dr. Bell in THE LANCET of July 25th, 1863, p. 95, is, to the