As yet, however, but slight attention was given to ordinary labours. When any difficulties occurred, or operations were required, all such questions were carefully examined, and rules laid down for practice; but the phenomena of natural labour were left, in the strictest sense, to Nature. On the continent, however, Saxtorph, Solayres de Renac, Baudeloque, and Naegele took a different view. Every case they attended was a subject of interesting inquiry. They educated their sense of touch to the highest point; and, carefully watching the progress of natural labour, they ascertained that the head did not descend in one position, as was supposed, but in several. The frequency of these positions is at present a subject of close observation with the scientific accoucheur.

The lesson which these eminent men have taught us is the value of patiently observing natural labour. They pointed out the instruction you derive from them. They showed the greater facility you acquire in at once recognising a difficulty. The acute sense of touch, which enabled Naegele to mark the progress of the head, at once enabled him to perceive what may retard its advance, and perhaps to remove the impediment before it obstructed the action of the uterus. I only ask you, gentlemen, to follow his example, not to be governed in your views of obstetric practice by what are called authorities, but to judge for yourselves. Nothing is so easy as to follow an authority, once you decide who is to be your guide. I should rather ask you to seek, at the bedside of your patient, a knowledge of the truth. I would ask you to make every case you attend a subject for observation, and briefly to note the facts you have ascertained. You will thus acquire that *tactus eruditus* so essential to successful practice.

I am induced to press this point upon your attention, more particularly because formerly—indeed, I might say, until very lately—no interest was taken in natural labours; all attention was given to cases of difficulty or danger. Hence the obstetric student was very anxious to witness operations, to watch the treatment of hæmorrhages, etc.; but the ordinary cases of labour he was given to attend were thought to be a bore. Six cases of labour were considered sufficient by the examining bodies as a test of practical knowledge; but when that number was increased to twelve, and to twenty, the students were startled, and many of them thought this to be too great a demand upon their patience. I have endeavoured to prove, from the history of midwifery, the reverse; and would convince you that it was the want of attention to natural labour which led to so many mistakes when it became difficult. I am anxious to prove to you that the most eminent men in the profession, like Mauriceau, like Smellie, like Hunter, noted all their cases; and these cases formed the basis of their future reputation. You can do the same; and, if you wish to practise midwifery successfully, I would say, in conclusion, do not trust implicitly to books, which can be read in your studies; neither be governed by authorities, which are often wrong; but let your study be the bedside of your patient, and your book, *the book of Nature*.

DRUGS IMPORTED FROM CHINA. During 1862 we received from China, of camphor 1222 cwt.; of cassia 345,140 lbs.; of oil of cassia 20,166 lbs.; of rhubarb 165,326 lbs.: and of other essential oils 62,634 lbs.

Original Communications.

ON DIPHTHERIA.

By J. WEST WALKER, M.B.Lond., Spilsby, Lincolnshire.

IN a previous communication to this JOURNAL (May 16th, 1863), I attempted to show that the theory of the nature of diphtheria might be embodied in the following conclusions.

1. The characteristic formation is but an external complication, and has no specific relation to any particular state of the general system.

2. The general diseases with which this formation is found to be associated are most various—ranging from the most trifling malaise to the most virulent septicæmia, and extending through the whole class of acute specific diseases.

3. Possibly during the prevalence of a diphtheritic epidemic there may be a distinct general disease altogether different from other known diseases, but we have no positive evidence on the subject.

4. Diphtheria, in the sense in which the word has hitherto been employed, is to be looked upon, not as one disease, but rather as many diseases, alike only in being associated with the common characteristic formation.

I shall endeavour now to explain how, by adopting such a theory:

1. The difficulties which have hitherto beset bibliographers in collecting the ancient history of the disease, are to a very great degree removed.

2. The various questions arising on the subjects of diagnosis, prognosis, etiology, and contagion admit of more satisfactory solution; and

3. Treatment having more reliable indications becomes less empirical, more rational, more successful.

One of the greatest difficulties with which those who have written on the history of diphtheria have had to contend, has been to determine whether the author they were quoting was describing diphtheria, in the generally accepted sense of the term, or merely instances of ordinary diseases complicated with peculiar manifestations. We frequently read of cases wherein the characteristic false membrane is so minutely and accurately described, both with regard to its physical and pathological properties, as to leave no doubt as to its perfect identity with the like phenomenon as at present seen; and yet in the same case or series of cases we find, perhaps, as clearly portrayed the diagnostic sign of some well known general disease, the eruption of an exanthem, the false membrane of croup, etc. Then it is we become impaled on the horns of a dilemma; we must either reject such cases as evidence of the previous existence of diphtheria as a specific disease, or we must acknowledge that of old, as at present, the pathognomonic sign was observed to present itself in connection with a great diversity of general symptoms. Should the conclusions set forth in a former part of this essay be correct, this difficulty no longer exists, and the study of the earlier history of the affection (or affections) becomes proportionately more simplified and intelligible.

My position would perhaps be best illustrated by selecting extracts from different authors who have written on the history of diphtheria. If we take Dr. Headlam Greenhow's classical work on the subject, and refer to his chapter on "Diphtheria in the Sixteenth, Seventeenth, and Eighteenth Centuries," I think that, notwithstanding the extreme care with which his cases are selected, abundant evidence can be

found to show that the difficulty in question exists, and that many different general diseases, alike only in being complicated with the common diphtheritic sign, have been confounded together and brought forward as supporting the fact of the previous existence of a *soi-disant* specific disease. We shall find sufficient to show that during the later centuries, as is familiar to every observer in the present, the diagnostic sign of diphtheria—the peculiar formation, was observed to be associated with scarlet fever, with epidemic sore-throat, on blistered and erysipelatous surfaces, with small-pox and with measles, with croup, with "putrid fever of any kind" (and this class would probably include those cases wherein purpura and petechiæ are mentioned as symptoms), with ague or remittent fever. A goodly list of general diseases which, were I disposed to quote from Dr. Greenhow's next chapter, on "Diphtheria in the Nineteenth Century", might be considerably increased. In short, the more nearly we approach the time of our own epidemic, the more closely particular cases are watched, and their general symptoms faithfully recorded, the more conclusive becomes the evidence that there is no one particular state of the general system to which the term diphtheria can, with any approach to precision, be applied, but that hitherto all the ills that flesh is heir to have been huddled together as one disease, simply because they present a common complication.

The query then presents itself: Can we receive records such as those alluded to as evidence of the previous existence of diphtheria? If we adopt the theory advocated in these papers, the question admits of ready and easy answer, for they unquestionably prove the previous existence of epidemics of diseases in connection with the remarkable diphtheritic product; but if, on the other hand, we agree with those who believe in the theory of diphtheria being an acute specific disease, the question is at once surrounded with difficulties, and we are compelled to resort to such apologetic phrases as "It seems probable, as has sometimes happened in more recent epidemics that scarlet fever and diphtheria were intermingled; that the cases of *scarlet fever* had a *diphtheritic character*; and that, whilst in all probability Dr. Fothergill saw some cases of uncomplicated diphtheria, he yet *confounded* the two diseases." Again, "That the epidemic of diphtheria of that day, like that of our own time, was apt, so to speak, *to impress its character upon other diseases* and included simple sore throats unaccompanied by exudation, as well as those of more malignant nature." Again, "That diphtheria in that day, as well as in our own, was frequently confounded with scarlet fever" (Greenhow, p. 28, 30, and 35); not necessarily confounded at all, when we believe in the theory that would make scarlet fever, in common with a host of other general diseases, amenable to the influence of an external complication. What other specific disease *impresses its characters* on such a number of fellow diseases? Why continue to believe in the existence of a disease, of which its strongest advocates are unable to adduce one diagnostic sign. Absent the peculiar complication, the diphtheritic false membrane, and what is diphtheria? Present it, and what is not?

Diagnosis. Much has been written to show what diphtheria is not; very little to define what it is; and without something like a definition, without some signs or symptoms, differing from the signs or symptoms of other diseases, diagnosis becomes almost impossible. Pages have been written, and particular cases cited, to show that diphtheria is not scarlet fever, nor croup, etc., and to point out the differential diagnosis between it and these several diseases; but on the other hand, equally well might pages be written to prove that in particular cases it is nothing more than one or other of these diseases peculiarly complicated. It is the specific disease theory which finds a difficulty in these apparently contradictory statements; and it is the same theory which demands from its votaries much, and to my mind, useless argument on the subject of diagnosis, but which has, I fear, succeeded only in making a very important subject a matter of confusion worse confounded. How often has every practitioner, whose lot it has been to practise during a diphtheritic epidemic, been asked the momentous question—Is it diphtheria? and found it perplexing at first how rightly to answer it. The term has become so associated with fearful consequences that he hesitates to apply it to mischief (in this particular case perhaps) so slight, and yet the diagnostic sign—there it is. He knows full well that he has frequently met in practice with the like sign in connection with a great variety of general diseases—the acute, the chronic, the benign, the malignant, the specific, the non-specific—that he naturally hesitates before giving a decided answer to the question, an answer which involves the application of a single term to a series of widely different phenomena alike only in one, and that by no means important feature. If we speak of diphtheritic diseases, diagnosis becomes at once rational and easy. When and wherever the formation presents itself—and I need not here enter on the diagnosis of that well known morbid product—the case is clearly diphtheritic; but what the general disease with which it is associated may chance to be, must be left to the ordinary rules of diagnosis applicable to such general diseases.

[*To be continued.*]

Reports of Societies.

LIVERPOOL MEDICAL INSTITUTION.

MARCH 31ST, 1864.

R. GEE, M.D., Vice-President, in the Chair.

PUBLIC BUSINESS.

Gratuitous Medical Attendance. Dr. IMLACH moved—"That the Medical Society of Liverpool regrets that sudden and dangerous accidents and illnesses occasionally occur among the poor of large towns, to meet which the present arrangements of the Poor-Law Board and of the Medical Charities are inadequate. That the public has no right to expect that the members of the medical profession shall, *nolentes volentes*, be burthened with gratuitous attendance upon such cases, and insulted at inquests for declining to attend strangers. That it is the duty of Boards of Health or of the police to secure medical attendance on such cases by paying, out of the public purse, fees for all such urgent cases as the police officer on the beat may report as requiring immediate attendance."

Dr. IMLACH said there had been a number of letters in the papers on this subject. He read the following memorandum, a minute of the Watch Committee, that had been handed to him by Major Greig, the head constable.

Memorandum relative to Medical Attendance upon Cases coming under the Notice of the Police. In August 1859, the following order was issued to the police: "When the attention of the police is called to poor people in the streets, or in their dwellings, in cases of sudden or extreme illness, they are at once to call on the nearest parochial medical officer. In all other cases, when time will permit, the relieving officer of