in 1870, with the object of establishing a pure oral school for deaf children and a training college for teachers of the deaf on the oral method, and by these means of reintroducing into the country generally the practice of the oral instruction of deaf children.

Both the school and the training college have continued their work uninterrupted down to the present time, mainly through the benevolence of a generous public. It is, however, to the training college I now wish to call attention, as the committee find themselves confronted with a task of exceptional difficulty in their endeavour to develop this side of the association's activities. That the college has been faithfully administered and successfully conducted may perhaps be best indicated in a word by stating that every Fitzroy-square student presented for the professional examination held by the joint examination board for teachers of the deaf, and open to candidates from all parts of the British Isles, has satisfied the requirements of the examiners, and one of them has each year headed the list of successful candidates. The work of the college has, however, been conducted under constant financial strain, and at no time has this strain been more severe than it is at the present moment. The college buildings are now inadequate for their purpose, and, further than this, the committee desire to broaden the basis of instruction given to the students and provide a training which shall not only satisfy the strictly technical requirements of teachers who seek service in certified schools for deaf children, but give them that degree of general culture and professional skill as will enable them to undertake with the fullest measure of competency the very important work to which they intend devoting their lives. *The State gives no grant towards the college expenses; consequently fees have to be charged and the benevolent public appealed to from time to time.*

An appeal is now being made for £3000, sufficient for the time being to enable the committee to carry out their proposals in a modest manner, though not to place the work on a permanent footing, and so far a fund has been collected amounting to about £1800. If the public could but realise the essential importance of an adequate supply of properly trained teachers, capable of giving the deaf children of the country a sound oral education, I feel sure the money would be forthcoming for our purposes. It is not enough that schools shall be opened and children compelled to attend them. They must also be staffed with properly trained teachers, and surely the work of providing the necessary training is one worthy of the most liberal endowment.

Mr. Macleod Yearsley's excellent articles on the education of the deaf child, which have just appeared in your journal, will, I trust, help not only to remove widespread misconceptions of the deaf child's educational capabilities and of his possibilities for useful citizenship, and bring about a better classification of deaf children at school, but help the public to realise how important the work of instructing these afflicted children is, and inferentially how essential it is that there should be a proper supply of competent teachers to undertake their instruction.

I am, Sir, yours faithfully,

G. SIBLEY HAYCOCK.

Fitzroy-square, London, W., March 15th, 1911.

THE SPHYGMOMANOMETER AND THE SPHYGMOGRAPH IN RELATION TO THE MEASUREMENT OF ARTERIAL BLOOD PRESSURES.

To the Editor of THE LANCET.

SIR,—I am indebted to Dr. George Oliver for his generous recognition of my address on "The Sphygmomanometer and the Sphygmograph in Relation to the Measurement of Arterial Blood Pressures," and I am, of course, glad to have the advantage of his support. Doubtless, as he says, the multiplication of the time points of the arterial cycle at which "pressure" may be read will increase the clinical value of the sphygmomanometer, but even when the maximum and minimum can each be estimated with confidence the rapidity, or otherwise, of the movement from one to the other will still be beyond the resources of the instruments at present at our disposal. Since Dr. Oliver's demonstration at the Royal Society of Medicine I have

had some experience of the ingenious apparatus he has given us, and so far there does seem to me some presumptive evidence that by the auditory method, leaving on one side the question of the resistance offered by the arterial wall, a measurement of the diastolic pressure may perhaps be effected. At least, in all cases in which I have found the maximum record abnormally high Dr. Oliver's tambour has indicated also a high diastolic reading, with one exception, and that a decidedly impressive one—namely, cases of aortic regurgitation. In these, in my experience, the tambour gives a very low figure as the diastolic record, sometimes, indeed, at, or close to, zero. As in aortic regurgitation the physical conditions are particularly favourable to the fall of the intra-arterial pressure during cardiac diastole to an unusually low level, the fact that in such cases the auditory method announces a very low "diastolic pressure" may reasonably, if confirmed, be quoted in support of the claim that by this method the "diastolic pressure" may be estimated.

Dr. Reginald Pratt (THE LANCET, March 18th, p. 765) raises another issue. He challenges without qualification the whole art of sphygmometry. Now it is no doubt true that the sphygmograph, like other apparatus, may be deliberately manipulated for a particular purpose, or may, in careless or inexperienced hands, yield, within limits, unreliable results. But these possible inaccuracies can be avoided by due attention and practice, and defects due to one or other of them can, for the most part, be detected, not only at the time of record, but even in the completed sphygmogram. For example, I feel sure that anyone accustomed to the use of the sphygmograph will readily recognise that the peculiarities of Dr. Pratt's tracing B depend on some artificial limitation of the free movement of the lever of the sphygmograph. The difference between his tracings A and B is not that one shows "ill-sustained pressure" and the other "well-sustained pressure," but that A has been taken with a sphygmograph so adjusted as to allow free movement of the lever, whereas B is the expression of a checked or limited movement. I do not know under what guidance Dr. Pratt recognises A as answering to the description of a "low tension" or "ill-sustained" pulse, but personally I should not, from a contemplation of the tracing, have come to this conclusion.

I am, Sir, yours faithfully,

Harley-street, W., March 20th, 1911.

C. O. HAWTHORNE.

"606" AND SYPHILIS.

To the Editor of THE LANCET.

SIR,—Allow me to make a few remarks in reply to the criticism of my article on "606" by Dr. James McIntosh and Dr. Paul Fildes, published in THE LANCET of March 18th. In the first place, I must object to their accusation of my having cast a slur on the name of the inventor of "606." In my article I expressly stated that, "as its inventor is one who holds a high position as a medical scientist, it is necessary to deal with this preparation on its merits." As to the manner in which this preparation was "boomed," I do not know who was responsible for this, and I am not the only one who has drawn attention to it. In the second place, I beg to state that the views expressed in my article, which Dr. McIntosh and Dr. Fildes say are "unjustified and at variance with the opinions of the vast majority of competent observers," are shared to a greater or less extent by many experienced syphilologists and other members of the profession who are certainly competent. Whether these views are unjustified or not time will show; at any rate, the time appears opportune for a little wholesome criticism. After this preamble, I will pass on to the consideration of some of the statements made by Dr. McIntosh and Dr. Fildes.

1. These authors remark that "on the average, if not invariably, '606' is far superior to mercury in the symptomatic treatment of syphilis"; yet, in a previous paragraph, they state that "'606' has no reference to symptoms—it is purely a parasiticide." How these two statements can be reconciled is not apparent. But if "606" is purely a parasiticide, I should like to ask Dr. McIntosh and Dr. Fildes

how they explain the undoubted healing effect of this preparation in cases of malignant syphilis, in the lesions of which, according to Buschke,² spirochaetes are absent. The explanation given by Buschke is that the action of arsenobenzol is not parasitotropic, as Ehrlich supposed, but organotropic, and that it acts in a similar way to mercury and iodides. This view also applies to the lesions of tertiary syphilis, in which spirochaetes are absent or few in number. I may add here that Dr. McIntosh and Dr. Fildes's assumption that, in my statement that treatment by "606" is symptomatic only and "not treatment of the disease itself," I "refer apparently to the spirochaete," is incorrect; the spirochaete is by no means the only factor concerned in syphilis, as Sir Jonathan Hutchinson has well pointed out in the new edition of his book. Dr. Levy-Bing,³ who has just published the results of six months' experience with intramuscular injections of "606" at the St. Lazare Hospital, Paris, also comes to the conclusion that this preparation only cures the symptoms of syphilis, not the disease itself.

2. Dr. McIntosh and Dr. Fildes consider that "'606,' applied at properly ascertained times, will prevent the onset of symptoms with far less inconvenience and effect upon the general health than mercury." But, so far, there is no evidence that "606" has any action in preventing the onset of symptoms; in fact, according to Levy-Bing, recurrent symptoms are more frequent, and often of greater severity, after treatment by "606" than after mercurial treatment. With regard to the effect of mercury on the general health, so far as my experience goes (and this agrees with that of Sir Jonathan Hutchinson) the general health is generally improved by a course of mercury.

3. Dr. McIntosh and Dr. Fildes are undoubtedly correct in stating that "'606' will probably be used by the unskilled," but that this will be "without danger" is hardly to be believed. Apart from the deaths and other severe accidents following the injection of this preparation, which may in part, I admit, be attributed to faulty technique, there is an increasing amount of evidence to show that "606" has a deleterious effect on the nerves. This neurotropic action has been pointed out by Buschke and Fischer,⁴ Finger,⁵ Stern,⁶ and others, who have reported cases of paralysis of the ocular muscles and affections of the internal ear. Buschke even goes so far as to consider that, in addition to the directly injurious effects of arsenic, "606" may cause deposits of arsenic in the internal organs, which predispose to further manifestations of syphilis. Finger, on account of this neurotropic action of "606," does not consider it indicated for the ordinary treatment of syphilis. "Aus den Gesagten glaube ich zum Schlusse kommen zu sollen, dass das Arsenobenzol sich vorläufig zur Anwendung in der Praxis nicht eignet."

I am, Sir, yours faithfully,

London, N.W., March 18th, 1911.

C. F. MARSHALL.

"TERRIBLE NEWS OF THE PLAGUE."

To the Editor of THE LANCET.

SIR,—I have read in this week's issue of THE LANCET a report from St. Petersburg forwarded by your Constantinople correspondent and entitled "Terrible News of the Plague." The news may be terrible, but is not in any way founded on fact. I have but a few days ago returned from Peking, travelling through Manchuria, and when at Mukden met two of the workers from Harbin—namely, Dr. Wu and Dr. J. M. Stenhouse—so that I had ample opportunity of learning about the exact position of affairs.

As has been already reported by your China correspondent, active and effectual antiplague work has been carried on in the Chinese town of Harbin (Fuchiatien) for the last two months or more by Dr. Wu and other Chinese doctors, assisted by Dr. J. G. Gibb, Dr. W. H. G. Aspland, and Dr. Stenhouse, who are colleagues of my own at the Union Medical College, Peking. At the beginning of February the death-rate in Fuchiatien was as high as 150 a day, but since then conditions have steadily improved, so that at the time of my leaving (Feb. 28th) it had fallen as low as only two a day.

² Berliner Klinische Wochenschrift, No. 1, 1911.

³ Annales des Maladies Vénériennes, March, 1911.

⁴ Loc. cit.

⁵ Wiener Klinische Wochenschrift, Nov. 24th, 1910.

⁶ Deutsche Medizinische Wochenschrift, Jan. 5th, 1911.

When these doctors first went up to Harbin the work was much obstructed by some of the local officials. Subsequently, by orders from Peking, these men were degraded, and thereafter the governor of the province and other officials did everything in their power to help the medical men in charge. The bodies of the dead were burnt to the number of about 4000, and have not been left lying about the streets as your correspondent describes. A large and efficient staff of sanitary police has been employed, while house-to-house inspection, rehousing, and isolation work have been steadily carried on, with the very satisfactory results described above. Naturally, the distress and suffering have been very great, but more from the intense cold and entire dislocation of trade than even from the plague itself. It is these conditions which have made the work so extremely difficult. Part of the Chinese Town had to be burnt down and large numbers of people had to be temporarily housed, whilst many more, as could only be expected and unfortunately could not be prevented, scattered to other parts of the country. In consequence, the town is in some measure deserted at the present time.

It is a pity, however, that these reports sent from St. Petersburg should be so inaccurate and misleading, as they bring discredit on the excellent work which has been done and which has been fully acknowledged by the Russian medical authorities. Similar reports have previously been circulated through St. Petersburg, an example occurring only the other day, when it was stated that 18 out of 19 Chinese doctors had died of plague at Kuanchengtze. The facts subsequently telegraphed by Dr. Young, also of the Union Medical College, Peking, were as follows: "Hospital under native doctors closed because four nurses, two doctors died. No precautions observed." This probably refers to a hospital manned by untrained Chinese doctors, many of whom have served bravely and faithfully in spite of their insufficiency of medical knowledge.

Before leaving Peking at the end of February I had abundant opportunity of seeing and hearing about the work done to prevent the spread of the plague. In consequence of ignorance of the danger and of the grievous insufficiency of trained medical men, the work was begun late and carried out under many difficulties; but, speaking generally, the necessary measures were widely and gladly adopted and the officials heartily coöperated in the work. It is to be hoped, however, that the present circumstances will help to awaken the Chinese Government and people to the intense need of national public health work, and of efficiently trained men to carry this out.

I am, Sir, yours faithfully,

H. V. WENHAM.

St. Bartholomew's Hospital, London, E.C., March 18th, 1911.

OPEN WINDOWS IN THE TREATMENT OF CERTAIN DISEASES.

To the Editor of THE LANCET.

SIR,—About three years ago I called attention to certain facts which seemed to indicate that the prevalence of adenoids among children resulted from making them sleep during cold and damp weather with the windows open. Among other things I then noted, of 49 children who had been brought up strictly on the open-window system 22 had been operated on for adenoids, while in 69 children who had been brought up on the shut-window system only two cases of adenoids were observed. Further investigation seems to me to have confirmed the supposition that cold and damp air predisposes to, or is in some way generally associated with, the causation of adenoids, and I understand that some laryngologists now admit my contention. Indeed, one told me that he was now "absolutely convinced," and another that he had been "completely converted."

Further considerations have led me to suspect the wisdom of treating certain diseases on the open-window system. That cold and damp weather has a very marked effect on the prevalence and severity of certain diseases is conclusively shown by the frequency of these diseases in winter and the mortality returns at particular periods of the year associated with cold and damp weather. On the other hand, as far as I have been able to learn, there are no satisfactory statistics which indicate that the open-window treatment of influenza, common colds, measles, and bronchitis is attended with more beneficent results than the shut-window system. The