The heart's apex was in the sixth interspace, one inch outside the nipple. The temperature varied from 101° to 103°. The appetite was capricious. There was no expectoration and no albuminuria. There was an opening discharging pus opposite the ninth rib, outside the angle of the scapula.

Fig. 4.

Fig. 5.

I enlarged the cutaneous opening, and, at the outer part of the subcutaneous abscess, discovered a small track passing into a cavity within the ribs. This gave exit to a large quantity of pus and hydatid membranes, and then it was found that the cavity was situated altogether beneath the diaphragm, which had been pushed up to this enormous height by the gigantic hydatid cyst.

I would here raise the questions, without venturing to answer them, Whether an enlargement of the liver often or always pushes the heart over more or less than a collection of fluid in the pleura. In this case the dislocation was very considerable. The cavity was washed out with solution of chloride of zinc (40 grs. to 3j), and the wound was dressed with boracic lint and salicylic wool. The liver rose one inch.

The case progressed favourably, though at one time she was in some danger of sinking from exhaustion, and she became still more extremely emaciated. Whether or not this depended upon the fact that there occurred an enormous flow of pure bile from the wound, half a pint to three-quarters of a pint *per diem* for some time, I will not pretend to say. The result was, however, that bedsores formed over the angles of the scapulae and the most prominent rib angles. In the course of time, however, this diminished and finally stopped. She began to put on flesh. The motions, previously pale, assumed their natural colour, and by the end of September (four months) the abscess had completely closed.

She is now well nourished, and in good spirits, but there remains a rounded swelling at the epigastrium, freely movable, and connected with the liver. Whether it is another cyst or a distended gall bladder I cannot say, but whatever it is, it appears to be diminishing in size, as may be seen by comparing Fig. 6 with Fig. 7.

Fig. 6, September 24th.

Fig. 7, November 29th.

The subject of these lectures has been so diffusely treated, that it will be perhaps convenient to summarise what has been said with regard to treatment, which is intended to be its essential part.

1. Pyæmic abscesses do not call for surgical interference, or, if in rare cases one should point, it is only opened to relieve symptoms, but without hope of doing permanent good.

2. The same observations apply to abscesses resulting from suppurative phlebitis of the portal vein.

3. Multiple abscesses associated with dysentery or ulceration of the bowels are very unfavourable for surgical treatment. They must however, be opened and treated on the same lines as the single or tropical abscess, because they cannot be certainly diagnosed.

4. Single abscess of the liver, whether tropical or not, must, if it approach the surface, be opened, the following precautions being adopted:

(a) If it present at the epigastrium, the presence of adhesions must be ascertained before incising the liver.

(b) If through the chest wall, a spot must be chosen below the normal limit of the pleura; but, if by chance either pleura or peritoneum be opened, the opening must be closed with a double row of stitches before incising the liver.

(c) Strict antiseptic precautions must be throughout adopted, either carbolic acid or some slightly soluble salt of mercury being employed for the dressings.

(d) The tube must be of large size at first, and a tube of some sort must be kept in until the discharge is reduced to a very minute quantity.

If the abscess have burst into the lung, pleura, pericardium, peritoneum, or kidney, and the position of the abscess can be clearly determined, it must be opened without delay. If the position of an abscess be only suspected and the patient be losing ground, it is right to puncture the liver in the most likely situations, bearing in mind that, though usually quite harmless, a slight amount of risk accompanies this very trivial operation. This rule applies to cases in which the abscess has ruptured into any of the cavities enumerated above. If, on the other hand, whether the abscess have ruptured or not, there are no means of diagnosing the whereabouts of the matter, and the patient be not losing or even gaining ground, the surgeon should hold his hand for a time.

5. Hydatids of the upper and back part of the liver are to be treated upon the same lines; but in cases of this sort, and in those of subdiaphragmatic abscess, it must be remembered that the diaphragm may be pushed up to a very great height, thus closely simulating intrapleural suppuration.

6. Empyema, pericarditis, and peritonitis caused by rupture of a hepatic abscess or hydatid must be promptly dealt with on general principles.

ON THE CENTRAL MOTOR INNERVATION OF THE LARYNX.

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THE communication, under the above title, by Professor Krause, of Berlin, published in the JOURNAL of January 18th, forcibly illustrates the truth of the old adage, that "it seems a pity to take offence where none is meant." Professor Krause evidently appears to think that the terms "investigation" and "discovery" are identical. It seems hardly necessary to point out that this is not so. Inasmuch as he himself found it necessary to controvert only last year, in a special communication, at the meeting of German naturalists at Heidelberg the opinion given out by François Frank—namely, that there was no cortical representation of the larynx¹, it was surely legitimate for us in 1886 to "investigate" the whole question of the motor innervation of the larynx.

As to the question of priority, Professor Krause, in his present communication, says in reference to his own work on the subject: "These results of mine the authors (namely, Semon and Horsley) have, on a former occasion (1886) recognised as the first investigations on the subject."

¹ See *Internationales Centralblatt für Laryngologie*, etc., January, 1890, p. 381.

This statement is not quite correct in this form, for we could not recognise Professor Krause's results as "the first" investigations, because we were well aware that Professor Ferrier had, as far back as 1876,² signalised the region in front and below the sigmoid gyrus in dogs as causing occasionally vocalisation, and that Dr. Duret, of Paris, had described in 1878³ the effects of ablation and of compression of this region with regard to the dog's barking. And we may say here in passing that it certainly appears to us not a little remarkable that Professor Krause, who judges so harshly the omission of his name from a "preliminary communication," should have himself failed, so far as we are aware, to mention the labours of these predecessors of his in any of his complete papers. But it is certainly true that we, in 1886, most heartily and ungrudgingly acknowledged the importance of Professor Krause's investigations on this subject. Why then should we, when we did this in 1886, attempt, as Professor Krause seems to believe, to deprive him of his rights in 1889? The thing is, on the face of it, absurd. If Professor Krause, in return for our politeness—namely, sending him directly a reprint of our preliminary communication—had taken the trouble, if he felt hurt at his name not having been mentioned in it, of writing to one of us and asking for an explanation, instead of taking the course—certainly very unusual under such circumstances—of at once lodging a claim for a priority, which we never contested, not only in the JOURNAL, in which our preliminary communication has appeared, but also in the *Berliner klinische Wochenschrift*, in which neither his nor our own papers on this subject have been published, he would have received a reply which we venture to believe would have satisfied him, and the present discussion would have been avoided.

We had hoped that both the title and the introductory remarks of our first communication would have left no doubt in the mind of any reader that it was intended simply as a report of our own experiments and of nothing else. If we had included a history of the subject, and had entered upon a discussion of the many controversial matters connected with it, the paper would have become very different from what it is now. Thus the fact that not a single name is mentioned in the paper finds an equally simple and natural explanation. The expression "vindication of our rights of discovery," which Professor Krause throws into the discussion, ill fits our purely objective communication. We have nowhere spoken of "discoveries," but have only given our results and the conclusions, which, we think, may legitimately be drawn from them. Surely, though this seems "out of place" to Professor Krause, this is a right which cannot be seriously contested, and if he had only followed the natural course above sketched or had waited a little longer, he would have seen that the use of this right by no means involves the infringement of the rights of others. For it is our intention, as it has been from the very beginning, to do every justice to Professor Krause's investigations in the full account of our work, which we hope to bring before the Royal Society in May or June, and we need not say that this painful and perfectly unnecessary controversy will not in the least alter our plans.

But as Professor Krause in the peroration of his paper is pleased to attempt to reduce our whole work to a mere repetition of his own experiments on different species of animals, we must be allowed to at once energetically protest against this attempt. No doubt we have corroborated by far the greater part of his own experiments, and the fact that we have been able to do so has given us so sincere a pleasure that it is now the more painful to us to be needlessly attacked by him. But at the same time not only are we not acquainted with any experiments of his on the medulla oblongata, not only do we differ on one most important point concerning the effects of cortical stimulation, as will be shown in the conclusion of this paper, but above all we are entirely unaware that Professor Krause has drawn, in print, any practical conclusions such as our own from his experiments, and it will be for him to show that he has done so. The only paper of his on the subject with which we are acquainted is a very short report of his experiments.⁴ In this, so far as we can see, not one word occurs as to their practical bearing upon clinical and pathological questions. Nor do we know of any later publication of Professor Krause's in which such important questions as the impossibility of unilateral laryngeal paralysis being caused by cortical lesions, the nature of

laryngismus stridulus, of the epileptic cry, etc., have been discussed. It appears to be our crime that we have supplied the deficiency!

Finally, we must decidedly maintain our statement, that we have not been able to find an area of representation in the cortex of the abductor movements of the vocal cords—except in the cat, in which, as stated in our two communications on the subject in 1886⁵ and 1889, both peripherally and cortically conditions appear to exist, curiously different from those observed in other animals.

To prevent all misunderstanding we wish to state distinctly that we by no means deny the existence of a general representation of respiration in the cortex. Indeed, it will be seen in our full report that we have observed an influence of cortical stimulation upon respiration especially in young animals, in which the differentiation of the several motor areas is not nearly so perfect as in the adult animal. But we have not been able to find in the monkey, dog, or rabbit, an area in the cortex from which abduction of the vocal cords could be produced in a corresponding manner to the adduction produced from stimulation of the area posterior to the lower end of the præcentral sulcus at the base of the third frontal convolution in the monkey, and of the præcrucial and neighbouring gyrus in the dog and rabbit. And we fail altogether to understand Professor Krause's reasoning, when, in the paragraph of his paper treating of the "cadaveric position" of the vocal cords, he argues from the fact that he obtained this position from stimulation of the extreme periphery of the laryngeal area, that it was "thus seen that abduction may also be brought about by stimulation of this area." To begin with: the expression "abduction" in this connection is absolutely misleading. The "cadaveric position" is that in which the vocal cords are found after death. They do not then, as a rule, stand quite close to the middle line (though sometimes they do), and are in this sense of course somewhat "abducted" from it; but at the same time they are under all circumstances much nearer the middle line (that is, the glottis is considerably narrower) than when they stand in the common respiratory position.⁶ How can anyone under these conditions speak of "abduction of the vocal cords being brought about" by stimulation of the præcrucial or præcentral area when the vocal cords become arrested in the cadaveric position? The very reverse of it is true. As to the explanation of this condition, we think it is simple enough, and entirely in concord with our results of stimulation of the medulla, given in our first paper. The electric cortical stimulation acting only upon the most peripheral part of the laryngeal area, that is, the one in which adduction of the vocal cords is but weakly represented, is just strong enough to interfere to some extent with the inspiratory impulses which probably pass rhythmically up the afferent fibres of the pneumogastric nerve to the respiratory centre, and having been transferred in the medulla to the accessory nucleus, are there changed into a tonic innervation of the abductor muscles of the vocal cords ("reflex-tonus" of the abductors, on which Professor Krause himself formerly used to lay great stress⁷), but not strong enough to induce complete closure of the glottis. Thus this phenomenon, that is, the cadaveric position obtained under such circumstances, represents, in our opinion, a conflict between the natural medullary and the artificial cortical stimulation, but does not signify that "abduction" is brought about by stimulation of a cortical area.

With these remarks we take leave of Professor Krause's communication. We deeply regret that personal matters should have been quite needlessly introduced into a purely scientific discussion, and we feel sure that when our full report is published, all our predecessors in the investigation—experimental as well as clinical—of this interesting subject will convince themselves that nothing could have been and is further from our intentions than to deprive any scientific worker of the fruits of his labour.

ANOTHER medical journal has perished for want of an editor. The *Centralblatt für Nervenheilkunde, Psychiatrie, und gerichtliche Psychopathologie* has ceased to appear, as the editor, Dr. A. Erlensmeyer, who founded the periodical twelve years ago, finds himself compelled by the pressure of other engagements to relinquish that post, and no successor is forthcoming.

² *The Functions of the Brain*. First Edition, 1876; p. 150.

³ *Etudes Expérimentales et Cliniques sur les Traumatismes Cérébraux*, 1878, pp. 142 and 250.

⁴ "Ueber die Beziehungen der Grosshirnrinde zu Kehlkopf und Rachen." Sitzungsberichte der Kgl. preussischen Academie der Wissenschaften, 1888.

⁵ "On an Apparently Peripheral and Differential Action of Ether upon the Laryngeal Muscles," JOURNAL, September 4th, 1886, p. 447.

⁶ Particulars of this, as well as of all the points raised in this last paragraph, will be found in our full report.

⁷ See Virchow's *Archiv*, vol. 98, 1884. Reprint, p. 38. *Ibidem*, vol. 102, p. 317.