the removal of the tumour would probably have been easily effected from the mouth, and with much less danger both to life and to voice. I may add that I took all possible care to avoid any interference with the tissue of the true vocal cord in the operation. In fact, the tumour was attached, not to the true, but to the false vocal cord. The distortion of the cord is, I suspect, the result of the cicatrisation which followed on the removal of the tumour. If it is objected that the same cicatrisation might have followed after removal by the laryngeal *écraseur*, I would reply that I think this is less likely, from the very circumstance that the base of the tumour would probably have been less easy to reach. In my zeal to extirpate the disease, after having removed the chief mass, I cut away another portion which still projected. If I ever repeat the operation, I would not do this. The stalks of these polypi, like those of uterine polypi, may doubtless be safely left to wither away, after the portion which produces symptoms has been removed. With this precaution, possibly the mere removal of the polypus from the larynx may not involve more danger to the voice in the one method than in the other. But surely the division of the whole larynx, from top to bottom, cannot be effected without risk to the integrity of its mechanism. If there is no risk that some unlucky deviation of the knife may injure the cords mechanically, is there no risk that their structure or muscular mechanism may be injured by the resulting inflammation and cicatrisation? I can hardly bring myself to believe this.

As to the danger to life, I will not interfere in the controversy between Dr. Mackenzie and Mr. Durham on this head, as judged by the results of practice hitherto. But, I think, no one can witness the operation without admitting that it is a very serious surgical proceeding, and that it ought to be reserved for cases of proved necessity. If I offended against this rule in the case under consideration, I can only plead want of experience. In another case I would follow Dr. Mackenzie's advice—viz., to leave the tube in until a full view of the larynx can be obtained, and I would only perform thyrotomy after the failure of a properly conducted attempt at removal by the mouth. The use of a tracheal cannula for a few years does not interpose any serious obstacle to the closure of the wound after its removal, and would not prevent the complete restoration of the voice, while any injury done to the vocal cord must render this latter result hopeless.

I am surprised to hear from Dr. Mackenzie that the view obtained of the larynx on division of the thyroid cartilage is not ample, and that the opening resulting from the wound is smaller than that of the glottis. I have made no experiments on this head; but anyone could easily satisfy himself of the accuracy of this assertion, by a few observations on the dead subject. In my case (having carefully extended my incision fairly into the thyro-hyoid membrane), I obtained a most ample view of the interior of the larynx by a far larger opening, as it seemed to us at the time, than the glottis. Perhaps this may differ at different periods of life, and in different conditions of the cartilages.

ON THE ANATOMICAL INVESTIGATION OF EPILEPSY AND EPILEPTIFORM CONVULSIONS.

By J. HUGHLINGS JACKSON, M.D., F.R.C.P.,
Physician to the Hospital for the Epileptic and Paralysed, and to the London Hospital; etc.

In his valuable paper in the JOURNAL of April 26th, p. 457, Dr. Ferrier has drawn attention to some investigations* I have made concerning the bearing of cases of convulsion on the localisation of movements in the cerebral hemispheres. It is very satisfactory to me to find that the results he has obtained from the new method of investigation—the artificial rousing up of the functions of particular parts of the encephalon in lower animals by direct faradisation—agree with the general conclusions I have come to from observing cases of paralysis, convulsion, chorea, etc., in man. The importance of his novel facts, and those of Fritsch and Hitzig to which he refers, for anatomy and physiology, is obviously exceedingly great. But for what is called the *pathology* of convulsions in man, they have a remarkable value.

What is above called pathology, would, however, be more conveniently named anatomy or physiology. Such convulsions, as I shall mention later in this article, are really experiments on parts of the human brain analogous to those Ferrier has made on the brains of dogs, cats, and rabbits—they are experiments made by disease. Indeed, very much of our clinical work is a study of quasi-anatomical and physiological *experiments*. For example, a large part of our investigations into cases of hemiplegia, convulsion, chorea, etc., is so. I think it is important to bear in mind that much of our clinical study of disease is *not* pathology; from not doing so, our notions on the "causes" of epilepsy, chorea, etc., are confused. Thus the word "cause" is used by medical men in different senses. It is used for the seat of a lesion, as when it is said that "hemiplegia is caused by disease of the corpus striatum;" for the functional nature of the lesion, as when it is said that "epilepsy is caused by increased excitability of the medulla oblongata;" and for the pathological process when it is asserted that "loss of speech is caused by local softening." But, strictly, the *three* lines of inquiry should be pursued; the causation is to be studied triply in *each* case of nervous disease. It is an Anatomical inquiry to seek the Organ or part damaged (the seat of disease, the Localisation as it is more technically called). It is a Physiological inquiry to search into the defective working of the nervous tissue of the organ damaged—the Functional affection. It is a Pathological inquiry to trace the processes by which the Nutrition of nervous tissue is altered. (Strictly, I suppose, we should speak of pathological anatomy, pathological physiology, and pathological nutrition?) Reversing the order, and putting these three things in the most abstract form, there are abnormalities in the absorption, in the expenditure, and in the distribution of force.

In the study of epilepsy and of epileptiform seizures, it is of very great importance to keep these several lines of investigation distinct; often it is not possible. The importance of doing so is that we shall more easily trace the fundamental resemblances of different symptoms in spite of their superficial differences. For example, a convulsion affecting one side, hemiplegia and hemichorea are alike in that the same muscles are affected, and, therefore, inferentially alike in that the same internal region is damaged; again a convulsion, a sudden stench in the nose are alike in that both depend on the same functional alteration in nerve-tissue, they are alike as disorders of function; thirdly, epistaxis, hæmorrhage in the retina and cerebral hæmorrhage are alike in that they are the accidental results of the same pathological process in arteries—results of the same abnormality of nutrition. A further advantage of thus differentiating our investigation is that we shall learn more exactly *where* our knowledge is deficient.

In this paper I shall speak of the first line of inquiry,—Anatomical, and almost solely with the intention of showing the bearings of Ferrier's researches on the methodical study of the *seat of the lesion* in epilepsies. In future articles I shall consider the Physiology of Epilepsies and their Pathology. As in this article the illustrations I give are from cases of *convulsions*, I can defer the definition I have to give of the term epilepsy as I use it.

It is to be hoped that Dr. Ferrier will make careful comparisons of the effects of the local discharges he artificially induces in animals and those artificial or, at any rate, abnormal discharges induced by local disease in human beings. Of course the inference is not to be drawn that the phenomena in the two cases are alike. The differences in the external conformation of animals imply differences in the normal functions of their nervous centres.* These differences will assert themselves, even under excessive and unnatural excitation, whether it be by faradisation or by disease as surely as they assert themselves during healthy activity. But, although this is what one would expect *à priori*, Dr. Ferrier's experiments have the great value of *demonstrating* special differences in different animals. He concludes that "striking differences corresponding with the habits of the animal are to be found in the differentiation of the centres." "Thus," he continues, "the centres for the tail in dogs, the paw in cats, and the lips and mouth in rabbits, are highly differentiated and pronounced." In fact we have in Dr. Ferrier's researches a starting point for a "Comparative Physiology" of the Convolutions. For, so far as comparative anatomists have ascertained homological cerebral structures, so far, it is to be hoped, will he be able to develope the homological functions.

Before we pass to speak of convulsion in man, it is necessary to state certain principles as to the constitution of nervous centres.

The nervous centres represent movements, not muscles; chords, not notes. This is evident from the effects of destroying lesions of the corpus striatum. From a *small lesion* of this body there does not result paralysis of *a small part* of the arm, nor of any such group of muscles as flexors, or extensors; there results *partial paralysis of the whole arm*, the most special parts of it suffering most. There is loss of a certain *number of movements* of the limb. Let us take a more striking example: in cases of *very grave* lesion of the corpus striatum (that is, of a centre far above the supposed deep origins of the ocular motor-nerves), there is, besides palsy of the face, arm, and leg, an ocular palsy. Now this palsy is not of the sixth nerve, nor of the third nerve, nor of

* "On the Anatomical and Physiological Localisation of Movements in the Brain," *Lancet*, Jan. 18, Feb. 1, and Feb. 15, 1873; also "Study of Convulsions," *St. Andrew's Reports*, vol. iii, 1870.

* I would here refer the reader to an article I contributed to this JOURNAL in October, 1869, p. 371, in which I have considered certain symptoms the result of disease or experimental injury of the brain in dogs, etc., in relation to corresponding symptoms in man.

the fourth, nor of any one muscle, nor of any random grouping of muscles. It is a *loss* of a highly special and widely associated movement; the patient has lost power *to look* to that side on which his body is paralysed; there is what is commonly called lateral deviation of the eyes. Similarly, in convulsion there is a *development* of *movements*. In a convulsion beginning in the hand, the spasm creeps up the whole limb, developing first the movements of the most special parts of it, but not picking out such groups of muscles as flexors or extensors. Among other movements, there is at a certain stage a *development* of that of the eyes for "looking" to one side. In this case the two eyes are *turned to* the side of the body convulsed. We must, however, draw attention to a very important qualification with which the expression "development of *movements*" is to be used.

Both in Dr. Ferrier's experiments and in cases of convulsion from disease in man, the results of the discharges (since they are sudden, excessive, and very local) are only exhibitions of the movements represented in the parts discharged in the rough. A great number of different movements are developed at once.* And it must not be forgotten that not only are the discharges unnatural in being excessive, sudden, and temporary, but also in that they are very local. We are reminded of the effects of putting one muscle into strong action by Faradising it; the result is a mere caricature of a normal movement. Duchenne insists that a muscle is never singly in action in health, except perhaps in the case of the facial muscles.

But in spite of these drawbacks, the study of discharging lesions, whether induced by Faradisation or by disease, is of great value. My own opinion is that *there is no other way* of finding out what movements parts of the convolutions near to the corpus striatum represent. The reason for thinking so is that the other process of experimentation—that by *destruction* of small parts of the cerebral hemisphere—produces no obvious symptoms—no obvious loss of movements, or no special loss at any rate. But it would be a great error to infer that the part *destroyed* did not represent special movements. The bearing of Ferrier's researches is very direct on this matter. For if we discharge that part, destruction of which produces no *loss* of movement, there will be a presentation of a mass of movements. See, then, the clearing of the paradox. Disease of the convolutions sometimes does, and sometimes does not, produce symptoms. The word disease is used vaguely; so far as it involves destruction there are no symptoms, but there are symptoms from discharge. The speculation I have put forward (*St. Andrew's Reports*, vol. iii) to explain these paradoxical results is as follows.

To begin with a "motor" centre. The study of cases of hemiplegia shows the constitution of the corpus striatum to be such, that *each* part of it represents movements of the *whole* of the parts which that organ governs. So to speak, the corpus striatum is a mass of corpora striata, each one of which represents faintly, and each in some slightly different manner, the whole of the parts which the corpus striatum in full represents in greater degree. Now, to pass to the hemisphere. The convolutions in the region of the corpus striatum *are* the corpus striatum "raised to a higher power". Each part of the brain in this region re-represents the whole of the movements which have been represented in the corpus striatum; so then, if *any* one part of the brain in this region be *destroyed*, there is no obvious loss of movements, because the movements it represented are still represented in each neighbouring part, although in different degrees and orders. But for this very reason, if *any* one part be strongly *discharged*, vast numbers of movements are developed.†

I now mention illustrative cases. Although I can only take enough space to give a mere outline of the cases, I think they show plainly that some of the "experiments of disease" on man are, notwithstanding the *special* differences I have insisted on, *fundamentally* like the experiments of physiologists on animals; that a large and definite part of the study of nervous diseases, of convulsion in particular, must be put on an anatomical and physiological basis; and that Ferrier's researches will be a most valuable help in thus methodising our work—in making it less empirical and more scientific.

The first illustration is an outline of a case I have already published (*Medical Times and Gazette*, November 30th, 1872). A man had convulsions, each of which *began* in his *left* thumb. He died: we found no other disease in his brain than a tubercle, the size of a hazel nut, in the hinder part of the third *right* frontal convolution. It will be particularly interesting to me to know what effects Dr. Ferrier will obtain by faradising the homologous part of a monkey's brain, for he tells me that these animals will shortly be subjected to experiment. Theoretically, I should not expect an identical convulsion, but an homologous one. I will, before giving the second illustration, remark on this point, and, at the same time, state some facts which show that the study of cases of convulsion bears on what is conveniently, if not correctly, called the physiology of the mind. One constantly hears, however, that the convolutions are not for *movements*, but for "ideas", "memory", etc. Yet those who use psychological phraseology to describe symptoms of disease—"loss of memory for words", "chorea the result of disorder of volition", etc.—have as much as other people to seek the anatomical and physiological substrata of mental phenomena.*

Among the fits which *begin unilaterally*, the commonest are those in which the spasm starts in the index finger and thumb. This is significant. It is an illustration of what I believe to be the law of the effects of lesions of the brain. In evolution (development, education, etc.), the progress is from the general to the special. In the opposite process of dissolution the more special parts suffer first. I generally use the term "voluntary" instead of "special"; it is a convenient counterpart to the term automatic. In physiological language, a voluntary part—the hand, for example—is one which has the greater number of *different* movements at a greater number of different intervals—shortly, the most varied uses; an automatic part—the chest, for example—is one which has the greater number of similar movements at the greater number of equal intervals—shortly, more similar uses. In brain-diseases parts suffer the more as they are voluntary, and the less as they are automatic. Now, the thumb and index finger are the most voluntary or specialised parts of the body; hence the suggestiveness of the case I mention. The thumb in man has a distinct flexor longus pollicis. In the *Anthropomorpha*, Huxley (*The Anatomy of Vertebrate Animals*) says: "The *flexor pollicis* is more or less closely connected with the *flexor communis perforans*, or with that part of the muscle which goes to the index digit." On the intellectual importance of this muscle Duchenne insists strongly. "En somme, ces faits cliniques demontrent que le long fléchisseur du pouce est l'un des muscles qui sont essentiellement destinés, chez l'homme, aux usages manuels les plus delicats; à tenir et à conduire la plume, le crayon, le pinceau, l'aiguille, etc.; qu'il aide, en un mot, à l'exécution des travaux manuels qui sont à la hauteur de son intelligence supérieure" (*Physiology of Movements*, p. 251). And in his work on *Electrisation*, he says that when the small muscles of the thenar eminence are atrophied, the hand loses its distinctive human character and approaches that of the monkey. The thumb in the monkey is less specialised than in man. If, then, we discover in a monkey the homologue of the part discharged in my patient, we shall expect from *its* discharge a fit of a less special kind; for example, not a fit beginning in its pollex, but more likely one beginning in the whole of its five comparatively little differentiated digits at once, if not in the whole arm.

I have yet to publish the case of a woman who had fits beginning in her left great toe (where fits beginning in the foot nearly always start); there was a small tumour in her right hemisphere. My colleague Dr. Gowers made a careful examination of the brain for me: he found that the tumour involved the lower part of the ascending frontal convolution. Although there were other lesions in this woman's brain—local indurations—there is a strong probability, amounting almost to certainty, that the fits depended on discharge of that part in which lay the tumour. Here, again, I await Dr. Ferrier's further researches in comparative physiology of the convolutions.

In the *Medical Mirror* of September 1869, I published the case of a

* In chorea, which I believe to depend on repeated small discharges of convolutions near to the corpus striatum, there is a succession of independent and quasi-purposive movements of great speciality.

† I have stated some of the facts on which this speculation is founded, and the speculation itself in a brief manner, in the *Medical Times and Gazette*, Dec. 14th and 21st, 1867. In a note, August 16th, 1868, *op. cit.*, the bearing on this principle of localisation of the facts supplied by cases of convulsions beginning unilaterally, is more particularly considered. (At that time, I supposed that such convulsions depended on discharges of the corpus striatum itself.)

The study of numerous different kinds of disease of the brain leads me to the conclusion that not only are the gross movements of the whole body represented or re-represented in the convolutions, but also the so-called "vital processes". A slow pulse and a lowered temperature are among the results of a large cerebral hæmorrhage. In epilepsies, we have pallor of the face and alterations of the secretions, as well as convulsions. In this article, I do not, however, speak of "vital" symptoms. They are best studied in some cases of cerebral tumour.

* Of what "substance" can the organ of mind be composed, unless of nervous processes representing movements and impressions; and how can the convolutions differ from the inferior centres, except as parts representing *more* intricate co-ordinations of impressions and movements than they do? Are we to believe that the hemisphere is built on a plan *fundamentally* different from that of the motor tract? What can the anatomical substratum of the "idea" of a ball, possibly be, except a process representing certain impressions of surface and particular muscular adjustments? Why, then, is there anything remarkable in the fact that discharge of a part of the "organ of mind" produces spasm of the arm and deviation of the two eyes? What can occur physiologically in recollection, but a faint revivification of such processes which, in the past, have become part of the organism itself? What is delirium, except the *disorderly* revival of sensori-motor processes organised in the past? What is a mistake in a word, but a wrong movement, a chorea? Surely the conclusion is irresistible, that "mental" symptoms from disease of the hemisphere are *fundamentally* like hemiplegia, chorea, and convulsions, however *specially* different. They must all be due to lack, or to disorderly development, of sensori *motor* processes.

man who had fits affecting the *right* arm. In this case there was a tumour in the hinder part of the first (uppermost) frontal convolution of the left hemisphere. (There was also a tumour in each lateral lobe of the cerebellum, to which I traced no symptoms.) I did not see this man in a fit; his arm was paralysed after the first seizure.

Lately I was allowed by Mr. Soutter to see a patient of his who had literally innumerable fits limited to the right arm. Mr. Soutter witnessed many; I saw several. The spasm passed down the arm except in the later fits, then it passed up. Shortly before the patient's death she had, Mr. Soutter tells me, universal convulsion. Here I correctly predicted disease of the hinder part of the first (uppermost) frontal convolution—not from physiological knowledge, but because of what I found in the other case just mentioned. Once more I ask, What would be the homologous series of convulsions from the artificial excitation of the homologous parts in a series of animals lower and lower in the scale?*

SYMPTOMS OF IRRITANT POISONING FROM PORK BRAWN.

By EDWARD MACKEY, M.B.,
Professor of Materia Medica in Queen's College, Birmingham.

CASES similar to the following deserve record, because they have a medico-legal interest, and may be required for reference and precedent.

On January 4th last, at Hampton-in-Arden, sixteen persons, including men, women, and children, aged from five to seventy-eight years, were suddenly attacked with violent vomiting and purging, accompanied in most cases with general and severe muscular cramps, soreness of eyes, and sense of burning and constriction in the throat. Mr. Adkins of Meriden (who favours me with these details), and Mr. G. W. Tait of Knowle, were with the sufferers for the greater part of the day and night. Some seemed to be in serious danger; but, under appropriate treatment, all were convalescent next day. It was found that all had eaten some "pork brawn", and had begun to suffer two or three hours afterwards. All who had eaten any suffered more or less. The brawn had been bought from one provision-dealer in the village, a respectable man who could give no reason for its bad effects. He had eaten a little of it (perhaps two ounces), and had thought it particularly good. Of eighteen pounds made, six had been sold; the rest he himself gave into the custody of the police. Dr. Wade was consulted, and at his suggestion a portion of the brawn was sent to me for analysis. When received, it was mouldy and partly decomposed. Of its original appearance and taste no complaint had been made by any customer, though Mr. Tait informs me it had to him an unpleasant smell, and "nearly all trace of difference between gelatinous and muscular tissue was gone". There was no suspicion of intentional poisoning by the vendor, neither had any copper vessel been used in the cooking; but two rumours had been spread—(1) that the pig had had mercurial ointment applied for foot-and-mouth disease; (2) that, as the furnace stood near a partly open window, some one might have, in malice, thrown in poison. Poisons whose effects would accord with the symptoms are oxalic acid, salts of copper, zinc, antimony, arsenic, or mercury. The reaction was not strongly acid, and the substance had not unusual taste or colour: oxalic acid and sulphate of copper were therefore excluded. Three portions of two ounces each were tested by the process of Reinsch, with negative results. For greater certainty, similar portions of ordinary brawn bought in Birmingham were mixed with fractions of a grain of white arsenic, tartrated antimony, and corrosive sublimate respectively, and readily gave evidence of these poisons with the same test. Marsh's hydrogen test was then used, with results equally negative as to the suspected brawn, and equally positive as to the brawn which was purposely mixed with antimony and arsenic. Sulphuretted hydrogen, passed for some hours through an acid solution, did not produce any precipitate—an additional evidence of the absence of copper as well as of the other poisons. Sulphate and chloride of zinc were tested for separately, and neither found. Under the microscope, portions of the muscular tissue appeared normal, and without trace of parasite. There was, therefore, no tangible poison in the suspected brawn. Did the symptoms arise from formation or decomposition of fatty acids during boiling?

The mode of its preparation was as follows. A pig, said to be quite sound, was killed on Wednesday; the ears, snout, feet, and some of the flesh and viscera, were put on Thursday morning into an iron furnace partly covered with a wooden lid, and were boiled with salt and spices till night; the fire was then let out, the meat left to grow cold, and then boiled again all Friday. On Friday evening it was "turned out" to cool, and cut up for sale on Saturday morning.

Why did this particular quantity cause bad effects when the man had prepared, apparently, similar brawn safely twenty times before? Inquiry into *differences* in preparation gave the following answers.

1. *As to Time.*—He had never before boiled any so long as this: he could not say exactly his usual time, but it was generally till the flesh came off the bones. He had never before let any grow cold all night and be re-boiled next day.

2. *As to Vessel.*—He had never before used his iron furnace for this purpose, but always an iron pot boiled over his kitchen fire. The furnace had been commonly used for boiling clothes: this time it had been also used for "rendering" lard for about an hour before the brawn had been put in; it had not been cleansed from the lard; had never prepared lard before in the same vessel as the brawn. Soda had not been used when the clothes were boiled, and the lard prepared just before the brawn was sold and used without any complaint.

These answers contain, I believe, whatever explanation can be given of the unfortunate results. I regret that the state of the material when received by me, a fortnight after the occurrence, vitiated any results obtainable from alcoholic extracts, as prepared by Buchner and Schumann in their researches on the sausage-poison (*vide* Christison *On Poisons*, p. 640). I certified to the absence of mineral poison and to the probability of formation of acrid fatty acids—a suggestion previously made by Dr. Wade.

It may be observed, in conclusion, that the dealer will never attempt brawn-making again, "for he suffered so much in his mind". His customers were considerate towards him, and no legal proceedings arose Somewhat similar cases might not always end with as little mischief.*

* The significance of the fact, that the hand is the part in which convulsions, beginning unilaterally, most often start; that the arm suffers first, or most, or both, in the greater number of motor affections from brain-disease (hemiplegia, chorea, paralysis agitans), will be better realised after reading Herbert Spencer's remarks on lacteal organs, in chap. VIII, vol. i, p. 359, of his *Psychology* (second edition), from which I give these extracts.

He points out and shows the significance of the "striking instances which the animal kingdom presents of unusual sagacity co-existing with unusual development of organs, which, by the help of complex muscular arrangements, give complex tactual impressions." After remarking, that it will perhaps be difficult to understand why *touch*, the simplest and earliest sense, should in its higher forms be more than any other sense associated with the advance of intelligence, he says: "The explanation lies in the fact that tactual impressions are those into which all other impressions have to be translated before their meanings can be known." Of the human hand:—"All that we need here notice is, the extent to which, in the human race, a perfect tactual apparatus subserves the highest processes of the intellect. I do not mean merely that the tangible attributes of things have been rendered completely cognisable by the complex and versatile adjustments of the human hands, and that the accompanying manipulative powers have made possible those populous societies in which alone a wide intelligence can be evolved. I mean that the *most far-reaching cognitions, and inferences the most remote from perception*, have their roots in the definitely combined impressions which *the human hands* can receive." [No italics in original.]

The study of cases of disease of the nervous system appears to me to supply coninual illustrations of the correctness of many of Spencer's deductions.

THE INVALID CRANE OR BED-HOIST: A NEW APPARATUS.†

By EDWARD ATKINSON, F.L.S.,
Senior Surgeon to the Leeds Public Dispensary.

IT not unfrequently happens in private practice that paralytics and other helpless persons who are long confined to bed, and whose bulk makes it a difficult matter to change their position, consult their medical attendant as to whether some means be not available for saving the labour of lifting, etc. Nor is it to the patient alone a matter of concern; for what surgeon is not familiar with instances where overstrained spinal or uterine ligaments, entailing life-long sufferings, are traced to a devoted attendance upon some heavy invalid parent or friend—perhaps daily, for months or years? Two or three such cases having lately occurred under my own observation, I became anxious to meet the difficulty, and projected several designs, but was never satisfied with any of them until I secured an ally in a relative of one of my patients, himself an engineer. The result of the apparatus which we have had constructed is so completely satisfactory that I am induced—having my friend's full permission for so doing—to communicate the accompanying description and drawings of it for the benefit of the public, and more especially of any surgeon who may be in need of such appliance. I may premise that the total cost did not exceed £10, of which sum the pair of patent pulleys alone cost half.

The invalid crane, as I propose to call it, is a modification of the Goliah travelling crane used in timber-yards and elsewhere, and con-

* Since the above has been in type we have the report of a fatal case, apparently of meat-poisoning, at Arbroath. A careful and rigid inquiry is to be desired.

† Read before the Leeds and West Riding Medico-Chirurgical Society.