the vitreous humour. It was pear-shaped in form, and barely altered its position after the patient had moved the eye quickly up and down, and from side to side. It appeared to be attached by its pointed extremity somewhat firmly, the base floating in the vitreous humour immediately behind the lens. It was impossible to get a glimpse of the disk. The opacity was extremely dense, resembling a piece of thick crape. There were not any vessels near it. Now and again, as the film moved slightly, a retinal vessel could be seen. The retina and choroid appeared healthy, and the eye was free from pain. There was no history of syphilis. The heart was healthy, the urine was normal, and there was no suspicion of brain-mischief. Vision was limited to shadows.

Mercury had no effect; for, after mildly keeping up its specific action for a fortnight, the film remained unaltered in character, and vision had not improved. Iodide of potassium, given in increasing doses, produced a marked change. Vision in a few days so far improved that fingers could be counted. Unfortunately, at this stage she ceased to attend. Some months afterwards, I accidentally heard that she recovered a fair amount of vision.

CASE II.—A healthy-looking young man, by trade a mechanic, assured me that he had enjoyed good vision. On April 25th last, he accompanied a militia regiment from their barracks to a church at some considerable distance. The sun was shining brightly at the time, and the red cloth of the soldiers' tunics sadly dazzled his eyes. As he continued to stare at the soldiers, the left eye suddenly "went quite dark." He had no pain in the head, nor any other distressing symptom at the time, and was in excellent health. On his arrival at home, his mother noticed that the pupil of the now blind eye was much too large.

I first saw him on May 3rd. He then, and subsequently, enjoyed perfect health.

His previous history was perfectly satisfactory. His family history was also good. He did not inherit syphilis, nor had he ever contracted it. He did not smoke tobacco; he abstained entirely from spirituous liquors. No symptoms of organic mischief were present; the lungs, heart, urine, etc., on examination, being found healthy.

He suffered from converging strabismus and a slight degree of hypermetropia. The eyes performed their movements properly, though there was slight weakness of the external rectus of the right eye, which converged more strongly than its fellow; but the weakness was not due to any central nerve-mischief. The cornea of the ailing eye was bright; the anterior chamber of a normal size; and the iris, which was of a dark brown colour, presented no appearance of disease. The pupil was dilated, and acted sluggishly, and qualitative perception of light was absent. Tension was normal. He could just discern letters of 20 Jäger with the damaged eye; the healthy eye read No. 1. The eye was free from vascularity, intolerance of light, and pain. There was great centripetal contraction of the field of vision. The form of the field, at eight inches, was nearly round, and measured only one inch and a-half by one inch and three-quarters.

Ophthalmoscopic Examination.—A large dark body, the lower half of which resembled in appearance a piece of dark crape, the upper half being more translucent, and not unlike a piece of muslin, as the eye diverged, floated through the vitreous humour behind the lens, and entirely prevented any reflection from the fundus. Its base had an attachment at the lower and outer side. As the eye converged, it receded, and I saw the disk, retina, and choroid. The disk was hyperæmic, but the retinal vessels presented no abnormal appearance. The pigment of the choroid was so abundant that I could not see its vessels. The retina had the appearance of health.

An interesting feature in the case was the peculiarity of the floating membrane. As the eye diverged, and it slowly floated through the vitreous humour, it could be seen to unfurl by degrees until it was completely stretched out, as if it had been lying in folds one above the other. No vessel was to be seen near it. He was ordered to take ten grains of iodide of potassium three times a day.

On May 5th, vision had so far improved that No. 10 Jäger could be read. The ophthalmoscope showed an unaltered condition within the eye. On May 7th, the pupil was less dilated, and acted more readily. Vision had improved to No. 6 Jäger, and the field of vision had increased, at eight inches measuring four and a half by five inches. Both qualitative and quantitative perception of light were moderately good. The opacity was less dark, and there was a slight reflection from the fundus through it. The disk remained hyperæmic. On May 10th, he could read No. 6 Jäger. The film could hardly be seen. The disk looked healthy. On May 12th, the field of vision was perfect; the film could not be seen; he read No. 4 Jäger. On May 17th, he read No. 1 Jäger; the film was gone.

The hypothesis that a large membranous opacity should be suddenly formed, and in a few hours give rise to total blindness, is, I think, untenable. A more reasonable supposition is, that such membrane has

been slowly formed, that during the time of growth it has been firmly attached, and, from some unknown cause, a large portion has become detached, and, floating through vitreous humour behind the lens, caused sudden loss of vision.

HOSPITAL VERSUS HOME PRACTICE.

By J. MATTHEWS DUNCAN, M.D.

[Continued from page 464 of No. for Oct. 30th.]

THE comparison of hospital and home practice would, of course, be quite easy if satisfactory data were obtainable. The difficulty lies mainly in getting good data of home practice. This difficulty is greater, I believe, if amputations are made the special subject of comparison, than if deliveries are used for the purpose. I have already given reasons which, added to those of Messrs. Holmes (*Lancet*, August 7th, 1869) and Callender (*St. Bartholomew's Hospital Reports*, vol. v.) are sufficient to demonstrate the unsatisfactory and incomplete character of the amputation data which have been recently used to shew the superiority of home practice. Now, I propose to try what can be done, with the view of contrasting hospital and home practice, if we use confinements for the purpose.

There is no difficulty in getting good hospital data. I shall use those of the Dublin Hospital alone. It is the largest and the oldest lying-in hospital in the British empire, and it has a vast mass of reliable experience to present in a statistical form—above 190,000 cases in the course of above 100 years. Here, it is well worth while to remark, that the data of a hospital with well kept records are far more valuable than those of home practice. If the hospital be large enough, its unmixed data, being sufficient alone, have an unity that cannot exist in a mass of little scraps of private practice sewed together to make a whole for fit comparison. Further, the whole of the hospital's results are to be got. A private practitioner may not be able to give the whole even of his small experience. The bad times of the hospital are recorded as well as the good. The private practitioner, who has had bad times in practice, has an indisputable tendency and an inalienable right to maintain reticence regarding them. The practitioner with fine results is not unlikely to give them to the world, whether he be asked to do so or not. The practitioner with bad results is not unlikely to keep them to himself, even when asked by a friend to give them up.

The results of the home practice of midwifery may be arrived at in several ways. I shall describe them. They are all indirect and inexact when compared with the scrutiny of hospital books.

First: A lower limit can be got; that is, a figure which certainly indicates a less mortality than the real one. The value of this is easily shewn; for, if an author make the mortality of his own practice, or of lying-in generally, under the lower limit, then it is certain that his own practice has been erroneously given, or is quite exceptional, or that his estimate of the mortality of lying-in generally is erroneous.

This lower limit is obtained by reference to the reports of the Registrar-General. Under the heads metria and childbirth can be got a number of deaths of childbed, which, it is known, do not comprehend nearly all the deaths *in* childbed, which last is the figure that is wanted. The deliveries can be nearly exactly estimated from the number of children born, and the proportion struck. No doubt, there are various sources of inexactness and inaccuracy in all this, but still the value of the figure obtained in this way is generally admitted, and it is certainly nearly as valuable as any used in the whole of this inquiry.

Many grave errors would have been avoided in the discussion of the question now before us, if the value of this lower limit had been known. I shall illustrate this by reference to the work of Dr. Evory Kennedy on Hospitalism, etc. Dr. Kennedy is a Dublin practitioner. The Registrar-General's reports shew that the Dublin practitioners generally have had a mortality in their confinement cases of at least 1 in 114. That is the lower limit. Metria and childbirth deaths alone bring the mortality up to 1 in 114. As there are many deaths in childbed not included in either of these categories, it is quite certain that the mortality in childbed in Dublin is higher than this lower limit, 1 in 114. Now, Dr. Kennedy, in the work referred to (p. 78), states his belief that 1 in 256 may be held to be the mortality in childbed, a figure to be contrasted with the mortality in the Dublin Hospital—a manifest and great error. The hospital is naturally expected to have a higher mortality in lying-in than that of the city generally. The city generally has a mortality above 1 in 114. The figure therefore suited for comparison with the hospital mortality is considerably above 1 in 114. 1 in 256 is thus shewn to be an utterly misleading figure for any comparison or use.

Now, the lower limit for Edinburgh, as obtained from the Registrar-

General's reports, is 1 in 160. For the towns of Scotland it is 1 in 178 (from returns of 1860-64).

Whatever may be the mortality of lying-in in private practice in Edinburgh, it must be above 1 in 160. In the towns of Scotland it must be above 1 in 178.

Second: The Registrar's reports may be corrected so as to bring up their statements of deaths of childbed (metria and childbirth) to the statements wanted—namely, deaths *in* childbed (from all causes). If this can be satisfactorily done, then we will have a statement of the mortality in childbed in home practice.

Now, this has already been done in two ways. Dr. McClintock (*Dublin Quarterly Journal of Medicine*, August 1869) has ingeniously suggested that it should be done on the following plan. He finds the figure indicating the excess of the mortality *in* childbed above the mortality of childbed (metria and childbirth deaths) and adds this to the mortality of childbed. In other words, he finds how many lying-in women die of other causes than metria and childbirth, and adds the figure to that indicating the deaths from these causes. I have no objection to this method of calculating when no better is available. But I have used a more direct and safer plan, that is, to search the public records for all deaths of lying-in women from whatever cause. I, some years ago, had this carefully done for Edinburgh and Glasgow in 1855. I did not limit the search for deaths to four weeks after delivery, but extended it to six weeks; and this is unfortunate, as four weeks is the usual time understood to be adhered to in inquiries like the present. But the error thus introduced must be very trifling and unimportant.

Now, this search shewed that the home practitioners of Edinburgh and Glasgow in 1855 had, among lying-in women, a mortality of 1 in 107.

Third: The third source of knowledge of the mortality in private practice is spontaneously published records. These form, for reasons already given, a source of data that is not very valuable. "There," says M. Danyau (*Fievre Puerperale*, 1858, p. 177), "there are secrets which every one keeps carefully to himself. If all the world were sincere" (he adds), perhaps "we would find that the results are less favourable than is generally supposed." Accordingly, in the discussion in which Danyau took part, neither he nor any one else said what was the mortality in his private practice, although everyone wished to know. Some darkly hinted at the greatness of the mortality, and there can be no doubt that the restraint arose from fear of revealing a high figure of deaths. The great Dublin discussion revealed some speakers bold enough to state the figure wanted. But the originator of the discussion satisfied himself with observing that he lost only 1 in 1200 from puerperal fever in his private practice; a statement equally valueless and misleading, hiding facts, not revealing them. To conclude, there can be no doubt that records of private practice will have a strong tendency to error in the way of representing the mortality as smaller than it really is. The data of unfortunate practice will come to light in unduly small proportion.

In private practice that is not exceptional and in the records of which we have reason to think all deaths are included, I find such mortalities as the following (*Edinburgh Medical Journal*, November 1869, etc.): 1 in 45, 1 in 56, 1 in 105, 1 in 108, 1 in 121, 1 in 159, and so on.

If we had the mortality of the practice of the practitioner who in 1852 flourished in Brackel, or the figures of those gentlemen who have had what is called an epidemic of puerperal fever, we should have high mortalities compared with those just recorded.

Now, let us come to a conclusion in regard to the mortality of lying-in women in private practice. I cannot, after all the care taken, be otherwise than indefinite. I can see no reason to suppose that the figure in private practice is far from 1 in 100. It may be worse. It may be better.

This has to be contrasted with the total mortality in the Dublin Hospital, 1 in 72; or with the mortality of large periods, as that of Collins, 1 in 100.

Considering the extraordinary aggregation of cases of difficulty and danger in such an hospital as that of Dublin, I can find room for no astonishment at its mortality. I can find no sufficient grounds on which to lay any blame at its door. I can find no reason to believe that the mortality among its lying-in would have been less had they been confined in their own homes; while I am certain that the loss in comfort and other valuable objects would have been immense.

The eminent Dr. Stokes has summed up the whole case of hospital *versus* home practice, in a few words, and in favour of hospitals. The mortality in private practice is, he believes (*Dublin Quarterly Journal of Medical Science*, August 1869, p. 313), with all its favourable circumstances, as great as or greater than in lying-in hospitals.

Whatever may be the exact truth on this point, the history of the Dublin Hospital makes it plain that metria is an awful invader of its beneficent precincts. There is every reason to suppose that its attacks

may be prevented or their baneful effects reduced in amount. To attain to this supremely desirable object, the efforts of science and art should be laboriously and incessantly directed.

AN AID TO PARTURITION, AND TO THE TREATMENT OF DISPLACEMENTS OF THE UTERUS BY A NEW MECHANICAL APPLIANCE.*

By PROTHEROE SMITH, M.D.,

Physician to the Hospital for Women, etc.

WHAT makes the contrast between the easy and rapid childbirth of the savage state, and the painful and lingering process in civilised life?

In answering this inquiry, I shall endeavour to elucidate the chief reason of this obvious difference, and thence to show what gives the advantage to the savage. I shall endeavour to prove that a force, which exists in the one, and is deficient in the other, explains to a great extent the dissimilarity of the two instances. I hope then to propose a remedy for the defect, and to give my experience of its efficacy. The same question applied to uterine ailments is capable of a similar explanation, and its solution will, I venture to say, elucidate an important cause of the frequency of diseases of women in civilised life.

Man being erect, it necessarily follows that the structure of the pelvis and abdominal muscles must be such as to retain the viscera in their proper position, and to make the necessary expulsive efforts, which defecation and parturition require, with impunity.

The spinal column thrown into a series of waves, not only lessens shock from concussion, but by its lumbar curve the plane of the pelvis is rendered oblique, constituting that graceful form, which, when absent, is artificially supplied by a pad or bustle, and which is so designed as to support the abdominal viscera on the pubes, aided by the abdominal muscular walls and spine, and to retain them within the pelvis. But when the plane of the pelvis becomes horizontal by the obliteration of the lumbo-spinal curvature, or, as it is called, the Grecian bend, the abdominal viscera tend to prolapse towards or beyond its outlet, and to displace the pelvic contents.

This lumbar curve being absent at the extremes of life, the infant is found to be straight-backed; whilst the common evidence of old age is the obliteration of the double antero-posterior curves, going on to the bowed back and drooping form of those who are well-stricken in years. But the deficiency of the natural spinal curves is more or less known in early adult life; it may be from disease of the bodies of the vertebrae, with which we have at present nothing to do, but its chief cause may be found to be defective muscular power, which, in its cause and effect, forms the chief subject of the present paper.

Unlike the ancients, who suspended their garments from the shoulders, the usages of modern society impose upon females, more or less from the period of birth, the habit of wearing a cinch round the waist. From this are suspended all the clothes, which, by their weight, keep up considerable pressure on the thoracic, lumbar, spinal, and abdominal muscles; and this persistent pressure often leads to degeneration of the tissue of these muscles. Thus the muscles used in diaphragmatic respiration, and for fixing the chest in forced efforts of expulsion, as well as those of the spine and abdomen, are enfeebled by absorption or degeneration of muscular fibre, and so their functions are impaired. This alteration in the muscular tissue is well known to all who have witnessed the result of tight lacing in the deadhouse, in the muscular atrophy which marks such cases, and attenuates the abdominal walls. The consequent obliteration of the lumbar curve, and the levelling of the plane of the pelvis, destroy the natural lines of the female figure, and open the door to all those maladies, which result from the above-named deformity of the spinal column, and of the pelvic outlet.

Two great canals are lodged within the pelvis, viz., the alimentary and the generative. For the expulsion of the contents of the one, in the act of defecation, there are necessarily: 1. The involuntary contractile force of the bowels; and 2. The voluntary force supplied by the spino-abdominal muscles.

In this act the direction of the pelvis is altered, so as to facilitate the passage of the fæces by means of an inclined plane, over which they are readily expelled.

In parturition, the expulsion of the child through the pelvic canal is protracted, in like manner, for lack of muscular power; and the process at times seems almost left to the unaided force of the womb. Hence this organ is overtaxed, and sometimes fails in its efforts till helped by instrumental traction.

* Read before the Midwifery Section at the Annual Meeting of the British Medical Association in Leeds, July 1869.