about two inches through these openings, it is found that they both impinge on dead bone, situated above the back of the acetabulum. On rotating the thigh, its head is found to be on the back of the ilium. The thigh is flexed on the abdomen, and the leg on the thigh, owing to the contraction of the hamstring tendons. He always lies on his right side, with the body bent forwards. Cod-liver oil, one drachm, three times a day, with meat and wine.

26th.—An abscess was opened by a small incision at the junction of the upper and middle third of the thigh, to the outer part, giving exit to four ounces of laudable pus.

Jan. 1st, 1857.—The purulent discharge from the abscess and the night-sweats more profuse. The appetite and strength are materially diminished. Continue diet and oil, with four ounces of wine.

7th.—The boy being under the influence of chloroform, Mr. Erichsen passed a probe through the upper wound, and found the head of the bone carious, and lying on the dorsum of the ileum, close above the acetabulum. He then made a T-shaped incision over the head of the bone, and divided the fibrous structures which held it *in situ*, and then the head being well pushed out of the wound, and the soft parts guarded by an assistant, he cut through the trochanters, from within outwards, by means of Butcher's saw; a very small portion of carious bone was gouged from the brim of the acetabulum. Three small arteries were ligatured. One suture was put into the lower part of the wound, and the whole dressed with wet lint. The limb was put up on a long splint, bracketted over the wound, but the leg could not be quite straightened, owing to the contraction of the hamstring tendons.

14th.—The patient's general health seems better; he is cheerful, and takes his food with relish. The discharge from the wound is profuse, but healthy. The ligatures came away on the third day, the suture on the first. To-day, the splint was re-applied, the child being previously put under the influence of chloroform, which easily affects him, but makes him feel sick all the rest of the day, and his head throb severely.

21st.—The boy is improving slightly in general condition; the discharge is profuse; a small collection of pus was let out close to the wound; the splint was re-applied. The leg is now quite straight, the rigidity of the hamstring tendons apparently having been due merely to confinement in bed. Charcoal poultices have been used from the day following the operation, and are to be continued.

28th.—The boy has greatly improved since last report. He sits up in bed, takes his food well, and the discharge is but trifling, the wound looking clear and granulating healthily.

I have thus endeavoured, gentlemen, to point out to you that there are three distinct forms of hip-joint disease, one commencing in the soft structures of the joint, the other in the pelvic bones, and the third in the femur. This division is founded in Nature, as I have had frequent opportunities of showing you in the wards, and as will be seen by the cases I have just brought before you, and it has an important practical bearing on the operation of excision, which is unnecessary in the first variety of the disease, improper in the second, and only requisite, in the third, in those instances in which Nature fails to effect a cure.

The operation of excision of the hip-joint, or more properly of the head of the femur, for the whole of the joint—that is to say, the cavity of the acetabulum, as well as the head of the femur, can never be removed—is a simple one. It consists in making an oblique T-shaped incision over the dislocated head of the bone, clearing this of the surrounding soft parts, turning it out by adducting and pushing back the thigh, and then removing it either with a saw or cutting pliers. I have used both in these operations, but prefer the saw. In the last case, in which I lately operated, I found great advantage from the saw invented by an excellent Dublin surgeon, Mr. Butcher, the blade of which was passed behind the bone, and then being turned by a screw in a horizontal direction, readily removed the head and trochanters.

The principle on which excision of the head of the femur is practised is twofold: first, to remove a mass of carious bone, which, by its irritation, keeps up hectic, and will ultimately destroy the life of the patient; and secondly, by taking away carious bone that is insusceptible of ankylosis, to establish firm union between the clean cut surface of healthy bone and the side of the pelvis, and thus restore a useful limb to the patient.

In excising a carious head of the femur, the first object we have in view is the preservation of the patient's life. It is a rule in surgery, that whenever a patient's strength is being

from carious bone, the cause of the constitutional disturbance, the diseased osseous tissue, should, if possible, be removed. It is, therefore, as much in accordance with sound surgery to remove the carious head of the thigh-bone, when its presence is threatening the patient's life, as it is to amputate for caries of the tarsal bones, or to excise a diseased elbow, under similar circumstances. In fact, in extreme cases of femoral coxalgia, the surgeon has to choose between standing by inactively and seeing his patient gradually sink exhausted by the suppuration resulting from the diseased head of the femur, or, by excising this carious structure, removing the cause of the wasting discharge, and thus averting a fatal termination. It is true that there would be another alternative—viz., amputation at the hip-joint, but I am not aware that the most strenuous opponent of excision of the hip has ventured to advocate such a proceeding in preference to the operation we have been discussing.

There is, however, another object besides saving the patient's life, to be attained by excision of the head of the femur. It is the restoration of a tolerably useful though necessarily a shortened limb. The shortening of the limb that is left is not, however, the result of the operation; it has already taken place as a consequence of dislocation on to the dorsum ilii before the head of the bone is removed. As a small portion of the upper end of the femur merely is excised, never more than what projects beyond the acetabulum, the already existing diminution in length is not increased by the operation.

After the operation, the limb should be put on a long splint, bracketted opposite the wound. No perineal band should be applied to the injured side, but the splint may be connected with one attached to a leather case put round the sound thigh, from which extension is to be made.

NOTE

ON

OVARIOTOMY

AND

OVARIAN TAPPING.

By J. MATTHEWS DUNCAN, M.D. &c.,

LECTURER ON MIDWIFERY, EDINBURGH.

IN THE LANCET of February 28th, I published an article entitled "Is Ovariectomy justifiable?" I there showed that the arguments hitherto used in justification of this operation were either loose and illogical or absurd. In THE LANCET of March 21st, I find an answer to my paper by Professor Simpson, the gentleman whose arguments chiefly were impugned. My first impulse, on perusing the paper, was to lay it aside, as it contained much disagreeable matter personal to me as a private individual, and nothing in the way of solid argument that demanded my attention; but further consideration has induced me to reply, even at the expense of some reiteration and of the painfulness of controversy, however amiably conducted. The importance of the subject is an ample apology for this, and I shall avoid private matters carefully. I shall make my remarks under two heads:—

I. WHAT DR. SIMPSON HAS DONE.

1. He complains that I do him injustice in excluding ovariectomy from amongst those operations of which he says that (to use his own words) "he particularly doubted whether surgeons were justified in so often subjecting patients to a great chance of speedy death, from a severe surgical operation for the removal of a disease which might still allow of the continuance of life for many months or years," &c. Re-examination convinces me that I used this quotation justly; but if Dr. Simpson ever intended it to be used differently, I have no desire to cling to it. I never used it as an argument against ovariectomy, but only as an illustration of bad reasoning. He insists that these remarks do include ovariectomy; that is, his labours to justify ovariectomy leave him particularly doubting whether surgeons are justified in so often subjecting patients to a great chance of speedy death from it. A curious result. The qualification he introduces with the words "*so often*" cannot be important, for I suppose all will agree that an operation which is justifiable once is justifiable a thousand times, and the reverse.

2. Dr. Simpson designates objections against the use of statistics in therapeutical questions "common platitudes usually offered by young medical critics." I am already older than Newton was when he had completed many of his greatest discoveries. (Readers will pardon my presumption in introducing such a name into so small a question.) On the other hand, I think I have little more than half the number of years of Dr. Alison, the most distinguished medical philosopher of this age, when writing the ominous warnings, designated as platitudes &c., concerning the dangers of applying statistics to therapeutical questions. The age of an opponent is but a sorry argument. I am rapidly correcting this fault of mine.

3. Dr. Simpson fixes upon amputation at the hip-joint as the arena on which to defend his statistics against my objections, that the circumstances of the cases included in the statistics, were not known so as to make us sure they had such reasonable amount of likeness to one another as to be fairly clubbed together. His defence consists in telling us only one circumstance, and that true only of twenty-four out of eighty-four cases. The one circumstance is, that the amputations were for the cure of chronic disease. But this is evidently not even a good palliation of the error I point out. I have again to ask, what was the chronic disease? When an ovary is cut out, we have a well-known multilocular dropsy of it. But, what is chronic disease? Is it cancerous, tubercular, or inflammatory? Is it of the bone, the joint, or of the whole limb? Alas, we are still in total darkness!

4. In concluding his remarks on cases of amputation of the hip-joint, Dr. Simpson points out that the amputation was "fatal in the proportion of 75 in 100 cases in which it was adopted for the removal of hopeless chronic disease of the limb—a mortality which is nearly double the mortality accompanying ovariectomy when performed for the removal of hopeless chronic disease of the ovary." Here it is evident we have two defects which vitiate the whole argument; for we do not know on what grounds, if any, the cases are said to be hopeless; nor do we know whether the hopelessness was of life or merely of cure of the disease.

5. Dr. Simpson publishes a long defence of the absurd statement that first-tappings are fatal in one case out of every five operated on. It would be tedious to follow him through his argument, and useless; for my original article, already referred to, disposes of the whole matter in an irrefragable manner; and Dr. Simpson's objections, so far from being at it, are scarcely beside it. For instance, he points out that a child passing through its sixth year has already passed through its first year; and adds something clever about a dull and dispassionate schoolboy. He forgets that Dr. Southam's table was first shown to be unworthy of any confidence, and then that it is not one of first-tappings, and that if a woman has been tapped twelve times, the first-tapping could not have proved fatal. But such diversions from real argument are unworthy of the subject.

After all, will Dr. Simpson, or anyone else, really attempt to defend Southam's table, made up as it is from a set of cases selected to illustrate the course, and especially the terminations, of ovarian disease, when used as a means of ascertaining the fatality of first-tappings? Again, will Dr. Simpson, or anyone, defend the proposition that first tappings are fatal once in every five cases? I may remark that I lately received a note from one of the most distinguished, oldest, and certainly of the most experienced obstetricians in the three kingdoms, informing me that he had never seen a death from tapping at all. Many others have told me the same.

All Dr. Simpson's argument as to this point is conducted as if he knew what use I would make of Southam's table, were it one of first-tappings. I have not entered on the investigation of the real mortality of first-tappings, and, therefore, have never indicated what use I would make of it. I shall only say I will never use it in the absurd way he too early attributes to me. But in his defence of his own use of it he has fallen into an error of reasoning, which, in deference to its author, I shall not class amongst the platitudes, &c. The error, or fallacy, consists in passing off what is true abstractly, as if it were true in relation to the question in hand. Thus it is most palpably true that the fifteen cases in Southam's table, which, according to himself, had undergone tapping once or several times without dying from it, must all have been tapped once safely. But this very simple truth as to fifteen cases surviving has no proportionate relation whatever to the other five that died, in respect of the question of the mortality of first tappings.

To use Dr. Simpson's own example. What would Farre or Neison think of the mortality of the first year of infancy being arrived at by selecting four cases dying in that year, and fifteen

who survived it. It would be true that four died in the first year, and that fifteen did not, but these numerical facts would have no relation to the question.

II. WHAT DR. SIMPSON HAS NOT DONE.

On this subject I cannot venture to occupy the valuable columns of this journal, further than to say that my whole original article, referred to, remains unanswered. I am quite willing it should stand without defence, as it does not need it. I beg those interested in the question only to read it dispassionately.

There is one aspect of Dr. Simpson's paper which I regret having even to notice. This is, the attempt made, directly and indirectly, to injure my private character. I am described by Dr. Simpson as having "totally misrepresented in various respects the statements" of himself and others; next, it is said that I only "misrepresent some of the opinions and statements" of Dr. Simpson; then it is proposed to "show the various forms of misstatement and error which he (Dr. D.) has committed;" then it is asserted in regard to a statement of mine "that the exact reverse of this is true," and it is implied, if not said, that I knew this to be the case; then it is said "Dr. Duncan is entirely wrong when he makes these very characteristic observations," and it is added, "'Nothing,' long ago observed Dr. South, 'is so haughty and assuming as ignorance when self-conceit bids it set up for infallible.'" Then it is tried to cast ridicule upon me for speaking even hypothetically of gangrene of a limb after fever requiring amputation, an operation which I have seen performed by a distinguished hospital surgeon. Further, it is said by Dr. Simpson that "he believes that no medical man, except Dr. Duncan, can have any doubt in his mind that they were instances in which the first operation of paracentesis was the more immediate source of death of the patient by exciting peritoneal inflammation." These words, "except Dr. Duncan," &c., were written by Dr. Simpson with the following words of mine under his eye. My words are: Case 1, *This is evidently an example of death from tapping*; Case 2, *This case is also selected in order to illustrate death from tapping*; Case 3, *Death was the result of the first of an intended series of tappings*. For my account of the fourth case I refer, without hesitation as to the result, to my original paper. Again, Dr. Simpson attributes the antithesis of the dangers of phlebotomy and tapping, made use of by M. Velpeau, to me. Again, he says, "it is really difficult to comment upon such sad affirmations and unhappy reasonings as these with due and proper gravity." And again, he adds, "If Dr. Duncan's communication had been confined to our own Society or Journal in Edinburgh, where he is sufficiently known, I would, in all probability, not have taken the trouble of publishing any correction of his misstatements," &c. &c.

Such are some of the statements in Dr. Simpson's paper on Ovariectomy and Ovarian Tapping. Are they arguments? No. Personal abuse is known to be a resort of those who feel the deficiency of good argument.—Are they worthy of professional writing or debate? No.—Are they palliated by Dr. Simpson's position in the profession, while I am perhaps still included amongst the juniors? No.—Did I give Dr. Simpson any example of such writing or debate? No. I can truly say, that while I hesitate not to call arguments or measures, illogical, absurd, or ridiculous when they so appear to me, I have never in any publication, anonymous or otherwise, stooped to characterize an author or an opponent in the manner in which Dr. Simpson so unprovokedly adopts towards me. Neither shall I so far belie my own reputation as to seek now to defend it. I appeal to the profession without hesitation and without reserve; and I shall not attempt the disgusting task of applying similar treatment to my opponent. I am proud to say that, so far as I know, the mass of the profession is on my side. Since my paper was published I have received numerous and valued testimony in its favour from my professional brethren. Several of these are from most distinguished, if not the most distinguished, obstetricians, physicians, and surgeons in London, Dublin, and Edinburgh. Their words are often far too flattering to me, and, of course, not intended for publication; but they have been the greatest possible comfort to me in entering, even in the careful way I did, into the troubles of controversy.

Castle-street, Edinburgh, March, 1857.

MEDICAL SOCIETY OF LONDON.—This evening (Saturday) a paper will be read by Dr. Septimus Gibbon "On the Identity of Typhus and Typhoid Fever."