COLLECTIVE INVESTIGATION OF DISEASE.

THE Metropolitan Counties Branch of the British Medical Association may be congratulated upon the very successful and influential gathering that assembled at the Royal School of Mines, in Jermyn-street, on Wednesday evening, to hear Sir William Gull and Sir James Paget advocate the adoption by the Branch of the work of collective investigation that is being inaugurated within the Association, and which, if steadfastly pursued, may prove of much importance not only to the Association but to medicine generally. The assembly was a thoroughly representative one, and the manner in which the addresses were received augurs favourably for the movement. Dr. Bridgwater, the President of the Branch, occupied the chair, and, after clearly and forcibly stating the object of the meeting, called upon Sir William Gull, who expressed his pleasure at being asked to fulfil the duty of forwarding a movement of such great value to the public and the profession. What the Collective Investigation Committee were undertaking was, he said, a scientific inquiry into those matters which come daily under notice, and, although the work was only just commencing, it promised much, for its sphere was not that of morbid anatomy, but more particularly the tracing out the beginnings of morbid processes, their modifications under varying circumstances, and their evolution. In general practice alone did the opportunity offer itself for noting the beginnings of disease, whereas hospital practice only dealt with declared disease where morbid changes were manifest. Diseases having a different origin and cause may merge finally into one type of change. What was wanted was the clinical analysis of the early states; he instanced "Bright's disease" as an example. He warned his hearers against being over-sanguine of the early fruits of such inquiry, which demanded the exercise of the intellect and the overcoming of prejudices. Much would depend upon the questions that were formulated; they should be simple and their purport easily explained. It was a duty they owed to the profession to collate their observations for this purpose. It was not necessary to wait until there were physiological explanations given for every condition. Clinical facts well observed were of great value by themselves. Such a movement, too, had an educational value, maintaining, in those who followed it up, that docility of mind which was the antithesis of prejudice, or so-called "experience," that was the chief obstruction to truth. Certain dangers beset such inquiries—e.g., that which dealt with the communicability of phthisis without reference to the form of the disease; and partial truth endorsed by a large body of inquirers was a far more serious thing than individual error. In that particular question there was great risk of confounding causes with effects. Sir William then dwelt at length upon the great value of family life histories, and alluded to the difficulty in obtaining them from the unwillingness of individuals to admit their ailments. But such histories were known to family practitioners all over the country, and it was impossible to estimate the gain to knowledge upon inherited disease, tendencies to disease, and transmutations of disease that would accrue from the in-gathering of all this experience. He had always fancied there was a certain parallelism between the diseases of age and childhood, and such an inquiry might show in what this consisted. It would also show how in some families there was a greater susceptibility to certain infectious diseases than in others, and the reasons for this; whilst the large class of cases that have no pathology, those in which there is life-long suffering, people with sensitive nervous systems and feeble digestions, might be elucidated by this means. New questions would constantly arise out of the old that had received answer. Sir James Paget, after remarking that Sir W. Gull had anticipated all he had to say, observed that the list of questions already issued showed that in medicine, unlike other departments, men were not afraid to express doubts upon even the most minor points, a frame of mind which he contrasted with that of the politician, who never has any doubts on any subject upon which he speaks. A chief design of the inquiry was to bring the general practitioner to add his knowledge for the

advance of science. There was absolutely no other way of obtaining family histories than through him, and the importance of such histories was very great pathologically. But to render the work useful they must labour in the spirit of Darwin—collating the commonest facts, embracing every opportunity for investigation, and, like Darwin, they might be enabled to draw the grandest conclusions from the facts of ordinary daily life. Some were not qualified to make use of their powers of observation; but it would be impossible to estimate the large amount of knowledge that is lost to science for want of record. The unveiling of mysteries was one of the most universal pleasures of mankind, and the whole object of scientific inquiry and observation was to this end. Prof. Humphry, in proposing a vote of thanks to Sir W. Gull and Sir J. Paget, declared his belief that the Association had undertaken no work so important or of such good influence as that now being done in the collective investigation of disease; and if the Association had grown numerically powerful, this power brought with it responsibility, and should be utilised to the good of medicine. One difficulty in the successful prosecution of the work was the inertia of those who should be concerned in it; not really because they were too "busy," for the busy men were those who were the most energetic and able, and the most anxious for the advancement of their art, and such men were the best supporters of such an investigation as this. He concluded by paying a warm tribute to the earnestness and incessant labour in this cause shown by Dr. Mahomed, the secretary to the committee. Dr. Quain seconded the resolution in order, as he said, to protest against the lines of inquiry already laid down, particularly that upon the contagion of phthisis. Dr. Strange, the President of the Association, supported the resolution, which was cordially carried. Dr. Hare then proposed that a committee of the branch should be nominated to supervise the whole; and, in the course of his remarks, eulogised the memoranda upon the subjects of inquiry already issued. Mr. Hutchinson seconded the motion; and the names of the proposed committee were read. Dr. Wade moved, and Dr. Acland seconded, a vote of thanks to the chairman, with which the proceedings closed.

Correspondence.

"Audi alteram partem."

SIR HENRY THOMPSON ON PROF. BIGELOW'S EVACUATOR.

To the Editor of THE LANCET.

SIR,—I should not trouble your readers with any remarks on Professor Bigelow's article on a "Simplified Evacuator" had he not made, relative to my instrument, statements which are extremely inaccurate. On this account alone it is that I am compelled, with more reluctance than I can describe, to contest any question with my respected friend, the Professor of Harvard.

First, he describes and draws an aspirator of mine, which he says I have "lately abandoned"! So far from having done so, I rarely operate without it, and use it nearly as often as the more recent model.

Next, the drawing of this aspirator is intended to prove that its action is so defective as to return into the bladder a large portion of the debris already removed. I have, moreover, long known that Professor Bigelow has been in the habit of exhibiting one of my aspirators for the purpose of publicly illustrating its alleged defects (with bits of coal and water). I do not make any great complaint of this, although it is a mode of controversy to which just exception might be taken.

All I have to say is this, that nothing is easier than to use another man's instrument, in that man's absence, so as to make the instrument appear inefficient. *But it is not so in my hands.* Had Professor Bigelow ever seen me use it—as scores of his compatriots have—he would know that, if properly used, there is no reflux of debris into the bladder. The best instrument in the surgical armamentarium may be misused, and grossly too, and such is the fate of mine in the professor's hands if he meets with the result which he is at such pains to publish to the world.

I have performed the operation of lithotripsy at one sitting more frequently, probably, than any living operator; and

I am delighted with it. Will anyone say—and there are abundant witnesses—that it is not a rapid and complete proceeding in my hands? How could that be possible if my aspirator is so defective?

And now I have only to congratulate Professor Bigelow on his present search for "simplicity." He well knows that I regard his suggestion to remove the stone at one sitting as a great advance. But his first instruments were a return to the time of Heurteloup! After years of patient experience, the mechanism of both lithotrite and evacuator had become marvellously simple and efficient. This simplicity he disturbed, disastrously, for a time; exhibiting elaborate and costly apparatus at the Congress here in 1881, wholly useless to the practical lithotritist. He could not see that not one new instrument was required to carry out his excellent idea. As we were to attack larger stones, we wanted larger and stronger instruments—that was all. Clover's bottle, a little modified perhaps (as I suggested last January in THE LANCET) is as good as, if not better than, any, and I still often use it.¹ Only make it larger than before, and attach a larger evacuating catheter, *when you want a larger*, not otherwise. All the perforated tubes and strainers get so blocked with débris (as I found long since) in the human bladder—not with coal in water—as to be practically useless there.

I am heartily sorry to have been compelled to reply in a spirit of criticism, but the statements and the mode of proceeding referred to above rendered it impossible for me longer to remain silent. I am, Sir, yours obediently,

Wimpole-street, W., Jan. 1883.

HENRY THOMPSON.

"CASE OF FEIGNED SKIN DISEASE."

To the Editor of THE LANCET.

SIR,—The case of factitious skin disease, put on record by Dr. Colcott Fox, reminds me of one that came under my observation some years ago when I had charge of the dermatological clinique at St. Thomas's. A girl of seventeen, a maid of all work, came before me one winter—that is to say, at a time when hands and arms which are exposed to air and wet are apt to get chapped—with considerable chapping and soreness of all these parts. She professed not to consult me about this however, but about a considerable number of holes in the skin of her forearms, which she said were produced by carrying pails of water and other heavy things up and down stairs. She stated that when thus engaged she would frequently feel something give way in her arm, and that, on looking, she always discovered a newly formed hole. At the moment I was inclined to think that the wounds might really be due to the sudden extension in depth and length of some of the pre-existing chaps, determined by the stretching to which the skin had been subjected. A little examination, however, satisfied me that the holes were of artificial production. They were all of exactly the same character; their direction was horizontal or nearly so; they were oval, or rather egg-shaped, with the pointed end always in the same direction; they were between $\frac{1}{4}$ and $\frac{1}{2}$ inch in length; their edges were smooth and vertical, and they looked as though they had been made with a punch; there was no surrounding thickening or inflammation, and their floor was formed of the healthy subcutaneous connective tissue. I concluded she had snipped out pieces of the skin with scissors. I explained my views to my class, out of the girl's hearing; and one of them left the room and presently returned with two or three wounds in his arm, which were the exact counterparts of the girl's. He had made them with scissors; and added that no pain attended their production. I said nothing to the girl on her first visit; but on the second, charged her with deceit, and threatened to inform her mistress. I need scarcely say I did not see her again.

I remain, Sir, yours truly,

Old Burlington-street, Jan. 5th, 1883.

J. S. BRISTOWE.

¹ Professor Bigelow persists in repeating that the size of the evacuating sound in Clover's instrument is No. 21. I do not doubt that he has seen one of that size, but there was no limit in that matter formerly, as there is none now; the operator used any size he thought proper. One now at my side, which I have had upwards of fifteen years, is No. 23. With Clover's apparatus I removed scores of stones before the one sitting system was proposed. I am glad to see that Mr. Berkeley Hill employed Clover's bottle recently (*vide* THE LANCET, Jan. 13th); I really do not think he will find one much better.

"PICRIC ACID AND SUGAR TESTING."

To the Editor of THE LANCET.

SIR,—Strange, almost incomprehensible, as it may seem, I need not go farther than Dr. Johnson and his son's admissions to substantiate my position in this controversy, which has now resolved itself into the simple question of whether or not a sulphide is produced on boiling albumen with a solution of potash.

To be quite sure that there is no misunderstanding about what has been stated, let us start with the following quotations. Dr. Johnson says that his son "demonstrates and explains the fact that while a lead sulphide is formed by boiling albuminous urine with liquor potassæ *mixed with lead*, no alkaline sulphide is formed when the same urine is boiled with *pure liquor potassæ*." The son, after mentioning that it is stated and accepted as a fact in chemical text-books and to be found in so many words in Watts' Dictionary that albumen gives rise to an alkaline sulphide when boiled with a solution of a caustic alkali, says that he was surprised to find that it "produced no trace of sulphide, even after boiling for half an hour with a strong solution of potash; the solution thus obtained gave no dark colour or precipitate with a solution of acetate of lead." Further on he states that "if the lead solution was added to the albumen and potash *before boiling*, and heat afterwards applied, an abundance of lead sulphide was produced. "The fact is," he continues, "that a sulphide can be produced only by boiling an alkaline solution of albumen (made in the cold) with a metallic solution, such as one of lead or copper."

Here we have it stated in unequivocal language that a sulphide is not formed on boiling albumen with potash. This is the stand-point of Dr. Johnson and his son. Now, what does the son say in his communication contained in your last week's issue? "With the object of allowing every possible chance for the production of an alkaline sulphide by the action of caustic potash upon solution of albumen at the boiling temperature, I boiled the alkaline mixture for 'half an hour,' as Dr. Pavy has correctly quoted me. The result was that *no trace of sulphide was produced*, as I have already (equally correctly) stated. Dr. Pavy, on the other hand, speaks triumphantly of an abundant production of sulphide observed on adding a solution of lead to one of albumen and potash which had previously been boiled for 'fully five minutes.' I have repeated this experiment of Dr. Pavy's, and can corroborate his statement, that 'it does not signify whether the lead solution be added before or after the boiling,' when the ebullition has been continued for so short a time, and when the quantity of albumen present is so large as it was in Dr. Pavy's experiment."

The question, then, resolves itself into the issue being dependent upon the length of time the boiling with potash is carried on. It is admitted by Mr. Sillingfleet Johnson that after boiling for five minutes the evidence of the formation of sulphide is obtainable, and stated that after boiling for half an hour no sulphide may be discoverable. To this latter statement I assent, and we are thus brought to an agreement upon facts.

Agreeing then upon the facts, what inference is to be drawn from sulphide being discoverable at the end of five minutes, and not at the end of half an hour? Obviously that during the process of boiling it undergoes destruction. If it is admitted to be there at the end of five minutes, how is it conceivable that anyone should argue that it is not formed because it is not found at the end of half an hour? It is useless to waste time and words with those who argue upon such grounds.

I have said that I assented to the statement that no sulphide may be discoverable at the end of half an hour, and I have to say further that the gradual loss of sulphide may be watched. For about ten minutes or a quarter of an hour the quantity of sulphide present goes on increasing, as shown by testing the product with lead solution from time to time, but after this it declines, and sometimes when the boiling has been continued for half an hour the sulphide is found to have disappeared, but more often a longer period of boiling than this is required. No wonder that the careful and elaborate experiments of the son, which have been so repeatedly expatiated upon by the father in your columns, failed to reveal the presence of a sulphide. The observer was searching for that which he had previously destroyed.

Mr. Sillingfleet Johnson may learn, if he will consult his