the pain; and he thinks also that vision has slightly improved. This patient is returning home to some distance, and is to apply a succession of blisters behind the right ear.

With regard to the precise structural changes which have given rise to defective vision in some of these cases, I have not ventured an opinion. It would be easy, no doubt, to form conjectures; but I prefer rather, having stated the symptoms and appearances, to leave them to be judged of by those who are familiar with this department of ophthalmic medicine.

Original Communications.

REPLY TO DR. THUDICHUM'S REMARKS ON DR. SKINNER'S CASE OF SUSPECTED POISONING BY OXALIC ACID.

By THOMAS SKINNER, M.D., Liverpool.

IN the JOURNAL for February 18th, Dr. Thudichum charges me with having "publicly and deliberately" made an "enormous accusation" against some one. I am not aware that in any part of my paper I have accused anybody; far less have I done so publicly and deliberately. Not even to the patient herself and the nurse have I *accused* the domestic. The safety of my patient demanded that my suspicions should be told them. But surely there is a manifest difference between suspecting a person to two private and interested individuals, and accusing the same publicly and deliberately. Moreover, I do not consider that I made the case public by recording it in a journal which is strictly limited to the discussion of subjects connected with the medical profession; and particularly when the names of the parties interested were withheld. As to the "summary discharge", it was no more than any sensible man would do with a hired servant who was *really suspected* of poisoning her mistress. Dr. Thudichum mistakes the object of the dismissal; it was never dreamt of as a punishment, for there was neither accusation nor conviction; the servant was dismissed, with wages and board wages, and *a present into the bargain*, to serve no other object than to prevent a repetition of the same conduct. For my part, I think she was too well served. Besides, there were other circumstances, sufficient in themselves, let me state, to warrant the dismissal of any domestic.

Dr. Thudichum seems to think, that, "above all things," it was my "duty to inform the person herself of the accusations against her." But would not such a course give to guilty persons an opportunity to escape? I am inclined to think, that the propriety of making such disclosures to suspected parties is better left to the discretion of those in attendance, than settled by any fixed rule.

It is not my intention to trouble the associates by following Dr. Thudichum through all his arguments; I shall content myself with replying to those points only, which are essential to the refutation of his view of his case; namely, that it was one of natural disease.

In the first place, let me premise, that I think that Dr. Thudichum has taken considerable liberty with the facts of the case. As a proof of this, I beg to refer to his brief statement of what he calls the "essence" of the case, at p. 127:—1. Let me ask, in what part of my paper have I stated that a "furred tongue" and "intense thirst" are *consequent** upon "occasional vomiting and retching?" 2. In stating the symptoms which occurred on December 2nd, he leaves out the most important one, namely, the *burning pain at the pit of the stomach*; and he modifies the second in importance, the *singultus* and *deadly collapse*, by leaving out the hiccup altogether, and making the collapse to have been only "*apparent*". In refutation of this, I have simply to refer to my description of the collapse, at page 107. So real was it, that in spite of stimulants, external and internal, artificial heat, etc., the patient was still in the same state when my friend Mr. Marsh joined me two hours or more afterwards; and it was many hours after this before reaction was established. (Compare this last clause with the last paragraph in the first column of page 128 of Dr. Thudichum's remarks.) 3. He continues:—"Next day she had twitchings, which, in another place, he designates as 'fidgets'."† Dr. Good's definition of *dysphoria simplex* is "general restless-

ness, and accompanied with a perpetual desire of changing the position." If Dr. Thudichum will take the trouble of reading the description of this affection in Dr. Good's *Study of Medicine* (third edition, by Cooper, vol. iv, p. 510), I think it will puzzle him to point out any nosological, physiological, or pathological resemblance, worth attending to. 4. In this same "essence" of the case, it is reported, that "six ounces of the fluid vomited by the patient on December 2nd, were examined, and found to contain no poison." Not a word is stated as to the period of the vomiting when this fluid was obtained. 5. "The patient.....had on a *previous occasion* six leeches applied to her on account of inflammation of the cervix uteri." The date of this previous occasion is not given (two years ago); and, although I have not stated the kind of inflammation, I now beg to say that it was not chronic, but acute; or, more properly, subacute. Few obstetricians at the present day are in the habit of leeching the os uteri for *chronic* inflammation of the cervix. Dr. Thudichum, in the sequel of his remarks, by placing this statement in conjunction with other misinterpreted facts, causes it to favour his idea, that the case was one of natural disease.

But I must not anticipate. I think I have given sufficient proof, without wading into the substance of Dr. Thudichum's remarks, to show that he has made grave errors, both of omission and commission; and yet, this is what he calls "the essence of the case, divested of its atmosphere of suspicion." (page 127.)

Now to the case itself. Dr. Thudichum grants as much as, that "the second and third attacks might be produced by oxalic acid." If so, then I have chiefly to satisfy him as to the existence of the peritonitis, or the non-occurrence of the uterine colic; and to demonstrate to him the relation which the peritonitis as well as the vaginal discharge bear to the suspicion of poisoning.

1. *The peritonitis.* Dr. Thudichum is strangely inconsistent in his manner of writing: for instance, speaking of "such a clear acute attack of peritonitis," he says: "Giving Dr. Skinner full credit for the correctness of his diagnosis, so far as regards the existence of peritonitis." A little further on, he says: "Which pain was treated as peritonitis, but is perhaps more properly described as uterine colic, etc." If the pain was not arising from peritonitis, how did it yield so readily to appropriate treatment on both occasions? If it was uterine colic, how did it happen, that, "the cause still persisting"—(the pent up effusion of blood undergoing alterations)—there should have been *complete remissions of pain* from the 15th to the 20th November, a space of five days, and from the 22nd November to the 2nd December, a space of ten days? How was it, the same cause still persisting, that, on the recurrence of the pain on the 2nd December, its character should alter from a lancinating peritoneal pain, with the pulse at 110, hard and wiry, to a burning pain felt internally at the pit of the stomach, with the pulse at 84, but small and easily compressible? And, if it was uterine colic, caused by an accumulation of a "*large quantity* of peculiar blood" in the uterine cavity, how was it that there was no uterine tumour? and that the hypogastric or uterine region should have been the part of all others the most exempt from pain of any kind on all of the occasions?

Dr. Thudichum seems to think, that "peritonitis, preceded by rigors, is a *very rare* disorder" (p. 127). Does Dr. Thudichum mean that the precession by rigors is uncommon, or that peritonitis itself is very rare? Among our standard authorities on such a subject, I may safely quote the words of Drs. Copland and MacAdam. Dr. Copland says—"The more general states of peritonitis occurring in a person of good constitution, or in the sthenic form, usually commence with rigors or chills more or less severe and prolonged" (*Medical Dictionary*, vol. iii., p. 69). Dr. MacAdam uses similar terms (*Cyclopaedia of Practical Medicine*, vol. iii., p. 192). And such has been my own experience. So far as peritonitis itself is concerned, I am aware that Dr. Habershon has lately attempted to show that genuine idiopathic peritonitis never does occur (see JOURNAL for 1859, page 1048). In the report of the discussion on Dr. Habershon's paper, read before the Royal Medical and Chirurgical Society, it is stated that he said—"In many of the best text books, peritonitis was regarded as idiopathic in its origin; but he had failed to discover a single case of that character" (*Medical Times and Gazette*, December 24th, 1859, page 644). This is not the place for a discussion on this subject; but I beg to record my dissent from such a conclusion, even although it is derived from the analysis of 3752 autopsies at Guy's Hospital. Certainly, in

* The italics in quotations are my own.

† I thought that the description which I gave of these *spasmodic twitchings* was unmistakable.

town and hospital practice it is rare enough; but in the country, in agricultural districts, it will be found far from uncommon. During about three years of practice in a strictly agricultural district of Scotland, I saw and treated many cases of genuine idiopathic peritonitis; and I feel confident that most country practitioners, in large practice in such districts, will corroborate this statement. I have seen idiopathic peritonitis, of the sthenic form, in a strong and healthy subject, happen as often as three times, at intervals of a few months, in the person of one woman, a field labourer; and, on all three occasions, the disease arose from over-fatigue, followed by exposure to cold, and its consequence suppressed perspiration.

So much for the rarity of idiopathic peritonitis; we shall now look to the cause of the peritonitis.

Dr. Thudichum puts great stress on the fact that I have placed the oxalic acid and the peritonitis in the relation of cause and effect. I am not aware that I have said much about this. In my narrative of the case, I have simply stated that the primary attack and the relapse were "seemingly arising from no very evident cause." In my after-remarks, I have certainly alluded the two inflammatory attacks, "without exposure to cold, damp, or fatigue; and the system previously, and at the time of the attack, robust and healthy," as proof of the possible nature of the poisoning. I freely admit that they do not prove the nature of the agent; and items 1, 2, 3, and 4 would have been more in their proper place under the previous head of "Question of Poisoning." The nature of the poison rests wholly upon items 5 and 6. As to the possibility of an irritant poison producing peritonitis, I am neither prepared to assert it nor deny it; but I do not think it improbable, that, if oxalic acid was introduced into the stomach of a person predisposed to peritonitis, the oxalic acid might become the immediately exciting cause of its development. We know that even neutral salts may reproduce it, and that "the mildest possible saline laxative" has done so, and proved fatal, "after every symptom of abdominal inflammation had subsided" (Dr. Stokes, *Cyclopædia of Practical Medicine*, vol. iii., p. 315). We have yet much to learn of the etiology of peritonitis, particularly of its predisposing causes. For instance, we know very little of what produces it in the intrauterine condition; and although Professor Simpson has thrown much light on this mysterious subject, we have still very much to ascertain. (See his *Obstetric Memoirs*, vol. ii., p. 171.) All things considered, I think we ought to hesitate a little before stating, that the ingestion of an irritant poison, such as oxalic acid, is incapable of being an immediately exciting cause of peritonitis, particularly in a system where there is a predisposition to inflammatory action of a sthenic type.

The next question is, as to acute or chronic poisoning. At one time I was inclined to believe that the case might be one of those where natural disease (peritonitis in this instance) is taken advantage of to screen criminal purposes. (*Beck's Medical Jurisprudence*, 5th ed., p. 644.) The circumstances which led me to adopt the view I have taken, were as follow:—I have attended this patient, occasionally, during the last two years, and I have frequently found her suffering more or less from nausea, nervous prostration, and sometimes considerable pain of the stomach. These minor attacks have always yielded to time and simple measures; but, since I have had leisure to weigh all the circumstances of the case, I am inclined to think that the moral effect produced by my bodily presence had a great deal to do with the convalescence on those occasions; and the more so am I now of this opinion, as, for the last two months and a half—4. e., from the date of the dismissal of the domestic—these symptoms have never recurred, and she has enjoyed uninterrupted good health. It was, undoubtedly, an "after-thought" of the girl, when she told me that she had had similar gruel given her by the domestic previous to her severe illness; but it was just this after-thought, taken in conjunction with my previous observations, which led me to suspect not only acute, but chronic poisoning.

Again, if we take for granted that the patient had an attack of acute peritonitis on the 8th of November last, proceeding from the ingestion of oxalic acid or any other cause, is it not suspicious, to say the least of it, that all the leeches applied on two separate occasions should die immediately, particularly when we consider that not one out of six leeches applied to the same individual two years before did die? and, that the death of leeches applied to persons, who were known to be labouring under the poisonous action of oxalic acid, has been observed by various competent authorities? Let me ask, is the inference overdrawn, which supposes that the death of the leeches may

have been caused by oxalic acid in the system, or by some deleterious substance in the blood resulting from its decomposition; or by some change in the blood itself, or in the cutaneous transpiration, caused by the ingestion of oxalic acid, and destructive to the life of the leech? If the inference is not overdrawn, then the probability is, that oxalic acid was administered previous to the date of the excessive vomiting and collapse (2nd December); consequently, the case was, most probably, not only one of acute, but also of chronic poisoning.

One word about the death of the leeches, which Dr. Thudichum treats of, as the "baseless fabric of a vision."—He says, that the case of Dr. Arrowsmith, reported by Professor Christison, "has evidently suggested the inquiry about the leeches." I beg distinctly to contradict this statement. The nurse herself can testify to the fact, that, I never asked her a single question about the death of the leeches until she first informed me. The nurse who put on the leeches is the same woman who was sent for to wait upon the patient on the 2nd of December. I can certify that she is elderly, and a most respectable married woman, that she is experienced in leeching, and that it was her own observation and experience, and not mine, which associated the death of the leeches with the possibility of poisoning. Dr. Arrowsmith's case was confirmatory, and not suggestive, of "the inquiry about the leeches."

2. *The Discharge per Vaginam, etc.* Dr. Thudichum places great emphasis on "the bloody discharge at the crisis of the disorder." In the whole of my report I have never once hinted at a bloody discharge. I have only spoken of a "copious flow of a dark-coloured fluid" (p. 108). A little further on he calls it a "sudden discharge of blood." This also is equally gratuitous; there was nothing either sudden or bloody about it, as will be further seen in what follows. Although I have stated that I was unable, under the circumstances, satisfactorily to account for this discharge, I have nowhere said that it was a "symptom or sequel of oxalic acid" poisoning. I have not so much as alluded to the discharge in my remarks on the case. But Dr. Thudichum, who never saw the patient in sickness or in health, endeavours to account for it. He says, "This blood"—(what blood?)—"This blood was not ordinary blood; for the stains from such blood do not resist the ordinary domestic efforts at removal. It cannot be fresh menstrual blood, for a similar reason; particularly if we give credit to the statement that the catamenia had appeared shortly before her illness." (Dr. Thudichum may give full credit to this statement; it is a most important fact. The patient informed me that she had been poorly only a few days before the rigor on the 8th November last.) On the head of this fact, Dr. Thudichum is not afraid to risk the opinion that, "if we consider this blood to have been effused into some internal cavity sometime before its discharge, and there to have been retained and altered, then the colour of the fluid, the copiousness of its flow, and the peculiar nature of the stains, are satisfactorily accounted for." I wonder how many obstetricians will believe this? (The "some" internal cavity turns out to be the uterine; and the time of the effusion is November 8th last.)

As to associating the convalescence with the appearance of the discharge, Dr. Thudichum forgets that it came away the day after the violent symptoms had subsided; there being an interval of at least twenty-four hours. And I now beg to inform him that, though copious, it began gradually, increased as it progressed, and decreased as it disappeared; it extended over three days and a little more, and at no period was it associated with pain. In fact, the girl told me, when it first came on, that she thought her monthly period had arrived; and, for all that has been said about this discharge, I am now much inclined to believe it was none other than the menstrual secretion; she was poorly during the first week of November; in the course of nature she ought to be so again in the first week of December. The chief causes of my doubts at first, were, the profusion and dark colour of the discharge, considering that the patient had so lately come through a severe attack of acute inflammation, and had lost so much blood; and the comparative difficulty of removing the stains. Of this last cause of doubt, I have since questioned several females; and they have informed me that it is not unusual to have some difficulty in removing stains caused by vaginal discharges. As I have previously stated, I now repeat, that I never associated this discharge with the suspicion of poisoning. I simply recorded the symptom as one which was neither to be expected nor easily explained, but requiring to be noted.

To account for a host of symptoms, peritonitis and all, Dr.

Thudichum "assumes" that the large quantity of peculiar blood had existed in the uterine cavity from the time of the rigor on November 8th, about twenty-five days, *although he knew that she had been poorly her usual time only a few days before that date*; and he considers that the assumption is supported by "great probability", as the "female had before been suffering from inflammation of the cervix uteri" (p. 127). Dr. Thudichum clearly finds it inconvenient to give the date of this previous inflammation, which happened in 1857. The attack was not what might be called acute, as the general circulation was not much disturbed; it was more local and subacute. The cause of it, I believe, was a sudden suppression of the catamenia, from over-fatigue and exposure to cold, on the second day of their appearance. The inflammation was permanently relieved at the time, and it has never returned. Her periods since have been perfectly natural in colour, quantity, and regularity, and comparatively free from pain; and they answered to this description during the period immediately preceding the rigor.

Dr. Thudichum, as he proceeds, seemingly begins to doubt whether an attack of inflammation two years ago would fully account for the present symptoms; so he says, "It was a case of chronic disease of the uterus, either chronic inflammation of the cervix or its consequences, *narrowing or obliteration, of the cervical canal*; or, what is also very likely, and accounts better for the alleged regularity of the catamenia, it may have been a case of *antero- or retroflexion of the uterus*, diseases which are not rarely accompanied by inflammation of the cervix uteri." It does not signify what it may or may not have been; we wish to know what it was which gave rise to the phenomena. And, let me ask, is it all likely that a chronic or permanent diseased condition—a strictured or occluded cervix uteri—an ante- or a retroflexio uteri of *two years standing* (?)—could give rise to a single and a temporary obstruction, lasting over twenty-five days; and the patient, previously or since, having in no way suffered from any form of dysmenorrhœa? That the symptoms were not arising from, or associated with, any of those conditions, is very evident, as none of them exist, or ever did exist, in the patient. In proof of which, I have to state that Mr. Marsh and myself, since the publication of Dr. Thudichum's remarks, have carefully examined the patient (C. W.), and the following is our report:—We found the external genitals perfectly natural and healthy, but not in the virgin condition. Examined digitally, the vagina was normal in length, calibre, sensibility, and moisture; and there was no stricture or cicatrization in any part of it. The os and cervix uteri were natural in size and form, and the sensibility to pressure was not increased. To the touch, the os and cervix felt elastic and firm, yet soft and yielding. The surface of the os was smooth and regular. The os and cervix occupied their natural position in the pelvis; and there was no induration, ulceration, hypertrophy, nor atrophy; neither could any other morbid condition or mechanical displacement be discovered. On introducing the speculum, we found the mucous membranes of the vagina and os uteri perfectly natural in colour, while the character and amount of their secretions were normal. No eruption, abrasion, nor ulceration, could be seen; neither was any blood, pus, mucus, or sanguinolent fluid, visible. The uterine sound was passed with the usual ease in a healthy subject; its point being directed upwards and forwards towards the umbilicus, as far as the fundus uteri. We found the uterine cavity to be of the natural length, two inches and a half; and it was empty. The sound was withdrawn, and re-passed by both of us; and with similar ease. In no part of its course was it grasped, leave alone obstructed; and we failed by the sound, as well as by touch, to detect the existence of ante-flexion, retroflexion, or any other kind of displacement of the uterus.—Thos. Skinner, M.D.; Nicholas Marsh, M.R.C.S.Eng. Liverpool, February 19th, 1860.

Where are Dr. Thudichum's ideas now? Where is the natural disease, the chronic inflammation of the uterus and its consequences, the "narrowing and obliteration of the cervical canal"? Where the ante- or the retroflexion theory? Where the explanation of the pain of the peritonitis, the *uterine colic*? Where the explanation of the *dénouement*, the critical vaginal discharge? Where the pent-up, peculiar, altered blood-fluid? And how was it kept pent up in the uterine cavity for *twenty-five days*? and that too, in spite of the "vomiting and retching", which "had no doubt a great share in *propelling the blood* through the narrowed or obstructed cervical canal" (page 128); and in spite of the "series of contractions and relaxations" of the uterus to expel it, and which Dr. Thudichum has "no manner of doubt", if the uterus had been examined atten-

tively during the attacks of pain, would have been observed" (p. 128). Where is Dr. Thudichum's explanation of "the hard wiry pulse, the furred tongue, the intense thirst", *et hoc genus omne* recorded at page 128? "All these", he says, "are symptoms characterising obstruction of the cervical canal of the uterus and collection of fluid in its cavity" (p. 128).

Transactions of Branches.

BATH AND BRISTOL BRANCH.

CASE OF TRAUMATIC TETANUS.

By C. HARPER, Esq., Bathaston.

[Read February 23rd, 1860.]

THE patient was a half starved boy, aged 12 years, living in one of the most beautiful suburbs of Bath. I first saw him on the evening of January 10th, when I learned from his mother that a fortnight previously he received a wound a little below the left eye, by a playmate shooting an arrow of elder-pith at about twenty yards from him. It entered some distance beneath the skin, and required force to withdraw it, when the wound bled freely; little was thought of it, and it healed in three days without any surgical aid. The boy remained well for a week after, when his mother perceived a slight squinting of the left eye, as well as a very unnatural appearance of it, from not being able to close it: for this, he was sent to the Ear and Eye Infirmary for treatment, without, however, any benefit. About four days after, the mouth was observed to be drawn to the right side, as well as some difficulty in opening the jaws. As this continued, and the boy appeared dying of starvation—the jaws not admitting anything but fluids, being firmly fixed—the mother applied to me for advice.

I found the patient suffering from partial paralysis of the facial nerve of left side, accompanied by locked-jaw. The features were very peculiar, the two sides of the face having a different expression, although both retained their sensibility. The left side was much swollen; the eye was wide open (that peculiar state called lagophthalmia, from paralysis of the orbicularis palpebrarum), the pupil being drawn directly outwards; the conjunctiva was much injected from exposure to the air and dust. The right eye was closed. Both pupils acted. The hearing was not affected. The mouth was drawn to the right side; the jaws were firmly closed and immoveable, unless on application of some force, when they could be slightly separated, and fluids introduced, which were swallowed with difficulty. The masseter and temporal muscles were as tense and hard as a board. He complained of no pain. The mind was not at all affected, and there were no general tetanic convulsions. The bowels were confined. The pulse was 60, low; the respiration was quickened. The wound was directly over the left infraorbital canal; and its cicatrix could be distinctly felt very hard, and of about the size of a millet-seed.

The treatment I employed was a free incision into the injured part, isolating it from the general nervous system: and I encouraged the bleeding, as a moderate loss of blood might not be without its use. I searched the wound for any portion of the arrow which I thought might be the exciting cause, but failed to find any foreign body. A blister was applied to the spine to relieve any inflammatory or congestive state of the medulla spinalis or its coverings which might exist, although I think functional derangement alone existed, influencing muscular action. Croton oil and calomel were given to cause a free evacuation of the bowels. He was ordered to avoid all excitement to allay muscular spasm; and to take as much nourishment as possible, in order to sustain life.

On visiting him the next day, I found that he had had no sleep during the night, wandering very much at times, complaining of great pain in the epigastrium. Exertion caused slight tetanic spasms of the abdominal muscles, with partial opisthotonos; also of the flexors of the thighs and legs, and some clenching of the hands. The muscular rigidity of the jaws and of the muscles of deglutition, was undiminished; the jaws being firmly and completely closed. He was fast sinking, and died the following morning.

REMARKS. This case was one of acute traumatic tetanus; interesting from the seat of the mischief being so well marked, as shown by the separate set of symptoms—the partial paralysis from the injury of the facial nerve. The amount of injury, the cause of so much mischief, was certainly small; but it is more