condition of the habit; little benefit to the local affection has been seen to follow its employment; and it is on the topical bleedings that we must rely. Fomentations to the limb have usually afforded considerable relief; but I have had occasion to place more confidence in mild stimulating liniments, and wrapping the limb in oiled silk, than on fomentations, which are also more troublesome and fatiguing to the patient in their application. Evaporating lotions have also been found very efficacious when much heat is developed in the limb.

With respect to the internal treatment, I am disposed to rely much on rapidly getting the habit under the influence of calomel, with the double view of subduing the inflammatory action, and also exciting the energy of the capillaries, so as to carry off, if possible, the fibrinous deposit in the affected veins. The reduced condition of the habit of the patient, in the present instance, precluded the addition of an antimonial to the calomel. On the contrary, it was necessary to support the strength, and in order to effect this intention without augmenting the excitement, the infusion of yellow cinchona was ordered. This might appear to you incompatible with the leeching and other means adapted to subdue the local affection; but the influence of a simple tonic is not at variance with such measures, nor calculated to augment the inflammatory action, but the contrary; and I have seen no reason for altering the opinion which I have frequently delivered to you, that, in numerous instances, debility furnishes the pabulum of inflammation. The internal administration of the biborate of soda when aphthæ appears in the mouth, in low states of the system, is not a very common practice in this country, but from my experience of its influence in correcting the condition of the mucous membrane which causes aphthæ, I have no hesitation in recommending it to your attention. In the treatment of the present case its salutary influence was conspicuously demonstrated by the disappearance of the aphthous state of the mouth, when the biborate was administered, its recurrence when the medicine was left off, and its disappearance again, when it was resorted to a second time. The administration of it in the infusion of calumba, appears to aid greatly its power; upon what principle I cannot say; it is sufficient, however, to refer to the well-known fact, that in many instances, compounds have properties not to be found in their constituents, and this is an example of it; for I have not found the simple aqueous solution of the biborate equally serviceable. Upon the whole, we may conclude that the treatment of such cases, in general, should be conducted on the same principles as that of rheumatic metastasis to any other internal organ, modified by the character of the local affection.

ORIGIN OF THE ERGOT OF RYE.

To the Editor of THE LANCET.

SIR:—In THE LANCET of June 22nd is a paper, signed "F. B." in which it is asserted that I have assumed to myself the credit of discovering the cause of ergot, which credit, it is said, is due to my friend, Mr. Smith, of the Royal Botanic Gardens, Kew. I shall, therefore, feel obliged by your insertion of the following; though I do not consider myself bound to answer anonymous correspondents, yet, as I am represented to have acted unhandsomely towards an individual for whom I have always entertained the greatest respect, both for his talents as a botanist, and his personal kindness to me (which feeling of respect appears mutual, if I may judge from his last letter to me, dated June 3rd, 1839); therefore I cannot consent to remain silent.

It appears somewhat strange that "F.B." should have allowed Mr. Smith's claims to slumber for eight months, more especially as during this time I have often met Mr. Smith, who has never once opened his lips to me on the subject.

To explain the matter, I must state that when at Kew Gardens, in the summer of 1838, Mr. Smith pointed out to me the "elymus sabulosus," a grass, as being ergotised; and as I, as well as he, doubted the opinions hitherto entertained of its nature, I was glad to have the opportunity of examining it in the recent state, and begged a few spikes of the grass (not "the fluid" only, as F. B. mentions), which I told Mr. Smith were for the purpose of investigating the matter. The specimens were kindly given me, and Mr. Smith did not say he was, or had been working, at the subject, more than watching the grass externally, and he told me that he conceived the liquid on the spike (the "certain fluid" of F. B.) to be deposited or produced by a peculiar fly that was often seen on the grass, and which fly he imagined to be more or less connected with the formation of the ergot, as the cynips is with the nutgall.

About a week after, having examined, by the aid of the microscope, the specimens I myself brought from Kew, I wrote for more, and mentioned that "I had discovered the whole secret," as I then thought, for I had found the fluid to consist of sporules, which could not be any excrementitious fluid of a fly; which sporules, sporidia, or jointed bodies, were, I conceived, the reproductive particles of a fungus,—of what kind I then had not made out.

In a letter sent me by Mr. Smith, bearing date of October 15, 1838, my request is acknowledged, and he offers to bring me the specimens to my house, which was done, and in the same note he mentions that he had not seen the fly lately, but had collected the fluid, and found sporules also, in which he meant to steep grains, for the purpose of making them produce ergots when they grew to perfection. When Mr. Smith was with me, I showed him specimens of the ergot under my own microscope, and pointed out that the relation of the ergot to the styles and scales, at the bottom of the flower, was precisely that of the healthy grain, and neither of us at that period knew how to account for the sporules in any way being capable of producing an ergot.

I heard nothing more of Mr. Smith's investigations till we met at the Linnæan Society, on November 6, when a paper was read on the ergot by Mr. Smith, the printed abstract of which F. B. has published in full. After the reading I believe I uttered the words to Mr. Smith, "you are wrong" (which F. B. seems to be acquainted with), because I did then differ in opinion from some of the points in that paper.

Now the truth must be told, that the abstract of Mr. Smith's paper, published in the "Proceedings of the Linnæan Society," does not contain all the opinions that gentleman entertained at that time; for, after his description of the fungus, and his discovery of it in the *anthers*, and his opinion that it caused ergot by communicating disease to the grain, he mentioned that these minute joints became *animated*, or, in other words, animalcules, when kept for a short time in the liquid that was obtained from the plant which contained them; which fact is in opposition to his former discovery; for one being cannot belong to two kingdoms, and I expressed my opinion on this and other points; and, as F. B. seems to recollect, I uttered the words "I am sure you are wrong."

In the interval between the meeting of the Linnæan Society, on the 6th of November, and that of the 4th of December, I carried on my examinations into the cause and structure of ergot; and at the meeting of the Society held on the evening of the latter date, I am accused of adopting Mr. Smith's views in the paper that was then read. I confess I did adopt his views of the nature of ergot, but I did so without borrowing his discovery of the fungus on the anthers to convince me; and it was by patient investigation, and experiments of a delicate nature, that I arrived at the conclusions I did, which took three weeks of continued examination to complete, and which substantially proved what I then considered had only previously been partially done.

These observations are recorded in another place, and are not required to be gone through again in the present instance; suffice it to say, that they consisted in proving that the external particles of the ergot were not animalcules, but sporules of a fungus, which I succeeded in causing to germinate, going through all the various states, from the commencement to the perfect state of a plant, up to its development of similar bodies to those from which itself was produced; which series of observations incontestably proved that the fungus was a separate plant from the grain, and I considered I had as much right to make known my discovery of the independent germination of the sporules, as a proof of the nature and origin of ergot, as what Mr. Smith had by his finding the fungus on the anthers.

I am accused by this anonymous writer of not giving Mr. Smith his share of the credit of the discovery of the origin of ergot, either in my paper or in the abstract; but the fact is, Mr. Smith's paper had been read and spoke for itself; and in the "Proceedings" both papers appeared as abstracts; and if I did not put forth what that gentleman discovered, I mentioned the essential matter of his observations. But I feel that some apology is due to him for leaving his name out of my account of the ergot, inserted in the "Medical Gazette" of the 19th of January, which was done inadvertently from a desire of brevity, and not with a view of wishing to take from him any share of credit to which he is entitled for his observations.

According to F. B. the credit which he wishes to claim for Mr. Smith is the discovery "that the ergot is not a fungus, but a diseased grain, occasioned by the growth of a fungus, not previously detected." But it is fair to other botanists, who have examined the nature of ergot, to state their discoveries and opinions before this claim is adjudged to any individual in particular.

And if F. B. reads Fries's description of *Spermœdia*, he will find that he considered it the diseased grain of grasses; and in a note in the "English Flora" (Vol. v., Part ii., p. 226), Berkeley entertains the same idea. If F. B. reads Phœbus's account in the "Deutschlands Kryptogamische Giftegewächse," 1838, he will find the ergot figured as a diseased grain correctly, and also the sporules of the fungus of F. B., "not previously detected," are there also figured with extreme accuracy; and it is there also stated, as well as in Christison's "Treatise on Poisons" (3rd edition), that Wiggers could produce ergot by infecting healthy grains with the seeds (sporidia) of the fungus; and in Philippar's "Treatise on the Nature and Origin of Ergot," the *viscid juice* is described, its supposed origin is pointed out, and its containing numerous sporules, is also related; he goes farther, and figures the sporules, and gives a drawing of the fungus on the *anthers* cementing them together into one mass, and occasionally calls the ergot an "ergotised grain;" but still in these several descriptions all but the discovery is made out, and some credit is to be given to these individuals for their observations, which I myself, and I believe Mr. Smith, likewise, were ignorant of whilst investigating the subject.

To Mr. Smith I will say is due that share of the discovery in which he proved and described what Berkeley and others previously imagined, viz., that the body known as ergot was not a fungus but a diseased grain, and I reserve for myself the substantiating of his views by a different and more perfect proceeding; and also as being the first to observe the parasitic fungus to develop throughout all its stages up to maturity, and to arrive at a perfect plant, unconnected with any part of a grass; which fungus, being new and undescribed, I considered I had the privilege of naming *Ergotœtia*, and did think of taking the specific name of it after Mr. Smith, as F. B. mentions, on account of my respect for him, and also because he was the first that I was then aware of to detect it in a place where it was not before observed, which fact went a considerable way towards pointing out the true origin of ergot; but by the advice and suggestion of a mutual friend, Mr. N. B. Ward, of Wellclose-square, I adopted the term *abortans*, for reasons which F. B. has assigned; and had I any doubt of Mr. Smith's share of the discovery, is it probable that I ever should have proposed his name as being fitted to form the specific one of the newly discovered genus?

I trust from what has been said, that I have acted in no way to deprive Mr. Smith of his claims; and, in fact, here I allow them; and if I did not speak of them so fully as he wished, I did not deny them, or speak in any way against his discovery in my paper.

There is one more point that I must take notice of:—F. B. remarks that I "may, by subsequent observation and research, have developed some minutiæ which do not appear in the paper of Mr. Smith, is what might be expected, and need not be denied." Now great care is taken to withhold the nature of these minutiæ, and, in fact, these minutiæ are all that I do claim, being the development of the fungus, apart from the plant, and proving that the bodies in the interior, which had been supposed to be sporules of the fungus, are nothing but fatty particles, incapable of producing ergot, being, in fact, the most substantial of the proofs that the ergot is a diseased grain, and not a fungus, containing sporules, as Philippar, and previous investigators, had imagined.

I am sorry to be obliged to make this public reply to accusations brought against me by an anonymous correspondent, but I trust that nothing I may have said will prove offensive to Mr. Smith; and I beg to assure that gentleman (for whom I, as well as many others, have always entertained the greatest respect), that nothing do I here state with the view of depreciating his abilities, or wishing to detract anything from his merits; and I do hope that Mr. Smith and F. B. will feel satisfied with this explanation. At the same time I must remark, that should a further correspondence be entered into, I shall not take notice of any more *anonymous* communications, and have to express my regret that I have taken up so much of your valuable pages in refuting accusations brought against me. I remain yours obliged,

Edwin J. Quekett.

50, Wellclose-square, June 26, 1839.

REDUCTION OF HERNIA.

To the Editor of The Lancet.

Sir:—I trust the following case will be considered of sufficient importance to merit a space in your valuable Journal. I am, Sir, your obedient servant,

J. Sawkins, M.R.C.S.L.

Towcester, June 20, 1839.

On the 28th of January I was called upon to visit Elizabeth Dunkley, aged 66, an out-door pauper, residing in Towcester, but belonging to the parish of Grafton. She was of a weak, emaciated constitution, and much troubled with dyspepsia. She complained of great tenderness, and pain on pressure, of the abdomen, which she had experienced two days previous to my visit, the bowels not having been opened since the 26th; a weak pulse; tongue white and coated, and considerable pain in the head. I imagined the above symptoms indicated constipation of the bowels, therefore I ordered her some aperient medicine, and told her I would call in the course of two or three hours. On my second visit she stated she could not retain the medicine, and, in fact, everything she took was ejected; her bowels had not been relieved, and there was now great anxiety depicted in her countenance. My suspicions led me to inquire whether she had any swelling in the abdomen; she replied in the affirmative, and, upon examination, I found a hard, unyielding tumour on the right side, about the size of a small egg, which, with the attendant symptoms, fully confirmed me she was labouring under strangulated crural hernia. I applied the taxis for some time, (first pressing the tumour downwards and then upwards, as the course strictly recommended by the works on hernia) but without success. I left her, to request Mr. Watkins to see her; he attempted the same means, but without avail. I then suggested to him the passing of the tube of the stomach-pump per anum. I inserted the tube its whole length; a great quantity of confined air escaped, the tumour gradually re-