asserted—viz., that he is not accustomed to use the instrument unnecessarily, or in a profligate manner, as the whole tenour of Dr. Simpson's observations would lead the reader to infer.

Finally. Whatever statements the patient may inadvertently have made to Dr. Simpson, either in reply to leading, inquisitorial, unprofessional, and perhaps imperfectly understood inquiries, that did not concern him, I cannot be answerable for. All I can declare, and that most solemnly, is, that no lady was ever treated with more consideration, kindness, delicacy, and decorum, than the party in question.

I have the honour to remain, Sir,

Yours respectfully,

E. W. DUFFIN.

Langham-place, Oct. 6, 1851.

DR. SIMPSON'S UTERINE SUPPORTER.

To the Editor of THE LANCET.

SIR,—I trust I may, without being considered in the light of a partisan, state the little experience I have in the utility of one of the novelties introduced to the notice of the profession, for the treatment of certain uterine affections, by Dr. Simpson. I allude, in particular, to his "uterine supporter," or the "infernal impaler" of Dr. Isaac Irons. The term "uterine supporter" appears to me an unfortunate one, and evidently has had the effect of misleading many—Dr. Irons amongst the number—who have had no opportunity of getting practically acquainted with its mode of action in appropriate cases. The instrument, in reality, is not a supporter, in the more obvious sense of the term. It is a contrivance to prevent the recurrence of the lateral deflections of which the organ has shown itself susceptible, and not to *support* a prolapsus.

Now as regards my own experience in the matter. Three cases of retroversion have come under my notice in the course of the past year, when the supporter performed its orthomatric duty with great comfort to the patients, who had already submitted to the usual expedients in such cases, without material advantage. It is very seldom, indeed, that we have not at the Hospital for Women several in or out-patients wearing the uterine segment of this orthomatron; and it is but fair to state, that not a single instance has come to my knowledge, where, if any inconvenience was sustained, that inconvenience, never distressing, did not speedily disappear after its removal.

Isaac Irons being, as I mentioned, practically unacquainted with the instrument in question, has been led away under the erroneous impression that the uterus was stuck on a piece of metal, something like the hat on his own head; but the truth is, the instrument will not sustain the organ at all on such a principle; and it only requires a careful consideration of its construction, with due reference to the axis of the womb in situ, soon to satisfy an unprejudiced mind that the term *impaler*, saying nothing about its fearful adjunct, *infernal*, is a very unjustifiable appellation.

If it were not for the spirit so sadly displayed between these two champions respectively of the old and new systems of handling the opposite sex, tending, perhaps not unnaturally, to draw the unfavourable notice of the profession on those appearing to concern themselves in it, I should be tempted to bring forward many other reasons for believing that this invention of Dr. Simpson's, with other inventions, not Dr. Simpson's, which we have frequent opportunities of testing at the hospital, are anything save diabolical contrivances.

I remain, Sir, your obedient servant,

HENRY SAVAGE, M.D.,

Physician to the Hospital for Women,
Orchard-street.

Gloucester-place, Portman-square,
October, 1851.

MEDICAL ASSISTANTS.

To the Editor of THE LANCET.

SIR,—The pages of THE LANCET have always been open to the complaints of medical assistants, and, very much to its credit, your journal has almost always shown a bias in their favour. In your number for October 4th, there are a couple of letters severally written by gentlemen who describe themselves as such, calling in question a communication of mine contained in your impression for the 13th ult.

It is somewhat remarkable that both these letters dwell most pointedly upon that which is the root of all evil—money. Yes, gentlemen, I did say, and more than this, do again repeat, that £30 or £40 per annum is sufficient remuneration for a medical assistant; and I will tell you why: it is because the majority of practices *can not* allow more; and further, I will venture to predict that as long as the practice of medicine

and surgery in this country holds together in its present form, the salaries, as a general rule, will not be more abundant. But there is something besides salary to be considered: I look upon every *bond fide* engagement of this kind worth from £80 to £100 a year to any young surgeon, for most certainly he must be very economical to be enabled to live in equal style and comfort upon such a sum, finding his own table and house room. The advantages in a professional point of view have been dwelt upon; but there are others. He has few trials except those of his own making, he has neither thought nor care for the morrow, no tax of any kind touches him, there is often a pupil to assist in dispensing, and in provincial practice a horse is generally kept for his express use. He certainly practises among the poor, but so do all junior practitioners; and is not disease the same whether in the pauper or the peer? There are many medical men now in practice who can look back with pleasure to bygone days when they were assistants; lasting friendships have been thus formed, and ties of a still closer nature have sometimes been the happy result. Very many in the profession insist on their sons acting in this capacity, well knowing its benefits. One correspondent sees fit to cast a libel upon general practitioners, and distinctly affirms that their treatment of assistants is dishonourable: this I stoutly deny, and there are thousands who will bear me out in asserting that the contrary is most certainly the case. That there are, here and there, some scurvy fellows who treat their assistants, as they treat their patients, very badly, it is impossible to deny, but it is base injustice to condemn the many for the sins of the few; for I would ask, are not the assistants of to-day the practitioners of future years, and is it likely that these same would act a dishonourable part towards their junior brethren? In conclusion I would add, that the letter in question was as duly considered as it was carefully worded, and the advice given is still recommended, notwithstanding the polite insinuations of A. T. C., or the dyspeptic grumbings of Experientia.

I am, Sir, your obedient servant,

Faversham, October, 1851.

W. N. SPONG.

CHEMISTRY AND ITS ERRORS.

To the Editor of THE LANCET.

SIR,—I have irrefutably shown, in my pamphlets,* and in papers which you have done me the favour to publish in THE LANCET, that water alone cannot produce the hydrogen and oxygen gases, and consequently that the alleged decomposition of water is decidedly a groundless assertion, arising from a total misapprehension of Lavoisier's experiment. Having many years since doubted the possibility of decomposing water, I most carefully and anxiously examined and considered the experiments of Cavendish and Priestly (in the *Transactions* of the Royal Society), as well as of Lavoisier, and also the subsequent ones of Dr. Wollaston and Sir H. Davy, bearing upon the subject; and the conclusion I have drawn for this investigation, and which I unequivocally and confidently assert must be ultimately admitted by scientific men, is that water has never been decomposed. I also unhesitatingly and with equal confidence assert, that from the same experiments it is clearly to be deduced that a METAL is an earth, *sui generis*, combined with electric matter—not merely that a metal is a conductor of electricity, but that the latter is chemically united to, and associated with, the earth, and that its metallic lustre, ductility, malleability, &c., is due to the electricity, which endues the earth with a seeming vitality.† It is also to be deduced that a metallic oxide is the union of the earth with oxygen after the exclusion of the electric matter, and which, unlike the oxygen, is, as far as we know, an imponderable substance.

The same experiments will be also found to establish the fact that oxygen and the electric matter are antagonistic elements, and are therefore never both found combined and in union with the same substance or base; but it appears that each element is able to eliminate the other, from any matter with which it may be united, as either happens to preponderate in quantity, and to be applied and directed to the other with that object. It follows, therefore, that when an acid is applied to a metal by which an oxidation of it is effected, that such oxidation goes on, *pari passu*, with the loss which the metal sustains of electric matter. The notion at present entertained that hydrogen and oxygen *unite* to form water, is

* Published by Ridgway, Piccadilly.

† If the definition of the vital principle be its independence of rest, electricity must be it, or its nearest ally.

therefore altogether erroneous, the fact being that electric matter not only chemically combines with peculiar earths and some other substances, liquid as well as solid, but also with water, and that hydrogen is the result of such last-mentioned union: the consequence is, that when the two gases (oxygen and hydrogen), in the proportions given by Lavoisier,* are ignited to form the water, the quantity of oxygen being sufficient to effect the separation of the electric matter from the water, or in other words, sufficient to decompose the hydrogen, the electric matter escapes, accompanied by an explosion and great heat, and the water which is then left behind unites with the oxygen and thereby becomes acid, as appears from Priestly's experiments, (of whose accuracy we have the Doctor's positive and repeated assurance,) while those of Mr. Cavendish afford a further confirmation of the fact of such acidity.

I think it will be also found to follow, from an attentive consideration of the same experiments, that the inflammability of most if not all combustible substances, depends upon the quantity of electric matter which they contain; and that what is termed combustion, is the exclusion or exit of such matter by means of its opponent, the oxygen, the intensity of the combustion depending upon the quantity of electricity, and also the velocity with which the electric matter is expelled.†

The clear, unquestionable, and important consequences, however, to be deduced from the experiments, are, that hydrogen must be eliminated from the list of chemical elements, and that two others must be added—viz., water and electricity.

That professional jealousy, and the interest-begotten prejudice, of which Bentham makes mention, may still retard this chemical reform, to the great detriment of science, is unfortunately too possible; but that any amount of opposition can prevent its being ultimately adopted, is not within the range of possibility.

I am, Sir, your obedient servant,

Reigate, Oct. 1851.

W. F. STEVENSON, F.R.S.

REMARKS ON PLACENTA PRÆVIA.

To the Editor of THE LANCET.

SIR,—If no other advantage arises from the communication I forwarded to you the other week, one great benefit has already resulted to the profession, and, I may add, myself, in the publication of some cases of placental presentation occurring in the practice of Mr. Lilley. He certainly starts with two very prominent advantages in his favour, which I can lay no claim to, and the value of these I most willingly concede without hesitation,—an experience of twenty-one years and a recent case of placenta prævia.

In the exercise of the right to comment upon the cases furnished by Mr. Lilley, as may seem most just and expedient, I request of him to believe that it is always in the spirit of courteous and candid remark, and without any wish to adopt even the appearance of a presuming or hypercritical tone.

A very careful perusal of these cases, then, fails to satisfy me of their value for any practical demonstration. The question at issue being the possibility of delivering a child through a lacerated placenta, I cannot perceive that their narration either militates against my position or substantiates his; indeed, I am not yet certain whether a careful analytic consideration would not lead to a conclusion totally opposite to what he seeks to establish.

Mr. Lilley has recourse to a singularly easy kind of argument, and assumes, as quietly as if it were a matter of course, that the placental substance is usually so broken down that you may pass your hand through it as readily as into an old glove, or so much hasty-pudding. Now, with all due deference, I do dissent most unequivocally from this broad doctrine, which at the same time begs and pre-judges the whole question, and deny that such a condition is necessary, usual, or peculiar. That diffuent placenta exist, I can well understand, but not that they are an essential part of such presentations, or occur as a natural consequence of hæmorrhage or dis-

organization, as it is termed. Let Mr. Lilley follow me in such a case as this, not of unfrequent occurrence:—

An accoucheur is hastily summoned to a patient who is flooding. This is her first attack. On examination he finds the placenta presenting, the os uteri dilated or dilatable—introduces his hand, turns and delivers—the whole affair being completed in two or three hours from the period of the seizure. Would it be said, in such a case, that the hand was passed *through the placenta*, or that there was time for it to become disorganized and “quite penetrable” in consequence of the “rupture of its blood-vessels, and complete infiltration with blood?” This alleged softening down of the placenta, I find no warrant for in any of the authorities I have been able to consult, nor has it even been hinted at by old and experienced practitioners, to whom my views are familiar, and who have supposed that the operation of perforating the placenta had been repeatedly performed by them. On the contrary, the diagnosis is a “spongy substance,” “elastic,” “like a cushion,” and certainly most unlike a pulpy, pulaceous mass, ingeniously and very conveniently *improvised* to meet and neutralize an unexpected difficulty. I do not deny the existence of placental cases, but withhold my belief from the assumed fact, that the hand can so readily penetrate their substance; nor am I inclined to credit the ease with which it could make for itself a passage through the thick and tough fetal membranes covering the placenta, which of themselves would materially resist and offer a powerful obstacle to the introduction of the hand.

Of the various cases cited by Mr. Lilley, I proceed to make a few remarks upon one which at the first glance seems to be decisive of the whole question; a more minute examination, however, reveals the very lax manner in which it is stated, and the little reliance to be placed upon it. The quotation is—“In another case I passed my hand through the middle of the placenta, and it came away after the birth of the child, with the large opening through which it was delivered.” Now as the mode of formation of the placenta precludes the possibility of the centre being directly situated over the os uteri, and as, moreover, the hand by no artifice or management could be made to pass through the root of the vessels—the middle of the placenta,—I will so far correct this error as to infer that a margin of placenta wider than usual was attached to that spot: but even then there is not space enough between the centre and edge of the placenta to permit the passage of a full-grown child; and the only way possible for it to pass would be by direct laceration through the placental edge. Again, it has never been pretended that more than a limited portion is detached; and it has been a matter of surprise to authorities how small that portion is. The great bulk of the placenta is adherent, natural, and healthy, and would consequently oppose all those resistances to the passage of the child through it, which I have previously insisted upon. While, on the other side, nothing seems to me more natural or more readily explainable than that a portion of the placenta, softened down or not, should give way before the introduction of the hand by doubling up, and thus permit its entry into the amnion membrane. Therefore, without in the slightest degree doubting Mr. Lilley's acumen or powers of observation, he must forgive me if I decline to be guided by such a case or accept it as final, partly because there are two material errors in fact, in as many lines of description, and further, because past observations, made under the influence of preconceived ideas, and having no reference to the point of discussion which I have mooted, are obviously unfitted for being received as conclusive evidence. The observations, as well as the decision, must be present and future.

And now, in conclusion, I cannot refrain from giving utterance to an opinion I have long entertained, which has been materially strengthened by the train of thought I have been led into in the composition of this paper, as well as by what I have seen, heard, and remarked—which is, that the existence of placenta prævia is much rarer than is commonly imagined; that many of the so-vaunted cases are ordinary instances of detachment of placenta; and that consequently, under these circumstances, the practice of turning adopted is meddlesome and mischievous. Let me be understood as speaking generally. I shall probably startle Mr. Lilley still further by the assertion that detached placenta and presenting placenta differ only in degree, the difference being simply the position, one, two, or more inches nearer to or farther from the uterine orifice. That in the latter cases, should the presentation be natural, the pains good and continuous, the placenta diffuent, or even otherwise, nothing then opposing the speedy termination of the labour, the wiser and certainly safer plan

* I have not mentioned Monge or the other French chemists, to avoid prolixity.

† Respiration must be a similar process. It will be found that the venous blood is charged with carbon combined with electric matter, and that this is decomposed in the lungs by the inspiration of oxygen, when combustion takes place, and the liberated carbon then uniting with oxygen, forms the carbonic-acid gas—the matter expired.