it seems scarcely decent that motions carried almost unanimously at a full meeting of the corporation should be overruled after what can at the best have been but a hurried discussion on the part of the Council, seeing the amount of business crowded by it into the work of the afternoon of the 8th inst. The College Council "trust that these questions will now be allowed to rest," for "they deem it best, in the interests of the College, that the discussion on the subjects which have been in dispute should close with the grant of the Supplemental Charter." Truly an impotent conclusion; the subjects are still in dispute, unless the Council agree that the Members have convinced them of the justice of their claims, for the Members have not been silenced by any of the frothy occasional utterances of the Council; the "dispute" cannot close with the grant of the Charter, seeing that it has already been extended beyond that ideal landmark of the Council, and the Council have not yet proved that they have the remotest idea of the best interests of the College, or they would not so lightly have sent up to the Privy Council a draft Charter from which the latter body insisted upon some most important excisions. The Council may think that it has now before it a long period of undisturbed repose, but as yet the storm has but been brewing. We have not only Lord Randolph Churchill and Sir Guyer Hunter on our side in Parliament, but an exceedingly strong body of members belonging to both political parties in the House, and action will immediately be taken to gain at the hands of the representatives of the people that representation which must find its justification and precedent in the very constitution of the House of Commons itself.

I am, Sirs, your obedient servant,

WM. ASHTON ELLIS,
Grosvenor-road, S.W., Nov. 1888. Joint Hon. Sec. Assoc. M.R.C.S.

MENSTRUATION AFTER REMOVAL OF BOTH OVARIES.

To the Editors of THE LANCET.

SIRS,—The point raised by Mr. Thornton, Mr. Meredith, and Mr. Barwell as to the arrest of the menstrual function as a sequence of the removal of both ovaries is open to different interpretations. Mr. Thornton and Mr. Meredith agree that "when both ovaries are entirely removed no recurrence of menstruation could occur." Mr. Barwell relates a case in which he and Mr. Sheild are certain that both ovaries were removed, "not the minutest part being left behind"; yet in this case the subject "has menstruated regularly just as she used to do before the operation."

Mr. Barwell's case does not stand alone. I have seen similar instances; but they do not invalidate the hitherto accepted law that the determining cause of menstruation resides in the ovaries. The occasional recurrence of menstruation after the loss of both ovaries may be explained in various ways. First, after the menstrual function has been fairly started during health, and the influence of periodicity upon the constitution has been established, it is quite in harmony with physiological law that a flow of blood representing menstruation should sometimes return after the removal of the ovaries. Every month we note an exaltation of nervous and vascular tension, which culminates in the menstrual flow and then subsides. Habit tends to keep up this order of events. The dominion of periodicity is one of the most interesting and instructive problems in biology. It must be considered in this connexion. Secondly, it is certain that a periodical discharge, assumed to be menstrual, more or less regular as to time and quantity, is frequent after the ordinary climacteric, when the ovaries have become atrophied and inert. The flushes and nervous phenomena attending it indicate the periodical exaltation developed in the age of ovarian integrity. Thirdly, in some cases a morbid stimulus persists in diseased ovaries for some time after the abrogation of their normal physiological function. Fourthly, in some cases there is a complication with uterine disease, as myoma or hyperplasia, which keeps up a determination of blood to the pelvic vascular system, and so discharges more or less strictly periodical recur for a time even after the removal of both ovaries. But in the majority of cases in which the ovaries are removed with the tubes, the hæmorrhagic discharges, menstrual or pathological, cease. On the other hand, I know of no case in which

menstruation has been established where the ovaries have been congenitally wanting.

We must not then, without more abundant proof than the evidence of an occasional apparent exception, abandon the old law, which declares that the ovaries rule over the function of menstruation. I will not now discuss what share the tubes may claim.

I am, Sirs, yours obediently,

Harley-street, November, 1888.

ROBERT BARNES.

To the Editors of THE LANCET.

SIRS,—I assisted Dr. Aveling in removing both ovaries more than two years ago, and the patient has menstruated regularly since.—I am, Sirs, your obedient servant,

West Cowes, Nov. 12th, 1888.

STEPHEN NOCKOLDS.

MEDICAL APPRENTICESHIP AND PRACTICAL TRAINING.

To the Editors of THE LANCET.

SIRS,—It is now some twelve months since a committee of the Medical Council recommended such a change in medical training as would ensure on the part of the young idea a greater acquaintance with the details of practice as well as more complete clinical knowledge. The committee appears to have been led to this recommendation by the repeated statements made as to the incompetence of at least many of the young practitioners educated under the present system to take sole charge of the sick. These allegations have been made over and over again by practitioners who have had to deal with assistants, and whose words are as reliable as those of any members of the profession. The committee must have felt that the charge rested upon very good evidence when, notwithstanding their personal interest in the present system, they gave to it such practical attention. Then there was the further question of utilising the immense amount of clinical *matériel* now running to waste in the union and other hospitals, and that at a time when even interested visitors were obliged to admit that the recognised *matériel* was in many places too scant. On this point something of the nature of an undertaking was given to the Privy Council that this unused *matériel* would no longer be allowed to run to waste. Most distinctly an expectation, at all events, was held out to that effect by the General Medical Council. Well, the outcome of all this was the following resolution, which was carried by the Council at its meeting of May 26th, 1888:—"That in addition to the requirements at present in force with respect to hospital attendance and duties, all candidates for the final examination be required to produce evidence that they have, under proper supervision, taken part as pupils for six months in the practice of a recognised public dispensary, including the visitation of patients at their own homes, or in the out-patient practice of a recognised hospital, or have acted for six months as pupil to a registered practitioner either holding such a public appointment or having such opportunities of imparting practical knowledge as shall be satisfactory to the examining bodies." This was the outcome of much labour on the part of that august and most expensive body, and I repeat here what I publicly stated before, that it is a perfectly illusory arrangement. It is illusory because by placing the out-patient department athwart the new apprenticeship system it so handicapped the latter as to practically prevent students adopting it. Why should a student become an apprentice when he need only present a certificate of attendance on that out-patient department which he always has been attending? How will the mere presentation of such a certificate supply that practical training which the attendance itself has heretofore failed to supply? Moreover, it is notorious that the conceit of the belectured student is such, and he is so utterly unconscious of his own ignorance, that nothing short of compulsion will make him serve an apprenticeship at the end of his course. I have myself had some experience in teaching having been for six years a private teacher in a medical school, and, with every respect for the opinions on the other side, I venture to express the matured conviction that it is at the commencement and not at the end of the course this apprenticeship should be served. I would make it twelve months; would recognise only those masters who

would furnish proof of the possession of the necessary capacity and means for teaching; and to prevent fraud I would require that each apprenticeship should be entered in the Students' Register from the very date of its commencement.

Here I may be allowed to quote Carlyle: "Properly thou hast no other knowledge but what thou hast got by working; the rest is yet all a hypothesis of knowledge; a thing to be argued of in schools, a thing floating in the clouds, in endless logic-vortices till we try and fix it." The Chelsea philosopher had clearly in his mind's eye the difference between the apprentice and the poor bedazed and bewildered youth whose head is stuffed in the lecture room with that garbage which Carlyle so heartily despised. It is very suggestive that the opponents of early apprenticeship are almost in every instance persons who have a personal interest in opposing it. On the other hand, it is cheering to find a man like Sir James Paget urging home the lesson of early apprenticeship whenever an opportunity is afforded to him. It is well known that the late Dublin scheme offered special inducements, or at least offered a clear stage, for the reintroduction of early apprenticeship; but the moment it was found likely to take, the Dublin grinding interest strangled it. The other day I urged Dr. Kidd to restore it, and, in reply, he informed me that there was not the least chance of doing so, adding the significant words, which every parent and every intending student ought to be made aware of: "*I wish students and their friends could be induced to establish it for themselves.*" I am sadly afraid that there is a percentage of the professional masses to whom the term "professional rabble" would not be inapplicable, for in no other way can I account for the blindness and the apathy which they display in matters vitally affecting themselves. Just look at the treatment the Privy Council the other day extended to the Members of the College of Surgeons. Would they for a moment treat the licensed victuallers, or the trade unionists, or the association of chimney sweeps with like contumely? The fact is, that medical men do not take that part in civic and political work which mere duty of itself requires of them, and hence one great reason why we are so helpless in Parliament. The privileged schools, hospitals, and licensing authorities, I hope, will be made to feel the power of the professional masses, expressed through their Parliamentary representatives, the first time that a general election shall place a democratic Government in office.

I am, Sirs, yours faithfully,
Cashel, Nov. 10th, 1888. THOMAS LAFFAN.

SHAKSPEARE AND HARVEY.

To the Editors of THE LANCET.

SIRS,—I have read with much interest a few of the communications in your columns relative to the immortal Bard antedating Harvey in the discovery of the circulation of the blood, and also the possibility of Shakspeare being possessed of the knowledge of Harvey's theory, either by gaining information direct from Harvey or from the rumours that were abroad concerning it from the time that Harvey made the discovery to the publication thereof.

"What damned error, but some sober brow
Will bless it, and approve it with a text."

Of all claims of Shakspeare's priority, that of his knowledge of the circulation of the blood previously to its discoverer's, while attracting the widest attention, is yet the most assailable. Mr. Hackett, who in 1859 first called attention to and upheld it, shows not only his ignorance of the most important part of Harvey's theory, but his utter lack of knowledge concerning the many other theories that had been presented years before and were accepted at the time Shakspeare wrote by different physicians.

Shakspeare died in 1616. Harvey first published his theory in 1619. Among the older theories were those of Hippocrates, Praxagoras, and Erasistratus, who held that the arteries contained air, and that therefore the *veins* were the *only* blood-holding vessels, and that they had their origin in the liver. Galen, the most celebrated of ancient medical writers, who lived as early as 150 A.D., taught that the left ventricle of the heart was the common origin of all arteries, and that the arteries of living animals contained

blood, *not* air; but he did not advance with his studies so as to learn in what direction the blood flowed, or whether it was movable or stationary. The distinguished Michael Servetus, who was burned with his books by order of Calvin in 1553, taught that the blood flowed from the right ventricle through the pulmonary artery to the lungs, and thence through the pulmonary vein and left auricle into the corresponding ventricle, from which it was conveyed by the aorta to all parts of the body. Sylvius had already pointed out the valves in the veins, and with this fact, together with the discoveries of Galen and Servetus, Harvey had his way well paved for the setting forth of the theory which rendered his name immortal. There is not one thought to be found in Shakspeare in any way relating to the circulation of the blood that is not applicable to the teachings of either Hippocrates, Galen, Servetus, or Sylvius, and it is very evident that he did not tie himself down to any one of these theories, but that sometimes he had in mind the theory of Michael Servetus (to which all the heart allusions will apply), and at other times that of Hippocrates, which accounts for all the thoughts regarding the liver as the propeller of the blood through the veins. Harvey was the first to show the true idea of the circulation, his chief points being that the left ventricle of the heart, acting as a muscular sac, forced the blood into the arteries; that it went to the extreme parts of the body and returned by way of the veins to the right auricle, thence to the right ventricle, from which it was squeezed into the lungs, and thence to the left auricle and corresponding ventricle, the same blood making a complete circuit, and ready again to be sent out on its life-sustaining journey; that the pulse is produced by the arteries being filled with blood and so enlarging, and that it coincides with the contraction, not the dilatation, of the heart. It is easy to see that many of Shakspeare's thoughts on the circulation might be made to apply to Harvey's theory, and he who is ignorant of the discoveries of the physiologists preceding Harvey might be led into the "damned error" which (to him) would rob the discoverer of his deserved honour.

I am, Sirs, yours faithfully,
Easton, Pa., U.S.A., Oct. 30th, 1888. B. RUSH FIELD, M.D.

THE CHEMICAL INCOMPATIBILITY OF ANTISEPTIC AGENTS.

To the Editors of THE LANCET.

SIRS,—In a communication under the above heading to a contemporary,¹ I pointed out that, granted the advisability of employing antiseptic agents in solutions of definite strength, the possibility of reducing the strength of the solution, or of altering its nature through the chemical incompatibility of the materials employed, is one of considerable moment. Taking corrosive sublimate, carbolic acid, iodine, salicylic acid, and Condy as examples, I showed that many instances occurred in which, when brought into contact one with another or with certain lubricants, chemical action took place. Carbolic acid and iodine afford one of these instances.

Dr. Percy Boulton, in THE LANCET of Nov. 3rd, speaks from clinical experience in defence of this combination. I do not pretend to say that the substance formed, whatever it may be, *is* antiseptically inert. It may be, as Dr. Boulton suggests, a colourless salt, which possesses antiseptic properties even more powerful than the iodine and phenol from which it is produced. But if so, let the substance be isolated, and let us know something of its chemical and physical as well as of its antiseptic properties. I cannot, however, accept Dr. Boulton's clinical experience, extensive though it is, as conclusive evidence in favour of its antiseptic power; and for this reason: *the substance, as employed by him, is not in an isolated condition.* In my paper I pointed out that an exceedingly small admixture with phenol is sufficient to fix the whole of the free iodine. Dr. Boulton's prepared solution, though it *contains no free iodine*, possesses undoubted antiseptic properties, for it *does contain uncombined carbolic acid*, a very small proportion of which is altered. Upon these two points direct evidence may be obtained. The disappearance of the iodine colour is in itself a proof that no free iodine remains. Inability to produce the blue iodide of starch by the addition of starch paste, and failure to impart a violet

¹ Brit. Med. Jour., April 28th, 1888.