higher operative surgery and bacteriology. As in the army, medical officers who qualify in these subjects ought to receive extra pay.

The Director-General, in his recent Edinburgh speech, said the naval medical officers thirty-five years ago "were not up to the mark." Many excellent young surgeons entered then, and if some failed to keep abreast of medical and surgical advance and became incompetent it was the fault of the service, which did not insist on, nor afford opportunities for, professional study. The Director-General asked young Scottish medical men to come into the service for honour and glory. Alas! in the piping times of peace there is not much of either, of necessity, for the medical branch. Entry for continuous service therefore resolves itself into a matter of sordid bawbees to a very great extent, I fear.

I have nothing to say against the service. I have had some of the bitters and some of the sweets, and, best of all, I have my modest pension. I have recommended several young men to enter, who are now serving. They have not cursed me so far, but they are still very young, I admit.—I am, etc.,

March 15th.

FLEET SURGEON (RET.).

PORTRAITS OF BURNS.

SIR,—Dr. John Findlay's courteous letter¹ gives me an opportunity of laying before your readers certain points of evidence relating to the authenticity of the Raeburn portrait of Burns which I had to omit from my lecture. It is quite true that in 1889, when Mr. Barrington Nash came to the conclusion that the portrait reproduced in my article² was a portrait of Burns by Raeburn, there was no record to show that the poet and the painter had ever met. It was known that they were in Edinburgh at the same time. Near the end of 1889 the following letter from Raeburn was discovered:

York Place, Edinburgh,
December 1st, 1803.

Gentlemen,—I enclose you a receipt for a case containing Burns' portrait, and I have no doubt you will get it soon and safe, and I flatter myself with the hope of its meeting with your approbation, than which, I assure you, nothing will give me more pleasure.

I have twenty guineas for a portrait the size of Burns's. I do not wish you to remit the money to me, for, as I have money to pay in London, I shall, after receiving your permission, draw upon you for the amount.

I am, with much respect, gentlemen,
Your most obedient servant,
HENRY RAE BURN.

That letter was found amongst the papers of Messrs. Cadell and Davies, who published the first London edition (1787) of Burns's poems, and in 1823 brought out an edition "elegantly printed, in 4 vols. 8°, with a portrait of the Author from an original portrait by Raeburn." It will be observed that, when Raeburn forwarded the portrait of Burns to Cadell and Davies (1803) the poet had been dead for seven years (1796). One cannot suppose that Raeburn would have supplied a portrait which was merely a copy of the Nasmyth. The small portrait which Raeburn did supply is now in the possession of Mr. Barrington Nash as well as the large one, which Mr. Nash permitted me to reproduce in my lecture. It is the small Cadell and Davies portrait which, to my mind, supplies the convincing evidence that the large portrait, which, Mr. Nash maintains, was painted by Raeburn and does represent a portrait of Burns, is indeed a true portrait of the poet. I admit that a comparison of the Nasmyth with the large Raeburn portrait does leave one in doubt, but when I placed the small Raeburn portrait between the two all my doubts disappeared. A strict comparison of the cranial cast—it is a misfortune that the lower part of the face is missing from that cast—with the Raeburn portrait convinces me that it does reproduce faithfully the essential anatomical features of the cast. I do not think it would be impossible in modern portraiture to find the same man represented with just as great divergence as in the Raeburn and Nasmyth renderings of Burns. The Nasmyth portrait represents a handsome, characterless young man about town; the Raeburn a real living, "thundering" man. If ever Raeburn's papers should come to light I feel with Mr. Nash

that the last doubt will be removed as regards the authenticity of the large Raeburn portrait.—I am, etc.,

London, W.C., March 15th.

ARTHUR KEITH.

THE NATURE OF PREGNANCY.

SIR,—I think that I can best reply to Dr. Young by quoting a sentence from Dr. Ballantyne's most interesting paper:

There is no denying the fact that the existence of these reactions in the blood of the pregnant woman, if it can be proved to the satisfaction of the bacteriologist and the clinician, does support the view that gestation has an effect upon the mother resembling that of an infectious fever and even of malignant disease.

Here we have expressed a reasonable caution as to the admission of immunity reactions as proved factors in pregnancy. No one can quarrel with this view. At the same time, when Dr. Ballantyne wrote this sentence he was obviously convinced that, if they be once admitted, they must figure very largely in our general conception of the relationships of the mother and ovum, as largely, in fact, as they have for long in infective disease and more recently in malignancy. I would have been loth to intervene with my original letter had I not—in imagination it may be—seen evidence in another part of the paper of a certain hesitancy in fully accepting the view so clearly laid down in the sentence I have quoted.

Dr. Young, with a plea for accuracy in scientific thinking, is pleased to write of the antigens of pregnancy as of trifling importance and as being immediately neutralized. As a worker for years in immunology, not alone in pregnancy but in disease generally, I simply do not know what he means, and I must clearly point out to him how greatly he differs from the considered opinion of Dr. Ballantyne. So long as this chasm separates us I see little good in continuing the discussion.

Dr. Young seems to imagine that, finding myself in a difficult position, I have attempted to extricate myself by "supposing" this or that. I must remind him that, in regard to the sensitization of both mother and fetus, I am not merely theorizing but stating results which I have obtained experimentally. Possibly my results may not be confirmed later by other workers, or even by myself; but, so far as they go, surely an actual result merits some other term than an "ingenious supposition." Further, I fail to see why my view of the placental link should be considered inadequate merely because, as we all know, that organ, among its other attributes, happens to be a diffusion membrane.

I drew a rough parallel between the engorgement of pregnancy and that of bacterial infection in an attempt to show that flushing of the tissues need not indicate "willingness" on the part of the organism. Dr. Young considers that the two processes differ in totality, and refers to the ultimate strangulation of invading organisms. He will not agree with me, I suppose (and, so far, I certainly cannot prove it to him), if I suggest that the invading trophoblast is similarly strangled in the course of pregnancy. I merely mention this to him to show that the evidence of "willingness" on the part of the mother is capable of an explanation other than that given by him.

Finally, I am content to leave the matter practically where Dr. Ballantyne (to whom all thanks are due for his very illuminating paper) puts it in the sentence quoted. The first necessity is to prove to the general satisfaction that immunity reactions do occur in pregnancy.—I am, etc.,

Liverpool, March 17th.

H. LEITH MURRAY.

THE PATHOLOGY OF CANCER.

SIR,—Dr. Crow, in his letter in the JOURNAL of March 14th, presumes too much. He must not infer that a regulator of cell growth is necessarily an inhibitor. It co-ordinates the various activities of the cell, and, like Justice with the scales, keeps the balance true. His statement that cancer is uncommon in very old people is rather sweeping; I should be inclined to say that these persons are old because their ductless glands have remained wonderfully efficient, and that, therefore, they do not develop cancer. All tumours, including cancer, become much more common as age advances, so that in old people they can often be counted by the score (Mansell Moullin).

¹ BRITISH MEDICAL JOURNAL, March 14th, p. 624.

² Ibid., February 23th, p. 461.

Dr. Crow must acknowledge that the ductless glands are relatively much larger in the young, where metabolism and growth are very active; anabolism is in excess of katabolism, but the secretions of the glands are in sufficient abundance to limit the lines along which multiplication of the cells takes place, as determined by long years of evolution.

During middle life, the metabolic balance is about even; the glands have diminished in size, as the regulating secretion is not so much required. But after the meridian of life is passed the metabolic balance is again disturbed; katabolism is now in excess of anabolism, and the amount of growth regulator is rapidly becoming less. The critical period is now approaching. Whereas normally the millions of cells which comprise the organism have developed, lived together, and now decay in unison, in cancer, cells which have for a long time been irritated, become mutinous, and decline to decay; on the contrary, they live and flourish at the expense of their neighbours, and in this sense are parasitic. Why is this? Simply because constant irritation connotes constant death and continual replacement, so that the cells at that part are particularly young and robust. Moreover, their functional activities are no longer kept in check by the secretions of the ductless glands, and they become insane. To put it in another way, the irritated cells are differentiated and unlike other parts, and therefore become a centre of unlike reactions to incident forces. I quote Sir James Goodhart:

Are we wrong to expect that, if a complex body is possessed of energy or many centres of energy for orderly growth and development, this disposition or force will not now and then slip its leash and run off on its own account? An analogy of this kind seems to me to convey a workable conception of a scheme of malignancy, where ordered growth diverges by successive steps of variation, of indulgence in function, until cancer appears, until malignancy becomes the insanity, shall I say, of function.

I hold that the "force" for orderly growth and development is the chemical action of the secretions of the ductless glands. I, of course, agree that there are difficulties in regard to the hormone theory. It is quite impossible to say, for instance, whether the failure of supply of the regulating secretion is or is not due to an all-round and equal degeneration of the glands; and, again, while we may admit that the total functional result of these secretions is perfect metabolism, which means perfect and even growth, there is an intermixture of effects; we do not know how much is due to each gland separately.—I am, etc.,

Wigan, March 16th.

J. THOMSON SHIRLAW.

THE FALLING BIRTH-RATE.

SIR,—I notice that one of your correspondents on this subject remarks that "in Paris the deaths from tuberculosis and typhoid fever are, as a rule, two or three times more numerous in relation to population than those in Berlin and London."

No doubt your correspondent is correct in matter of fact, but the implication that the mortality from typhoid fever is heavy is totally mistaken. On reference to the health and mortality statistics issued weekly by the municipality of Paris, I find that typhoid fever caused last week four deaths, as against 3 in the previous week, the average number being 5; 43 new cases were reported, as against 31 the previous week and 36 the average of the season. In a population of some 2½ millions, this gives us a mortality of less than 1 in 500,000 and a disease-incidence of about 1 in 75,000—a fairly safe risk.

In London's population of 4½ millions I find, for the month of January, 1914, 85 notified cases of enteric fever with 16 deaths, or a weekly average of about 21 cases and 4 deaths.

Of course typhoid fever is a preventable disease, and should be absent from our statistics, but in these days of unlimited expenditure for military and naval purposes and very limited national expenditure on urban sanitation, we must be thankful for small mercies. It is, at least, obvious that typhoid fever is of small account, as a causal factor, in a decreasing or stationary population.

With regard to tuberculosis, I fear that your correspondent is nearer the mark, for in Paris the mortality from that disease totals well over 200 weekly.—I am, etc.,

Paris, March 15th.

A. A. WARDEN.

¹ BRITISH MEDICAL JOURNAL, March 14th, 1914, p. 625.

SIR,—Mr. Henry Sewill says I suggested "that there exists some causal relationship between a falling birth-rate and a rising death-rate in France." Yet here is what I wrote: "Even in France the death-rate declines with the birth-rate; moreover, her recently published records for the first six months of 1913 show that there were 8,000 more births than during the same period of the previous year, but that there were 12,000 more deaths; in other words, that a rise of her birth-rate was accompanied by a still greater rise of her death-rate."

It does not surprise me, therefore, that the rest of Mr. Sewill's letter is very much a repetition, with which Malthusians are familiar, of the *répopulateur* misunderstanding of the population question. For example, he says that "the population of France during the past thirty years would have shown a decrease had it not been for immigration." Page 135 of the Registrar-General's seventy-third annual report happens to give France's population, births and deaths for every year from 1881 to 1910. The population in 1881 was 37,590,000, in 1910 it was 39,339,000—an actual increase of 1,749,000. The columns of births when added up make 25,687,627, and the deaths 24,317,527—a natural increase of 1,370,100. Therefore the immigration in these thirty years was 1,749,000 minus 1,370,100, or only 378,900 of the 1,749,000 increase.

As Mr. Sewill points out, "The French death-rate has long been, and still remains, much higher than that of any of her northern neighbours." Fortunately, like theirs, it still falls with every decline of the birth-rate. To make it fall faster than the birth-rate, and thereby to quicken her rate of natural increase, France must either increase her food-producing and food-buying powers more quickly or she must imitate Holland and specially reduce the birth-rate among the poor and unfit. She does not seem to have the huge supplies of coal and iron which have enabled her northern neighbours to multiply so rapidly.

Being diffident about taking up more of your valuable space, I beg you will allow me to repeat my offer to give to any doctor interested a copy of *The Small Family System*, recently published, which discusses this subject. I have already replied to a large number of applications for the book and received many expressions of hearty agreement with it. I was as ignorant as the majority of my colleagues are of Malthus's supremely important population doctrine, and of neo-Malthusianism, until my independent essay on over-population happily led me to hear of the Malthusian League. I now realize that an excessive death-rate (that is, over 10 per 1,000 per annum) is due to people begetting more children than they can afford to maintain; and that if the birth-rate of this and other countries were now to become stationary, nothing that medical science could do—not even by discovering a cure for cancer—would prevent the death-rate from becoming stationary. I would like to add that Dr. Drysdale is to read a paper on "The Empire and the Birth-rate," and show his original population diagrams, on Tuesday, March 24th, at 4 p.m., before the Royal Colonial Institute.—I am, etc.,

24, Alexandra Court, S.W.
March 14th.

BINNIE DUNLOP, M.B., Ch.B.

SALARIES OF RESIDENT MEDICAL OFFICERS.

SIR,—I desire very heartily to endorse the sentiments expressed by "R.M.O." in the JOURNAL of March 14th, and more particularly those relating to the medical superintendents of sanatoriums. It is an extraordinary fact that the salaries of resident medical officers and medical superintendents of sanatoriums are, as a rule, much smaller than those of tuberculosis officers.

Dr. Halliday Sutherland stated recently that to call a building a sanatorium did not make it one, and this is only too true. In any sanatorium, properly so-called, there must be constant methodical medical supervision or else the results obtained will be disappointing or even disastrous. A certain tuberculosis officer who, I may say, acknowledges the efficacy of sanatorium treatment, informed me recently that many of the phthisical patients sent from his dispensary to a neighbouring sanatorium returned in no way benefited. A number, indeed, were rather the worse of the treatment received, or perhaps one should say became worse as a result of the absence of treatment. In the sanatorium referred to, the medical