SIR,—“Peace hath her victories no less renowned than war” could hardly be better illustrated than by the abbreviated *Journal* of the last fortnight, upon which, Sir, the profession must most warmly congratulate you.

May I plead that some of this excellent editorial précis writing is continued? Without it how many of us would have learnt that “outfitters won’t give long trousers to son aged 7 without doctor’s certificate”; “certificates must be renewed that leg still off each time applying for extra soap allowed”; or the story of the night porters and the hansom cabs. *Floreat brevior B.M.J.*!—I am, etc.,

London, W.1.

HENRY WILSON.

SIR,—I feel I must write and congratulate you on your two fine efforts. You and your staff deserve the highest praise for a grand show. If our rulers had half the guts you people have our country would not be in its present condition.—I am, etc.,

Hemel Hempstead.

E. S. PHILLIPS.

SIR,—While not particularly anxious to dim the glow of complacency which, I presume, appears on the faces of all at B.M.A. House, I must protest against your efforts in producing the *B.M.J.* during the fuel crisis.

In my opinion only two courses were open to you: either (a) fall in line with good grace like the other periodicals, or (b) defy the “ban” and produce the *Journal* as usual, since no legal proceedings could have been taken against you. Sixty-two thousand sheets of paper and sixty-two thousand envelopes—what a waste of paper and effort!

The first issue I scanned with startled amazement; the second I put at the back of the fire unread. Am I alone in my feelings in this matter?—I am, etc.,

Nelson, Lancs.

A. J. P. DALY.

Statistics: Retrodisplacement and Fertility

SIR,—The burdens of the research worker in medicine have been added to in recent years by the vociferous advent of criticism from the statisticians. Conscious of the immense amount of labour that has gone into the preparation of even a short paper he is apt to resent what he considers are the facile criticisms of the man who wields a slide-rule in an arm-chair. It is indeed much to be regretted that so often the statistician’s summing-up ends in the words: “Your data are insufficient on which to base any conclusion.” Of course such a picture of the statistician as a sort of kill-joy is extremely one-sided. On the contrary, statisticians have repeatedly pointed out that ignorance of statistical method may lead to a conclusion *not* being drawn, that significant answers to questions may be missed.

In the hope that this preliminary apology will avert wrath, I would like to discuss the article by Mr. H. H. Fouracre Barns, “Retrodisplaced Gravid Uterus” (Feb. 1, p. 169). It is not my intention to deal with the logical errors in his conclusions, for they do not make much difference in the end, and in any case they are no worse than is to be found in many other articles. His general conclusion, that his figures do not disprove the hypothesis that retrodisplacement (without incarceration) does not significantly predispose towards abortion (I hope he will accept my re-wording), is quite correct. It would have been easier for his readers to see if he had arranged his figures (p. 170, col. 1) in a table:

TABLE I.—Incidence of Abortion up to Twelfth Week of Pregnancy

	Aborted	Not Aborted	Total
Retrodisplaced uterus	2	25	27
Not displaced	54	393	447
Total	56	418	474

It is scarcely necessary to do a χ^2 test to demonstrate that there is no significant difference in the number of abortions in the two types of uterine position.

Hidden in the mass of figures in his article are some which are of profound significance. Mr. Barns quotes the figures of Polak, 1926, who found “18% of congenital retroversions in nulliparous women and 35% in post-partum women.” Mr. Barns found, at University College Hospital, the following

figures: 381 primigravidae, of whom 21 had a retrodisplaced uterus, and 93 multiparae, of whom 6 had malposition. If we take a comparable number from Polak’s figures (the conclusion is even stronger if we take his total number) we can arrange them as follows:

TABLE II.—Incidence of Retrodisplacement of the Uterus in Nulliparous Women

	Displaced	Not Displaced	Total
U.C.H.: pregnant	21	360	381
Polak: not pregnant	69	312	381
Total	90	672	762

$$\chi^2 = \frac{(69 \times 360 - 21 \times 312)^2 \times 762}{90 \times 672 \times 381 \times 381} = 29.1. \quad P = \text{less than } 0.001$$

The test means that if there were no difference in the incidence of pregnancy between the women with displaced uterus and with normal uterus, the chances of getting such figures are less than one in a thousand. The discrepancy is due to the small number of pregnant women with displaced uterus. The figures clearly disprove the hypothesis that displacement has no deleterious effect on the incidence of pregnancy, and they disprove it as conclusively as could be desired. I would hasten to add that this does not invalidate Mr. Barns’s statement: “that any woman complaining of sterility in whom the only abnormality to be found is a retrodisplaced uterus should be investigated and treated along the same lines as would be a woman with an anteverted uterus.” Retrodisplacement is only one factor in the causation of infertility, and careful investigation is the common-sense procedure of the careful surgeon who does not rush to operate without sufficient reason.

A measure of the association between retrodisplacement and infertility may be obtained from the above figures. Since the variables fall each into two definite categories, the best coefficient would be the ϕ coefficient, which is the correlation for point distributions. This comes to just below 0.2, which explains why, although the association has been observed, there has been much uncertainty over it.

A similar table could be constructed comparing the U.C.H. multiparae with Polak’s post-partum women, but on turning to the original statement of Polak I find that he declares that post-partum retroversion is not a fixed condition but may return to normal in the course of several months. The two sets of figures (U.C.H. and Polak) are therefore not comparable, and conclusions drawn from them may be fallacious. This point reinforces the oft-repeated statements of my teachers: statistical techniques are only the numerical form of the logic of scientific method; the right time to consider statistics is before an investigation is begun, not when it is over.—I am, etc.,

London, W.C.1.

M. HAMILTON.

Intestinal Myiasis

SIR,—Dr. Alan M. Easton has been kind enough to send me an article with the above headline published in this *Journal* by Dr. D. S. Sharpe on Jan. 11 (p. 54). As my name is mentioned in that report it may easily be inferred that I concur with the views expressed in it. I am therefore compelled shortly to discuss the matter in this *Journal*. When returning the larvae and stating their scientific name as *Ptinus tectus*, Boield., I added: “It is hardly possible that the specimens should have passed the stomach of the patient, and I suggest that they have got into a vessel used for receiving or transmitting the excrements.” Unfortunately the author did not mention this warning in his report, and it appears therefore desirable to elaborate my statement in the following lines.

The evidence for an actual passing of the larvae in the stool of the patient does not hold good to scientific scrutiny. The account does not mention in what type of vessel the stool was examined by the patient nor where this vessel was kept before use. It has obviously not been a water-filled lavatory basin, and there is a great possibility of the larvae having fallen into the vessel from some infested material, such as a mattress, cushion, blanket, or the like. As the larvae are of a more or less creamy colour they would to all intents and purposes be invisible to a layman if lying in the usual type of vessel.

On the other hand there are very serious reasons for disbelieving any possibility of these creatures having passed the intestine. The chemical objections have to some extent been considered by Dr. Sharpe, and he himself discounts the possibility of the soft-skinned larva resisting the protein-digesting secretions of the stomach. He therefore assumes that the eggs with their "resistant chitinous coating" can pass the stomach unharmed and that the larva can hatch from the egg in the more alkaline portion of the bowel. In actual fact the egg membrane of the *ptinids* is very fine and thin, and in the absence of experiments does not justify the assumption of its being much more impervious to pepsin and hydrochloric acid than the chitinous integument of the larva. In order to obtain conclusive evidence on this subject I have treated eggs, newly hatched larvae, and almost full-grown larvae of *Ptinus tectus*, which were kindly placed at my disposal by the Director and Mr. R. Howe of the Pest Infestation Laboratory, Department of Scientific and Industrial Research, with artificial gastric juice. The specimens were first admixed to chewed bread, and the experiment was kept at 36-38° C. for three hours. The material was then washed in distilled water and scrutinized under a dissecting microscope. Full-grown and newly hatched larvae were dead but not disintegrated, and larvae more or less ready for hatching had been killed inside the eggs. There is no evidence so far (after ten days) that any of the less developed eggs have hatched. The fact that young larvae have definitely been killed inside the intact egg-shell seems to prove at any rate that the egg-shell does not afford a considerable protection against the influences prevailing in the human intestine.

Dr. Sharpe has also stressed the mechanical difficulties for the larvae of remaining in the intestine for a period of a week or more in view of the action of the peristalsis. According to him the larva holds itself in position by its claws and jaws. In actual fact the rather thin claws would probably be of little avail, while the jaws would be wanted for the ingestion of the food, so that they could not possibly keep their grip. On the other hand this point is of minor consequence, as an insect larva of this type developing in the intestine would find a nook at the base of the villi or somewhere else where it would be relatively safe, and it would soon create a small excavation round its body in which it could rest securely.

Ptinus tectus lives in dried animal or vegetable matter, such as dried crayfish (e.g., of collections or of food for aquarium fish) and casein (in which latter it is a regular pest) on the one hand, and in rye, maize, cayenne pepper, cocoa, ship's biscuit on the other hand. It is therefore very well adapted to a life in a medium containing between 5 and 15% humidity. The larvae would, however, be quite unsuited to a habitat in a stiff semi-liquid material with a moisture content of more than 90%, as is found in the human intestine, and they would very quickly be clogged up and asphyxiated. While experimenting with artificial gastric juice I kept another batch of eggs and newly hatched and almost full-grown larvae in chewed bread in a moderate quantity of saliva without gastric juice for 31 hours at about 20° C. This treatment, too, killed the larvae, but it is too early yet to decide whether all the eggs have been killed in this experiment. Mr. Howe has informed me of some tests with much lower degrees of humidity in hygroscopic foods with an estimated water content of up to 40%. As only a small percentage survived in these materials it is obvious that a further increase of the water-content would soon prove lethal, as shown in my own experiment.

The gravest reason, however, for scepticism is afforded by the question of temperature. It is known from experiments with eggs of the flour-moth (*Ephestia kuehniella*, Zell.), an insect which lives under very similar ecological conditions, that they cannot hatch but will perish at a prolonged exposure to a temperature of 34.4° C. or more. Even in a moist cell it has not been possible to raise this temperature (Hase, A., 1927, *Arb. biol. Reichsanst. Land u. Forst-wirtsch.*, 15, 116). As to *Ptinus tectus* itself Mr. R. Howe informs me from his experience, gained at the Pest Infestation Laboratory, that only large larvae survive for long at 28° C. or above, and that only few first-stage larvae reach the second stage at all at as moderate a temperature as 29° C. The most conclusive point against Dr. Sharpe's view, however, is the fact that, according to Mr. Howe's experiments, eggs do not hatch above 28° C., so they would hardly remain alive for a day and certainly never hatch at the temperatures prevailing in the human intestine.

Lastly a point of terminology should not pass without comment. The term myiasis, derived from *μύια*, a fly, designates the "infestation of any part of the body of living animals by the larvae of diptera," or two-winged flies (Smart, J., 1943, *A Handbook for the Identification of Insects of Medical Importance*, p. 810, London: British Museum). A similar infestation by beetles or their larvae is called cantharidiasis. Entomologists have for some time been aware of the fact that many reports of myiasis and similar ailments collapse on closer examination, and modern textbooks like that of Smart duly stress the necessity

for a thoroughly critical examination of each reported case. My personal impression is that in temperate European countries the larva of the lesser house-fly is most likely to produce genuine cases of myiasis, and that certain tyroglyphid mites can probably produce similar and sometimes rather serious symptoms.

I would like to express my thanks to the Director and Mr. R. Howe, Pest Infestation Laboratory, Department of Scientific and Industrial Research, Slough, Bucks., for breeding material of *Ptinus tectus* and for the information cited above; to Mr. N. D. Riley, Keeper of the Department of Entomology, British Museum (Natural History), and Mr. F. Barnett of the same Department for their, unfortunately unsuccessful, efforts at obtaining a quantity of pepsin for me; and, lastly, to the John Innes Horticultural Institution for providing me with this substance.

—I am, etc.,

Imperial Institute of Entomology.

F. I. VAN EMDEN.

SIR,—I was most interested to read Dr. D. S. Sharpe's account of a case of intestinal myiasis caused by the larva of *Ptinus tectus* (Jan. 11, p. 54). I am, however, startled by his implication that the larva of *Ptinus tectus* is insectivorous, and I should be glad to know which insects normally form the food for this larva.

I feel also that Dr. Sharpe may be adding to the anxieties of parents by his lurid threat of intestinal myiasis to those children who eat fallen fruits. The only dipterous families which are likely to infest fruits attractive to even the most omnivorous (or insectivorous?) child are the Trypetidae or the Drosophilidae, and, so far as I am aware, no member of these families has so far been incriminated with causing myiasis. The adults of the Muscidae and the Calliphoridae—the myiasis-causing species *par excellence*—certainly feed on fermenting fruit, but I think they prefer a rather more decayed pabulum for breeding purposes.—I am, etc.,

Lagos, Nigeria,

P. J. L. ROCHE.

Control of Measles

SIR,—After a depressing week-end of Arctic weather, snow shifting, and fuel crises (national and domestic) it was a welcome relief to open the *British Medical Journal* and read the three leading articles (Feb. 8, p. 225). The first of these ably reviewed the present position with regard to the control of measles, a very topical subject; the second set out clearly the reasons why a severe outbreak of influenza is unlikely this winter; while the third had the significant title "The Minister Accepts."

I found myself in general agreement with the opinions expressed in the article on the control of measles, but would like to make one or two comments with regard to the availability and use of convalescent measles serum, normal adult serum, and gamma-globulin. The last is at present unobtainable, while convalescent measles serum is in my experience a very rare product indeed; but adult normal serum could and should be readily available for the use of any doctor requiring it.

From the beginning of this year Leeds doctors have been able to obtain adult measles serum through the Public Health Department. The supply is obtained by arrangement with the Regional Blood Transfusion Service, who kindly carry out the necessary bottling and undertake the essential tests. The few weeks that have elapsed since the inauguration of this scheme have already proved its value, and in addition to numerous inquiries from doctors within the city there have been several requests for serum from areas outside the city and two from as far afield as Newcastle, so that it would appear that as far as the north-east of England is concerned the facilities for obtaining this product either do not exist or are not sufficiently well known. In this latter respect the statement in the leading article that normal adult serum is available from most of the constituent laboratories of the Public Health Laboratory Service was news to me, and I am sure it would be helpful if further publicity could be given to this source of supply.

I have found that some confusion exists as to the use of adult serum for attenuation. In one or two instances the serum has been given to the case rather than to the contact. Claims have been made as to the value of convalescent and adult serum in the treatment of a case of measles, but these have not been substantiated. When using adult serum for prevention the usual dose recommended is 10 ml., and this should be given to the