can be excluded is false. This must be emphasized because patients can have all the signs and symptoms of brucellosis without any demonstrable antibodies. The following case history illustrates the truth of this statement.

In May 1967 a 37-year-old man stayed on a farm during an abortion storm and drank raw infected milk every day. On 8 July 1967 he was thought to have "flu" and was in bed for a fortnight with a very severe headache, general aches and pains, and sweating which was so severe that he gave up wearing pyjamas. He was treated for a viral infection and was given tetracycline for three weeks. He felt fit until 29 July, when his symptoms returned. On 1 August brucellosis was diagnosed and he was admitted to hospital on 5 August.

Blood culture taken on 8 August grew *Brucella abortus* biotype 1; this was reported on 27 September and he began treatment with tetracycline and streptomycin for a month with a good response. At the end of October 1967 he had a relapse. He had a further relapse lasting 14 days in May 1968, when his spleen was found to be palpable. On 1 July he was given an intradermal injection of brucellin. The next day he felt a little unwell, and on 3 July the brucellin test when read was completely negative.

He had two further relapses, in November 1969 and August 1970, and was treated each time with tetracycline. When seen on 1 March 1973 he had been complaining of severe mental depression and feeling tired. This commenced a week after an influenza vaccine injection in December. He thought he had "flu" and could not understand the symptoms because of his recent vaccination. Since then he has greatly improved, and when last seen was feeling tired but much better.

Over a period of years his blood was examined and the serum agglutination, antihuman globulin (Coombs), and complement fixation tests carried out. These were done on seven occasions between 1969 and 1973 with negative results. Three sera were examined using an antigen prepared from the organism isolated from the patient's blood, also with negative results. In blood taken on 21 May 1968 the serum IgM level was 104 mg/100 ml (normal range 50-170).

In the past I have received numerous specimens from patients complaining of symptoms suggestive of brucellosis who were in close contact with infected cattle. The question must arise, when it is impossible to demonstrate antibdies to brucella and no other diagnosis has been made, as to whether these patients are suffering from brucellosis, particularly in the light of the case quoted. Unfortunately blood cultures yielding *Br. abortus* are rare in brucellosis except in the acute case; hence the difficulty in establishing the diagnosis of brucellosis in a patient who does not produce antibodies.

One further point in the leading article that I would like to take up is the statement that the acute disease usually dies out in the patient within a year of infection. I wonder how many people would agree with this statement? Certainly it is no reason to allow a case of acute brucellosis to go untreated, and such an action or lack of action would amount to negligence. The acute case generally readily responds to treatment, while in the chronic case treatment is extremely difficult and very disappointing. No one who has made the diagnosis of brucellosis in the acute stage should neglect to give adequate antibiotic therapy for at least six weeks.—I am, etc.,

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Coping with Minor Casualties

SIR,—Before more words are wasted would someone please define "minor casualty" for me? Mr. D. Lamont (23 March, p. 573) appears to believe that the ambulant state of the patient is the criterion.

The "young man" he advocates for the "lowly function" of "eliminator" will fail to diagnose the ruptured metacarpophalangeal ligament or the severed tendon or nerve concealed beneath the most trivial of cuts if he is denied the right to perform a full, unhurried examination of the injured part. He would also need more than an "apartment" to provide tetanus prophylaxis, simple dressings, and "etc [?]." Heaven forbid that he should even think about removing foreign bodies from eyes in inadequate surroundings and without proper examination.

It is not the ambulant patients who block the casualty officer's time; it is the ubiquitous "collapse". These patients are all brought by ambulance in response to 999 calls and all require full examination. But most of these are cases of social problems, long-standing abdominal pains, minor cerebrovascular accidents, faints, drunks, hysterics, and various psychosomatic disorders, all of which require a great deal of time to sort out and which, I feel, could be dealt with far more effectively and efficiently by the G.P.

Surely if an eliminator is required in a casualty department, he should be the most experienced doctor available and not "the most junior medical member of the staff".—I am, etc.,

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SIR,—When one reads the correspondence on "Coping with Minor Casualties," and especially about the poor fellow who had his Sunday lunch disturbed, one wonders what on earth the present-day doctor is in practice for.—I am, etc.,

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Treatment of Meningococcal Carriers

SIR,—With reference to the paper by Dr. D. M. Easton and others (16 March, p. 507), I feel that I should point out that the statement that tetracyclines are not effective in the treatment of nasopharyngeal meningococcal carrier states is incorrect for the most recent of the tetracycline antibiotics—namely, minocycline (7-dimethylamino-6-deoxy-6-demethyl tetracycline).

It has been shown[1-6] that minocycline 100 mg twice daily for a period of probably not less than five days significantly reduces the number of nasopharyngeal carriers of meningococci. In the studies completed to date there has been no evidence of resistance of *Neisseria meningitidis* to minocycline.—I am, etc.,

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1 Devine, L. F., *et al.*, *American Journal of Epidemiology*, 1973, **97**, 394.
2 Devine, L. F., Hannah, J. M., Hogerman, C. R., Rhodes, L., and Peckinpaugh, R. O., *Military Medicine*, 1973, **138**, 20.
3 Guttler, R. B., and Beaty, H. N., *Antimicrobial Agents and Chemotherapy*, 1972, **1**, 397.
4 Guttler, R. B., Counts, G. W., Avent, C. K., and Beaty, H. N., *Journal of Infectious Diseases*, 1971, **124**, 199.
5 Devine, L. F., *et al.*, *American Journal of Epidemiology*, 1971, **93**, 337.
6 Ito, S., *et al.*, *Medical Consultation and New Remedies*, 1972, **9**, 127.

Effects of Oral Contraceptives on Endogenous Hormone Secretion

SIR,—We were interested in the comments of Drs. H. S. Jacobs and Anne M. Jequier (23 February, p. 328) concerning our paper on the effects on endogenous hormone secretion of a combined low-oestrogen contraceptive containing mestranol (5 January, p. 11).

Though we agree that the urinary assay for luteinizing hormone is not specific for biologically active hormone, nevertheless it does detect the presence of LH fragments, whether desialylated or not, which have been derived from pituitary LH. We therefore consider that this is a useful measure of pituitary activity which we have used as a means of comparing cycles with and without the exhibition of a low-dose oestrogen oral contraceptive. Furthermore, as we indicate in our paper, urinary LH levels measured by precisely the same technique have been previously used by one of us in the assessment of the effects of different contraceptive formulations. In these previously reported studies[1] we found a more significant inhibition of LH with the higher-dose norethisterone-containing preparations which also contained ethinyl oestradiol. It is probable therefore that these differences are primarily related to an effect of mestranol or perhaps the lower dose of norethisterone.

The other more significant point we wish to make concerns the high output of oestrogen in some of the oral-contraceptive-treated cycles. The fact that there was this evidence of marked ovarian activity in the absence of ovulation in two of these women and that active steroidogenesis was taking place in most of the other treatment cycles merits further comment and investigation.

We await with interest the results of the studies of Drs. Jacob and Jequier, particularly as they suggest that there may be differences in the effects of ethinyl oestradiol and mestranol on the pituitary ovarian mechanism, as suggested in our paper.—We are, etc.,

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1 Orr, A. H., and Elstein, M., *Journal of Endocrinology*, 1969, **43**, 617.

Contraception and Abortion

SIR,—King's Termination Study II (9 March, p. 418) really tells us very little that is new and also draws some conclusions for which no basis exists in the published findings.

It is widely accepted that the closer the doctor-patient relationship the better are the results of treatment. The King's College Hospital figures have shown that this is also true when applied to enthusiastic contraceptive advice and its success. However, I remain to be convinced that it was necessary for them to carry out 360 abortions as a necessary prelude to this success. A less liberal but equally compassionate approach (the two are not mutually exclusive) to requests for termination, coupled with proper contraceptive advice and enthusiastic follow-

up, should yield equally impressive results. If the authors are sceptical of this they should carry out a further study along these lines.

The authors' belief that "there will always be some women who will be forced to seek legal termination" prompts me to ask who do they think is going to apply this pressure? If it is the "force" of suggestion by society via husband or boy-friend, then surely the doctor must redress this imbalance by ensuring that his patient understands as far as possible the full implications of her request.

In conclusion, the authors' hope that the unhappiness of many of their patients has been replaced with the possibility of a brighter future by this policy of abortion and contraception is, with respect, a little naive and also premature. They are no doubt aware of the increasing weight of evidence linking therapeutic abortion with subsequent infertility,[1] miscarriages,[2] prematurity, increased perinatal mortality rate,[3] and various gynaecological problems.[4] Have they any evidence that the 360 women in their study are going to be exempt from such consequences?—I am, etc.,

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1 Stallworthy, J. A., Moolgaoker, A. S., and Walsh, J. J., *Lancet*, 1971, 2, 1245.
2 Wright, C. S. W., Campbell, S.. and Beazley, J., *Lancet*, 1972, 1, 1278.
3 Klinger, A., *International Journal of Gynaecology and Obstetrics*, 1970, 8, 680.
4 Brudenell, M., *Proceedings of the Royal Society of Medicine*, 1971, 65, 155.

Antibiotic Discs Active against Resistant Organisms

SIR,—The letter by Mr. D. F. J. Brown and Dr. J. B. Selkon (23 March, p. 573) prompts me to describe a similar experience.

While I was attempting to demonstrate that penicillinase-producing *Staphylococcus aureus* may show large zones of inhibition around penicillin discs the organism was inoculated on to DST agar medium (Oxoid) on which discs stated to contain 5 units of benzylpenicillin and 2 μg, 10 μg, and 25 μg ampicillin respectively were placed. The following day, while the other discs did not show any zones of inhibition, a zone of 24 mm diameter was seen around the 2-μg ampicillin disc. The edge of this zone did not have the typical heaped-up appearance seen with penicillinase-producing organisms. Furthermore, other 2-μg ampicillin discs of the same batch did not show a zone of inhibition when tested against *Escherichia coli* (NCTC 10418), a known sensitive strain.

The zone of inhibition produced by these evidently faulty discs when tested against the Oxford strain of *Staph. aureus* (NCTC 6571) was neither reduced nor eliminated by the addition of penicillinase to the medium, whereas penicillinase prevented zones being produced by 2-μg, 10-μg, and 25-μg ampicillin discs of other batches. Obviously the suspect discs did not contain the antibiotic with which they were labelled. Attempts to identify the antibacterial substance in these discs were not successful but seemingly it was some substance active against *Staph. aureus* but not against any of the strains of *E. coli, Klebsiella* spp. and *Pseudomonas aeruginosa* tested.

Six vials of this batch of faulty discs had been recently received in the laboratory and stored at 4°C. Several discs from each vial gave similar results. Had these discs not produced a satisfactory zone with the Gram-negative control strain of *E. coli* (NCTC 10418) in the routine tests it might have been assumed that they contained an incorrect amount of the intended drug. The fact that they contained a substance other than that intended would not have been immediately evident.

Unlike the experience of Mr. Brown and Dr. Selkon all the discs tested in the batch were faulty and this should have been detected by the manufacturers' quality control procedures. Clearly, there is a need for urgent action to ensure that antimicrobial drug sensitivity discs comply with the manufacturers' description of them.—I am, etc.,

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Growth after Renal Transplantation

SIR,—Your leading article (1 December 1973, p. 505) raised pertinent questions concerning the crucial role of growth in the rehabilitation of children treated with chronic dialysis and transplantation. I do not believe that your statement that "the growth problems on dialysis are usually less than those following transplantation" is supported by the little evidence that is available. Evaluation of growth data of 46 children dialysed longer than a year in four centres [1-4] and seven children in our centre reveals that only six of the 53 grew at a normal rate for their age and sex (four of seven in our centre). Growth after transplantation appears to be better. Grushkin and Fine[5] reported normal growth of six of 26 children followed up longer than a year, though a more pertinent figure would be six of 18, since eight children already had fused epiphyses at the time of transplantation. In children treated with alternate-day prednisone McEnery *et al.*[6] found normal growth in four of 10. Of a total of 38 children in our centre, only seven grew at a normal rate; all seven were in a group of 21 who were treated with alternate-day rather than daily prednisone.

The factors causing growth retardation in children on dialysis and after transplantation are incompletely understood but seem to be primarily calorie deficiency in the former and steroid therapy in the latter. Some improvement in growth has been noted with the use of calorie supplements in children on dialysis[7] and alternate-day prednisone regimens in children post transplant.[6-8] At present, however, the long-term growth of most children treated with either dialysis or transplantation is poor and the available evidence does not support the statement that dialysis is preferable to transplantation in this regard.—I am, etc.,

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1 Baillod, R. A., Ku, G., and Moorhead, J. F., *Proceedings of the European Dialysis and Transplant Association*, 1972, 9, 335.
2 Broyer, M., Kleinknecht, C., Loirat, C., Marti-Henneberg, C., and Roy, M. P., *Proceedings of the European Dialysis and Transplant Association*, 1972, 9, 181.
3 Borra, S., and Kaye, M., *Canadian Medical Association Journal*, 1971, 105, 927.
4 Cameron, J. S., *Nephron*, 1973, 11, 221.
5 Grushkin, C. M., and Fine, R. N., *American Journal of Diseases in Children*, 1973, 125, 514.
6 McEnery, P. T., Gonzalez, L. L., Martin, L. W., and West, C. D., *Journal of Pediatrics*, 1973, 83, 806.
7 Simmons, J. M., Wilson, C. J., Potter, D. E., and Holliday, M. A., *New Zealand Journal of Medicine*, 1971, 285, 653.
8 Wilson, C. J., Kaye, J., Belzer, F., Kountz, S. L., and Potter, D. E., *Pediatric Research*, 1972, 6, 415.

Correction of Plasma Calcium Measurements

SIR,—The two recent papers on this important subject (15 December 1973, pp. 640 and 643) have left some confusion in their wake. Dr. A. M. Parfitt (16 March, p. 520) has already made the very important point that as the point of correction is to gain a better indication of the ionized calcium level, a proportional rather than an absolute correction should be applied. Other correspondents (9 February, p. 245, and 2 March, p. 393) have commented on the discrepancies between the correction factors given in the two papers. In fact the factors proposed (0·91 and 0·99 mg calcium/100 ml per 1 g albumin/100 ml respectively) are less strikingly different from each other than they are from other recently reported estimates which lie in the region of 0·7.[1-3]

The figure of Dr. Berry and his colleagues (15 December, p. 640) must be an overestimate for their cuffing technique leads to an increase in globulin and other macromolecules capable of binding calcium as well as albumin. Their factor thus represents not albumin binding but total plasma binding expressed in terms of albumin. It is appropriate only as a correction for changes due to cuffing and cannot properly be applied to the more useful purpose of allowing for variation in albumin levels in individual patients.

Dr. Payne and his colleagues (15 December, p. 643) have estimated a true albumin correction factor, but should their result supplant previous lower estimates? Their factor was derived from a study of a group of patients including many with hyperglobulinaemia. If, as seems reasonably likely, there were an association between albumin and globulin, the estimate of the regression of calcium on albumin would be biased, a positive correlation leading to overestimation because of the added binding effect of globulins. Unless the authors are able to discount this possibility it would seem wise for those wishing to correct calcium measurements to hold to previous lower estimates of the factor.—I am, etc.,

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1 Moore, E. W., *Gastroenterology*, 1971, 60, 43.
2 Orrell, D. H., *Clinica Chimica Acta*, 1971, 35, 483.
3 Pedersen, K. O., *Scandinavian Journal of Clinical and Laboratory Investigation*, 1971, 28, 459.

The "Filth Row"

SIR,—Cleanliness in public buildings conduces to morale as well as to health. Schools, which should be educative, and hospitals, where the frail are congratulated, are par-