low figures were found in all three cases of P.N.H. in which this test was performed. Ham's test and the sugar-lysis test were also strongly positive. A low figure of AChE (less than 0.550 pH units/hour) was also found in several cases of myeloproliferative disorder, including polycythaemia vera, myeloid sclerosis, di Guglielmo's disease, thrombocythaemia, and myeloid reticulosis. In some of these cases there was also slightly increased lysis in the sucrose-veronal buffer.

Though there was not complete correlation between a low AChE and increased sugar lysis, the high proportion of abnormal results in the diseases under discussion supports Dameshek's⁵ speculation that they are connected.—I am, etc.,

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Elusive Tumours

SIR,—For a good number of years I have observed the difficulty among medical students and even medical practitioners in eliciting a small subcutaneous tumour by palpation. This is particularly so in small breast tumours.

Palpation of such small tumours can be improved by wetting the skin surface over the affected site. The use of a soapy solution will increase the sensitiveness of this examination. Small subcutaneous tumours can become very obvious by rubbing the palmar surface of the terminal phalanx of the index finger (or any other finger) over the wetted skin surface.

I am sure a lot of clinicians know this little trick, although I do not recall reading about it in medical publications.—I am, etc.,

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Maternal Rh Immunization

SIR,—In opposition to Dr. W. Q. Ascari and his colleagues (15 February, p. 399), Dr. D. F. Hopkins (29 March, p. 843) upholds the accepted view that a proportion of Rh-negative women of blood groups A and B, as well as O, are protected against Rh immunization if they are married to ABO-incompatible husbands. Dr. Hopkins bases his case—a case disregarded by Dr. Ascari and his colleagues (7 June, p. 634)—on the three series quoted by the latter,¹⁻³ plus a series of his own,⁴ and if to these four are added the other six relevant series,⁵⁻⁹ as in the accompanying Table, his already convincing case becomes conclusive.

The Table gives the observed and expected numbers of Rh-immunized wives in the aggregate of eight of the ten series.¹⁻⁷ It shows that the ratio of observed to expected wives of ABO-incompatible husbands is 0.76 (296:390.3) for A wives and 0.66 (208:316.2) for B wives. Such massive

departures from expectation speak for themselves, and essentially similar results are given by the aggregate of the remaining two series,^{8,9} the data from which are differently presented.

Incidence of Rh Immunization in Wives of ABO-compatible and ABO-incompatible Husbands Respectively (Combined Data¹⁻⁹)

Type of Rh-immunized Wives	Wives' ABO Group	Rh-immunized Wives		
		Observed	Expected	O./E.
Wives of ABO-compatible husbands	AB	256	254.7	1.01
	A	3,084	2,484.2	1.24
	B	513	432.1	1.19
	O	1,948	1,655.6	1.18
Wives of ABO-incompatible husbands	A	296	390.3	0.76
	B	208	316.2	0.66
	O	1,049	1,819.9	0.58
Husbands	ABO-compatible	5,801	4,826.6	1.20
	ABO-incompatible	1,553	2,526.6	0.61
Total		7,354	7,353.2	1.00

Thus there is overwhelming evidence that a proportion of ABO-incompatibly mated A and B, as well as O, wives do indeed receive protection against Rh immunization.—I am, etc.,

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Patient-monitoring on a General Ward

SIR,—The general concept of the range of equipment carrying the name Monitron was developed in these laboratories, and I am therefore not unnaturally concerned with the future image of the equipment. Drs. J. M. Rawles and G. S. Crockett (20 September, p. 707) acknowledge the collaboration of members of my staff, and we have no quarrel with any of the results quoted or with their interpretation. What I feel is unfortunate is that certain factors which would put the trial into perspective have not been given sufficient emphasis, and that as a result a whole range of equipment may in future be viewed in a rather worse light than it deserves. These factors are:

The equipment used was so-called Mark I equipment, merely one step removed from laboratory prototypes. The quantity manufactured was only sufficient to equip 12 beds, and it was one of the purposes of the trial to obtain operational experience with rather complex equipment, so that a viable commercial range could be designed. This has now been done thanks to the work of people like Dr. Rawles, and current equipment has been shown to have an incidence of technical faults about one-tenth of that of Mark I equipment.

When allocating the equipment to five different hospitals a deliberate attempt was

made to obtain a wide spread of operational situations ranging from a busy multiple injury unit to a general medical ward. This was done in the full realization that in some situations the equipment would prove to be more useful than in others, and not entirely unexpectedly it has been demonstrated that a complex installation is unsuited to a general medical ward.

Monitron equipment is basically modular, and much simpler and cheaper installations can be assembled which provide less facilities but bring the cost down to a few hundred pounds per bed. The full range of equipment was supplied so as to identify the facilities which were useful in a particular situation.—I am, etc.,

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Platelets and Aspirin

SIR,—Drs. A. H. Youssef and P. Barkhan (16 August, p. 394) and your leading article (p. 371) implied that acetylsalicylic acid is bound to platelets via the acetyl group, and that this may be the explanation for the effect of this drug on platelet aggregation and associated release reactions.

However, careful consideration of their data does not necessarily support the above conclusion. They found that virtually no ¹⁴C-carboxyl-aspirin was bound to platelets, whereas a small amount (2–5%) of ¹⁴C-acetyl-aspirin was bound. They explain the lack of carboxyl label on the platelet as being due to a splitting off of this group when the aspirin was attached to the platelet via the acetate group. However, the carboxyl group of the aspirin is firmly attached and no enzymes or other mechanisms for splitting it off are known. By contrast the acetyl group is easily hydrolysed and there are known enzymes in plasma and other tissues to accomplish this.

A more plausible explanation of their data is that under the conditions of their incubation of labelled aspirin with plasma and platelets a portion of the ¹⁴C-acetate is split off, and that it is this free acetate ion that either binds to or enters into the platelet. Preliminary data from our laboratory using ¹⁴C-acetate support the latter explanation. The mechanism of action of aspirin on the platelet remains obscure.—I am, etc.,

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Postoperative Gas Gangrene

SIR,—Your leading article "Postoperative Gas Gangrene" (20 September, p. 665) and the article to which it refers by Dr. M. T. Parker (p. 671) are salutary reminders of the prevalence of this postoperative complication. It will surprise many surgeons to know that during this two-year investigation, by no means nation-wide, clostridial infection was reported in 85 cases, "of which 55 were gas gangrene, fatal in 31." Advocacy for penicillin operative cover for higher-risk patients is also welcome, but where do you draw the line between these and "perfectly safe, clean operations" (is there such a

thing?), for which you condemn prophylactic antibiotics. Our experience of 22 cases of gas gangrene suggests that this condition was unexpected and arose from a variety of orthopaedic operations—only four had had amputations.

We find it difficult to understand how you can avoid the mention of hyperbaric oxygen in the management of this dreadful condition. An earlier leading article¹ was followed shortly afterwards by a letter² in which it was quoted that a multi-centre investigation of 200 cases treated by hyperbaric oxygen revealed a mortality rate of 2.9%, excluding those moribund on admission and dying within 24 hours.³ We reported 16 cases with one death and have now had 21 cases with one death.⁴ This contrasts with 31 fatal cases out of 85 in Dr. Parker's investigation. Although the manner of collection of these various groups may have differed, the vast improvement of the prognosis in those treated with hyperbaric oxygen is too clear to be avoided, but the earlier treatment is commenced the better the outlook. Unfortunately there is delay, partly because the condition is not anticipated and may be confused with other forms of postoperative pyrexia, etc., and also because knowledge may not exist as to the availability of the nearest hyperbaric unit. Any surgeon would do well to know where the nearest unit is situated.

Dr. Parker found that infection in most cases arises by autocontamination (from organisms arising from the anus of the patient) and not from outside sources, particularly in the theatre, where it is known that clostridial organisms are ubiquitous. This should give confidence to those surgeons who are worried about taking a patient with established gas gangrene into the theatre for operative procedures. Dr. Parker mentions that if this were done together with a theatre failure in sterilization disaster might follow, and he quotes four cases in which gas gangrene did develop. We know of an occasion which, if not the same, is very similar, and these four patients developed the undoubted signs of gas gangrene. All commenced hyperbaric oxygen early and all had normal wounds healed by first intention; two of these cases had metal prostheses which did not have to be removed and which subsequently functioned adequately.—We are, etc.,

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Hazard of Self-inflating Resuscitation Bags

SIR,—Drs. R. Loveday and D. G. Hurter (11 October, p. 111) describe a hazard of the use of Ambu bags. They write that "the Ambu bag in question had been in use for about one year" and "had never been autoclaved or subjected to any sterilizing technique," which suggests that they may not have experienced a further hazard.

During an outbreak of infection with *Pseudomonas aeruginosa* in an intensive care unit the organism was isolated in large numbers from the inside of an Ambu bag. The strain was indistinguishable by serological and bacteriophage typing from strains isolated from tracheostomy wounds in three patients. The Ambu bag had been used in chest physiotherapy on all the patients.

As resuscitation bags of this type are difficult to sterilize, autoclavable bags are now used and one bag is kept for each patient. Wire-mesh filters may retain sponge particles but not micro-organisms. The potential dangers from sponge particles and contaminating bacteria necessitate resuscitation bags which can be sterilized repeatedly. The strains of *Pseudomonas aeruginosa* were typed at the Cross-Infection Reference Laboratory, Colindale.—We are, etc.,

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Suppression of Lactation

SIR,—Dr. C. A. Hakim and colleagues (11 October, p. 82) confirm a clinical impression concerning the lower incidence of thromboembolic episodes with hexoestrol. I am surprised that the authors found it necessary to use as high a dose as 45 mg.

For several years I have been using a single injection of hexoestrol dipropionate 15 mg. in oily solution, by deep intramuscular injection, as soon as possible after delivery for the suppression of lactation. Out of 200 or so patients, I have had to use another similar dose seven to ten days later on only three or four occasions, complete suppression of lactation having been achieved in the majority by the single dose of 15 mg.—I am, etc.,

Bracknell, Berks.

P. J. W. YOUNG.

Contraceptives and Cervical Carcinoma

SIR,—Dr. G. I. M. Swyer (23 August, p. 471), referring to Table I in our report on prevalence rates of cervical carcinoma in situ for women using the diaphragm or contraceptive oral steroids (26 July, p. 195), draws conclusions that result from a misreading of the paper and require correction.

The Planned Parenthood Centers of New York City had been in operation for many years at the time this study was begun and the first cytological examinations were carried out. Column 0+ in Table I refers to the uncorrected prevalence rates that were found on the initial survey for the total of all women choosing and/or using oral steroids or diaphragms for all lengths of time. We had thought that this was clearly stated in the caption and emphasized in a footnote to Table I. The figures include the women using the method and do not give any information about the status of any women before starting either method of birth control. Nor can one obtain that information, desirable though it may be, from the prevalence rates of disease in newcomers to Planned Parenthood of New York. Virtually all women

coming to the centres (except the very young) give a history of using some type of contraceptive previously. Approximately 30% of the women who chose oral steroids on their first visit indicated that they had used them in the past, although they often could not recall for how long or when. Thus, even the newcomers who are part of the population in column 0+ of Table I include a very significant proportion of steroid "choosers" who were actually "users" before they came.

I think it is not possible to find a sizable population of sexually active women today in the United States (or in the United Kingdom) who have not been using some form of contraceptive. For that reason prevalence rates of disease are unavailable for a "control" group of women using no contraceptive, and meaningful differences must be sought in comparisons between matched populations where the contraceptive used is known. This is what we have done. Clearly, the matter of free choice of contraceptive is important and may be influenced by unknown factors affecting the probability of developing carcinoma. We have so stated in our discussion of results. Our next step is to follow the incidence rates of disease in matched populations that are initially normal, and will include a third group of women using another form of contraception (intra-uterine device).

So far, nearly 50,000 women have been examined, and large matched populations of normal women who are using the diaphragm, oral steroids, or an intrauterine device for contraception are available. We are prepared to continue for at least five to 10 years more to determine incidence rates of the disease in these three different groups. If there are clinically meaningful differences, they should be apparent within that time.—I am, etc.,

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SIR,—Dr. Myron R. Melamed and colleagues' paper (26 July, p. 195) obviously has extremely important significance in the urgent search for acceptable and safe methods for reproduction control. The authors have attempted to contribute evidence to bear on the question of cervical cancer hazard resulting from a year or more of exposure to the oral use of steroids for contraception. They have presented data acquired opportunistically from populations of women differing on many important variables related to cervical cancer risk. As they are aware, the differences found between the mechanical contraceptive barrier (diaphragm) users as controls and the oral steroid study group may reflect protective effect for the former rather than increased hazard for the latter.

Because of differences between the study and control groups on many important variables the data presented are relatively meaningless, except for the groups matched on the five vectors representing the important variables associated with cervical cancer risk. The twofold difference indicated by these data between oral contraceptive and diaphragm users hinges on the validity of the carcinoma in situ prevalence-rate determinations. Prevalence rates for carcinoma in situ can only come from the findings of cytological examinations performed for the first