led to both feet regaining a normal appearance, and to the complete healing of all the areas of gangrene.

I have given doses up to 400 mg. at a time (100 mg. in 1 ml.), but there appears to be no advantage in using more than 100 mg. at a time. I use this routinely now, mixed with some 6 ml. of 1% lignocaine. Injections may be given daily or less often. There is probably no advantage in giving more than one injection daily. Pain down the leg at the time of injection is considerable but not intolerable, and is lessened by giving the injection slowly. If given slowly only minimal quantities of acetylcholine are carried back to heart and lungs, and no bradycardia or bronchospasm of any severity occurs. Slow injection is important, since cardiac arrest can occur with rapid injection. It is, of course, vital to avoid injection of acetylcholine into the femoral vein, and to be certain that the artery has been entered it is advisable to use a syringe with a freely moving plunger so that the blood from the artery enters readily. A disposable syringe has a plunger which is too stiff in its movement to be forced outward by arterial pressure, and this type of syringe is unsuitable. I use a 21 S.W.G. 1½ needle.

It is presumed that acetylcholine is beneficial through improving the collateral circulation in the affected limb. This may be in part due to vasodilator effect, but in view of the apparent permanence of improvement it might be conjectured that some local or spinal vasoconstrictor reflexes, possibly arising in the diseased main vessel, are inhibited and removed and do not recur.

No ill effects have been observed from this therapy, and it is hoped that others may see fit to give it a trial. It is, of course, a matter of common sense to vary the point at which the artery is punctured.—I am, etc.,

R. J. T. WOODLAND.

Paignton, Devon.

Psoralens

SIR,—In July 1960 S. W. Becker, jun., summarized our knowledge concerning these drugs.[1] He stated that there were many psoralens but only a few had been used clinically. His theme was largely that psoralens are potent controllable photosensitizers which can be used to increase or decrease the effects of sunlight in human skin. Becker described how under their influence the horny layer of the epidermis becomes thicker and more dense and the *stratum lucidum* is also changed; if the skin is then exposed to sunlight so that an erythema is obtained, pigment is retained in the epidermis in a way which does not happen in untreated skin, and sun-tanning is apparently accelerated. But the erythema must be carefully produced. To take psoralens and then expose oneself haphazardly to intense sunlight or to the rays from an ultraviolet ray source may produce a severe blistering dermatitis; damage to the eyes may also occur. Becker therefore deplored the use of psoralens as "sun-tan pills."

My remembrance of this matter was recently quickened when a patient recently showed me a residual oedema of the legs and a good deal of cutaneous damage following the use of psoralens for "sun-tanning." He had been recommended by a friend to take the tablets during a holiday in France, and the acute reaction which he incurred has cost him four months of invalidism. In practice it seems likely that many take psoralens, then sunbathe, and, possibly from a fortuitous combination of circumstances, get away with it; but in a proportion of cases a very acute generalized dermatitis with blistering and oedema (and perhaps with renal complications) may occur. Therefore it is advisable not to use the drugs casually merely as an aid to "sun-tanning." Other side-effects include nausea, vomiting, insomnia, and mental depression, and some have suspected that psoralens are at least potentially hepatotoxic.—I am, etc.,

London W.1. R. M. B. MACKENNA.

REFERENCE

1 Becker, S. W., jun., *J. Amer. med. Ass.*, 1960, **173**, 1483.

Hypnosis for Asthma

SIR,—The report "Hypnosis for Asthma—a Controlled Trial" (12 October, p. 71) and subsequent correspondence raise a point of very general significance. Dr. B. J. Freedman (2 November, p. 329)—and also Dr. R. J. Walden (14 December, p. 706) by implication—asks "What value can be placed on a patient's personal assessment of his wheezing when under the influence of post-hypnotic suggestion." It was only an assessment of this kind that gave any clear indication of the therapeutic superiority of hypnosis over the control method. In drug trials the patient's own statement of how he feels may be the most valuable criterion of the effect of treatment. Its essential subjectivity presents no problem in properly designed double-blind trials. In the present case, however, the situation is more difficult because the therapy itself is likely to persuade the patient to *say* he is better. The situation is akin to a drug trial in which the therapist gives to the treated patient, but not to the control patient, strong suggestions that he will get better.

On the other hand, the patient who feels well is better off than the patient who feels ill, and, as Dr. G. P. Maher-Loughnan (30 November, p. 583) has pointed out in reply, the patient's $F.E.V._1$ and V.C. measured once a month may be poor indices of either initial impairment or subsequent progress. When reliable direct observation is impossible there is nothing for it but to use the patients as observers of themselves, but the investigator must realize that his observers may be biased. If necessary he must eliminate the bias or make due allowance for it. The "Hypnosis for Asthma" investigation was rightly criticized by Dr. Freedman for failing to do this.

In most illnesses, that which can be directly observed is but a small part of the whole constellation of events that constitute the illness. The doctor is therefore obliged to rely on information obtained by verbal communication with the patient. Psychologists already know quite a lot about the biases affecting such communication. While it is generally impossible to eliminate these distortions entirely, they can be minimized, and sometimes measured, by adopting appropriate methods. Much more research is needed into these problems, so that better methods of communication between doctor and patient can be devised. In the meantime we must ensure that all the currently available knowledge is brought to bear upon choosing the most suitable method for each type of inquiry.—I am, etc.,

J. G. INGHAM.

M.R.C. External Staff, Llandough Hospital, Penarth, Glamorgan.

Management of Depression

SIR,—In the article (7 December, p. 627) on the management of senile psychiatric disorders by Dr. Felix Post, no mention is made of the new tricyclic antidepressant iprindole.

A group of 12 patients in my practice were treated with this drug for four weeks. Each was suffering from a depression of endogenous or of reactive origin. The Max Hamilton Rating Scale was used to measure the initial and final degrees of depression, with a dosage of 30 mg. t.d.s. In the final assessment, with an average initial score of 20 and an average final score of 7.125, four cases were much improved, four cases were improved, in two cases there was no change, and two cases were worse. Side-effects were observed in one patient only and consisted of increased agitation, nausea, and weakness in the legs. The frequency of atropine-like side-effects as occurring with other antidepressants was not noted and generally speaking the response was gradual and sustained.

Professor L. G. Kiloh (28 December, p. 813) states that a high proportion of patients do not take their tablets. Consequently an antidepressant drug with a significant reduction in side-effects could be a welcome addition to the therapeutic armamentarium.—I am, etc.,

D. WAXMAN.

London W.1.

Inhibition of Lactation

SIR,—The results of three of the four early trials of quinestrol analysed at the colloquium on quinestrol (Estrovis) in 1966 have been recently discussed in your columns (21 December, p. 769; and 17 January, p. 184). As there is some discrepancy between them, the results of the fourth trial performed at the same time by me at St. George's Hospital furnishes further relevant information. In this, a double-blind trial was performed comparing a single oral placebo with a single 0.8-mg. oral dose of quinestrol and an intramuscular injection of 45 mg. hexoestrol plus placebo capsule. The treatment in each case was given within two hours of delivery, usually before the patient left the labour ward bed. Fifty patients were randomly selected into each of the three groups. The results are shown in the Table:

Drug	No. Patients	Results			
		Excellent	Fair	Poor	Fail
Placebo only	50	0	1	1	48
Hexoestrol	50	30	7	3	10
Quinestrol (2 mg.)	50	17 (34%)	6	2	25 (50%)

An "excellent" result indicated that objective daily examination of the breasts until the seventh day, and again three weeks later, revealed that they had never been other than soft and had not leaked at all. In addition, the mother had subjectively felt that both breasts had been completely comfortable throughout.

A fair result implied that there had been mild engorgement or slight leakage, or slight discomfort on one day only throughout this four-week period.

A "poor" result implied that two of the above three factors had been present on one day, or that one of the above factors had been present on any two days.

A "fail" result was registered if on any one occasion there had been severe engorgement or considerable leakage or painful breasts, or that suppression had been required either during the week in hospital, or during the subsequent three weeks.

As quinestrol is stored in fat and then slowly released, it might be expected that the fatter the patient the higher would be the failure rate. This is indicated by an analysis of the heights and weights of the patients in the "excellent" and "fail" groups of those given quinestrol.

Result	Number of Patients	Mean Height (in.)	Mean Weight (lb.)	Standard Deviations of Weight (lb.)
Excellent	17	62	128	17·3
Fail ..	25	63	140	18·5

There was no significant difference between the mean heights of the groups.
Student's t test (with Bessel correction) was applied to the mean weights of the two groups, and the difference was significant at the 5% level ($P = 0.05$, "t" = 2.1, d.f. = 40).

I found conclusively that some form of suppression of the initiation of lactation was required if the patient was to be saved undue discomfort. This is contrary to the report of MacDonald and O'Driscoll,[1] but the trials are not comparable, as in theirs the maximum length of follow-up was five days, and neither objective evidence of engorgement or secretion nor subjective degree of discomfort of the breasts was assessed. As some advocate fluid restriction[2] and some fluid overloading with 2 to 5 litres per day,[3,4] I must point out that in the present trial the patients were allowed to drink what they wanted.

It will be seen that the above criteria for success or failure of the trial could hardly be more strict, yet my results using the 2-mg. dose (34% no trouble whatsoever, 50% requiring further suppression) were much better than those of Mr. P. N. Gillibrand and Professor P. J. Huntingford (21 December, p. 769). They combined the results of eight patients given the 2-mg. dose with those of 19 patients given the 4-mg. dose. Twenty-one out of the 27 cases (78%) required further suppression of lactation. Although I have no doubt that they assessed their patients most carefully, it is obvious that the discrepancy cannot be vaguely accounted for by my observer error whereby clinical signs were ignored or not observed owing to inadequate frequency or length of follow-up In the same way, the much better results using quinestrol reported by both Barbour[5] and Kuku[6] cannot be explained by observer error on their part, as the follow-up was equally thorough in these trials. No satisfactory explanation could be given for these marked discrepancies between the results at the colloquium in 1966, and in my opinion none has been given since.

The 2-mg. dose of quinestrol clearly inhibited the initiation of lactation in 34 to 50% of patients. Although too low a success rate for clinical acceptance, the better results in the patients with lower mean weights held out hope that with higher dosage the success rate would improve. This appears to be confirmed by more recent trials.—I am, etc.,

PETER S. FIRTH.

St. George's Hospital,
London S.W.17.

REFERENCES

1 MacDonald, D., and O'Driscoll, K., *Lancet*, 1965, **2**, 623.
2 McCracken, J. S., *Lancet*, 1965, **2**, 688.
3 Vartan, C. K., *Brit. med. J.*, 1969, **1**, 50.
4 Vartan, C. K., *Lancet*, 1965, **2**, 1022.
5 Barbour, E. M., and Baruah, N. K., *Scot. med. J.*, 1968, **13**, 277.
6 Kuku, S. B., *J. Obstet. Gynaec. Brit. Cwlth*, 1968, **75**, 103.

SIR,—Further to Mr. S. J. Steele's letter (30 November, p. 578) regarding oestrogen inhibition of lactation, I have just completed a small series with this same point in mind. Large doses of stilboestrol for inhibition of lactation had been recommended to me during my formative years, and I wondered whether I was justified in continuing with this treatment. The dose involved is 10 mg. stilboestrol four times a day for four days, 5 mg. four times a day for four days, and 5 mg. twice a day for two days.

In a double-blind trial with stilboestrol and a placebo tablet, 88 mothers who did not wish to breast feed received these tablets at random in the dosage as above; 45 received the placebo; 43 stilboestrol. Each patient was asked daily—twice daily on the second, third, and fourth days—to describe her breasts as "comfortable," "uncomfortable," or "painful," and she was examined to determine whether they were "soft," "tense or patchy tense," or "uniformly very tense." By allocating one adverse point to "uncomfortable" symptoms and "tense" signs, and two adverse points to "painful" and "uniformly very tense" the following figures came out of this series: Symptoms: 14 adverse points were allocated to six of the 43 patients taking stilboestrol whereas 82 were allocated to 27 out of the 45 placebo takers. Signs: Four and 68 points respectively.

The incidence of the uncomfortable leaking of milk was noted. Thirty-nine patients taking the placebo complained of this against 13 taking stilboestrol. I traced eight patients having had the stilboestrol course who needed further treatment for engorged breasts after leaving hospital, as against three who took the placebo.

On the dosage outlined, I would suggest that stilboestrol plainly is effective in suppressing lactation, but that there is a distinct rebound phenomenon. And, of course, as noted by Dr. Steele, there are the worrying thoughts of thromboembolism. There must be a better way of producing symptom-free inhibition of lactation. Preferably a swing away from the present anti-breast-feeding trend.

I am very grateful to the matron and staff of the Bolitho Maternity Home, Penzance, and to Mr. Miller, West Cornwall Hospital, Penzance, for their help.

—I am, etc.,

ROBERT E. SENIOR.

Penzance,
Cornwall.

Scabies in Negroes

SIR,—I can bear out the impression of Dr. F. A. Ive (14 December, p. 706) that scabies may be rare in patients of Negro origin in this country. During six and a half years of clinics in the Hackney E.9 area of London I saw a great deal of scabies. The area is one which has a large immigrant population, mostly from the West Indies. During that time I saw scabies only once in immigrants; this was in a family of West Indians.—I am, etc.,

CONSTANCE M. RIDLEY.

Harold Wood Hospital,
Harold Wood, Essex.

SIR,—I was interested to read Dr. F. A. Ive's letter (14 December, p. 706) in which he points out that during the present scabies epidemic he did not find a single case of scabies in patients of Negro origin. In my eight years of dermatological experience in Britain, at the three big industrial towns where there is a considerable coloured immigrant population, I hardly remember seeing scabies in a coloured immigrant patient, in spite of sometimes substandard living conditions, overcrowding, etc.

I wondered whether it was just a question of personal cleanliness or does Mrs. Acarus show host preference?—I am, etc.,

M. GANPULE.

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Manchester 3.

Vulval Pruritus

SIR,—I am sure that Dr. M. J. V. Bull (11 January, p. 120) has correctly interpreted Dr. L. Tann's (21 December, p. 776) observations, and that the most likely cause of his patients' vulval pruritus is candidiasis associated with the use of the contraceptive pill.

This prompts me to make some further observations on this matter, as in my experience vulvo-vaginitis due to *Candida albicans* is now the commonest cause of a vaginal discharge in women using oral contraceptives. I would agree wholeheartedly with his remark that "the full clinical picture of vaginal candidiasis is not always apparent." In a majority of cases the classical picture of irritation with thick, curdy white secretion is entirely lacking, the patients complaining only of vaginal discharge. This discharge is often copious, frankly purulent, and without any special characteristics. The fungus is often difficult to find in stained smears, a prolonged search being necessary to find an occasional spore or mycelial element. Culture is the most effective means of diagnosis, but even here several specimens may have to be examined before a positive result is obtained.

In these patients cure is often difficult to obtain, and attention should be paid to preventing reinfection from the intestinal reservoir by giving a course of oral nystatin. In a not inconsiderable number it may be necessary for the patients to stop using the pill for some months. The possible co-existence of sexually transmitted disease