very first year, and this could be the resultant of some real diminution in contraceptive efficacy in the third year combined with the group's lower fertility for the above reason. But, as Dr Sivin says, the third year rate should be obviously higher if there were serious interference by deposits with the contraceptive action.

Mr Newton states that there was no difference in age and parity between the two groups. But the difference in rates of removal for planned pregnancy (zero for the "replacement group," 10% for the "continuation group") could point to an important difference in attitudes to the risk of unplanned pregnancy and hence possibly in behaviour. When electing for replacement or continuation with the same IUD the women were presumably aware of the possibility of reduced contraceptive protection if they took the second course. The "replacement group" might therefore accumulate an excess of users of additional methods such as spermicides. They might also check more obsessively for lack of cervical displacement of their IUD (a potent cause of pregnancy with this particular design).

Pending more clinical data, what guidance can be offered to clinicians? A blanket policy of replacement at two years makes medicolegal sense but might be unnecessarily early for those whose IUDs are least affected by incrustation phenomena. Measuring copper levels in cervical mucus, as suggested by Dr Gosden and her colleagues, may one day be practicable. For the present, if use beyond two years is desired it would be wise to discuss the available facts carefully with the IUD user herself.

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¹ Loudon, Nancy. Personal communication.

² Johnson, A B, et al, *Contraception*, 1976, **14**, 507.

Maternal antibody against pertussis

SIR,—In your leading article on whooping-cough immunisation (2 July, p 5) in which you aptly support the advice of the Joint Committee on Vaccination and Immunisation¹ you state in previous studies "maternal antibody against pertussis was found (my italics) not to be transferred to the fetus across the placenta, so that there was no objection to early immunisation, as was the case in diphtheria prophylaxis." The joint committee was more cautious, saying that whooping cough "is different from other infectious diseases of childhood in that maternal protective substances are not generally believed (my italics) to be transferred to the newborn baby."

This belief is part of the pertussis folklore and is presumably based on the readiness with which infants can develop whooping cough in the first few months of life. It even leads some to the false deduction that antibodies play only a minor role in protection against whooping cough or that only IgM is of significance. Actually the levels of pertussis agglutinins in infants' sera are directly related to those in the mothers' sera.^{2,3} For example, mother's titre 128, infant's titre 16 at 3 months and 8 at 4 months; mother 64, infant 8 at 3 months and <4 at 4 months; mother 16, infant 4 at 3 months and <4 at 4 months; mother 8, infant <4 at 2 months.

However, the mothers' sera rarely contain

detectable amounts of antibody to pertussis factor 3 and so the infant is susceptible to type 1,3 infection, just as vaccinated children were before 1967 when many batches of those earlier vaccines lacked antigen 3.

If, as you suggest, the presence of maternal antibody against diphtheria is reason for avoiding early immunisation (before 3 months of age), then the same applies to pertussis. The joint committee's recommendation that vaccination should start at 3 months of age is therefore not the compromise that you imply but is consistent with the currently available evidence.

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¹ *Whooping Cough Vaccination. Review by Joint Committee on Vaccination and Immunisation*. London, HMSO, 1977.

² Abbott, J D, Preston, N W, and Mackay, R I, *British Medical Journal*, 1971, **1**, 86.

³ Preston, N W, et al, *Journal of Hygiene*, 1974, **73**, 119.

Carbon dioxide-dependent *Staphylococcus aureus*

SIR,—We read Dr M Rahman's letter (30 July, p 319) with interest, as we have recently isolated a similar strain of carbon dioxide-dependent *Staphylococcus aureus* from a clinically infected occipital wound in a child. These strains, though rare, are well documented as dwarf or G strains and usually produce tiny colonies in the absence of added carbon dioxide after prolonged incubation.^{1,2} Although they may be isolated from clinical lesions in man their virulence as judged by mouse inoculation has been reported as low.² Dwarf strains are described as being induced by antibiotic administration,³ and a previous isolation in this department of a carbon dioxide-dependent strain was from a patient with osteomyelitis of the radius who was receiving penicillin.

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¹ Wilson, G S, and Miles, A A, *Topley and Wilson's Principles of Bacteriology, Virology and Immunology*, 6th edn. London, Arnold, 1975.

² Slifkin, M, et al, *American Journal of Clinical Pathology*, 1971, **56**, 584.

³ Wise, R I, and Spink, W W, *Journal of Clinical Investigation*, 1954, **33**, 1611.

Genital yeast infection

SIR,—The question whether genital yeast infection should be considered a sexually transmitted disease (STD) (Dr R N Thin and others, 9 July, p 93) might benefit from a clarification of the terminology. Is STD defined as the presence of micro-organisms that can cause disease and are usually transmitted from one host to another by sexual contact? This is the concept that seems implicit in the routine STD statistics, and the problem is then to determine whether or not sexual contact is the usual mode of transmission of genital yeasts.

On the other hand it seems implicit in the name itself that a patient with STD should (a) have a disease, and (b) have acquired that disease by sexual contact. If the presence of genital yeasts in the absence of any clinical

problem is not a disease, then the fact that they might be "usually sexually transmitted" would not make their presence an STD; and similarly, if genital yeast infection can occur without the yeasts having been sexually transmitted, then any such infection would not be STD.

It would be of value statistically, and maybe also clinically, if we were to differentiate clearly between micro-organisms that are sexually transmissible and diseases that are sexually transmitted.

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Infectious mononucleosis complicated by hydrocele

SIR,—Infectious mononucleosis is now recognised as a systemic disease, but its association with hydrocele has not so far been described. The following case history may therefore be of interest.

A 4-year-old boy was admitted with a 36-h history of swelling of the right scrotum. A few days before the appearance of the swelling he had felt unwell and feverish and had noticed a swelling on the left side of his jaw. On examination there were large masses of firm, painless lymph nodes in both posterior triangles of the neck, the axillae, and the groins. His spleen was palpable to 3 cm below the costal margin, but the liver was not palpable. There was a right hydrocele with both testicles easily palpable. Investigations showed haemoglobin 13 g/dl, leucocytes $25.6 \times 10^9/l$ (25.600 mm^3) with 40% lymphocytes and numerous (47%) atypical mononuclear cells, and normal platelet count. The Paul-Bunnell test was strongly positive. Screening for cytomegalovirus, toxoplasmosis, and mumps was negative. He was discharged home and when seen later in the outpatient clinic the hydrocele had resolved and the spleen had also decreased in size.

The aetiology of the hydrocele was obscure, but it may be speculated that it was due to infiltration with large atypical cells, which has been described in other complications.^{1,2} Pleural effusion with infectious mononucleosis has been described and so has the urogenital complication of acute urinary retention.³ The patient reported presented with hydrocele and generalised lymphadenopathy, both conditions resolving during the same clinical course, and infectious mononucleosis was shown to be the primary diagnosis.

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¹ Michel, R G, et al, *Archives of Otolaryngology*, 1975, **101**, 486.

² Sarka, T K, *Diseases of the Chest*, 1969, **56**, 359.

³ Sperber, A, Tessler, A N, and Berczeller, P, *Urology*, 1973, **2**, 456.

Measuring airways obstruction in asthma

SIR,—Your recent leading article (13 August, p 414) is a timely reminder of the importance of frequent measurements of airways obstruction in the management of asthmatic patients.

The instruments you mention, however, the peak flow meter or gauge, are very useful but relatively expensive. For this reason I was associated with the development of a cheap and simple instrument¹ to monitor changes in airways obstruction and provide a warning of