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specialist opinion, since the capricious nature of the condition determines that when retested six to eight weeks later most children will have no glue detectable, and only a small proportion will be referred to hospital. Without adequate screening procedures up to 60% of children with deafness due to glue ear may go undetected for two years or longer.[1]

It is fallacious to assert that the rate of adenotonsillectomy is inversely related to that of grommet insertion. Since the introduction of sulphonamides in 1939 and penicillin in 1945 the incidence of sore throat has been declining. The accusation of indiscriminate tonsillectomy as a surgical panacea was muted nearly 30 years ago, and at that time there was concern that the pendulum would swing too far in the opposite direction.[1] The notion that the decrease in the rate of adenotonsillectomy, due mainly to the use of antibiotics and improved social welfare, bears a direct relation to the rise in rate of grommet insertion, which has filled a professional vacuum, is surely unsubstantiated in clinical practice. Furthermore, the increased use of antibiotics may have contributed to the increased incidence of [illegible] otitis media, some cases of which may [illegible] incompletely treated, partially resolved [illegible] suppurative otitis media; antibiotics have not been shown to hasten the ultimate resolution of effusion in cases of non-suppurative otitis media.[4]

In Dr Black's discussion and summing up of the evidence against the overzealous use of grommets he quotes a two year follow up study as recording tympanosclerosis of the drum head in almost half the children who underwent surgery for glue ear.[7] However, it is not made clear that almost half of these children had myringotomy alone, a fact which illustrates how readily tympanosclerosis (chalk patches) will form on the tympanic membrane. In itself tympanosclerosis of the drum head is not associated with hearing loss and is a common otoscopic appearance after any inflammatory process affecting the tympanic membrane, particularly untreated otitis media. In the same paper the authors report no cases of cholesteatoma after grommet insertion although they acknowledge the limited follow up period. By present day experience their grommet extrusion time seems rather rapid.

In conclusion, the rising rate of surgery for glue ear is probably related to the increased awareness of [illegible] condition which may account for speech, [illegible] onal, and behavioural problems in young [illegible] en, coupled with the unproved efficacy of [illegible] term antibiotics and decongestants in the treatment of the middle ear effusion. The cost of this effort is balanced against the need for less frequent review of patients with grommets in situ and the expense and side effects generated by prolonged use of drugs. It is unnecessary to invoke the comparison of treatment with a "surgical epidemic" and unlikely that if one had existed for the last 25 years the long term consequences of the surgery would be unknown.

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HTLV-III, haemophilia, and blood transfusion

SIR,—Professor A L Bloom and his coauthors (22 June, p 1901) state that cryoprecipitate should no longer be used. This recommendation by the reference centre directors will have a major, and we believe unfortunate, effect on treatment policy in many haemophilia centres.

One implication is that patients with von Willebrand's disease and haemophilia A of mild or moderate severity will have to be treated with large donor pool factor VIII concentrates (albeit heat treated) when desmopressin is not appropriate. Initially because of concern about chronic liver disease in haemophiliacs,[1,2] and, more recently, with HTLV-III also in mind, we have for the past five years tried to restrict the exposure of our patients to large donor pool concentrates. Cryoprecipitate has played a major part in this policy, being used in the treatment of patients with von Willebrand's disease and those with mild to moderate haemophilia. Children with severe haemophilia are also treated with cryoprecipitate until they go on to home treatment. Even so, we used 1 million units of factor VIII concentrate in 1984, 60% in the form of commercial concentrate. This exposure is limited by buying as much as possible of a batch from a single commercial supplier. In this way patients have been treated for as long as 18 months using 100 000 units or more of the same batch.

This policy has resulted in a low prevalence of HTLV-III antibody in our patients. We recently tested 76 patients (including 27 children) who have received blood products at this centre during the past five years. Seven of the 28 who had received commercial concentrate were HTLV-III antibody positive (25%). There were no positive results from patients treated with NHS factor VIII concentrate only (5), NHS factor IX concentrate (12), cryoprecipitate (28, or fresh frozen plasma (3). The seven positive patients all have severe haemophilia A (factor VIIIC < 1 IU/dl) and constitute 37% (7 of 19) of this most at risk group. All are adults, aged 23 to 54 years, and none have clinical features of HTLV-III infection. All patients with mild or moderate haemophilia A, Christmas disease, von Willebrand's disease, and factor X deficiency were HTLV-III antibody negative. Three severe adult haemophiliac patients were positive for HTLV-III antibody on transfer to the centre in recent months, bringing the total number of our positive patients to 10.

Recurrent treatment with blood products is hazardous. Apart from the danger of HTLV-III infection, the severity and progressive nature of chronic liver disease in haemophilia has recently been re-emphasised.[3] Cirrhosis of the liver has been reported in mildly affected, infrequently transfused haemophiliacs,[1] and in children.[2,3] Although the pathogenesis of chronic liver disease in haemophilia is not completely understood, it is undoubtedly related to treatment with large donor pool concentrates, all of which contain non A, non B hepatitis viruses.[4] We would agree that only heat treated concentrates should be used since this may protect the recipients from HTLV-III infection, but heat treatment has not, so far, been shown to eradicate hepatitis viruses.[5]

The best approach seems to us to be a treatment policy which is designed to reduce, as much as possible, all the risks associated with blood products and which is tailored to the needs of each individual patient. HTLV-III antibody testing of all blood donations is expected to start in September 1985 (W Wagstaff, personal communication). During the intervening two to three months we believe it is preferable to continue using cryoprecipitate rather than treat patients with large donor pool concentrates, which otherwise they might never need receive. Adoption of the proposal by Shore *et al* that cryoprecipitate should be prepared only from the plasma of female donors would decrease the risks of HTLV-III infection during the interim.[6] This policy could be extended to fresh frozen plasma and platelet rich plasma, and plasma from male donors could be used for preparing NHS heat treated factor VIII and IX concentrate. Desmopressin and antifibrinolytic therapy should be used when possible to avoid treatment with blood products.

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Progesterone and the premenstrual syndrome: a double blind crossover trial

SIR,—The conclusions of the report looking at the therapeutic efficacy of progesterone in the premenstrual syndrome by Dr L Dennerstein and others (1 June, p 1617) prompt several comments.

Firstly, the authors, commendably, provide us with detailed information of their results, including the analysis of a series of paired *t* tests which were used to compare the effects of placebo and progesterone. However, the nature of the test, whether one or two tailed, is not stated, and that is critical in the interpretation of significance levels. In such a controlled study, where a considerable placebo response is to be expected, it would be incorrect to use a one tailed test as it is not possible to predict the direction of the difference between the treatments. This is confirmed by the authors' own results, which show that progesterone not only improved but also worsened certain symptoms compared with placebo (tables II and III). Under these conditions, the erroneous use of one tailed testing would have the effect of increasing the type I error of the results—that is, the chance of finding a statistically significant difference between progesterone and placebo when there is not one.

Therefore, on the assumption that two tailed paired *t* tests were used in the assessment, some of the significance levels quoted in favour of progesterone are overestimates. For instance, during the first month of treatment, progesterone is concluded to be significantly superior at the 5% level for the relief of symptoms of water retention in the menstrual distress questionnaire (table II: $t = 1{\cdot}95$, $p < 0{\cdot}05$). However, with 23 pairs of data and two tailed testing, t must be greater than 2·07 before a significance at the 5% level is proved.[1] In fact, three of the eight results tabulated as showing a significant benefit of progesterone over placebo in table II are actually not significant at the 5% level (water retention (menstrual distress question-

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