analysed completely. Nevertheless, the initial results in these prophylactic trials do not appear to support the totally negative view which arises from the experimental work on the therapeutic effect of ascorbic acid which has been reported from Salisbury. These results will be described when the analysis has been completed.—I am, etc.,

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Lead Absorption in Children

SIR,—The indiscriminate use of the terms absorption, exposure, intoxication, and poisoning by Dr. Neil Gordon and his colleagues (20 May, p. 480) is misleading. They measured only blood lead concentration and inferred that it reflected absorption.

The blood lead concentration represents an equilibrium in which absorption, excretion, and tissue deposition participate. Recent animal studies in this department have shown that lead can accumulate in the liver following oral administration of lead compounds without significant change in the blood lead. Conversely, while the significance of a raised blood lead is not disputed, it is not necessarily indicative of absorption but could represent release of tissue lead.

Lead poisoning in childhood usually occurs between the ages of 1 and 5 years; death and cerebral damage are well-documented sequelae.[1] It is questionable whether the measurements reported by Gordon and others in older children and adolescents are relevant in any way to the aetiology of mental handicap.—I am, etc.,

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REFERENCE

[1] Byers, R. K., *Pediatrics*, 1959, **23**, 585.

Treatment of Choriocarcinoma

SIR,—Dr. K. D. Bagshawe (15 April, p. 178) appears unduly concerned that our paper (4 March, p. 521) may have weakened the case for the treatment of choriocarcinoma in specialized centres such as his at Fulham Hospital. We wish to assure him that we have the highest regard for his good work, and, although we do not support the view that all cases of choriocarcinoma should be treated in specialized centres, we do agree that difficult problem cases should be channelled without delay to such centres for chemotherapy by experts like himself.

In his eagerness to defend his position Bagshawe has made certain statements which require examination. In the opening paragraph he says: "Before chemotherapy was applied to choriocarcinoma no clinician ever saw more than a few cases." This categorical statement may have been true of clinicians in Britain, but not in Asia. Before chemotherapy was introduced into our hospital we had seen two dozen cases or more, and there must be other clinicians in Hong Kong, Indonesia, the Philippines, and other parts of Asia who have seen many more. Such inaccurate statements do not strengthen the cause which Bagshawe is trying to promote.

Two papers on choriocarcinoma from Singapore have recently appeared.[1,2] Bagshawe compared the figures and concluded: "The virtually simultaneous presentation of totally opposite conclusions, drawn from the same data, must cast doubt on their value." This hasty conclusion ignores the fact that the first paper[1] was on pulmonary choriocarcinoma and the second[2] was on all types of choriocarcinoma. Our second paper also included more recent material than that reported in the first, although the duration of follow-up was up to 1966 in both presentations. There is no contradiction in our conclusions or discrepancy in our figures. Neither was there recourse to "spontaneous resurrection," as Bagshawe terms it. The careful reader will find that Bagshawe's assumption is in error. The deaths in the "hysterectomy-chemotherapy" group were 12 in both papers, with one case under treatment. Bagshawe repeatedly refers to our cases of malignant trophoblastic tumours as "hydatidiform mole." He may disagree with our classification, but there is no need to misrepresent the facts.

We find it difficult to understand the self-contradiction in Bagshawe's thinking on the value of hysterectomy. To quote him: "The reader is led to believe that some workers have claimed that hysterectomy is valueless. . . . Such claims have not, I think, been made. . . ." Here he appears to hold no bias against hysterectomy. Then, in the second last paragraph he describes at length the evils of elective hysterectomy and concludes, "unless the uterus has perforated, hysterectomy is better deferred." Uterine perforation is a rare complication in choriocarcinoma. This means that Bagshawe opposes hysterectomy in the majority of cases. In his paper,[3] which we had referred to, Bagshawe also elaborated on the alleged evils of hysterectomy and concluded: "Hysterectomy is thus not only ineffective but also disadvantageous in some patients." Let his own statements be his judge. In our paper we had advocated that such claims "deserve careful study before hysterectomy is abandoned altogether." Indeed, unless a strong protest were raised these claims (based on flimsy evidence) would mislead gynaecologists into giving up hysterectomy altogether.

In our paper we have not claimed that our "results equal those obtained elsewhere," as Bagshawe asserts. We have reported the results of all 80 consecutive cases of malignant trophoblastic tumours seen, treated, or both, irrespective of the state on admission to hospital. We have included no fewer than 12 deaths in patients who did not have the benefit of chemotherapy. Bagshawe's results were based on 23 treated cases out of 28 seen. The hopeless cases were excluded from his results. Obviously no worthwhile conclusions could have been drawn by trying to compare our results with his.

Bagshawe places great faith in the Joint Project[4] which amassed "806 cases of suspected trophoblastic tumours" from 18 different centres in Asia over the period 1948–52 for study by a panel of pathologists in the U.S.A. The non-representative nature of this pooled material led the authors themselves to conclude that "an accurate ratio of choriocarcinoma to hydatidiform mole cannot be established . . . and there is no way of estimating whether the large number of choriocarcinomas is due in part to a large number of hydatidiform moles." Yet Bagshawe makes the bold statement that the Joint Project "found no evidence that mole was a relatively more frequent antecedent to choriocarcinoma in South-east Asia than in the U.S.A." We are of the opinion that the value of the Joint Project is seriously limited by the absurdly small number of cases studied—806 out of a population of several hundred millions or more. Your readers might be interested to know that for the four-year study period of the Joint Project the Singapore participants contributed 41 cases and that these were rejected from the final analysis because of inadequate data. We have studied over 500 consecutive cases in the past seven years. Our results may be expected to give a more truly representative and accurate picture than the collation of hotch-potch data from 18 different sources.

We find Bagshawe's mathematics perplexing. In calculating the ratio of molar to non-molar pregnancies in the last 25 cases of our series he arrives at the figure of 12.5:1. We are at a loss how this figure was arrived at. He states that the "expected number of fatal cases without treatment" in our series would be "about 24." On what grounds? He also states that our "fatality rate is twice that of a somewhat larger series" from his own hospital. May we suggest that all these unfounded statements and self-contradictions do not strengthen the case for specialized centres and may weaken the faith of Bagshawe's supporters. Finally, may we reassure Bagshawe that we do not oppose the treatment of problem cases in his specialized unit, but we do strongly contest the unfair allegations which he has made against hysterectomy.—We are, etc.,

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REFERENCES

[1] Tow, W. S. H., *Proc. roy. Soc. Med.*, 1967, **60**, 239.
[2] —— and Cheng, W. C., *Brit. med. J.*, 1967, **1**, 521.
[3] Bagshawe, K. D., ibid., 1963, **2**, 1303.
[4] Joint Project for Study of Choriocarcinoma and Hydatidiform Mole in Asia, *Ann. N.Y. Acad. Sci.*, 1959, **80**, 178.

Pressurized Aerosols in Asthma

SIR,—I am grateful for your published disclaimer (27 May, p. 584) that I do not support the views of Dr. R. Munro Ford (6 May, p. 375) or others of your correspondents who condemn the use of aerosol preparations in the treatment of bronchial asthma. Indeed, the agent which I find most useful and use most frequently in the treatment of chronic asthma is 5% ocipreneline delivered from a De Vilbiss No. 40 hand nebulizer. As with other medications, I take pains to explain clearly to the patient the way in which it should be used and its limitations.

I deplore the present "hue and cry" approach to the problem of deaths from asthma, and still more the emotional seizing on one valuable form of therapy. A recent paper by Tai and myself[1] indicates how tenuous is the functional state of many patients with asthma who do not appear clinically very ill. This paper suggests that we should look afresh at our abilities in the field of "expectation," rather than talk too glibly of "unexpected" deaths from asthma.—I am, etc.,

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REFERENCE

[1] Tai, E., and Read, J., *Lancet*, 1967, **1**, 644.

SIR,—I have been following with great interest the correspondence in your columns concerning the excessive use of pressurized aerosols in asthma. Little has so far been written about the older asthmatic child in this context apart from the very instructive case described by Dr. W. Pickvance (25 March, p.