SIR,—Clark and Renfrew[1] state that "the hypothesis of a common calibration curve for the bristlecone pine and Egyptian data over the time period 3100 BC to 1800 BC is not contradicted and suggestions to the contrary are not well founded". But according to Fig. 2 of ref. 1, all but two of the 14 Egyptian data points for the period 2200 BC to 1800 BC indicate ^{14}C dates that are less recent than indicated by the curve shown for the bristlecone pine, on average by one or two centuries. Consequently the statistical conclusion is difficult to accept; this viewpoint was put to one of the authors of ref. 1 by McKerrell at the 1973 Oxford Archaeometry Symposium, in support of his contention[2] that the bristlecone pine calibration is not consistent with the Egyptian data; but no explanation of the apparent paradox was then offered.

A close inspection of the figure gives an explanation. In the period 2100 BC to 1900 BC there is a substantial difference between the bristlecone pine curve and the tree ring data points that were used for the statistical analysis; this difference is in the same sense as for the Egyptian data. So the analysis by Clark and Renfrew cannot be held to contradict McKerrell's contention.

The tree ring data were taken from the unpublished thesis of Houtermans[3] and comparison of these with the data points shown by Suess on the bristlecone pine curve[4] confirms that there are differences between the two sets; since both sets originate from the same measurements one must presume that corrections have been applied to one set which have not been applied to the other. In the absence of other indications it seems reasonable to accept the published data rather than the unpublished. It is also relevant that using either of the bristlecone pine curves put forward by the Pennsylvania[5] and Arizona[6] radiocarbon laboratories (which incorporate the Suess data with later measurements) the Egyptian ^{14}C dates shown for $\sim$ 2000 BC are still significantly less recent than bristlecone pine ^{14}C dates, though by not quite so much.

Yours faithfully,

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DRS CLARK AND RENFREW REPLY: We agree with many of the points made by Baxter and in a paper now in the press[1] discuss the substantial statistical problems involved in constructing an adequate calibration curve from the data available. But the construction of a calibration curve was not, as Baxter seems to imply, the purpose of our recent paper[2]. Instead an evaluation was sought of the general applicability of the bristlecone pine data and their associated radiocarbon dates. The radiocarbon dates obtained from dendrochronologically dated bristlecone pine samples will match radiocarbon dates from historically/archaeologically dated Egyptian samples only if satisfactory answers could be given to his three questions: a precise equivalence would give exactly the independent corroboration which he is seeking. In fact, although the two sets do not contradict within the limits of their associated errors, the match is an inexact one because of the magnitude of those associated errors.

There should be no question here of overworking experimental data. The procedure used was to present, apparently for the first time, a formal analysis of the consistency of the determinations from different laboratories, working always through a consideration of the measurement errors involved (both as reported by the laboratories themselves, and as established from replicate analyses). Is this not in fact the only acceptable way of comparing two independent data sets?

The geophysical problems which Baxter indicates are not new, and will only be answered by statistical analysis of existing data and by the gathering of new data in such a way that it will be susceptible to statistical analysis. He is certainly right to stress that the appropriate form of the calibration curve is not yet clear, and that an error of perhaps ±200 yr is to be associated with the curve[3].

Aitken is correct that the data presented by Houtermans and by Suess are not in precise agreement: we chose to use the former because they were more recent and did not have to be read from a graph. Aitken's reference to Fig. 2 of our paper is not, however, to the curves used by us to compare the bristlecone pine and Egyptian data, but to Suess's original curve presented there for purposes of comparison only. If the data points of Fig. 2 are compared with the curves derived from those data, there is no contradiction. Consider, for example, the least-squares straight line. If the hypothesis (of a common calibration curve) is correct, one would expect roughly half of the data points to lie below this line. Inspection of Fig. 2 shows that, between 2200 and 1800 BC: (i) 5 of the 14 Egyptian points are below this line; (ii) 10 of the 19 bristlecone pine points are below this line.

Such a distribution of the 14 Egyptian points is not unusual; the probability of five or fewer points out of 14 being below the line is about 0.20 (from the Binomial distribution), so that there is no reason to doubt on this basis that the Egyptian points satisfy the same calibration relationship as the bristlecone pine data. The same argument could be applied to the entire interval from 3000 to 1800 BC. Figure 2 shows that 23 of the 52 Egyptian points lie below the fitted straight line, whereas 27 of the 57 bristlecone pine points lie below this line. One could hardly expect to get much closer to 50% than that.

It remains our conclusion that, on the basis of the available data considered, and within the error limits discussed (themselves unfortunately wide), there is no contradiction between the Egyptian and bristlecone pine data. This is an important conclusion since for some parts of prehistoric Europe the calibrated radiocarbon chronology differs by as much as 2,000 yr from archaeological chronologies put forward before the application of radiocarbon dating. At the same time, as Baxter rightly stresses, the error limits are large, and much more work will be needed before the form of the appropriate calibration curve will be known with precision.

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Na^+ transport defect in cystic fibrosis

SIR—Using the technique of ^{3}H-ouabain binding to isolated cells Quissell and Pitot concluded that the Na^+–K^+ ATPase functions normally in fibroblasts from cystic fibrosis patients[1]. However this finding may not be very revelant to the Na^+ transport defect occurring in this genetic disease.

The Na^+ transport abnormality in cystic fibrosis seems to be confined to exocrine glands[2]. It is unknown at present whether the defect resides in an active or passive transport step of the cation. At least with isolated sweat