ice sheets were initially triggered by massive volcanic eruptions is not contradicted by the geological record". But until further data are available from Patagonia, and especially studies of the ash deposits by volcanologists, the evidence from southern South America should not be used either to support or contradict the argument.

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BRAY REPLIES—The main point of Francis[1] is that the evidence of late Pleistocene and Holocene volcanism in southern South America[2] is based on a small number of published radiocarbon dates. The reason I originally included the southern South America volcanic data was that both the radiocarbon-dated ashes and the indirectly dated ashes all occurred at the same time as periods of volcanic activity in New Zealand and Japan. This occurrence seemed to be more than coincidental and I suggested that the apparent synchroneity of volcanic ash data from the three areas reinforced the hypothesis that there have been global waves of volcanic acitivity separated by periods of relative quiescence. Since then, South American volcanic ash dates of 9,000, 11,000, 21,000, 22,000–26,000 and 30,000 yr BP have been published[3] which are also coincident with the New Zealand and Japanese volcanic phases. In addition, I have made a preliminary compilation of North American and European volcanic ash dates which show the following distribution (in 1,000 ^{14}C yr b.p.): 0.2–0.4; 0.6–0.8; 1.2–1.3; 1.6–1.8; 2.0–2.6; 2.8; 3.3–3.5; 3.9; 4.4; 5.0; 6.6; 8.3–8.9; 11.3; 11.7–12.0; 13.1; 18.6; 19.7; 28.5; 31.0; 35.0; 39.5–40.5. A comparison of these dates with the New Zealand and Japanese data in Table 1 (ref. 2) shows that all but five dates are synchronous.

Francis' other point, that some of the southern South American eruptions were minor is correct, but not contradictory to the theme of global waves of volcanic eruptions in the late Pleistocene which have influenced climate and perhaps triggered glacial expansion. In any case, if the large mid-continental Northern Hemisphere ice sheets were triggered by massive eruptions[4], then it is likely that these eruptions occurred in the Northern Hemisphere or perhaps the tropical portion of the Southern Hemisphere. It is in these regions that many of the largest Pleistocene eruptions have occurred as shown by the land record and especially by the ash layers in the ocean[5].

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Acoustic Bragg diffraction from human tissues

NICHOLAS AND HILL[1] claim to have presented "qualitative evidence that the method can provide objective characterisation of tissue structure", and that the "diffraction phenomenon . . . can be quantitatively related to the specific structure of the type of tissue".

They present their evidence for the objective characterisation in Fig. 4, where three sets of tissue specimens (of unspecified condition) have a statistical significant difference over a portion of the curve, the curve having been obtained by an arbitrary seven-point smoothing. Whereas the first claim of the authors may be true, the second, regarding the specific structure of the type of tissue, is surely not substantiated by the evidence presented. Differentiation between tissue types may have been demonstrated but there is no evidence presented for its relationship to specific structure. Indeed, it is known[2,3] that the Fourier transform of angular correlation is a spatial spectrum, so that the peaks in the spectrum of Nicholas and Hill's Fig. 3 indicate distances of significance in the tissue specimens, presumably related to structure, as the parallel with X-ray diffraction suggests. This potential source of helpful fundamental information has been removed by cavalier smoothing.

In a field characterised by complexity[4], bedevilled by incomplete reporting[5], and yearning for independent confirmation, the significance of the results reported[1] demands more careful and precise consideration.

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NICHOLAS AND HILL REPLY—We accept the criticism[1] of our statement[2] that "this diffraction phenomenon . . . can be quantitatively related to the specific structure of the type of tissue". This statement is out of place in the article concerned and should have been omitted: its truth, however, has been confirmed in other publications[3,4]. The other points raised by Chivers are, in our view, unjustified and in some cases inaccurate.

In all of our investigations the tissues were freshly excised, pathologically normal specimens: although this was not stated explicitly it was thought that the implication of comparing pathologically abnormal tissues with the reported results, justified such an omission.

Chivers' criticism[1] of the curves depicted in our original Fig. 4 (ref. 2)—that the range of the seven-point average was not specified—is, we believe, taken in context, not valid. It was stated in our report[2] that the power spectra were calculated using a fast Fourier transform and, as such, they are both finite and discrete. We also stated that the data in each resulting spectrum was averaged over consecutive seven-points. Thus it is that the range of our averaging was over the whole spectrum, as should be obvious if Fig. 4 is compared with Fig. 3.

Finally, Chivers' comments on the validity of our smoothing procedure should be considered in the light of our objective. We clearly stated that "several possible analytical approaches could be taken towards establishing the significance of differences or similarities between such traces and we report here the results of one of these". Useful information has indeed been removed by smoothing, but the advantage gained is the relative ease with which different traces can thus be compared. It is apparent from our Fig. 3 that the peak separation is of the order of 0.01 cycles per degree, which corresponds to a separation in the angular traces of Fig. 2 of approximately 100°; hardly a significant contribution when considering the 50° of trace illustrated. Chivers' comments on this point, although strictly valid, are therefore of limited relevance to the results presented. We did not imply that the peaks in the power spectrum were artefacts, but merely indicated that the picket-fence effect must have led to some degradation of the resulting power spectra.

Our article was intended purely as an introduction to a new and exciting aspect of tissue characterisation and did not claim to be a complete report. It remains our belief, however, that the results we presented are both significant and precisely defined.

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