CORRESPONDENCE

Lunar Conductivity

SIR,—Your geomagnetism correspondent[1] commented on the problem of lunar conductivity where he reviewed the current status of the problem of inversion of lunar magnetometer data into a profile of the bulk electrical conductivity. His statements tend to centre only on the recent work of Hobbs[2] where the uniqueness of lunar data is discussed in terms of the Backus–Gilbert resolution criterion[3–5]. Both the comments in *Nature* and those of Hobbs are based on our very first preliminary report[6], now over two years old, of the lunar transfer (response) function and the inversion resulting from these data. Thus, the comments in *Nature* completely bypass our later reports[7,8] which extend the data both in accuracy and bandwidth as well as making use of a theory in the inversion which includes higher magnetic multipoles of the fields. The discussion in *Nature* and that by Hobbs, both of historical interest only, unfortunately do not give the background upon which our publication of the conductivity spike was based. In that work, there was some evidence that the transfer function "rolled over" beyond a frequency of about 0.01 Hz. In addition, the basalt lunar rock conductivity work published by Nagata *et al.*[9], when used together with published values of typical ultrabasic rock conductivity, provided a smooth temperature profile. The latter must be regarded as a key element in our early conclusion since the existence of the "rollover" phenomenon forced our solutions towards a "spike" irrespective of a variety of starting conditions in the iteration. The importance of this can best be understood when it is noted that in Hobbs's calculations the iterations and resolution tests are restricted to an upper bandwidth limit of 0.01 Hz, exactly where difficulty ensues in fitting a monotonic conductivity function to the data. The spike can thus be avoided by the use of dipole (or long wavelength) theory, and the combination of ultrabasic and Nagata-basalt conductivity functions which led to the conclusion that a conductivity "spike" exists in the lunar mantle.

If Hobbs had used our later data, published well before his paper and over the full frequency range, he should have reproduced our results showing that higher order magnetic multipoles are responsible for the prominent "rollover" in the lunar transfer function, that the higher order theory is indeed required in iteration, and that we had already produced models which are bounded by the Backus–Gilbert criterion.

The latter is no panacea for testing the uniqueness of lunar magnetometer data. Indeed, for the very limited resolution presently available, a strong argument can be made that better intuitive physical insight is gained by considering a spectrum of electrically equivalent models, just as we did. Further, Backus–Gilbert calculations carried out by us even earlier than the generation of our present model spectrum (unpublished) show no disparity with the set consisting of two-layer, three-layer, four-layer, core-plus-current layer, and two-current layer models as determined by the residual mean square error difference between model forward calculations and the data, and the mean square average noise in the data itself[7,8].

The Backus–Gilbert criterion, exquisite though it is, provides no intrinsic measure of confidence in the models beyond what we already have calculated, considering the overall quality of the data, and perhaps even more critical, the "goodness" of the theory which will always remain an external constraint upon the final model attained. A more careful reading of the literature would show that, though the time for use of the Backus–Gilbert criterion may be fast approaching, there is still considerable work to be done to improve the inversion by inclusion of the full higher-order theory arising from cavity asymmetry, short wavelengths in the solar wind, separation of data according to propagation vector direction, and a host of smaller but nevertheless still germane problems. In short, Backus–Gilbert tests are not required to show that the "spike", though still with us, is non-unique. Finally, the Kuckes two-layer model[10], or indeed any two-layer model, has already been shown by us to lie outside the error tolerance of the data[8].

Yours faithfully,

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Missing Magnitudes

SIR,—The recent article on polypeptide configurational kinetics entitled "The Missing Magnitudes" (*Nature*, **243**, 186; 1973) is, we feel, premature in its conclusions. An explanation, based on sample polydispersity, of differences between the two ranges of molecular times inferred from experiments is inadequate. One of us has recently fractionated poly-γ-benzyl-L-glutamate (J. B. Milstein and J. A. Ferretti, *Biopolymers*, in the press). The n.m.r. spectra of sharp fractions exhibit α C-H proton peak doubling, in direct contradiction of results by Nagayama and Wada (*Chem. Phys. Lett.*, **16**, 50 ; 1972 ; and *Biopolymers*, in the press). Thus the problem remains unresolved.

We assert that an explanation based on polydispersity cannot logically be extended to comprehend the experimental chain length dependence of the double peaks. Therefore, we would still maintain that the origin of the n.m.r. double peaks lies in the existence of a slow nucleation time ; in terms of our models this is the time for forming an initial α-helix from an all random coil molecule.

Even if this is not the finally accepted explanation of the n.m.r. behaviour, a treatment of helix nucleation in chain molecules is requisite to a detailed understanding of conformational kinetics. Theories developed by Miller (*Macromolecules*, **6**, 100 ; 1973) and by Jernigan, Weiss and Ferretti (*Macromolecules*, in the press) should have applications to time dependent properties