Geomagnetic excursions and climate change

KENT has demonstrated rather convincingly that the intensity of natural remanent magnetism (NRM) in deep-sea sediments is sensitive to changes in sediment type, and hence is not an accurate indicator of the true strength of the geomagnetic field. The reported correlations between NRM intensity and climatic parameters in deep-sea cores may be largely or entirely a function of climatically induced changes in core lithology.

This explanation is not, however, directly relevant to suggested correlations between climate change and other aspects of the geomagnetic field. For example, excursions of the Earth’s magnetic field, as recorded in sediments, have been correlated with climate fluctuations and with variations in the eccentricity of the Earth’s orbit. These excursions appear to represent changes in magnetic inclination and/or declination and do not necessarily involve variations of NRM intensity. The recorded excursions are not readily explained by changes in lithology and magnetic mineral content, although some workers have suggested that excursions in sediment cores are the result of disturbances of the deposits before, during or after sampling. The discovery of several of the excursions in non-sedimentary materials, such as lava-flow sequences and ocean-floor magnetic striping, lends support to the idea that these events were real fluctuations of the geomagnetic field.

The proposed connection between geomagnetic excursions and climate may come about through a direct cause and effect relationship involving fluctuations in field strength that can accompany reversals and excursions, atmospheric effects of variations in geomagnetic pole position, or perhaps some other mechanism. It is possible that excursions and climate are only secondarily related and that orbital factors are the driving force for both phenomena. Seven of the reported excursions during the last 700,000 yr seem to correspond to peaks in the approximately 100,000-yr eccentricity cycle. These excursions are sometimes, but not always, correlated with changes in NRM intensity in cores. Therefore, although Kent’s work casts doubt on the significance of correlations of climate with NRM measurements of field strength, it does not offer an alternative explanation for the proposed connections between excursions, climate and orbital parameters.

MICHAEL R. RAMPINO

NASA, Goddard Institute for Space Studies,
New York 10025, USA

I argue that these intervals, and in fact the magnetic data from the entire upper 300 cm of V20-108, are unlikely to be a representation of the geomagnetic field. In two nearby cores—V20-107 taken 227 km to the south and V20-109 taken only 208 km to the north of V20-108—the magnetic data are of high internal consistency and no such anomalous inclinations are observed in the Brunhes sediment. Such localized occurrence of large departures from an axial dipole field direction is difficult to explain with sources in the Earth’s core and is more likely attributable to a distorted record. Indeed, analysis of the Lamont coring and curatorial logs for V20-108 reveals that at least the upper 300 cm of the sediment was drawn through a bent pipe during coring, a condition that could easily lead to disturbance of the sediment and the magnetic record.

The same logs moreover show that the sediment core was broken in several places on board the ship, in particular at 298 cm to facilitate transport and at 460 cm where coring pipes were uncoupled; single-sample measurements of anomalous magnetic inclination closely correspond to these levels. In view of these observations, the interpretation of anomalous inclinations in V20-108 as records of geomagnetic field excursions and their correlation to global phenomena is highly dubious.

This illustration is not meant to imply that all geomagnetic excursions or short-polarity intervals reported in the literature are artefacts of poor or distorted records; several, such as the Mono Lake excursion recorded in sediments, and the Cobb Mt microchron in lavas, appear to be well documented. Rather, this unfortunate choice of data suggests that a more critical and conservative attitude should be taken in the documentation of geomagnetic field behaviour, considering the numerous and more mundane sources of anomalous palaeomagnetic data. A much improved knowledge of geomagnetic excursions and short-polarity intervals is required not only to provide a legitimate basis for comparison with the far better established record of Pleistocene climate change but also to gain a more fundamental understanding of the entire spectrum of geomagnetic phenomena.

This work was supported by grant OCE 81-19695. Lamont-Doherty Geological Observatory contribution 3452.

D. V. KENT

Lamont-Doherty Geological Observatory and Department of Geological Sciences, Columbia University, Palisades, New York 10964, USA


References:

Female choice in widowbirds

FIELD studies of the long-tailed widowbird, *Euplectes progne*, by Andersson1 involved experimental manipulation of the tail-lengths of territorial males and measurement of their breeding success in terms of the number of females that nested in the male's territory. The Darwin–Fisher theory2,3 of the role of sexual selection in the evolution of bird plumage would predict that females should prefer to mate with males with longer tails. Although Andersson interprets his data as supporting this prediction, we feel that this may not be justified in view of the following oversights.

First, the number of new nests was not least for males with shortened tails as claimed, there being no difference between males with short tails and normal males (four new nests in each case). Second, Andersson claims1 that any tendency of females to mate in one territory but nest in another (which is known to occur in other polygynous birds4) would fail to produce a spurious bias in favour of the Darwin–Fisher theory. This claim is only justified, however, if the Darwin–Fisher theory is true and females prefer to mate with long-tailed males but not in the territories of other males. If the theory is not true, and females mate randomly or with a preference for short-tailed males but nest in the territories of long-tailed males, such behaviour could produce a spurious bias in the results in favour of the theory.

Third, over the range of tail lengths that sexual selection would be assumed to have acted in the evolutionary past (that is, short to normal tail), there is no hint that females prefer longer tails (for example, no correlation between tail length and number of nests on the territory before treatment began; absolutely no difference between short-tailed and normal males; and a tendency to prefer normal-but-cut tails to normal males even though the tails of the former were on average 1 cm shorter). Males with long tails did significantly better than normal males with shortened tails, but did not do significantly better than males with normal-but-cut tails. The proper conclusion from the data should therefore be that females preferred to nest in the territories of males with 25 cm of feathers glued on to them, irrespective of the total tail length. Such a conclusion could be interpreted in terms of predation hypotheses for the evolution of bird coloration5.

We suggest the reason that female widowbirds prefer to nest in the territories of males with long feathers glued on to them, irrespective of total tail length, is that such males, through marginally more ponderous flight, are better decoys than normal or short-tailed males. Perhaps in a non-experimental context females choose to nest in the territories of males that happen to be good decoys through other non-genetic factors (for example, age, senility, damage). It does not follow, necessarily, that females also choose to mate with such males.

R. ROBIN BAKER

Department of Zoology, University of Manchester, Manchester M13 9PL, UK

G. A. PARKER

Department of Zoology, University of Liverpool, Liverpool L69 3BX, UK

ANDERSSON REPLIES—Baker and Parker question the assumption (supported in other *Euplectes* species) that females tend to nest on the territory of the male with which they mate. If females mate randomly or prefer short-tailed males, but nest in territories of elongated males, the result is not evidence for Darwin's hypothesis. However, there seems no plausible reason to expect such a peculiar difference in female choice of nest site as opposed to mate.

In their other two points, Baker and Parker’s criticism depends entirely on their unsupported assumption that the (small and nonsignificant; $P>0.6$) difference between control males I and II did not arise from chance variation, but because elongated and cut-and-restored males through ‘marginally more ponderous flight’ attracted more females to nest (but not mate) on their territories. However, there is no indication that cut-and-restored males had more ponderous flight than the other control males. They showed almost identical changes in mean rates of flight display and attack after manipulation. Moreover, because feather vane was removed along the 1 cm joint to avoid vane overlap, the operation lightened each cut-and-restored feather by ~0.5 mg, including glue. The two parts of the feather were carefully aligned, and two specialists on bird flight, Drs Ulla and Ake Norberg, on inspection considered any noticeable effect on the bird’s flight performance as highly unlikely. The two control groups should therefore represent one single category from an aerodynamic as well as from the females’ point of view. Their difference of 1 cm compared with a total tail length of ~50 cm would seem too small for females to notice; the difference to the other two groups was ~25 cm. It is therefore unlikely that the cut-and-glue operation rather than changes in male tail length caused the significant trend in the result. Further, because cut-and-restored males were more successful than ‘shortened’ males, there is a hint that females preferred longer tails also within the normal range of lengths, contrary to the claim of Baker and Parker.

They favour their own predation hypothesis6, and suggest that females chose to nest on the territories of the best ‘decoy’ males, most likely to display and attract females to themselves and hence to reduce the risk for female and young. But attraction of predators to the territory might just as well raise the risk also for the female and young. This alternative explanation therefore seems logically doubtful, in addition to being based on very unrealistic assumptions about differences between the control groups.

I therefore think it is most reasonable to interpret the trend of higher male success with increased tail length after manipulation as evidence that tail length influenced female choice as suggested by the Darwin–Fisher theory. However, further experiments are desirable in this or other species with similar ornaments and mating system, such as whydahs, peacocks and other pheasants.

I thank Frank Gömterm and Åke Norberg for helpful comments.

MALTE ANDERSSON

Department of Zoology, University of Gothenburg, PO Box 250 59, S-400 31 Gothenburg, Sweden

Matters Arising

Matters Arising is meant as a vehicle for comment and discussion about papers that appear in *Nature*. The originator of a Matters Arising contribution should initially send his manuscript to the author of the original paper and both parties should, wherever possible, agree on what is to be submitted. Neither contribution nor reply (if one is necessary) should be longer than 500 words and the briefest of replies, to the effect that a point is taken, should be considered.