news and views

No consensus yet on climate

In this issue of *Nature* (pages 368 and 370) is another instalment of the saga in which non-meteorologists suggest to the professionals processes, hitherto unrepresented, that may account for their lack of forecasting skill. The meteorologists' usual response is, as Sawyer's here, that it is not dubious new processes that are required but better understanding of those already known to be important.

Meteorologists are acutely aware that their data are so abundant that fortuitous associations of some of them with unrelated phenomena are bound to exist. To some extent King's scholarly conjectures about the association of magnetic field and climate are liable to this reservation. Sawyer shows, moreover, that there has been some selection of data in King's quoted evidence—the association he describes is much better for latitude 60° N in the winter than for other latitudes and seasons.

But I was still a little surprised at Sawyer's comment which is based on the fidelity of current numerical models of the atmosphere and in particular with his degree of confidence in what is, after all, one of the less sophisticated models of global-scale processes. To my mind this is deftly and succinctly qualified by King's reply, and one should indeed bear in mind a further qualification. The numerical models of atmospheric circulation do not represent quite such pure physics as might be supposed at first sight, for they have to be 'tuned'. By this process values of parameters appearing in the models are adjusted to make the model behave more realistically—such processes as surface friction, the transfer of energy by electromagnetic radiation and many cloud effects usually have to be treated in this way. Although this is a legitimate way of establishing a mutually consistent set of interacting processes, there is clearly room for King's as yet unidentified process.

The evidence for an association of geomagnetic and meteorological patterns presented by King is tempting (correlations of 0.96 are not to be sneezed at). Causal connection is a remote possibility awaiting a plausible quantifiable hypothesis that is not even remotely hinted at by King. His speculation that "Coriolis force, inertia and viscosity of the air" may account for some differences seems naive. Meteorologists know about such things and have taken them into account in many elegant and detailed treatments which show how the atmosphere responds to any forcing, whether by anomalous heat sources, undulation of surface elevation or indeed ill-defined but energetically small magnetic effects. Such analysis shows that motion of large scale penetrates upwards high into the atmosphere, where the energy is partially reflected downwards. Such reflection is governed by the large-scale properties of the upper layers—like the general wind and temperature structure, which may indeed be conditioned by some much less intense energy sources.

Given finally that the large scale systems may be close to resonance (particularly in the winter) we have the beginnings of some process by which climate may be modified by weak energy sources, especially those in the upper atmosphere but, it must be stressed, dependent not on local anomalies of energy but on variability of the global scale circulation.

If causal effects of one on the other are unlikely, is it possible that both climate and magnetic field perturbations have a common origin? It is widely believed that the details of the geomagnetic field originate at the core-mantle boundary, if not in the core itself—an unlikely spot, one would think, for climate to be controlled from. But the obvious influence of continents on climate and the less obvious influence of continents on upper-mantle resistivity profiles could, just conceivably, be a link—the upper mantle being a window through which core effects are seen.

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The self control of myosin

Two recent papers have reopened the debate about the existence of a Ca^{2+}-sensitising mechanism associated with the thick filaments in vertebrate muscle.

By the late 1960s it was generally believed that contraction of vertebrate skeletal muscle—and by assumption also of other muscles—was triggered solely by the binding of Ca^{2+} ions to the troponin components of the thin filaments. The evidence for this was that troponin was apparently the only myofibrillar protein which could reversibly bind Ca^{2+} in the presence of Mg^{2+} ions and that troponin, together with tropomyosin, was required to confer Ca^{2+} sensitivity on the ATPase of vertebrate skeletal actomyosin.

The idea that all muscles were controlled through the thin filaments in this way was scotched by a series of papers by Szent-Györgyi, Kendrick-Jones and Lehman. They were able to show that whereas some invertebrate muscles had this troponin control, others (most notably certain molluscan muscles like the cross-striated adductor muscle of *Pecten*) lacked troponin but instead contained a special kind of myosin which could bind Ca^{2+} in the presence of Mg^{2+} ions. Correspondingly, the actin-stimulated myosin ATPase from such muscles was Ca^{2+} sensitive. In other words, such muscles were controlled through the myosin of their thick filaments. Yet other invertebrate muscles had both forms of control. The test devised to determine which kind of control is present in any given muscle is simple. The effect of adding pure actin to the contractile apparatus is observed. If the Ca^{2+} sensitivity (that is the ratio of the ATPase activity in the presence and absence of Ca^{2+} ions) is reduced, then only the troponin control must be present since the thin filament activity has been swamped by the excess unregulated actin. But if the sensitivity is unaffected there must be myosin control.

Szent-Györgyi *et al.* examined both smooth and striated muscles of invertebrates but only the striated muscles of vertebrates. Now Bremel (this issue of *Nature*, page 405) has applied their test to a vertebrate smooth muscle (chicken gizzard), about whose control little is known. The