Book Reviews

A Practical Question in Astronomy

Possible Relationships between Solar Activity and Meteorological Phenomena. Proceedings of a symposium, Greenbelt, Md., Nov. 1973. WILLIAM R. BANDEEN and STEPHEN P. MARAN, Eds. National Aeronautics and Space Administration, Washington, D.C., 1975 (available from the Superintendent of Documents, Washington, D.C.). x, 264 pp., illus. Paper, $4. NASA SP-366.

This volume reports the proceedings of a symposium held at the NASA Goddard Space Flight Center in November 1973. It was a historic occasion: the last public appearance of Charles Greeley Abbot, faithful shepherd of the solar constant and longtime advocate of solar influence on local weather. The conference was dedicated to Abbot, who died, aged 101, before the following month had gone. And while the meeting did not produce any breakthroughs in sun-and-weather studies, it was certainly a notable event in the much-maligned field: not so much for the papers that were presented—for they were unspectacular—as for the simple act of bringing the subject out of the closet into the light of day.

The symposium sought to answer what is one of the few practical questions in all astronomy: Do changes on the sun affect the weather and, if so, how? It is an honest question that has been around, unanswered, for a long, long time. It is the question most often asked of solar astronomers by what I call the "wily layman"—the annoying and odious inquiry that pops up at the end of popular talks on the sun. And it is a question that clever astronomers have by and large avoided, for sun-and-weather study has been tainted by unproven claims and suspected data selection and tangled in a hundred years of snarled statistics. Such scientists as do get involved in it get more and more defensive and less and less constrained. The only winner in this spiral is the press, which, it seems, is always hungry for a story that links the sun or sunspots to some weather anomaly.

Aggravating the situation, I suspect, are emotional and civic overtones. Does *anyone* have a detached view on the subject? I have found that most people, scientists and otherwise, badly want there to be a connection between weather or climate and the sun. The sun surely drives the weather machine, so why aren't the variations in the one linked to the vagaries of the other? More important—and here is where objectivity ends—the sun is regular and to a certain extent predictable, with cycles, evident in sunspots, of 11, 22, 80, and who knows how many years, and an extended interplanetary atmosphere that is nicely divided into sectors of opposite magnetic polarity that sweep around past us. If we could only find the key we could use this regularity of an easily observed object to predict changes in our weather and climate, coming to the aid of an oppressed and hungry world and proving for all to see that astronomy, and science itself, are useful after all. Thus, if the mark of the sun is not obvious in weather records (and unfortunately it isn't), it must be hiding. If the sun is stable in its significant energy outflow (and it now seems to be), then there must exist a "trigger mechanism" that amplifies one of its insignificant energy changes in particles or fields to shake the mighty troposphere. The original question of whether there is a connection soon slips into the old rut: since the statistical evidence is unclear, let us get to work on the possible mechanisms. Since sun-and-weather study stands at the frontier of science, let us invoke frontier justice: bring in the guilty bastard (the sun) and we'll give him a fair trial!

The trouble, as the old-line meteorologists keep pointing out, is that weather and climate changes can happen in spite of the sun. Observable solar changes might have tropospheric effects, but not necessarily. The earth could enjoy dramatic weather and varied climate, with droughts and heat waves and ice ages, even if the sun were locked to a perfect thermostat and didn't change its outputs at all, in photons, particles, or whatever. There are lots of inputs to the weather machine, and many of them are intrinsically more variable, more immediate in their effects, and more poorly known than the rather stable sun. Moreover, if nature works in her usual way these causes are probably all at work all the time, intertwined and inconsistent in dominance, so that the attempt to isolate one of them as a predictor is probably unrealistic.

There are, as well, different regimes of change in the atmosphere, which may have different balances of cause: very long ones which could be dominated by earth-orbit changes (see J. D. Hays, J. Imbrie, N. J. Shackleton, *Science* **194**, 1121 [1976]), intermediate ones of hundreds or thousands of years for which the sun may well be important (see J. A. Eddy, *Science* **192**, 1189 [1976]), and the more immediate day-to-day and year-to-year ones which may be intrinsically unpredictable in a complex, global atmosphere. The sun may indeed prove guilty on any or all of these charges, but for now it is but one in a long lineup of suspects, and one of the least suspicious-looking at that.

Thus professional meteorologists have generally sided with the astronomers who stay clear of the field, yawning at the perennial victory claims of sun-and-weather enthusiasts or groaning at the press accounts.

It was into these confused straits that the NASA symposium sailed, and it is to be commended for doing it at all and forgiven for loading the ship mostly with enthusiasts. For the case is not at all clear and the rewards, if the sun is really responsible, are indeed great. In the most negative case, it would be of immense value to meteorology to demonstrate, once and for all, that the sun could be ignored in weather and climate prediction. Perhaps it is time, after more than a century of frustration on the opposite tack, to propose, arbitrarily, that the sun does control weather and climate: to throw the burden of proof, or disproof, on the skeptics. Indeed, this seemed to be the theme set forth by a number of the speakers at Goddard that November.

The book, as did the symposium, has four parts: a search for evidence of a correlation (11 papers, all claiming to find it), a discussion of the known variability of the solar output (10 papers), an attempt to elucidate the mechanisms (5 papers), and, most valuable of all, a lively panel discussion that succeeded, in my

view, in placing the problem in perspective.

Unfortunately, the conference contained nothing new. The selected statistics that excite the enthusiasts still caused the skeptics to shake their heads. The variations in solar output that were described still looked like miniscule fluctuations indeed, and the depth in our atmosphere to which their effects might reasonably extend was still the thin vacuum of its highest reaches. No one, neither skeptic nor enthusiast, proposed any definitive tests in the old debate. Charles Augustus Young, the eminent solar astronomer of the 19th century, had said it all in 1895 (*The Sun*, p. 161):

> In regard to this question the astronomical world is divided into two almost hostile camps, so decided is the difference of opinion, and so sharp the discussion. One party holds that the state of the sun's surface is a determining factor in our terrestrial meteorology, making itself felt in our temperature, barometric pressure, rainfall, cyclones, crops, and even our financial condition, and that, therefore, the most careful watch should be kept upon the sun for economic as well as scientific reasons.
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> The other party contends that there is, and can be, no sensible influence upon the earth produced by such slight variations in the solar light and heat . . . .
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> It seems pretty clear that we are not in a position yet to decide the question either way; it will take a much longer period of observation, and observations conducted with special reference to the subject of inquiry, to settle it. At any rate, from the data now in our possession, men of great ability and laborious industry draw opposite conclusions.

It would probably not surprise the sagacious Young that nearly 80 years later almost 200 such men were still hotly arguing the same issues. What he might find heartening, however, is that since the Goddard meeting, and in part because of it, his simple recommendation that more observations of the solar output be made is at last being implemented, by NASA and other agencies. We have subsequently brought together a new definition of the total output of the sun and its variation which should serve as a baseline for the gauging of future variations (O. R. White, Ed., *The Solar Output and Its Variation*, University of Colorado Press, in press). And computer modeling efforts are bringing us closer to the day when we can determine, in theory at least, whether these measured solar changes can have any significant effects in an atmosphere such as ours. In 1977, as in 1973 and 1895, we still don't know.

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Issues in the Study of Behavior

Growing Points in Ethology. Proceedings of a conference, Cambridge, England, 1975. P. P. G. BATESON and R. A. HINDE, Eds. Cambridge University Press, New York, 1976. viii, 548 pp., illus. Cloth, $38.50; paper, $11.95.

The 25th anniversary of the Sub-Department of Animal Behaviour (University of Cambridge) at Madingley was celebrated by a conference, in which representatives of widely different fields were invited to discuss some of the issues that seem to be emerging in ethology today. The present volume is the proceedings of this conference. Because the participants included many prominent and active ethologists, because they took seriously the request to identify and discuss issues that were likely to be important, and because the conference was organized in an intelligent and meaningful fashion, its proceedings deserve to be read by every serious student of behavior.

The book contains 18 contributions that collectively cover a wide range of topics. Several, such as Peter Marler's analysis of vocal communications in gorillas and B. C. R. Bertram's examination of kinship relationship within lion prides, are highly focused. Others, such as Richard Dawkins's essay on hierarchical organizations and P. P. G. Bateson's discussion of control theory as applied to developmental data, point to new and potentially useful tools in the analysis of behavior. Several essays take a broad view. For example, N. G. Blurton Jones examines potential contributions of ethology to the social sciences, and Peter Medawar takes note of various ways in which ethology has or is likely to cast light on human behavior. In the final, and in a sense capstone, essay N. Tinbergen looks to the practical implications of ethology and asks how its present and future findings might be best employed to the benefit of society.

The essays have been assigned to sections according to their perceived relevance to what have often been described as the four basic questions that ethology asks about a given behavior. What are its immediate causes? In what ways does it benefit or harm the individual? What is its relevance to the survival of the species? How did it evolve? In no case, however, are the essays focused on any one of these questions. And because most, if not all, have been revised to take account of the discussion they generated, they have a coherence that transcends both their topical specialization and the subject matter of the particular section in which they happen to appear. Another factor that contributes to the coherence of this highly diversified set of essays is the lucid and extensive editorial commentary. This commentary along with the editors' conclusion—an essay on asking the right questions—is in large part what gives this collection its distinctive flavor.

The paper by Bertram, "Kin selection in lions and evolution," provides a good example of the thinking one encounters here. Extensive field observations have revealed that a representative lion pride contains two adult males and seven adult females. Four of the females give birth at about the same time, and they rear their cubs together. Three female subadults remain with the pride, replacing females that have departed or died. All male subadults are driven from the pride, but they remain together and eventually form their own pride. Finally, the adult males in a pride do not retain tenure long enough to father more than one batch of young female recruits.

Bertram notes that with this system the adult males are typically related to each other and the adult females are related to each other, but the adult males are not related to the adult females—hence no inbreeding occurs. He then proceeds to calculate (using certain additional information) that the males in possession of a pride, like the cubs in the pride, are related on an average by about 0.22 (that is, they are almost half-siblings) and that the female adults are related on average by about 0.15 (that is, they are a little closer than full cousins).

For Bertram the significance of these calculations lies in the fact that it has often been observed that animals that are directly related are more cooperative with one another than with unrelated members of their species. In the case of lions, competition with conspecifics and killing of young that are not members of the pride are not unknown. Within the pride, on the other hand, one sees tolerance on the part of an adult male toward all the cubs, whether its own or its half brothers'. Moreover, a female will readily suckle cubs other than her own, and competition for an estrous female is seldom observed. Bertram shows how each of these observations can be explained in terms of the kinship that exists within a representative pride.

By focusing on the social and ecological setting in which a given behavior occurs (in this case the behavior is cooperation) and by seeking the genetic relation-