anyway due to economic, political and social forces internal to South Africa) and thus derived support for their criticism of their government from the presence of a concerned visitor having a different perspective.

I was not exposed to the temptation to appear on a radio or TV talk show, to which academics visiting South Africa, especially those in the social sciences, often succumb. Inexperience in South African affairs has sometimes resulted in visitors' being manipulated on such shows into appearing to hold views supportive of apartheid, when their intention was the converse. I was advised by a staff member at Witwatersrand University that a visitor should always consult about media contacts, usually with people in the host department. The African National Congress also should be informed of the visit well in advance, since its members know people who might like to meet scientific visitors.

In sum, I recommend that scientists who have the opportunity should visit South Africa, but should keep their eyes open and take advice on how to find out for themselves what is going on. Such a visit will provide invaluable insight into life there, which may help clarify the picture for concerned colleagues who have not had the opportunity or have been disinclined to visit. I traveled to the Soviet Union in the 1970s and early 1980s, at a time when 8000 physicists had vowed not to visit until Andrei Sakharov, Yuri Orlov and Anatoly Shcharansky were released. The arguments for and against this position have already been exhaustively rehearsed, but my visits to both countries led to a serious commitment to work for change, which is difficult to arouse in a person who has not thus *de facto* become involved.

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'Earth's Radiation Budget' Items

The article by V. Ramanathan, Bruce R. Barkstrom and Edwin F. Harrison (May 1989, page 22) contains some technically inaccurate physics. The authors give a logical argument that states that the 10–20% reduction in sunspot temperatures (below the undisturbed photosphere) multiplied by the assumed sunspot "filling factor" of 1–2% provides the modulated solar irradiance of 0.1–0.2% seen when sunspots cross the solar disk.

Simple use of the Stefan–Boltzmann law shows that the above argument is in error. The radiation from a blackbody varies as AT^4, where A is the surface area. Differentiating this formula implies that small temperature changes influence the surface irradiance four times more strongly than would be suggested by the temperature percentages given by Ramanathan and his coauthors. In other words, sunspots are incredible inhibitors of the Sun's surface radiation.

To deal correctly with the energy fluxes, one must study the faculae (Latin for "torches") that surround sunspots and, to lowest order, reradiate their "missing" energy. The subject is fairly complex, since sunspots and faculae have differing directional and spectral radiation patterns as well as temporal histories. Thus the interested reader is directed to a paper by Sabatino Sofia, Ludwig Oster and myself (*Solar Physics* **80**, 87, 1982) for a description.

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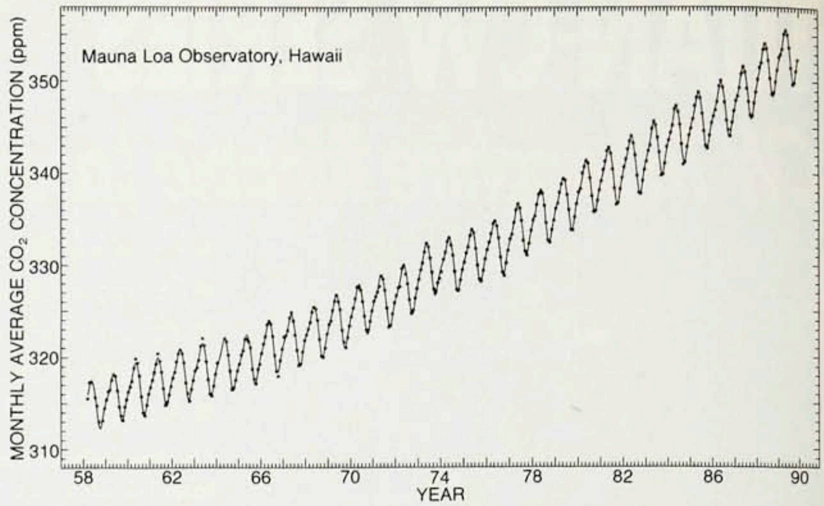

I read with interest the article "Climate and the Earth's Radiation Budget" by V. Ramanathan, Bruce R. Barkstrom and Edwin F. Harrison.

The authors state that the albedo of the Earth's surface and atmosphere, which governs the surface temperature, has a value of 0.30 ± 0.03, as measured by satellite. The Earth's precession about the normal to the ecliptic (with a period of about 25 000 years) and the fact that the surface of the sphere is not uniform (the Northern Hemisphere has three times more dry land that the Southern Hemisphere) suggest that although the albedo may be considered constant over several years, it should vary over a period of several thousand years. The change, albeit small, is significant; the temperature difference between an ice age and the hottest interglacial period is only a few degrees.

The authors provided excellent arguments in support of the greenhouse effect. They stated that over the last 100–200 years, the CO_2 concentration in the atmosphere increased by 25%, while from 1975 to 1985 it increased by 4.5–5.0%. This point is illustrated by the well-publicized Keeling curve[1] (shown above). The fluctuations in this curve reflect the life cycle of plants in the Northern Hemisphere; plants absorb CO_2 in the spring and release it as they decay in the fall.

The Keeling curve has already received worldwide acceptance. Its ascent is man-made: It is the result of deforestation and the burning of oil and coal. But the effect of its rise on temperature is not yet clearly understood, although it seems to point in the direction of a greenhouse effect.

Reference

1. C. D. Keeling, R. B. Bacastow, A. F. Carter, S. C. Piper, T. P. Whorf, in *Aspects of Climate Variability in the Pacific and Western Americas*, Geophysical Monographs of the American Geophysical Union 55, D. H. Peterson, ed., AGU, Washington, D. C. (1989), p. 165.

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Big Blue's Average Green

I was surprised to have a statement attributed to me in PHYSICS TODAY (July 1989, page 63) on IBM starting salaries for new physics PhDs. First of all, I did not make the statement, and furthermore, salaries offered differ substantially depending on individual qualifications. In point of fact, the salary quoted is significantly higher than the typical starting salary for a PhD hired to perform physics research in our laboratories, and I believe that with regard to salary we are very comparable to other industrial labs.

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Two-Slit Experiment: Derivation Dilemma

Thank you for publishing a lengthy review of my book *The Structure and Interpretation of Quantum Mechanics* (October 1989, page 124). Your readers may, however, be misled by the reviewer's statement that in discussing the two-slit experiment I make "an elementary (but unoriginal) error" by writing down the expression $p(X\&A)$ for the probability that a particle reaches region X on the screen via slit A. In the book this expression appears in a calculation that, as I emphasize, has to be *discarded* because it conflicts with experiment; in fact, immediately after this calculation I write, "Where might [this] derivation be challenged?" and a page later I state, "We must reject the whole derivation." The offending expression nowhere appears in my subsequent account of how the quantum mechanical probabilities are to be obtained.

If an author reports, say, Descartes's theory of refraction prior to showing why we must reject it, she does not thereby make "an elementary error." No more did I when I wrote "$p(X\&A)$."

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THE REVIEWER REPLIES: My remark concerned a probabilistic derivation concluding that the double-slit diffraction pattern should be the sum of two single-slit patterns, contrary to experiment. What conclusion should be inferred from that contradiction? Some of the writers whose work R. I. G. Hughes discusses suggest a need to revise probability theory, or even logic. Hughes concludes that "we must reject the whole derivation," without first discovering why it went wrong. I suggest that an "elementary error" lies in Hughes's use of his equation 8.18, $p(A \vee B) = p(A) + p(B)$. Physically significant probabilities are conditional, so that equation must be interpreted as an abbreviation for $p(A \vee B|C) = p(A|C) + p(B|C)$, with C denoting the conditions that affect the quantum state preparation. But Hughes uses equation 8.18 in a context in which the meanings of the three terms are $p(A|C_1)$, $p(B|C_2)$ and $p(A \vee B|C_3)$, where C_1 denotes that only slit A is open, C_2 that only slit B is open and C_3 that both slits are open. There is no reason to expect the equation to hold under such conditions.

We learn from the failed derivation that certain conditions that classical physics (*not* classical probability theory) suggests should be irrelevant are in fact relevant in quantum physics. I have discussed the relations between quantum mechanics and probability theory in detail in other publications.[1]

Reference

1. L. E. Ballentine, Am. J. Phys. **54**, 883 (1986). L. E. Ballentine, *Quantum Mechanics*, Prentice-Hall, Englewood Cliffs, N. J. (1990).

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