

Borie has little confidence that we will ever have the ability to model climate properly. She argues that we ought to spend a lot of money, increase taxes and tell people what to do, even in the absence of observational data to show whether the climate is slowly getting warmer or colder. The world's people have already had enough trouble with command economies based on fallacious theories without embarking on worldwide economic changes based on a hypothetical cause and effect.

Alley suggests that more research and study would be "self-serving." I myself am retired and have no grants or proposals pending. He says we ought to tax gasoline to reduce greenhouse gases. We are already taxing gasoline heavily. Moreover, most proposals for political and economic action go much further than a simple tax on gas. We are now in a position, if we spend the money, to produce mass balances for the various ice sheets, and after we have assimilated and analyzed the data over a few decades we should have a sound basis for evaluating just when and if Alley's disaster might happen.

Bentley cites data from a paper presented after I wrote my letter. It has always proved difficult to consider unpublished information when you prepare a critique. I await Bentley's next paper with anticipation.

All of the robust observational data that I have been able to obtain indicate that the climate is getting colder, *not* warmer. The northern line of orange production in Florida has moved south over the past 20 years, not north. For those who enjoy anecdotal evidence, let me refer to chapter 39 of Mark Twain's *Life on the Mississippi*, concerning Natchez, Mississippi. Twain agreed with Mrs. Trollope's 1827 statement that "Natchez is the furthest point to the north at which oranges ripen in the open air or endure the winter without shelter." This is no longer true. Louisiana oranges were commercially grown south of New Orleans beginning in the early 1940s, but the last commercial grove was destroyed by frosts in the 1980s.

Finally there is the new "Plant Hardiness Zone Map" issued by the Department of Agriculture, which shows the low temperatures controlling plant survival: The 1990 map shows that the zones in the 1965 map are now 5–10 °F colder. At this rate, maybe we should be concerned about a new ice age and should promote the production of greenhouse gases to counteract the cooling. Let me emphasize that I do not advocate this—

but we do need more research, and substantiated models, before the scientific community begins to advocate expensive restrictions on entire populations to avert a hypothetical anthropogenic climate change.

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3/91

Batting Around Ideas on Curveball Physics

Geoffrey F. Chew's review of *The Physics of Baseball* by Robert K. Adair (September 1990, page 103) led me to read and enjoy that delightful book. I was intrigued, but not entirely convinced, by Chew's reference to the mechanism of the curveball (the Magnus effect) as being "simpler than the Bernoulli effect." According to Georg Joos's *Theoretical Physics*,¹ from which I learned much of my physics, the Magnus effect is derived from the Bernoulli equation. Joos points out that this derivation assumes no separation of flow from the rotating surface, that is, it assumes no turbulence; and it follows from his discussion that *with* separation the lateral force is reduced by about half. Inclusion of turbulence, it seems to me, makes the mechanism more complex, though more realistic.

Adair expresses the Magnus effect in terms of the drag force due to flow separation and the experimentally derived drag coefficient, and he makes a point of distinguishing the Magnus and Bernoulli effects. He describes experimental results showing that the lateral (Magnus) force on a baseball varies with speed and reaches a slight maximum at about 60 miles per hour and a slight minimum at about 80 mph. The average magnitude in this speed range is roughly half of the inviscid-flow Magnus effect.

It seems to me that the inviscid-flow solution has unique conceptual and heuristic value, and that the experimental results might best be explained as departures from the inviscid-flow solution due to flow separation.

Reference

1. G. Joos, *Theoretical Physics* (trans. by I. M. Freeman), Hafner, New York (1934), pp. 197–199.

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11/90

ADAIR REPLIES: As another who learned much physics—and something of the Magnus effect—from Georg Joos's wonderful *Theoretical Physics*, I have no important disagree-

ment with Robert G. Fleagle's physics. My use of Isaac Newton's simple description of the Magnus effect was based partially on pedagogical concerns: My book was addressed to the lay audience and the late baseball commissioner Bart Giamatti. The Bernoulli pressure-velocity relation that follows from the conservation of energy applied to irrotational laminar flows surely plays an important role in the Magnus effect, but the trailing vortices at low baseball velocities and the turbulence that follows Nolan Ryan fastballs generate effects outside of the Bernoulli conditions. And Joos's instructive calculation of the Magnus effect was derived from a model that did not account for the drag force.

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2/91

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Geometric Phase's First Formulators

In an illuminating article (December 1990, page 34) Michael Berry writes about people whose work anticipated his discovery of the geometric phase. The earliest reference on his list is to the work by Sergei M. Rytov and Vassily V. Vladimirovskii in the Soviet Union, to whom he attributes the discovery of the law of the parallel transport of the polarization vector in electrodynamics.

In fact, as we wrote in our paper on Berry's phase in the relativistic theory of spinning particles,¹ this discovery was made in 1926 by a mathematician, E. Bortolotti, who was working on the applications of the absolute differential calculus invented by Tullio Levi-Civita. In a very clearly written paper published in the proceedings of the Lincei Academy, Bortolotti described the propagation of linearly polarized light in an inhomogeneous refracting medium and found the correct propagation law for the polarization vector.² He ended his paper with the following conclusion: "The light vector of the linearly polarized ray Γ , propagating through a medium with a varying index of refraction $n(x,y,z)$, is transported along the ray Γ by a parallelism with respect to a metric connection (in the sense of Weyl) in $\mathbf{R}_3$, whose components are determined by the vector $\text{grad} \log(n^2)$."

Since B. L. Markovski and S. L. Vinitzky have already proposed the name "Rytov-Vladimirovskii phase" for Berry's phase as it appears in the propagation of the polarization vectors in electromagnetism, I believe

that the least one should do is add "Bortolotti" to this name.

We would have never been able to get all these historical facts correctly while writing our paper on Berry's phase of the spinning particles without kind guidance from Emil Wolf, who pointed out that all the relevant references can be found in his book with Max Born, *Principles of Optics*.

References

1. I. Bialynicki-Birula, Z. Bialynicka-Birula, *Phys. Rev. D* **32**, 2383 (1987).
2. E. Bortolotti, *Atti R. Accad. Naz. Lincei Rend. Cl. Sci. Fis. Mat. Nat.* **4**, 552 (1926).

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Divert SSC Funds to Physics at NSF . . .

I have just finished reading the February issue, in which Roman Czujko, Daniel Kleppner and Stuart A. Rice (page 37) report on the APS Physics Planning Committee survey, which reveals a dismal state of funding for young physics faculty. A news story in the same issue (page 75) focuses on Leon Lederman's report "Science: The End of the Frontier?"

I find it ironic that Lederman should now be taking up the cudgels for the funding of small-scale university-based physics research and calling for a doubling of the NSF budget. He was, after all, one of the leading proponents of the SSC, which is swallowing up enormous sums of money that could otherwise be spent in accomplishing exactly the goals targeted in Lederman's report. In my opinion, the success in funding the SSC program is a major contributor to the current funding shortfall elsewhere.

For the life of me, I can't understand why anybody should be surprised by the present state of affairs. There is nothing new about it; it existed in 1988. In a letter to *PHYSICS TODAY* in July of that year (page 9) I stated, in part: "The advocates of the Superconducting Super Collider vehemently protest that it is not in competition with other branches of physics—that they are asking for 'new money.' I believe that this view of the situation is unrealistic in the present climate of massive budget deficits and the necessity to economize at every level of government. Any money provided for this project will be diverted away from government support of other science. *At the very*

least, it will siphon off funds that could be used to provide desperately needed increased funding for eV physics" (italics added).

In a letter to the fellows of the APS dated September 1989, President James A. Krumhansl pointed out that "the most sobering aspect of this erosion [of funding] is that it has progressed almost unnoticed in Congressional and executive actions. Indeed, many of our legislators believe that, by funding a few high-visibility projects, they are doing quite well by science."

The SSC appears to be budgeted at \$243 million in fiscal year 1991. I don't have the budget for the physics division of NSF in front of me, but in fiscal year 1990 it was about \$130 million. My solution to the critical problems addressed in the Physics Planning Committee survey and in Lederman's report is simple: Cancel the SSC program as currently constituted, and transfer the funds to the physics division of NSF, doubling its budget—with \$100 million left over to expand the rest of the activities funded by the Mathematical and Physical Sciences directorate. Then all of the problems addressed in both the survey and in Lederman's report will go away. Moreover, money spent in support of university-based small-grant science by and large will be in support of the science of what happens on Earth. As I pointed out in my 1988 letter, such research is infinitely more likely to produce the economic benefits that society has a right to expect in exchange for its support than the same amount spent on the SSC.

It is not that the nation is not spending enough money in support of science. It's just spending it foolishly.

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. . . or Use Some to Tutor Taxpayers

The Department of Energy has recently announced an official price tag for the Superconducting Super Collider of $\$8.249 \times 10^9$. It may be argued that this unprecedented sum will be spent almost exclusively for the intellectual exaltation of a handful of people. How much more usefully and effectively could these dollars be spent if only the US government would also provide the means for "fanning out" the excitement that the SSC will engender in the knowledgeable few!

Consider what might be possible if only 0.1% of the cost of the SSC itself, which is to say, $\$8.249 \times 10^6$, were

invested in educating interested persons around the world in the physics the SSC will elucidate. With such funds, AIP or some one of its constituent societies could exploit video technology and the talents of motivated and gifted lecturers and teachers to create an archive of knowledge with which to broadcast to the greatest possible audience the state of contemporary particle physics. An off-line, supranational classroom could be thusly created, in which persons like me, who wouldn't know a Higgs boson from a huge bison, could participate in the great adventure our tax monies will be paying for.

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Crediting Some Polymer Pioneers

Due to an oversight on our part, Harvey Scher, Michael F. Shlesinger and I neglected to acknowledge in our article on time-scale invariance in disordered materials (January, page 26) thanks owed to Donald G. LeGrand and William V. Olszewski of the General Electric Research and Development Center for their efforts in preparing the polarized-light samples of polycarbonate shown in the photographs on the January cover and in our figure 1. We also did not point out the central role LeGrand and Olszewski's work played in demonstrating the applicability of the Kohlrausch-Williams-Watts ("stretched exponential") decay function to mechanical relaxation and recovery in polycarbonate and (subsequently) in a wide variety of other engineering thermoplastic resins.

A surprising result of their small-strain research on high-molecular-weight polycarbonate was the discovery (or renewed appreciation) of the fact that all mechanical deformation below the glass transition T_g could recover as long as the polymer chains were not broken. It had in fact been known to experimentalists for many years that yielded and crazed polymer recovers upon heating above T_g , but many theorists (and rheologists) are surprised to hear that there is no true plastic flow in these *plastics*. The reason is that it is difficult for the chain entanglement network to reorganize in the glass state, so that while this network may distort, it retains an almost perfect memory of the original geometry frozen in at T_g .

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