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COMPUTERS IN PHYSICS

A Site-SSCer's Guide to the States

During debate on funding for the Superconducting Super Collider at a House science committee meeting, it hit me that the site selection process could be pushed along with the help of a redesign for the particle accelerator.

The obvious scientific benefits of a circular tunnel should be scrapped in favor of the squiggly line approach to policymaking, which is much more likely to win Congressional approval.

Wayne, New Jersey, may not be as well known as the Silicon Valley or North Carolina's Research Triangle for its scientific prowess, but science committee chairman Robert Roe has a great deal of political muscle when it comes to projects like the SSC. From New Jersey, the SSC designers should consider a swing to Dixie, or at least South Carolina (Senator Ernest Hollings), before moving across West Virginia (Senate majority leader Robert Byrd), over to Illinois and up along the eastern part of that state beginning in Carmi. (That part makes me happy.)

At the Fermi National Accelerator Laboratory in Batavia, Illinois, we might actually introduce the particles for study and stick up a few magnets before heading south through Peoria (House minority leader Bob Michel) and into the great states of Alabama, Mississippi and Louisiana (pleasing the chairmen of important Appropriations panels).

Winding into Fort Worth, Texas, on the way to Merced, California, and Spokane, Washington, would take care of the House leadership and end the SSC.

The only problem is we cannot offend Senate minority leader Robert Dole and his Kansas constituents. What better way to take care of that problem than building a visitors' center and 800-room hotel for tourists wanting to meet the SSC designers?

Now that we've ensured Congressional approval, we need the White House to sign the many bills to fund the project. President Reagan will be placated with stops in Illinois and California, but the new guy will need his little piece of the deal: Visitors' centers again seem appropriate. If George Bush wins, we build them in Texas, Maine, Washington, DC, and whatever other hometowns he claims. Mike Dukakis would probably be satisfied with sending research grants to MIT, which might end up happening anyway.

With all the funding approved, construction begins. Seven years later, the building phase is finished. There's only one problem left to be conquered. Realizing that we are not always best equipped to deal with a concern of this magnitude, Congress poses a question to the scientific community: How do you get it to work?

TERRY L. BRUCE
US House of Representatives
19th District, Illinois
Washington, DC

7/88

Middle Ground Sought on Bolide Impact

From the letters in the January 1988 issue (page 13) addressing Luis Alvarez's article "Mass Extinctions Caused by Large Bolide Impacts" (July 1987, page 24), and from Alvarez's reply, many in the physics community may have been given the impression that Alvarez's theory for the cause of the end-of-Cretaceous extinctions is now unequivocally accepted by most geologists and paleontologists. But as a recent review article on the topic by Anthony Hallam attests,[1] skepticism still remains about evidence for a megaimpact event and any relationship to the extinctions of that time. This skepticism cannot simply be dismissed as the opinions of "reactionaries" who are unwilling to accept catastrophism in geologic interpretation. Instead it more often represents the desire for full and rigorous testing of an important and innovative theory whose consequences challenge how we interpret both geologic and evolutionary history.

In his article Alvarez lists 15 "predictions" and "postdictions" as conclusive evidence of a Cretaceous-ending extraterrestrial event. However, acceptance of each of these as an independent test leads to a number of "contradictions." Increasingly, proponents of the impact extinction hypothesis are calling upon closely spaced, multiple impacts to explain the stepwise character of the extinctions at geologic boundaries and to allow for repeated extinction events via periodic comet showers.[2] If this modification of the theory is now accepted, then Alvarez's tests 1, 3 and 4, which predicted a single, unique iridium layer deposited simultaneously on a global scale, must be regraded. Under the multiple-impact hypothesis, the type section for the K–T boundary would not be the single "knife blade" layer at Caravaca, Spain, but a geologic section showing multiple layers, each corresponding to an extinction horizon. Since volcanic eruptions are well known to be episodic, short-lived events, while

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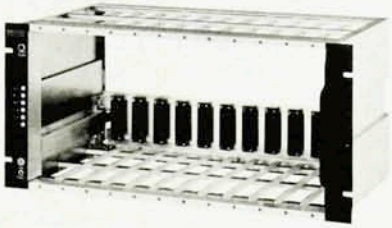

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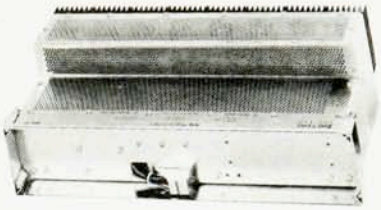

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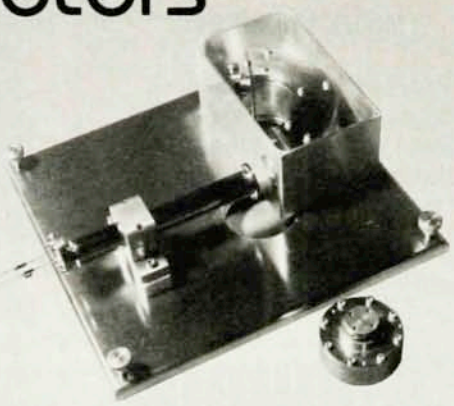

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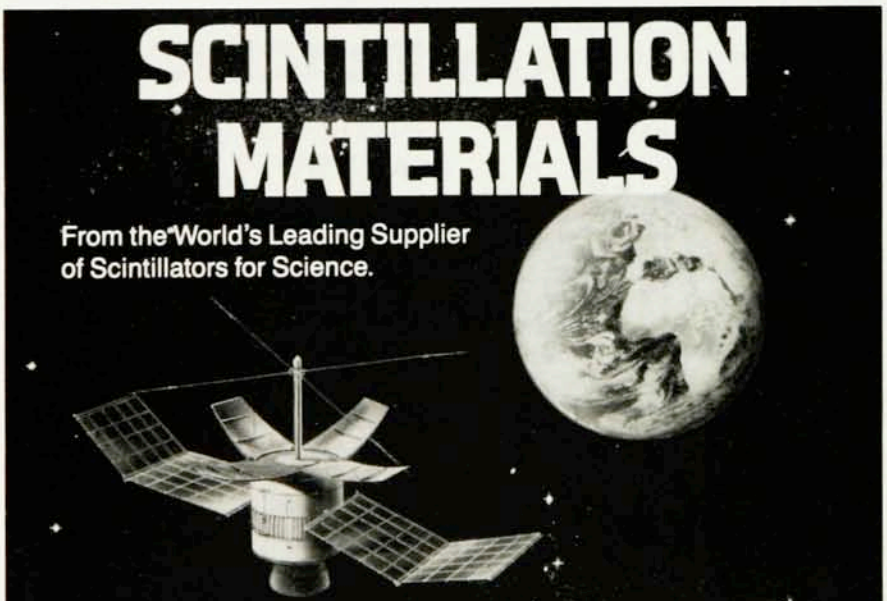

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comet showers distributed over several hundred thousand years are only a hypothesized phenomenon, such a section favors the volcanic case. Also, because the occurrence of abundant shocked quartz (a mineral not generally associated with oceanic crust) at the K–T boundary is at odds with the oceanic impact favored by many bolide supporters, "postdiction" 12 remains enigmatically inconsistent with the absence of a suitable continental crater site.

Geochemical evidence relating to the K–T boundary, although generally consistent with the impact hypothesis, introduces further ambiguity into the discussion. While the ratios of siderophile elements associated with the boundary clays are often close to undifferentiated meteorite (chondritic) values ("postdiction" 10), osmium isotope ratios (prediction 9) exclude a chondritic source, but instead indicate an iron meteorite or a volcanic source.[3] Also, the "chemical fingerprint" of the boundary clay (prediction 8), although unique and global in extent, is much too high in elements such as arsenic and molybdenum to represent a simple mixture of meteoritic material and average crustal material.

In "postdiction" 11, Alvarez refers to the presence of sanidine spherules in the K–T layer as evidence for high temperatures generated by an impact event. However, recent detailed analysis of these same objects indicates that at least some originated via organic processes, belying a high-temperature genesis.[4] Glen Izett, although favoring an impact scenario, concludes that the spherules "are not altered basaltic melt droplets formed during impact."[5] The magnetic spherules and spinel crystals found in some K–T clays seem more definitive of high-temperature conditions synchronous with the boundary, but they differ in a number of fundamental ways from tektites and other known products of meteorite impact.[6]

The evidence for massive burning referred to as "postdiction" 13 cannot be considered a strong test of the impact hypothesis, since other, less exotic processes, including extensive volcanism, could equally be a cause. Perhaps more of a test of the impact scenario is that this same study, to the surprise of its authors, found no trace of meteoritic noble gases or refractory mineral phases in the boundary clays.[7]

More recent descriptions of "*in situ*" dinosaur fossils occurring above the K–T boundary,[8] although still open to reinterpretation, illustrate the danger in asserting that the

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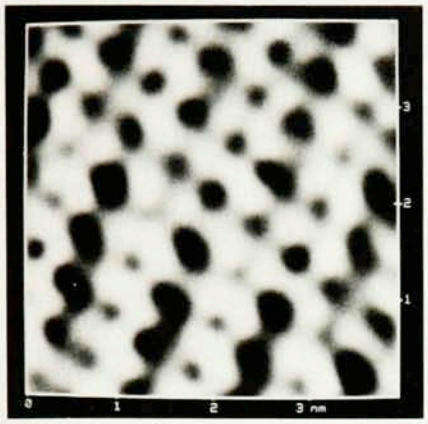

This month's image is an atomic resolution image of a cleaved surface of mercury telluride. The sample was cleaved and imaged in a nitrogen atmosphere using the NanoScope II. Note the distinct shift in the atomic pattern of the lower left region of the image with respect to the pattern in the rest of the image. The division between the two shifted regions starts in the lower right corner of the image, and runs diagonally upward and to the left. The atomic corrugations in the image are one angstrom high.

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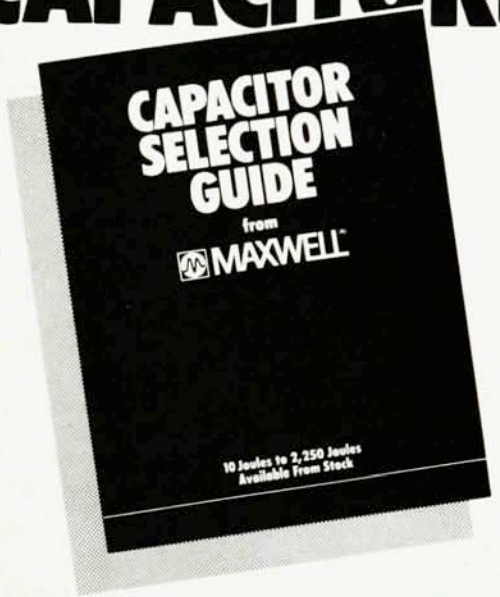

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globally synchronous extinction of so poorly distributed a fossil type as late Cretaceous dinosaurs has been proven (prediction 14) or that the dinosaurs' extinction can be precisely correlated with the much-better-defined marine extinctions. In fact one of the conclusions of the Os isotope study was that the marine and continental isotope signatures, if cosmic, would have to be derived from separate impact events.[3] This finding, along with the recognition by impact proponents of the need for multiple events, would seem to make prediction 6, the synchronicity of plant and animal extinctions, a moot point.

In summary, 8 of the 15 tests offered in support of the impact extinction hypothesis seem either mutually contradictory or in conflict with the multiple-impact hypothesis, probably the only impact scenario consistent with the periodic extinction events alluded to in prediction 15. Other tests, such as the description of the K–T spherules as high-temperature artifacts of an impact event, appear to have been evaluated prematurely. Despite these inconsistencies, the impact hypothesis remains a stimulating and viable, although still unproven, theory.

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STANLEY M. CISOWSKI
University of California,
Santa Barbara

5/88

After reading the letters about the bolide impact at the Cretaceous–Tertiary boundary and Luis Alvarez's response in the January issue, I see no more urgent need than for a few complete scientists. Outside of their disciplines, most scientists are hopelessly lost. The case for the impact at the extinction boundary is well made, but the consequences for life on Earth are not completely understood. Until and unless scientists show the proper respect for each other's disciplines, a more accurate understanding of the Earth's history will elude them.

I would say that the iridium layer is best explained by Alvarez's impact theory, but it does not follow that the extinction event is solely explained by the impact. Alvarez must be satisfied to offer the paleontologists his new evidence of major climatic upheaval and let them run with it for a while. The paleontologists must be willing to consider the impact along with the fossil record to come up with a clearer picture of what died when, and why.

Life on Earth has survived despite the Sun's gradual warming, despite the Earth's tectonic activity and despite occasional collisions with solar system debris. The true understanding of how this marvelous story unfolded will require much hard interdisciplinary study. Unfortunately there are too few scientists, paleontologists in this case, who can look at new evidence as a chance for increased understanding. Instead most scientists become overprotective of their pet theories. Yet Alvarez has made the mistake of treating the impact not merely as new evidence, but as the entire answer to a problem that is largely beyond his area of expertise.

Also, regarding Sanford Aranoff's letter and Alvarez's response, what could be more absurd than to plan to protect ourselves from a bolide impact, an event that occurs "only every 100 million years or so"? Instead of wasting their time on such an improbable extinction event, those "many scientists" who have studied it would be better put to studying the current mass extinction episode caused by *Homo sapiens*!

RUSSELL W. AGREEN
Fulton, Maryland

2/88

(*Editor's note:* Luis Alvarez died on 1 September. His illness prevented him from replying to these letters.)

Book Review Rebuttal

I am writing to correct certain misunderstandings and errors in the review by S. James Gates of my book *Introduction to Supersymmetry and Supergravity* (World Scientific, Singapore, 1986) in the November 1987 issue (page 92). The curious opening sentence, "Through the cooperation of a number of researchers . . . ," is a result of Gates's confusing the book he was reviewing with another book I have written with Peter van Nieuwenhuizen (to be published by Cambridge U. P.). The relation between these works is clearly stated on page 1 of the former book.

Midway through the third paragraph the review states, "Unfortunately, West's discussion of the superconformal algebra contains an error that causes a problem in a later 'proof' of the finiteness of $N=4$ Yang–Mills theory." Chapter 2 of my book gives the equations that make up the superconformal algebra. The two equations Gates is referring to are

$$[Q^{\alpha i}, A] = -i(\gamma_5)^{\alpha}{}_{\beta} c Q^{\beta i}$$

$$\{Q^{\alpha i}, S^{\beta j}\} = \ldots + d\, 4i(\gamma_5 C^{-1})^{\alpha\beta}\delta^{ij} A$$

I give the values

$$c = -(N-4)/4N, \qquad d = 1$$

while a book of which Gates is coauthor[1] gives

$$c = 1, \qquad d = -(N-4)/4N$$

The reader will notice that these two choices are related, except for $N=4$, by a *redefinition* of the generator A. For $N=4$, the Jacobi identities imply that $cd=0$. The case $N=4$ is not discussed in chapter 2 of my book; however, clearly one can obtain either $c=0$ or $d=0$ by rescaling A before setting $N=4$. Gates and his coauthors insist in their book that $c=1$ and $d=0$. However, both choices lead to a consistent algebra. Following the method given in chapter 14 of my book, one easily finds that both possible algebras admit faithful superspace representations. In short, there is no error.

In chapter 18 of my book, one of the arguments for the finiteness of $N=4$ Yang–Mills theories uses the well-known fact that the maximal symmetry this theory can admit is SU(4) and not U(4). Clearly, because the ± 1 helicity states are in the same supermultiplet, the case $c \neq 0$ is precluded. If one takes $c=0$, then, as discussed in my book, A must be trivially realized. There is no problem here either. In fact, recent work has established the anomalies argument for finiteness as perhaps the most rigorous one.

Let us now turn to Gates's reference in the review to my "discussion of the patently false notion of a finite set of auxiliary fields." This comment relates to my chapter 22, which discusses the now widely used result[2,3] for the free gauge covariant bosonic string action, namely XQX. In reference 2 this result was found by