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better ones soon, in plenty of time to meet the most optimistic schedule for the installation of the rest of the system.

Rahn's letter stating that glass is available as a waste form now perpetuates—unwittingly I am sure—other gross misconceptions of the public, including most scientists. Glass as a waste form, far from having the maximum experience behind it, is the latest entry, well behind its two competitors, ceramics and concrete-encapsulated forms. The fact is that in the entire world there is a total of some 35 megacuries of real radwaste glass. But the world has not been told of DoE's real success stories. For ten years at Hanford, some 300 megacuries of radwaste have been converted to crystalline ceramics (albeit in poorly chosen phases) and doubly canned in hastelloy and stainless steel. Waste *solidification* into *crystalline* forms is not only available, it has been and is going on *now* in the US. Even more dramatic is the ignorance, that waste *solidification and disposal* is going on every day *now* in the US. I refer to the exemplary work of Oak Ridge's Chemical Technology Division in in-situ emplacement and solidification into concrete, which also has been going on for ten years. The USSR in a simpler variant had already disposed of 100 megacuries in 1967; by now several hundred megacuries have been *ultimately disposed of* by in-situ solidification in mineral-encapsulated forms. Not only does experience therefore come out on the side of crystalline options, but cost and process safety would suggest that for low-temperature wastes (below 100 °C) equally insoluble but much simpler and cheaper options based on concrete will save the taxpayer billions of dollars.

The danger to the public's health from radioactive waste, in whatever solid, glass or concrete on any comparative scale, are essentially trivial. Yet as we have seen in Harrisburg, much damage can be done mainly from psychological trauma and societal gross over-reaction. One canot cure such trauma by apparently accepting the validity of the exaggerated threat and turning to speedy action. Exactly the undesired effect is caused by suggesting urgency—it exacerbates things. The pro-nuclear forces of the "don't just stand there, do something" school forget that the absolute minimum time between authorization and pronouncement of success of any kind by "demonstration" is 25 years. Can nuclear power wait that long? No. Active vigorous education and interpretation is vastly more important. Among the most important facts to be conveyed to the public is that nuclear wastes have been for ten years and are being solidified and disposed in concrete, and that enormous amounts of nuclear waste have been successfully solidified into crystalline ceramics for a decade.

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Refereeing justice

Although there are substantial differences between a judicial trial and the procedure involved in refereeing a technical paper, in the cases in which an adversary relationship does arise between author and referee, the similiarities are great enough to invite comparison. Viewed in this light, with the author standing as the accused, the journal editor as judge and the referee serving at once as witness for the state, prosecuting attorney and jury, a more biased and inequitable system would be difficult to imagine. The author does not have the right to face his accuser; the judge not only favors the prosecution but often does not scrutinize the evidence offered by the defendant; the defendant is often found guilty on the vote of a single juror (referee); and the accused is presumed to be guilty until he proves himself innocent. In general, every safeguard offered the accused in a judicial trial is not only lacking, but is reversed and given to the referee.

The reason for this state of affairs is not only that it is difficult to find a system which both preserves the rights of authors and also maintains a reasonable limit on the time and effort going into the refereeing procedure but also, to a large extent, that those responsible for setting up and operating the system not only make little effort themselves, but often actively oppose efforts made by others to effect improvements.

Because of space limitations it is not possible here to detail the various (some relatively simple) changes in procedure which would improve the system; but a single illustrative example, namely the institution of reciprocal anonymity, will suffice. This suggestion, which is by no means new, is widely opposed by those in positions to make changes, and a common reason given for this opposition is that "in most cases the referee would recognize the author so it wouldn't do much good." In fact, I doubt whether anyone has the slightest idea of the fraction of cases in which an author would be known without his identity appearing explicitly on the paper. In any event, if the anonymity were maintained in even a relatively small fraction of cases it would represent a definite improvement in the existing system.

The reason for the opposition to such

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change, however, is that many referees want to know who the author is, because having this information enables them, unfairly, to prejudge favorably or unfavorably the result of their reading and thus reduce the effort involved and/or the risk of antagonizing a prominent author in judging the paper. There are undoubtedly also some established authors who wish their names to appear so that referees will favor them, regardless of content. By their opposition to change, these aims are supported by those responsible for the operation of the refereeing system.

I suggested the institution of reciprocal anonymity to the officers of the APS several years ago, with no effect. No matter how often or how many suggestions of this type are made, they are simply ignored. One possible way improvements might be effected would be if they were presented to the general membership via a ballot. However, efforts on my part to obtain information from APS officers on the procedure necessary to form an effective and representative committee and thence institute a referendum to vote on its recommendations have been met with a refusal to supply me with the information I requested.

Until the officers of the APS come to place the objectives of fairness and justice above particular group interests, the refereeing system will continue to manifest gross inequities.

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10/9/80

Black-hole physics

The article by Jacob Bekenstein on black-hole thermodynamics (January, page 24) brings into sharp focus the physical significance of the choice of coordinates in which the Schwarzschild metric is expressed. In particular, the spherical event horizon at $r_0 = 2M$, where M is the mass of the gravitating source, is given the significance that its area represents the entropy of all that lies within. It is interesting to note that the area of the event horizon being $4\pi r_0^2$ is a necessary condition for deriving Schwarzschild's metric as a solution to Einstein's field equations for the case of static spherical symmetry. The condition $A = 4\pi r_0^2$ is not covariant, nor was it motivated by any conservation laws (or other physical considerations) prior to Bekenstein's formulation. Therefore, it is reasonable to ask if Schwarzschild's solution to Einstein's equations had enough prior legitimacy to warrant being the basis for the impressive formalism of black-hole thermodynamics.

It would seem the answer to this question is no, for there is quantum-mechanical trouble incurred by endowing Schwarzschild's r with operational significance as the "real" distance from the center of the black hole.[1] The trouble is the inconsistency of the speed of light as a particle and as a wave. As a particle in a curved spacetime, light travels with a speed computable from the line element in the spacetime; as a wave, light travels with a speed computable from the wave equation (covariant d'Alembert equation) in the spacetime. Equivalence of these two speeds is necessary for the complementarity of particle and wave aspects of light. Schwarzschild's coordinates do not support the complementarity.

This difficulty is reparable by changing the Schwarzschild radial coordinate to r', where $\exp(-2M/r') = 1 - 2M/r \geqslant 0$, a transformation that moves the event horizon to the origin of coordinates, which is the site of the collapsed matter. (The new coordinates shall be called *propagation coordinates*.) However, this renders the speed of light anisotropic. Experiments performed by Vernon Hughes[2] and independently by R. W. P. Drever[3] show no such anisotropy to a precision of 10^{-22} (see reference 1 for interpretation of these experiments).

It happens that the gravitational theory of Huseyin Yilmaz[1] circumvents this problem: In coordinates for which wave and particle speeds of light are equal, each speed is also isotropic. In this theory one has the static spherically symmetric line element

$$ds^2 = e^{-2\phi}dt^2 - e^{2\phi}(dx^2 + dy^2 + dz^2)$$

where $\phi = M/r = M(x^2 + y^2 + z^2)^{-1/2}$.

The theory of Yilmaz differs from that of Einstein in several respects, one of which is the interpretation of ϕ instead of g_{00} as the static gravitational potential. Also, the strong principle of equivalence is implemented to all orders, whereas this principle is known to hold only to first order in Einstein's theory.

Neither in Einstein's nor in Yilmaz's theory do propagation coordinates permit an event horizon to surround collapsed matter at a finite radius. Both theories, however, display a spacetime singularity at the propagation coordinate $r = 0$ (that is, at the collapsed matter). It is useful in the present context to examine the area of a sphere of vanishingly small radius about this singularity. In Einstein's theory the limiting area is $A = 16\pi M^2$, the same as the area at $r = 2M$ in the usual Schwarzschild coordinates. (This does not mean A is generally coordinate-independent—other than under transformation of r. Even in Special Relativity, areas vary under Lorentz transformations.) Although finite area at $r = 0$ might be considered a basis for the area law of black-hole entropy in Einstein's theory, no such basis exists in Yilmaz's theory, for the $r = 0$ limiting area is *zero* in that theory. Thus there are no absolute event horizons in Yilmaz's theory, and hence none of the following conceptual trouble with collapsed matter hiding behind such a horizon: If the horizon forbids the exit of radiation (electromagnetic or gravitational), it must forbid the static gravitational field of the source from manifesting itself outside the horizon. In that case black holes would be completely unobservable as they would have no way of interacting with the rest of the universe.

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THE AUTHOR COMMENTS: The letter by Michael Brill and Winfield Salisbury is less a comment on "Black Hole Thermodynamics" than a not-so-coherent polemic proposing Huseyin Yilmaz's 1976 gravitational theory as a replacement for Einstein's 1915 general relativity. Evidently, the issue of which theory is the better description of nature must be settled by experiment, not polemics. Yilmaz's theory is *not* included in the latest lists of experimentally viable theories of gravitation[1]; by contrast, general relativity has been in accord with all known experiments throughout its 65-year-long career. In the light of this, Brill and Salisbury's comments are not convincing. In addition, they contain a lot of confusion about basic issues in gravitational theory. Let me try to clarify the situation, especially for the benefit of readers do not actively work in gravitational theory.

First, black-hole surface area A is *not*, as Brill and Salisbury would like to suggest, coordinate dependent. It is defined as the invariant area of a spacetime slice of the horizon (the horizon at a "given time").[2] A is computed from the metric which the four-dimensional spacetime metric induces on the horizon (a three-dimensional hypersurface). A is invariant under transformations of the four spacetime coordinates, or of the coordinates used to parametrize points on the horizon. Thus it is consistent to relate it to entropy, which is also coordinate invar-