

(non-theory laden) observation of the single big-bang model cosmology as a refutation of the principle of relativity! I do not see that such an observation has ever been made.

I have discussed some of the underlying ideas of the theory of relativity in a recent book,² and I have recently demonstrated the Hubble law as a first approximation for a *bona fide* covariant equation of motion in a generalized mathematical expression of general relativity theory, where no absolute global time measure appears in the metric and where the oscillating universe cosmology is automatically incorporated.³

References

1. A. Einstein, *Autobiographical Notes*, in *Albert Einstein—Philosopher—Scientist*, Evanston, Ill. (1949), page 75.
2. M. Sachs, *Ideas of the Theory of Relativity*, Jerusalem and New York (1974).
3. M. Sachs, "The Hubble Law and the Spiral Structures of Galaxies from Equations of Motion in General Relativity," *Int. Jour. Theoret. Phys.* 14, 115 (1975).

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7/1/77

THE AUTHOR REPLIES: I disagree with Mendel Sachs's assessment that a conclusive "observation of the single-bang cosmology" (by which he means evidence that the universe will expand forever from the past big bang as in some Friedman models) would constitute "a refutation of the principle of relativity." It is possible for the laws of nature, the field equations, to be independent of the reference frame, while the solution describing the large-scale behavior of the universe has a high degree of symmetry in certain frames.

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H. G. Wells and isotopes

After working in the area of discharge isotope separation for the past two years, and thinking it to be a rather novel approach, it was with great amusement and chagrin that we read William Squire's letter (August, page 66), describing the possibility that the first discharge isotope separation experiments actually occurred in 1895. Although our comments are not meant to detract from the inventiveness of H. G. Wells, we would like to point out that based on experience in this laboratory (essentially another case of 20/20 hindsight), an isotopic interpretation of Baly's work seems improbable due to the extremely large enrichment factors required. Instead we feel that, in his dis-

charge, Baly produced ozone, which increased the measured oxygen density.

In experiments on discharge enrichment of N^{15} (*Applied Physics Letters*, 28, 372, 1976), the highest enrichments observed were less than 30%, and these were obtained only at cryogenic temperatures. At room temperatures and above, the enrichments were always negligible. In the case of deuterium enrichment in the hydrogen methane system (T. J. Manuccia and E. E. Geosling, submitted to *Appl. Phys. Lett.*) enrichment factors as large as 6 were indeed observed, but again, this was at low temperatures, and it should be pointed out that the H/D system will always show the largest isotope effects due to it having the largest $\Delta M/M$ of any pair of stable isotopes. To achieve the observation of Baly, namely an average density increase of about 1% starting from natural abundance of 0.2% an enrichment factor of over 40 would be required. This is extremely unlikely, especially at room temperatures and high (350 torr) pressures. More probably, Baly was observing the production of ozone at the 2% level in his mixture. Alternative explanations could involve impurities outgassed from his electrodes.

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8/24/77

Sexism in science

No one would disagree with A. V. Ferris-Prabhu (July, page 69) that the effort to seek out and encourage capable women is as important as the preoccupation with changing words. But I have a gut fear that the capable, independent, self-motivated woman is just the person who might object to the following:

- Papers she submits to the *Astrophysical Journal* will go out to referees with a form letter asking, "Is his work . . . , does he . . . , does his work . . . , and so on."
- Applications to NASA for position of astronaut ask for names for recommendations. Forms to these people ask, "Is he . . . , does his work . . . , and so on."
- Her work may be discussed in a national magazine under the title "Wise men from the South peer toward what may be limits of the universe," (*Smithsonian*, April 1977). At least her husband will find this funny.
- A distinguished scientist, the president of the Board of Directors of the US National Observatory, will look her (the only female) hard in the eye and call each Directors' meeting to order by firmly saying, "Gentlemen."
- She will read in an international scientific magazine (*Nature* 265, 17, Feb. 1977) an article entitled "Strategy for detection of cancer hazards to man," in

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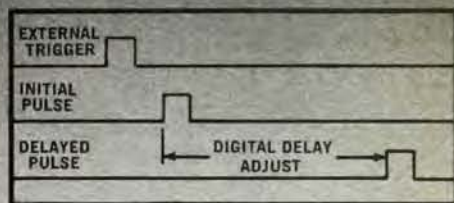
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letters

which the confusion the author introduces by attempting to use the noun "man" to be sometimes male only, sometimes male and female, and sometimes female only, reaches the ludicrous. For example, the title of Table 1 reads "Range of variation in the incidence of common cancer in men (unless specified φ)." Even the author could not accept entries for cervical, uterine or breast cancer in a table labeled "men."

► If she is a council member of an AIP Member Society, she will receive mail from the Director of AIP addressed to "Councilmen" (as well as to "Chairmen").

In a very real sense, we are what we say. And what much of the scientific community appears to be saying to young women is that it is male. If this appearance is to be altered, changes in language may have to lead the way.

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8/19/77

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Electron beam fusion

The news of the success of electron-beam fusion efforts at Sandia and Kurchatov reported in August (page 17) is of considerable interest to us in the Plasma Physics Division of the Naval Research Laboratory. I would like to clarify, however, the description provided of similar NRL programs. Within the NRL Plasma Technology Branch, there exist two quite distinct efforts which involve the pulsed compression of fusion plasma.

One program is concerned with the generation, transport and focussing of intense proton beams to implode thermonuclear pellets, in the manner of laser or electron-beam-initiated inertial fusion schemes. The present work, by G. Cooperstein, S. A. Goldstein, R. E. Lee, D. Mosher and S. J. Stephanakis, has concentrated on the delivery of energy to the target area, without detailed consideration, as yet, of sophisticated pellet features such as magnetic thermal insulation. (The influence of magnetic fields on particle-energy deposition in targets has been a subject of separate interest.)

The other implosion-generated fusion program (referred to in the news item as "the linear program at NRL") is actually the *liner* implosion research effort. The concept, called LINUS, involves the use of rotationally stabilized imploding cylinders, compressing plasmas in which magnetic fields provide particle containment, not merely thermal insulation. As in the proton-beam work, the present effort, by P. J. Turchi, R. L. Burton, D. L. Book, A. L. Cooper *et al.*, has involved the study of techniques for concentrating energy to achieve high energy densities.

Stable, implosion-reexpansion cycles have been generated allowing the repetitive action required for pulsed fusion-reactor systems. Separate investigations of plasma-magnetic field configurations suitable for liner compression are underway by other groups.

Both implosion efforts will eventually make use of material inertia to hold fusion plasmas at high energy density for times long enough to achieve net energy gain. In the proton-beam work, however, such times are measured in nanoseconds, while the liner systems will require tens of microseconds. The common interest in the Plasma Technology Branch that connects these two efforts is the development of high-energy-density systems, but the detailed physics is quite different.

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8/22/77

In your recent news story on electron-beam fusion (August, page 17), credit for having proposed both electron thermal insulation and to a lesser degree also alpha-particle confinement, both by the magnetic self-field of the electron beam, is given to the Russians, notably Leonid Rudakov.

I do not think that this is quite correct. At a meeting of the International School of Physics "Enrico Fermi" in Varenna, Italy, I had proposed both concepts, as can be seen from the proceedings published by Academic Press in "Physics of High Energy Density." My presentation was also attended by scientists from Livermore, Los Alamos and the Soviet Union.

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8/19/77

Conserving helium

It is presently our national policy to allow all helium associated with natural gas to escape into the atmosphere¹ and thence irretrievably into interplanetary space. The escape from atmosphere to space is slow, so that some helium can be recovered from the atmosphere, but the energy cost of such recovery is prohibitive and rising. As alternative sources of helium we may turn to the planet Jupiter or to the Sun itself. The disadvantages of such energetically distant helium sources by comparison with earthbound natural-gas deposits are plainly evident.

The quality of our future is critically dependent upon the availability of abundant and inexpensive helium. Without it, the large-scale applications of superconductivity recently described by Brian Schwartz and Simon Foner (July, page 34) can never materialize. In par-

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