RESEARCH FACILITIES AND PROGRAMS

Fusion research in the US

Research on controlled thermonuclear fusion in the United States is "declining in stature relative to that of the rest of the world." So remarked the Controlled Thermonuclear Research Review Panel in its recent report to the Atomic Energy Commission and the Joint Congressional Committee on Atomic Energy. Although the panel had praise for the work that is being done or has been done on controlled thermonuclear research (CTR) in the United States, it was concerned that the program was neither large enough nor moving as rapidly as it ought and concluded that it would "deteriorate rapidly to a secondary role if the present static budget of the AEC is continued."

The report points out that during the early period of CTR investigations this country held a commanding position. Four years ago, the United States effort in terms of weighted expenditure and personnel in the field represented nearly one half the total world effort and its contribution to progress (not otherwise defined in the report) was well over half the total. Today, the US effort is about one fifth the world total and the contribution approximately one third. During those four years, the AEC budget for fusion remained "essentially static and largely inflexible," a circumstance that both discouraged the influx of new people with fresh ideas and caused a lack of speed and flexibility in adapting and acquiring equipment to test new ideas. Meanwhile, programs in Western Europe, the USSR and Japan have gone ahead with new equipment, and vigorous, youthful staff. The panel was worried that: "After carrying through with the difficult groundwork and making major contributions to the foundation we shall be in a relatively poor position to reap rewards as they come."

To remedy the situation, the panel proposes that the AEC adopt and promote a fiscal policy that would double the number of scientists and engineers engaged in CTR under AEC auspices in five years. AEC was advised to take immediate steps toward formation of a national center for plasma studies and nuclear-fusion research. The national center should have an identity of its own and should be free of all security restrictions so that it could engage in cooperative ventures with other nations conducting programs in the field.

It should have close links to other fusion laboratories maintained by frequent visits and by exchange of personnel for periods of one to two years. The center should also have close ties to one or more universities and should play an important role in teaching and preparation of students for CTR careers.

The panel made detailed investigations of CTR programs in four major AEC-supported laboratories (Princeton, Oak Ridge, Los Alamos, Livermore). It recommended that for the immediate future AEC continue to rely on the four large laboratories for the bulk of CTR effort, that it support energetically a number of current experiments and novel excursions and that it "exercise courageous management in terminating and redirecting approaches which reach the point of diminishing return." The panel commended most but not all current fusion efforts of the four laboratories. It urged enlarged theoretical and engineering support.

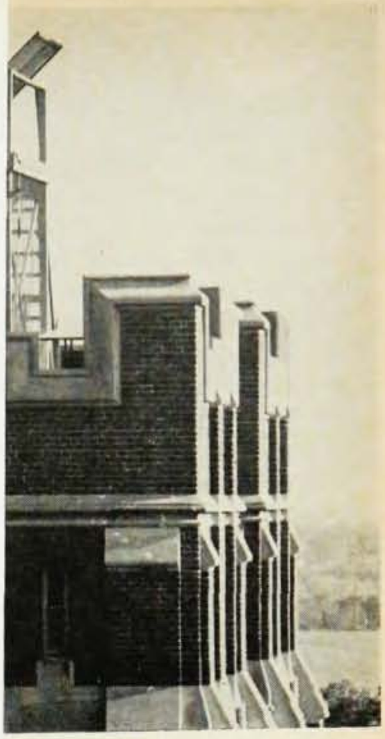
Other CTR programs are under way in laboratories not directly related to AEC, including the Naval Research Laboratory, General Atomic, General Electric, Aerojet-General Nucleonics. Their programs received the panel's commendation for their valuable additions to the total effort. But AEC was warned that it could not rely on outside sources to carry the burden of research "in this difficult and time consuming field." AEC should strive "to set the pace and capitalize on the good fortune of having active collaborators."

The panel began its work under the chairmanship of the late Samuel K. Allison of the University of Chicago. After his death, R. G. Herb of the University of Wisconsin succeeded to the chair. Besides Herb, the report was signed by Peter L. Auer of the Department of Defense, Gordon S. Brown of MIT, S. J. Buchsbaum of Bell Telephone Laboratories, David D. Jacobus of Harvard, Thomas H. Johnson of Raytheon, and Eugene N. Parker of the University of Chicago.

A bang, not a whimper?

Speakers at a recent session of invited papers before the American Physical Society (28 Jan.) discussed the question whether in its remote past our present universe may have been a small "cosmic fireball," which, among other things, burned deuterium into helium at a temperature of about 10^{10}°K. R. H. Dicke first suggested that black-body radiation from the fireball may still exist as microwaves. The theory was discussed subsequently by Dicke and three other Princeton professors (P. J. E. Peebles, P. G. Roll, and D. T. Wilkinson) in the 1 July 1965 issue of the *Astrophysical Journal* [142, 414 (1965)] in connection with new observational evidence reported by Arno A. Penzias and Robert W. Wilson of Bell Laboratories in the same issue. The observations showed background radiation at 7.5 cm with an antenna temperature of around 3°K above contributions from the atmosphere and the antenna itself. Some time later Roll and Wilkinson (*Phys. Rev. Letters*, in press) found a temperature of about 3.0°K at a wavelength of 3.2 cm. Both determinations fit the spectrum of a black body of about 3°K, and it is proposed that this spectrum may represent the greatly redshifted radiation of the cosmic fireball. The speakers at the meeting were Peebles, Wilkinson and Wilson.

The observations reported so far show the intensity rising with decreasing wavelength (see illustration). This would be surprising even if they did not also fit the



ɴ. The horn used by P. G. Roll
›wave background measurements
University's geology building.

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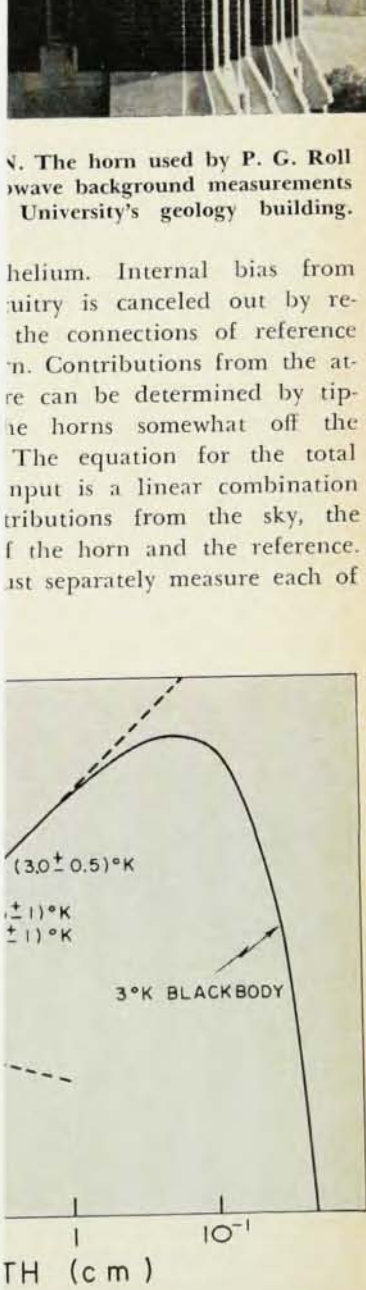

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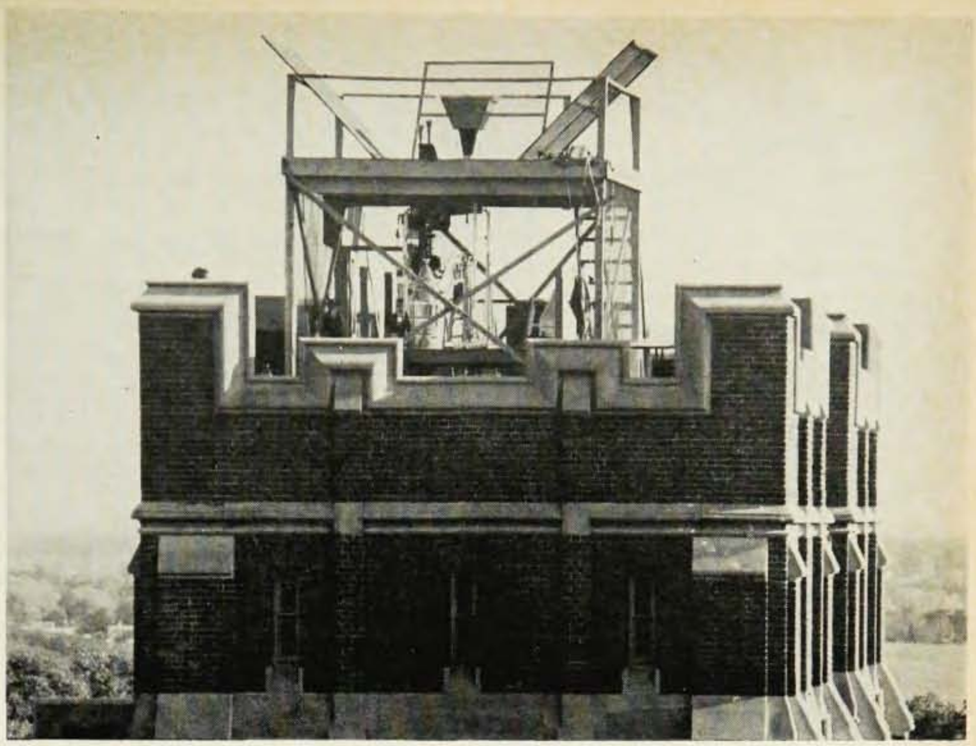
INSIDE THE 20-FT. HORN. Robert Wilson (on ladder) and Arno Penzias inspect the Holmdel instrument.



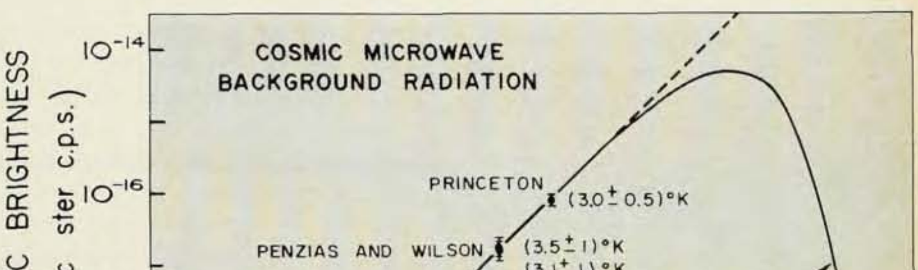
PRINCETON INSTALLATION. The horn used by P. G. Roll and D. T. Wilkinson for microwave background measurements stands on the roof of the University's geology building.

3° black body since the previously recorded galactic background (shown in the graph) would be expected to continue to show a linear decrease in monochromatic brightness if extrapolated into the centimeter-wave region. Two points, however, do not define a curve uniquely. The present two fall on a portion of the black-body spectrum that could just as easily be the tail of a gray-body distribution. Continuation of the gray-body distribution would be a rising straight line tangent to the curved portion of the black-body spectrum (indicated on the graph by dashes). Measurements in the wavelength region below one centimeter, it is hoped, will distinguish between the two. If extension to shorter wavelengths continues to rise along a straight line or follows some unknown third alternative, things will be up in the air again, and a new theory to account for the radiation will be necessary.

Meanwhile, the present data conform to the fireball theory by showing the radiation to be isotropic, unpolarized and free of seasonal variation. This is what would be expected if the whole universe is or was a black body.

The Bell Labs result was obtained with a 20-ft parabolic horn located at Holmdel, N. J. The Princeton observations come from a smaller, straight horn on top of the univeristy's geology building in what was formerly a bird cage used by the biology department.

The radiometers attached to both horns are similar to the Dicke type used in many radiotelescopes. They provide a measurement by comparing the horn signal with one from a reference source partially immersed in liquid helium. Internal bias from the circuitry is canceled out by reversing the connections of reference and horn. Contributions from the atmosphere can be determined by tipping the horns somewhat off the zenith. The equation for the total signal input is a linear combination of contributions from the sky, the walls of the horn and the reference. One must separately measure each of



3°K BLACK-BODY SPECTRUM. Observational points are indicated.

these in order to determine the excess temperature.

The radiation would have originated when the universe was much more highly concentrated than now. One assumes that in the early universe matter was uniformly distributed throughout space and was hot so that the matter uniformly filled space with black-body radiation. As the universe expanded, its contents (matter and radiation) would have been adiabatically decompressed and cooled with the blackbody radiation ending up at 3°K. Peebles has considered the conditions, especially those of helium production, that might have existed very early in the expansion. He discussed these matters at the January meeting and will publish a paper on them in *Physical Review Letters*. When the temperature was 10^{10}°K, thermal reactions would have produced neutrons and some protons. Thermal pair production would have flooded space with electron and neutrino pairs and these leptons would have reacted with nucleons according to one of the following eractions: proton-plus-electron yields neutron and neutrino or proton-plus-antineutrino yields neutron plus positron or the reverse of either reaction. The neutrons and protons together form deuterium, but photodissociation keeps the abundance low until the temperature has dropped to 10^{9}°K. Then deuterium starts to burn into helium, and the amount of helium increases as the temperature drops.

This calculation does not take account of possible gravitational radiation or a conceivable new kind of radiation. If they are present, the time scale for expansion of the fireball is reduced. The neutrons then become more abundant, and so does the helium.

For "a reasonable value of the mean mass density in the universe," Peebles concludes that the theory requires a large primeval helium abundance (27%–30%). If observation shows the helium, it will be a "remarkably stringent" test of general relativity. On the other hand, if the primeval helium is not very abundant and the 3° black body is confirmed, Peebles believes that the result will be difficult to explain by conventional general relativity. It remains for observation to determine whether the helium is there.

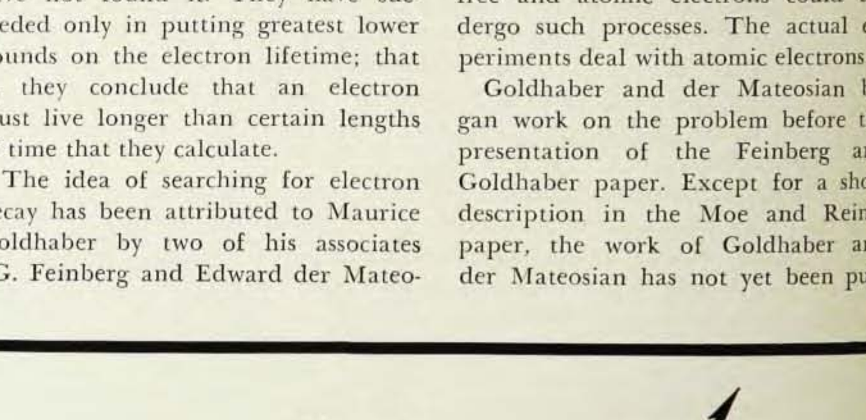
How long does an electron live?

Is the electron a stable particle? Much of physics goes on the assumption that it is. Electron stability is a necessary consequence of the law of conservation of charge since all particles lighter than the electron are electrically neutral and therefore it cannot decay into them and conserve charge at the same time. A lot of elementary-particle "spectroscopy" has been built on the assumption that the electron (along with the proton) represents a kind of "ground state" than which there is no lower. But as H. K. Moe and F. Reines point out in *The Physical Review* for 22 Nov. [**140**, B992 (1965)], "regardless of how attractive a law may appear theoretically, its validity rests on experimental evidence. The danger in the application of conservation laws to unexplored domains can be seen in the dramatic failure of parity conservation for weak interactions." In this spirit experiments are seeking evidence of electron decay. So far they have not found it. They have succeeded only in putting greatest lower bounds on the electron lifetime; that is, they conclude that an electron must live longer than certain lengths of time that they calculate.

The idea of searching for electron decay has been attributed to Maurice Goldhaber by two of his associates (G. Feinberg and Edward der Mateosian). It was formally presented by Feinberg and Goldhaber in a paper delivered at the National Academy of Sciences in April 1959 [Proc. Natl. Acad. Sci. U. S. **45**, 1301 (1959)]. The paper considered the theoretical status and experimental justification for three physical conservation laws that are usually considered absolute: conservation of charge, conservation of baryons and conservation of leptons. Various suggestions were made (and some preliminary results given) regarding experiments to test the laws. Indeed the tone of the entire paper is set by a statement that Moe and Reines seem to have paraphrased in the quotation above.

To search for a violation of a conservation law, as Feinberg and Goldhaber point out, one would like to begin by looking for an interaction that would be permitted by all other conservation laws but would violate the one in question. For conservation of charge, spontaneous electron decay would do this. If charge is not conserved, two reactions are possible according to Feinberg and Goldhaber: an electron yielding a gamma and a neutrino or an electron yielding two neutrinos and an antineutrino. Both free and atomic electrons could undergo such processes. The actual experiments deal with atomic electrons.

Goldhaber and der Mateosian began work on the problem before the presentation of the Feinberg and Goldhaber paper. Except for a short description in the Moe and Reines paper, the work of Goldhaber and der Mateosian has not yet been pub-



ELECTRON DECAY MODES. Could they happen?