

letters

of graduate studies are already superior in less measurable ways, such as sensing that more satisfying individual achievement is found off the beaten track.

The remarks in the paragraph "Faculty attitude" are amusingly understated. The prevailing attitude reminds me of the arguments I was given in junior high school to study Latin: it would help me appreciate French, and so on. (I have yet to understand why it would help me anywhere near as much as studying French itself). But I am biting the hand that fed me: it is largely thanks to this narrow-mindedness of physicists that I was able to parlay one year of graduate schooling in an offbeat subject (geodesy) into a comfortable professorship at a fairly respectable institution.

WILLIAM M. KAULA
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Laser-fusion budget

Although you mentioned the Lawrence Livermore laser-fusion construction project in your April "State and Society" section (page 101) it was included in a discussion of the magnetic-confinement fusion budget. The two complementary programs are separately funded and managed. The research is pursued by different technical groups. Budget information for the laser and electron-beam fusion programs in millions of dollars is:

	FY 1975 (est.)	FY 1976 (est.)
Operating budget		
total	41.4	54.9
Capital equipment	4.1	4.5
Construction	15.4	9.6
Total	60.9	68.1

With the projected FY 1976 budget, the ERDA laboratory laser-fusion efforts at Livermore, Los Alamos, and Sandia will remain at approximately the same level of effort as in FY 1975.

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Defective point

We all realize that in an interview as broad in scope as the one with Professors Amaldi and Weisskopf (November, 1974, page 23) mistakes are bound to slip through. In fact, my colleagues and I were impressed with the breadth and accuracy of the information that your two distinguished scientists furnished. However, one point attributed

to Amaldi is in error and I should like to take this opportunity to correct the error before it is propagated further. I refer to the comment on page 26 that Alfred Seeger of Stuttgart was the first to obtain information about point defects from positron annihilation. As an old friend of Seeger's I know that he insists upon accurate reporting, and would want the record set straight.

The facts are roughly as follows. As early as 1964, I. Ya. Dekhtyar¹ of the Soviet Union reported that two-photon angular distributions were affected by the state of the metal in which the annihilations were occurring. Changes in angular correlation as a function of temperature were reported by I. K. Mackenzie, *et al*² in the same year for several metals. In 1967, MacKenzie and his colleagues³ at Dalhousie University in Canada, and S. Berko and J. C. Erskine at Brandeis University published papers suggesting a relationship between point defects and positron annihilation. A model first proposed in 1965 by W. Brandt⁵ of New York University was subsequently applied to the experimental results by several groups independently in 1969. Among these were B. Bergensen and M. J. Stott,⁶ and D. Connors and R. N. West.⁷ We entered the field at Brookhaven in 1970 as did several other labs, so that by the time of the International Conference on Positrons, held at Queen's University, Kingston, Canada, there was a session devoted in part to papers dealing with positrons and defects. By then it was clear that several groups had begun to determine vacancy formation energies from positron measurements.

In September 1972 at an International Conference on Point Defects and Defect Clusters that Seeger helped to organize, he elected to review the status of positron annihilation as it applied to the study of point defects. He subsequently has published that review⁸ and at least three others. Seeger's knowledge of the physics of imperfections in solids is profound. Thus his reviews have been very helpful, the more so because they were objectively written at a time when there was no experimental activity in this field taking place within his own institute.

The third International Conference on Positrons was held at Helsinki in 1973. This time the number of positron papers relating to defects was large. New positron groups are still being formed throughout the world. I understand that one of them will be in Stuttgart. But for nearly a decade progress toward the application of positron annihilation to defects in solids has been taking place elsewhere.

References

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A. N. GOLAND

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The error attributed to Amaldi was introduced by our staff in the process of editing the original tape-recorded interview and not caught in proof.

EDITOR

More on gravity waves

Joseph Weber's reply to my letter in December 1974 (pages 9, 11) in no way addresses my claim "that the Maryland group has published no credible evidence at all for their claim of detection of gravitational radiation." He states "My first-hand knowledge is based entirely on other data including real-time counting and pen-and-ink records," but this evidence has not been published. Q.E.D.

Weber brings my own experiments into discussion,^{1,2,3} but these have nothing at all to do with the point of my letter. Indeed, the reader may judge the correctness of our conclusions shown graphically in figure 3 of reference 3,—that one month of data for our 480-kg bar show conclusively the absence of pulses intense enough to have been detected with good efficiency by Weber,⁴ assuming that his published events were indeed gravitational waves.

I shall not reply to Weber's innuendo, but I must respond to two criticisms he makes of our experiments:

1) That our equation 2 (reference 2) cannot be correct "because the detector relaxation time is not contained in it,"⁴ and

2) that "Garwin apparently overlooked the great importance of temperature control or automatic tracking of his cylinder with a reference oscillator."

Briefly, Weber seems to have misread our papers:

1) Equation 2 calculated P_{fa} , the "probability of false alarm"² (in one antenna). The rate of false alarms was

calculated in the next paragraph² as

$$r_d = (P_{fa})^2 / \tau_E$$

where τ_E is precisely the detector (energy) relaxation time.

2) In fact, our published results are insensitive to frequency offset of the reference oscillator or to temperature drift of the bar because for each data block (3 min¹ or 455 sec³) "Each data block is then processed by a computer which first computes the autocorrelation function and from it the decrement δ of the bar ($\delta = \pi \tau f_0 / Q$) and its offset ($f_0 - f_B$). These data are then used to predict from each pair of amplitudes [a vector $\mathbf{v}(t_n)$ in the phase plane] the amplitudes of the next point τ seconds later,

$$\mathbf{v}^*(t_n + \tau) \equiv \mathbf{v}(t_n) \exp(-\delta)$$

after obvious corrections for frequency offset."¹

Indeed, the effect of any imperfection of compensation is included in the measured system noise temperature T_e of 29.2 K¹ or 18.5 K.³ James L. Levine has reviewed the computation for a typical 4-hr stretch of the data³ and finds that the contribution to T_e from this source was 4×10^{-2} K. The details are available to anyone writing the author.

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R. L. GARWIN

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WEBER RESPONDS: Real-time counting data were sent to Richard Garwin in a letter dated 8 February 1974.

It is regrettable that Garwin continued to publish incorrect information about Maryland experiments after submission of his earlier letter in PHYSICS TODAY.

In the 23 September 1974 *Physical Review Letters* (page 797), Garwin states that a Maryland computing error "was shown to account for essentially all of the zero delay excess events on a four day tape . . ." Figure 1 is a histogram for this four-day tape. The computing was checked by three independent scientists. The zero-delay excess is 5.7 standard deviations.

It is also regrettable that Garwin's clever instrumentation gives poor sensitivity. The experiments search for excitation of the normal mode of an elastic solid, by gravitational waves. Two

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