some other observers. But if tachyons can have curvilinear trajectories, then their trajectories are not uniquely determined by the position and velocity at one instant of time; there must be other parameters, presumably interpretable as new dynamical quantities.

What is needed is a relativistic statement of a law of motion for charged tachyons or (equivalently) a description of the complete family of possible trajectories.

A possible "out" is to assume that tachyons are uncharged, and also lack all other electromagnetic properties (for example, dipole moments) that could cause Cerenkov radiation. Even this stringent requirement is insufficient; one must also consider the possibility of Cerenkov-like radiation with quanta other than photons. In fact, it is not obvious to me that tachyons can have *any* interactions with non-tachyons, without automatically becoming sources of Cerenkov-like radiation.

HOWARD ROBBINS
Thousand Oaks, Calif.

Rutherford portrait

I should like to congratulate you on the cover of your December 1971 issue, the reproduction of Oswald Birley's portrait of the late Lord Rutherford. It gave me great pleasure for it is the best reproduction of this portrait that I have ever seen; and, in my opinion, Birley's portrait is the best portrait of Rutherford that exists.

I remember the occasion very well, for it was my good fortune to have some talk with Birley about it.

A few years later, Birley painted a copy for the Cavendish Laboratory. This gave him much more trouble, but he thought that the copy was a better picture if not a better portrait.

JAMES CHADWICK
Cambridge, UK

For more on Rutherford and Chadwick see article by Charles Weiner, page 40.

Second-class currents

I read with interest your report "Do second-class currents exist in beta decay?" in the November 1971 issue (page 18). However, a few important facts should be added.

First of all, I note that only half of the reasoning is quoted from my paper [*Phys. Lett.* **34B**, 395 (1971)] explaining why the mass-18 and mass-30 values should be excluded from the discussion. The original reasoning, where besides the fact that these points are comparisons of successive positron emissions, the smallness of the energy differences in the decays are also used, enables us to exclude the above points from the discussion independently from the concrete form of the anomaly of the $(ft)^+/(ft)^-$ values.

(To see this in some detail we write $(ft)^+ = (ft)_0(1 \pm \delta(W^\pm))$ where $\delta(W^\pm)$ is the small charge-dependent correction term. Then $(ft)^+/(ft)^- \approx 1 + \delta(W^+) + \delta(W^-) = 1 + \delta$, and $(ft)_1{}^+/(ft)_2{}^+ \approx 1 + \delta(W_1{}^+) - \delta(W_2{}^+)$, where $\delta(W_1{}^+) \approx \delta(W_2{}^+)$ if $W_1{}^+ \approx W_2{}^+$.)

Using the remaining points we could conclude that the anomaly is energy independent with large probability. Later this was proved by the excellent work of Denys Wilkinson and David Alburger (*Phys. Rev. Lett.* **26**, 1127 (1971)).

Another important point is the use of the K/β^+ ratios to decide whether the second-class currents exist or not. This unique approach gave the first evidence that the magnitude of $G_{\rm IT}$ necessary to explain the $(ft)^+/(ft)^-$ ratios is impossible.

After one rules out the induced tensor interaction, the situation is indeed not too clear with the $(ft)^+/(ft)^-$ ratios. However a very promising contribution to the problem was made by J. Blomquist [*Phys. Lett.* **35B**, 375 (1971)], who recalculated the overlap integrals for the $A = 12$ system using the Cohen–Kurath intermediate coupling. He was able to explain the $(ft)^+/(ft)^-$ ratios in this case, and the same is expected for the $A = 8$ system. It remains to be seen what the calculations give in other cases.

ENDRE VATAI
Institute of Nuclear Research
Debrecen, Hungary

Question of genius

As a retired engineer and physicist I have been collecting material and planning an essay on the characteristics of professional technical people. My interest was again stimulated by the recent article "On Trying to Understand Scientific Genius" by Gerald Holton, published in the Winter 1971–72 issue of *The American Scholar*.

I differ from Holton's philosophy in some respects and am making the effort to have these differences published. In particular, I would like to use an article published in one of the New York newspapers in about the years 1930–1940. Unfortunately I have lost all references to the published story and am hoping that a reader could aid in naming the person involved and the approximate date of occurence. The story as I remember is as follows:

A physicist, a man whose name, very unfortunately, I have lost, was publicly

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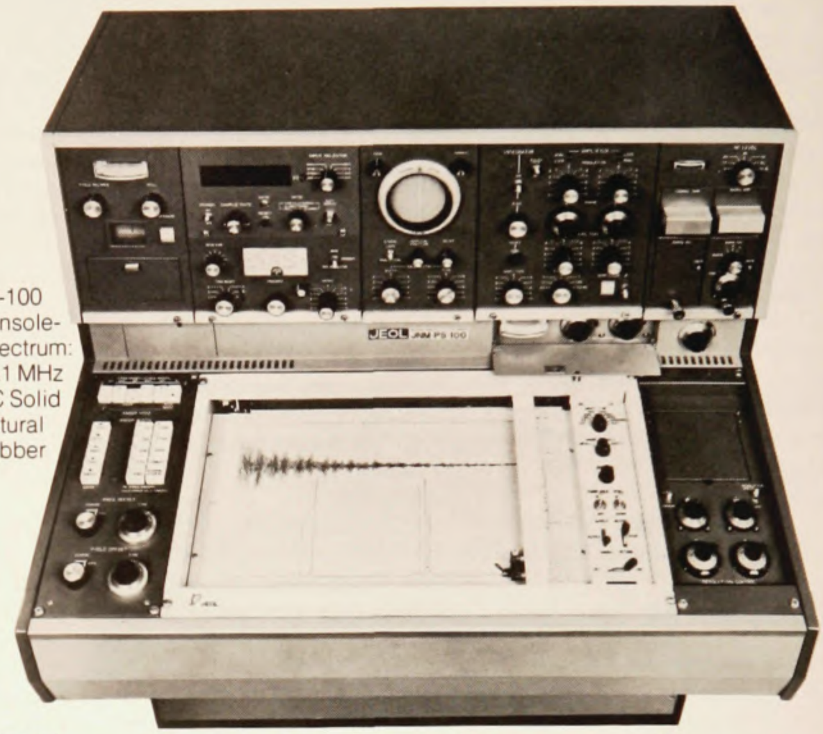

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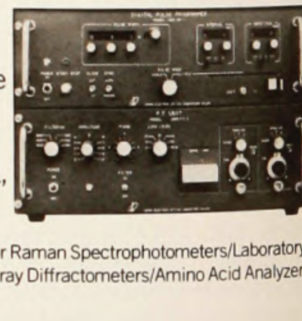

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banqueted and honored at a gathering of scientists and interested people. He was honored for his achievement in extending our knowledge in physics in a brilliant piece of work. After the meeting was over and the man in question had retired to his hotel room a reporter knocked at his door to request permission for an interview for the publication of the physicist's personal story.

Invited into the room, the reporter said that he felt very timid in the presence of such a great genius. The physicist replied that if the reporter intended to use the word "genius" he would withhold his permission to publish. The reporter agreed to the request.

He told the reporter that while conducting his experiments for his PhD thesis he noticed a strange phenomenon unconnected with the purpose of the thesis. He became so interested in this situation that his adviser warned him that he had better "get back on the track" or his time would run out and he would not be granted his degree. Heeding his adviser's warning he went back to work, completed his assignment successfully and was granted his degree.

Taking a position on the faculty of a university he was permitted to continue his work on his unexplained phenomenon. He told the reporter that he had spent something like five hours every day thinking, experimenting and studying. There were a succession of failures and new efforts. Finally, after many years, the pieces of the puzzle fit together and he published his findings. His classical comment: "If I had been a genius I could have solved the problem in a day . . ."

CHARLES S. THOMPSON
618 South Hadley Road
Fort Wayne, Indiana 46804

Solar economics

In their paper on solar power (February, page 44), the Meinels state: "The central problem is economics." Perhaps because it was intended to emphasize physics, they devote only one rather confusing paragraph to the economics. I have attempted to work their problem "backwards" but come to some conclusions that do not agree with theirs.

Solar input to one square meter is one kilowatt when the collector is normal to the sun's rays on a clear day. Assuming use of tracking mechanisms on the collectors, about eight hours of such collection can be achieved per day. Using the Meinels' figure of 330 clear days per year, the annual average input per square meter is 0.3 kW. At their assumed efficiency of 30%, this produces 0.1 kW of electric power. At $60 per square meter, the cost of the collecting system is $600/kW, about twice that of nuclear plant using a light-water moderated reactor. The cost of the steam generator, the turbo-generator, and the steam condensing and recycle equipment must be added to this. This would run about $200/kW more. We thus have about $800/kW total capital costs. With the Meinels' economic ground rules, this comes to 10.5 mills/kWh for the power cost at the generator. The cost of the collector-storage system alone would be 8 mills, somewhat more than the 5.3 mills taken as a target.

The actual situation may be considerably worse than this. J. E. Rink and J. G. Hewitt (*Proceedings 1971 Intersociety Energy Conversion Engineering Conference,* Boston, Mass. pp. 15–22, August 1971) by an examination of Arizona weather records concluded that, because of the successive cloudy days that would be encountered during winter, the collection area and energy storage capacity should be increased by a factor of 8.8 rather than the 3.3 used in the analysis above. In this case, the collector-storage system cost would rise to $1600/kW of electric power and the contribution to the power cost would be 21 mills/kWh.

It would be interesting to know what the cost of a collector-storage system would be if it were built today. How much improvement will be required to meet the Meinels's target cost, or the even lower costs that may be required in view of the above?

PAUL F. GAST
Argonne National Laboratory
Argonne, Ill.

THE AUTHORS COMMENT: Paul Gast focuses upon several points that in brevity were either omitted or only briefly discussed. The costs we quote refer to "fuel" costs, since only the source of energy has been changed. Our figure of 5.5 mills/kWh should therefore be compared to 5.0 mills/kWh for the cost of natural gas when used as fuel for a power plant. The cost of the generating equipment and heat exchangers are common to both energy fuels and additive to the final bus-bar cost of the power. Our cost figures say that solar power will cost not twice that of a nuclear plant, but more like three times as much at a unit cost of 60 $/m².

As for the winter-weather situation, if one makes the size of the solar collector large enough to meet the winter power demand there are three consequences:

- Increased collector area
- Increased thermal storage capacity
- The need for a rapid recovery of the energy in storage (before the next cloudy period occur), which also amplifies the

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