protons and alpha rays on atomic nuclei. In the last-named irradiations one finds only nuclear transformations involving release of electrons, protons and helium nuclei, whereby in the case of heavy elements the mass of the irradiated nucleus is changed only a little, so that near-neighboring elements come into being. It would be thinkable that in the case of bombardment of heavy nuclei with neutrons, these nuclei disintegrate in several rather *large* [Noddack's emphasis] fragments, which are no doubt isotopes of known elements, but are not neighbors of the irradiated elements.

She was not taken seriously,[3] and the identification of fission was postponed for five years.[4]

#### References


1. E. Fermi, Nature **133**, 898 (1934).
2. I. Noddack, Angew. Chem. **47**, 653 (1934).
3. I. Noddack, Naturwissenschaften **27**, 212 (1939).
4. O. Hahn, F. Strassmann, Naturwissenschaften **27**, 81 (1939).



JOHN MCCUE

*Lexington, Massachusetts*


*2/84*

THE AUTHOR COMMENTS: It is true that Ida Noddack's idea of nuclear disintegration was ignored. But Noddack herself did not perform any further experiments to support her speculation, even though as co-discoverer of the element rhenium she was in a good position to investigate the chemistry of the supposed element 93, at the time presumed to be a higher homologue of Re. Possibly she was not taken seriously because in 1925 she and her husband claimed to have discovered element 43, but their findings could never be verified.[1] Noddack's suggestion thus remains a curiosity in the history of physics—more a premonition that came true than a scientific accomplishment. In his review article,[2] L. A Turner commented,

> If [Noddack's] early suggestion of what has turned out to be the correct explanation was anything more than speculation it is regrettable that the reasons for its being considered plausible were not more fully developed. It seems to have been offered more by way of pointing out a lack of rigor in the argument for the existence of element 93 than as a serious explanation of the observations. . . . It seems to have had no influence on the subsequent course of events.

#### References


1. Fritz Krafft, *Im Schatten der Sensation, Leben und Wirken von Fritz Strassmann*, Verlag Chemie, Weinheim (1981), page 316.
2. L. A. Turner, *Rev Mod Phys* **12**, 1-29 (1940).



RUTH L. SIME

*Sacramento City College*

*Sacramento, California*


*3/84*

## Rewards for students

Allen Rothwarf (March, page 142) identifies the crisis in education to be that we "expect children to do what no adult would do . . . work hard for no payment." I am sure he means *immediate payment in money*. Many adults do their hardest work in early years, learning, founding businesses, investing in themselves, toward the prospect of future rewards of only estimated probability. Rothwarf seems also to confuse rewards with money, which, while absolutely necessary at some level, is often not the major incentive compared to those of intellectual satisfaction, discovery and peer recognition. What led Rothwarf to Drexel rather than to General Motors?

I must suppose that there are children who will respond to "hard subjects" in response to the carrot of a bicycle or $50. For those who would, and otherwise would not, there's no place on my staff.


HENRY ERNST SOSTMAN

*Yellow Springs Instrument Co., Inc.*

*Yellow Springs, Ohio*


*4/84*

## Mediterranean summer

Last summer, while sailing among the islands off the Mediterranean coast of France, my wife and I came across a sloop named *Phys Rev*. This seemed odd, so we went over and discovered that its owners are both physicists for whom the *Physical Review* has the status of an icon, hence a logical name for their boat. Since there are French boats with names like *Oesophage Boogie* and *Credit Agricole* around (not to mention American boats with names like *Katrinka Finklesplatt*), this seems an unusually reasonable choice these days.


ARTHUR BEISER

*Le Brusc, France*



## Pledge against nuclear arms

It is heartening to see, in recent issues of PHYSICS TODAY and other publications, that more members of the scientific community are slowly opening their eyes to the ever-increasing threat from nuclear weaponry. It is high time that we of the scientific community come out of the thick shells of elitism with which we have long covered ourselves. In our view, most scientists in nonsocialist countries can today be broadly divided into two types: those who either lock themselves away in their labs or become grossly obsessed in their intellect as contained on a chalkboard, and those who are more than happy to indulge in "corporate prostitution" and sell themselves as a commodity without stopping for a moment to think about the socio-economic motives and influence of their employers. In both cases, we consciously or unconsciously push our social responsibilities under the rug. We are oblivious as to whether our work is irrelevent or even harmful to the masses. If we believe that our work is universal and are aware that the consequences of our work can lead toward destructive ends, then we must acknowledge our obligation to all people without regard to national or political boundaries.

We conclude that there is not a lot we can do, given the present socio-political morass in which we are embedded. Since this very situation ultimately directs scientific activities, however, we should take a clear and conspicuous stand on relevant issues and try to influence political decisions. This thesis can be easily applied to the case of nuclear arms, which most of us, we hope, consider an urgent and pressing issue. Though the governments of various countries are to blame for the monstrous nuclear calamity facing us today, we scientists are guilty of perpetuating the nuclear threat. After all, we are the ones who actually carry out the sinister projects for our governments. We, then, can do far more than sign petitions that do not seem to take us anywhere; an organized campaign by the scientific community as a whole would have more influence in removing the threat of nuclear war than any other group would have. The campaign can begin with each of us pledging that we will never involve ourselves in any way with the development or production of nuclear arms.

Of course, this campaign would result in a greater competition for fewer jobs. We may have to settle temporarily for less lucrative jobs. At least we won't be a part of the death racket any more. And once we have achieved our goals, the industries would have to invest in less harmful sectors to make profits (and reinforce our job market).

So let us take the step to redeem ourselves of the guilt. Let us prove to the world that we are not a bunch of spineless impotents. We must show the world whose side we are on.

MICHAEL KELLISON
*Rutgers University*
*Piscataway, New Jersey*
*4/84*

## Birth of synchrotron

In his article on the birth of the synchrotron (February, page 31), Edwin McMillan mentions his letter to the editor of the *Physical Review*, in which he said in reference to his getting the idea for the synchrotron, "It seems to be another case of independent occurrence of an idea in several parts of the world, when the time is ripe for the idea."

He probably should have added to that statement, "and you are lucky enough to be in a place where people will listen to new ideas."

The first time I heard this idea—in almost the same words McMillan wrote to Lawrence—was from Robert Moon at the University of Chicago. It was in 1939 at the seminar where Sam Allison first spoke about the discovery of nuclear fission and the possibilities of a nuclear bomb; perhaps it is because of the juxtaposition of the two events that I so vividly recall it. Moon said to me, "People say that there is a relativistic limit to the power of a cyclotron, due to defocusing with the relativistic increase in mass. But I think it would be easy to overcome this by just frequency modulating the Ds to keep up with the particle mass."

Nothing happened to Moon's idea at the time, just as he was unable to get the co-ax line used on the cyclotron he had designed and built in 1936: The head of the project said it would have to be built like that at the University of California, since Lawrence was the expert! So it was Dunning who built (and received credit for) the considerably improved cyclotron with the far more efficient co-ax tuned circuit.

Moon is still active at the University of Chicago, where he now has been for over fifty years. And I have been unhappy about his not getting the credit he should have received, for over thirty years!

I have checked my recollection with Moon, who confirms my memory precisely. We seem neither of us to have lost all our memory, despite our advancing years!

FRANKLIN F. OFFNER
*Northwestern University*
*Evanston, Illinois*
*3/84*

THE AUTHOR COMMENTS: The condition for resonance in a relativistic cyclotron demands a certain relation between the magnetic field strength, the frequency of rotation and the particle energy; to maintain this relation as the energy increases during the course of the acceleration, one (or both) of the other quantities could be made to change with time according to a properly designed schedule. This fact has been known to accelerator designers ever since Bethe and Rose pointed out the existence of the relativistic limit in 1937.

Franklin Offner in the foregoing letter tells of one case of the recognition of this fact, by Robert Moon at Chicago in 1939. He does not say whether Moon accompanied his suggestion with the idea of phase stability; if not, it would understandably have been considered impractical because of the high degree of precision that would seem to be required to maintain resonance over large numbers of turns of the particles. I suspect that many people made the same suggestion but never carried it farther because of just such practical considerations.