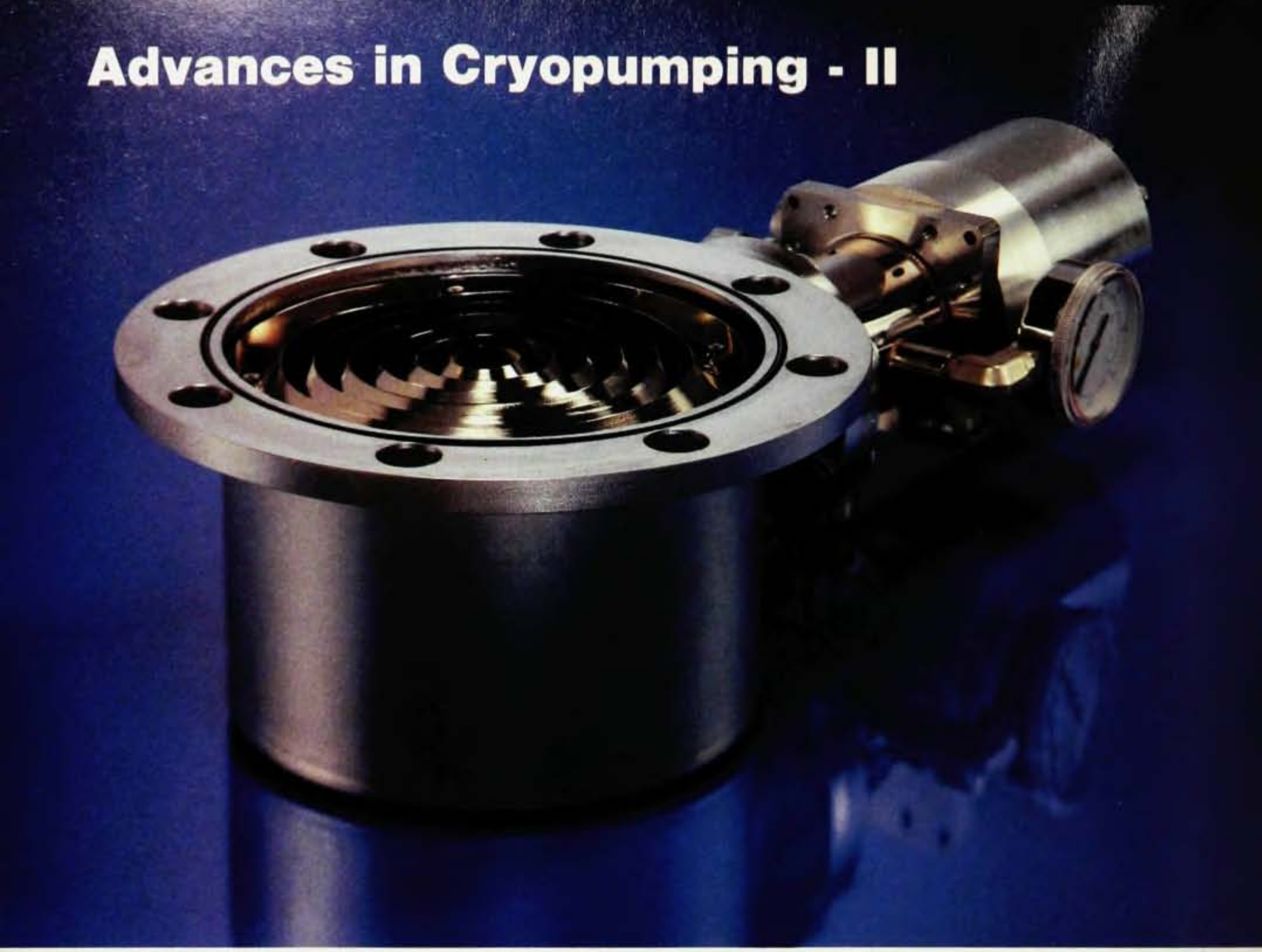
letters

More unpublished physics

I found the article "Some physics not in the *Physical Review*" by Robert Varney in October (page 24) exceedingly interesting. I went to Berkeley as a graduate student in physics in 1928—the same year Ernest O. Lawrence arrived as an associate professor. While at Indiana University I had learned that Lawrence was going to Berkeley and I had decided then that I would like to work for my PhD degree under his direction. As a consequence, I became one of Lawrence's first graduate students.

I offer the following additions to Varney's review: I received my PhD degree under Lawrence near the end of 1931. My thesis was entitled, "The Photoelectric Properties of Thin Alkali Metal Films." Father Macelwane, a well-known seismologist, came to Berkeley about this time looking for a new faculty member for his institution—St. Louis University. He talked to Lawrence, who recommended me for the position, after which Lawrence convinced Macelwane that it would be in their interest to pay me a monthly salary as a research associate while staying at Berkeley until the term opened in September 1932 at St. Louis. They agreed to do this, and so I was being paid by St. Louis University when I started and completed the first nuclear disintegration in the US. Livingston had completed his work on the 11-inch cyclotron and Lawrence asked me to work with Livingston on the next larger cyclotron, the 27-inch. I worked on the 27-inch from January 1932 until about mid-April of that year. Lawrence wanted to assign Milton White to work on the 11-inch, but White had a summer job to go to starting about the middle of May. Lawrence then suggested that I work on the 11-inch. White and I, in our first discussion, privately questioned whether Lawrence and Livingston had sufficient evidence for the existence of high-speed protons. We decided to make a direct test ourselves by seeing whether or not these protons would penetrate an appropriate thickness of mica. We constructed a fan-like disk, composed of different thicknesses of mica, which could be rotated in the path of the proton beam by turning a ground-glass joint from outside the vacuum chamber. From the mica thickness that the beam penetrated we found that we indeed had high-speed protons—of the order of one MeV. As far as I know, this work was never published in a scientific journal, for, at the time, we did not want Lawrence or Livingston to think we doubted their claim of having high-speed protons.

When White left for his summer job, I prepared to study the ionization probability of high-speed protons in nitrogen using the 11-inch cyclotron. Some time after making preparations for the ionization experiment, I received an urgent telegram from Lawrence, who was on his honeymoon on the East Coast, telling me to stop the work I was doing. Cockroft and Walton, he said, had disintegrated lithium with high-speed protons. Would I see if I could duplicate their work with the cyclotron? Cockroft and Walton had used metallic lithium, but from my previous work with lithium metal I was familiar with the difficulty of controlling the metal and I therefore decided to use a lithium salt. The stockroom clerk in the chemistry department opened up a display case with a number of lithium salt samples. I selected a lithium-fluoride crystal which appeared to be near perfect (transparent) and just the right size for our purpose. Next, we spent considerable time preparing and altering the cyclotron, including installing and positioning the lithium fluoride crystal, to prepare for the disintegration experiment. Toward the end of July, Donald Cooksey and Franz Kurie, who had come out from Yale University on a vacation, came into my room at 216 LeConte Hall. They were as excited as I about the experiment and asked if I would like them to work with me. I was particularly pleased to welcome them, as Cooksey was an expert on Geiger counters. They then said to me, "You see that there is a proton beam and we'll see that the necessary equipment for detecting helium nuclei—the product of the disintegration of lithium—will be ready." Cooksey and I proceeded to build the Geiger counter according to his design. It consisted of a brass



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cylinder supported above a glass plate by short sticks of red sealing wax. Along the axis of the cylinder was placed a thin platinum wire with a bead on the end. The cylinder was open to the air at atmospheric pressure and was positioned within a few millimeters of the exit port of the cyclotron vacuum chamber. Before positioning the Geiger counter, we tested it for the detection of alpha particles from a polonium source. Franz Kurie secured some glass radon "seeds" (from a hospital, I believe), smashed them in a small dish, poured in acid and then inserted a nickel wire. We deposited polonium on the nickel; this served as an excellent source of alpha particles. The potential on the Geiger counter could then be adjusted so that it could be used to detect alpha particles only and not respond to gamma rays or other background radiation.

It was August 1932 when all the equipment was tested and found to be in working condition. A vacuum-tube amplifier and a pair of headphones were connected to the output of the Geiger counter. With the proton beam adjusted for maximum current, we listened on the headphones for the characteristic "clicks" produced by the helium nuclei from disintegrating lithium. We obtained the desired results—the lithium was disintegrated by the bombarding protons.

I left Berkeley the last week in August for my upcoming marriage in Bakersfield and for my new position at St. Louis University. As Lawrence was expected back "at any minute," I left the 11-inch cyclotron with its lithium fluoride crystal, the Geiger counter and other detecting equipment in such good working condition that Lawrence, Livingston and White sent their letter to the editor of the *Physical Review* on 15 September, just a little more than two weeks after we had left!

James J. Brady
Oregon State University
Corvallis, Oregon
12/82

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Varney wrote, "Late in 1932, heavy water was discovered by Harold Urey." In fact, *heavy hydrogen* was discovered by H. C. Urey, F. G. Brickwedde and G. M. Murphy late in 1931. This date is important because the discovery of deuterium preceded the discovery of the neutron in 1932.

The clinching evidence for a heavy isotope of hydrogen was obtained Thanksgiving Day 1931 and the discovery was reported at the annual meeting of The American Physical Society at Tulane University, 28–30 December (1931) by the three authors named above.

The method for separating the isotopes of hydrogen by the electrolysis of water (the oxides of hydrogen and deuterium) was discovered in 1932 by E. W. Washburn, Chief Chemist, National Bureau of Standards. Urey recognized this in his Nobel Laureate Lecture.

Varney also reported: "In the summer or fall of 1933, Lawrence had visits with Rutherford . . . . and they thrashed out the topic of the name for the isotope of hydrogen. According to the story, Rutherford agreed to give up the term diplon [for the nucleus of the heavy isotope of hydrogen] in favor of the term deuton provided the latter were modified to include his initial, *R*, in it and so the term 'deuteron' was coined."

Soon after the discovery and availability of deuterium, Lawrence and Rutherford and their laboratories were busy using the heavy hydrogen nucleus as target and impinging particle in nuclear experiments. Interesting and unexpected results were obtained. For convenience in discussing these experiments and results, a name was needed for the heavy hydrogen nucleus. Lawrence and his co-workers chose *deuton*, Rutherford and his co-workers used *diplon*.

Urey was urged by colleagues to propose a name for the heavy isotope, as it was considered the prerogative and responsibility of the discoverer of a new chemical element to propose a name. In a letter, submitted 15 June 1933 to the editor of the *Journal of Chemical Physics*, Urey, Murphy and Brickwedde proposed the term deuterium for heavy hydrogen. Protium and deuterium were derived from the Greek words *protos* and *deuteros*, meaning *first* and *second*. As proton was already in general use for the protium nucleus, before the discovery of deuterium, the term deuteron was a natural extension.

F. G. Brickwedde
The Pennsylvania State University
University Park, Pennsylvania
12/82

More on classification scheme

John T. Scott's reply on behalf of AIP to Jeff Hecht's letter complaining about the Physics and Astronomy Classification Scheme (PACS) used for the subject indexes of AIP and most member-society journals (June, page 83) needs some supplementation. The editors of the AIP and member-society journals, who comprise the AIP Publication Board, are well aware of the existence of complaints about PACS, and are concerned about them. PACS has been a much-discussed agenda item at recent Publication Board meetings. Largely as a result, two separate efforts are