Letters

"Pay As You Go" Cyclotron

I read with a great deal of interest and pleasure the reminiscent article by Kamen on carbon-14 [*Science* **140**, 584 (10 May)]. There is a little more to this story. At Yale, between 1936 and 1938, I had been endeavoring to induce the administration to support the construction of an accelerator of some kind, and in 1938 we succeeded in getting approximately $2000 as a start on a cyclotron, with the promise of perhaps an additional $1000 per year. So we began the construction of what must surely be the cheapest "pay as you go" cyclotron of any size ever built. We had progressed to the stage of having a fair amount of steel and some pole pieces when Ernest Lawrence paid one of his periodic visits to his old haunts. The moment he walked in the door he said "Oh, you have a real magnet, this is a reasonable size. Why don't I give you our old 28-inch 'can' and you make this fit it?" We took him up on this on the spot, introduced some sleeves in the steel of the pole faces, which radically altered the characteristics of the magnet, of course, and wound coils on it; and then in due time the freight company delivered the Lawrence and Cooksey cyclotron. It came complete with a list of leaks, which we found later to be quite correct though not exhaustive, and by 1939 we were beginning to get a very feeble beam out of the completed cyclotron. By the time the first beam came in, this instrument must have cost about $3200. Howard Schultz, Bill Davidson, and I did the work. This actual vacuum chamber, to which I here claim title, is now on loan to the Smithsonian Institution.

After the machine had been running a little while, we had a beam up to perhaps half a microampere at best, on an internal target, and an energy of 3 Mev. The poor performance was due to the fact that we could not afford the tubes for the power output stage and had to operate off the driver stage. One evening I began thinking about the problem of C^{14} and realized that it should be possible to observe the protons from the reaction $C^{13}(d,p)C^{14}$ even though C^{13} is present at such a small percentage, if the energy change value is much higher than that of the $C^{12}(d,p)C^{13}$ reaction, as it should be. If I could measure the energy change in this reaction and prove it to be of the right value, I would know the mass of C^{14} and could then estimate whether it was stable or not.

Since Schultz was busy building his cloud chamber and Davidson was busy on his Ph.D. thesis, I put a target in the little bombardment chamber and ran an absorption curve on the protons that came from it. To my great pleasure, I found a reasonably good supply of them and a moderately good cross section; I was able to measure their range with an accuracy which, for those days, was fair. Using the range energy relation, I was able to get the mass of C^{14}, and it seemed sufficiently great to guarantee that C^{14} must be radioactive, with, I then thought, a maximum energy of about 300,000 ev. This was sent off as a letter to the *Physical Review* [**56**, 1168 (1939)] with some auxiliary information from the $B''(\alpha,p)C^{14}$ and with the suggestion that C^{14} would be unstable.

I made an attempt to measure the half-life, and I was fascinated to read that the same problem was being encountered at Berkeley not much later. I put a carton probe on the internal target and kept the machine running for some 10 hours, which was a major achievement, and then having let it decay for a while to eliminate N^{13}, I tried it for activity. I suppose I could have detected ½ disintegration per second. I estimated that I had somewhere between 10^9 and 10^{10} C^{14} atoms in the target, and if the activity had been under a year or even a few years I would have detected it for certain. As we know, it turned out to be many years.

The actual process of establishing the character of a radioactive element involves much more than looking at an activity on a target. I was well aware that the group in Berkeley was much better equipped to produce the sample and conduct the chemical proof of its nature. It seemed sensible then, and even more sensible now, to leave it to them.

I think there must be a third story, for quite independently, as far as I know, Bower and Burcham observed the same reaction in the Cavendish Laboratory.

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Volcanic Eruption in Bali

Early in the morning of 17 March 1963, the volcano Gunung Agung in east Bali erupted and great quantities of lava poured from the northern edge of the crater. Volcanic ash was ejected and carried west by a steady wind. Ash deposits of 15 to 30 cm were reported in western Bali, but the fallout was very light in Den Pasar in the southern part of the island. Altogether some 1150 persons were reported to have lost their lives.

By noon the ash had reached Surabaja, 340 km (215 miles) by air to the west. The skies became dark and one would have thought it was midnight. About 1 P.M. light ash began to fall, and by 5 P.M. its rate had reached 660 grams per hour on each square meter. After about 15 hours the ash rain ended, and the average fallout thickness was approximately 11 millimeters and weighed 6.1 kilograms per square meter—17,000 tons per square mile. These figures are believed to be accurate to plus or minus 20 percent. The aqueous extract of the brown-gray pumicelike deposit was definitely acidic, having a *p*H of 5.

All of east Java was covered. The fallout of volcanic ash was perceptible, but slight, as far west as Bandung and Bogor.

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