work. This is precisely what followed at the Argonne National Laboratory after Bartlett's discovery. The opening sentences of the report claiming the preparation of xenon (IV) fluoride and submitted to the *Journal of the American Chemical Society* on 20 August 1962 (*4*) confirms this:

> The first true compound of xenon, $Xe^{+}PtF_6^{-}$, recently was reported by Bartlett. This suggested to us the possibility that under some conditions of temperature and pressure, xenon might be oxidized by elemental fluorine.

Information of Bartlett's work, having thus removed academic skepticism, necessitated a crash program, and this is precisely what the Argonne National Laboratory ably provided.

Of all the scientists engaged in this fascinating new field of inorganic chemistry, the only skeptical ones, perhaps, were to be found among Hoppe's group at Münster, West Germany (*5*). These workers had been engaged on the possibility of synthesizing compounds of xenon and fluorine early in 1962, and had it not been for problems of supply, they might well have prepared the first authentic noble gas compound, namely xenon (II) fluoride, before May 1962 —the date of submission of Bartlett's report.

Finally, the lead to present-day knowledge of over 20 noble gas compounds came indirectly from unrelated work on the preparation of $O_2^{+}PtF_6^{-}$ and the subsequent correlation of molecular ionization potential of oxygen and of atomic xenon, and surely not from skepticism.

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My attempt at a broad-brush sketching of the status of science and scientists in the 1960's has evoked, in addition to numerous expressions of interest and commendation, several letters of criticism. My critics have been lenient, and I welcome this opportunity to correct any errors or misimpressions.

Gordon Tullock, of the University of Virginia, points out in a private communication that I was correct in saying (p. 14) that the first significant naval action of World War I (the battle of Coronel) was fought off the coast of Chile, but that the engagement was won by Admiral Spee's German squadron, not by the British, and that nitrate ships, if involved, were probably not German but British.

Claassen, "the young physicist" I referred to in discussing my fear that our present-day highly organized, large laboratories might stifle the creativity of the individual scientist, says in his letter in *Science*, "How serious this danger is I do not know, but the example he uses on page 16 about the discovery of xenon tetrafluoride serves rather to indicate the opposite from what is implied." I cannot but agree with this statement. In order to raise the hypothetical questions asked about the number of instances in which circumstances were unpropitious, it was necessary to use a favorable illustration, in which the outcome was, as I stated, a brilliant success, for the simple reason that we rarely learn of the instances of the opposite kind. I regret that my use of this example should have been taken to imply criticism of the handling of research at Argonne. No such implication was intended, since quite the opposite is the case, as the outcome itself demonstrated.

Moody and Thomas give in their letter a clear account of the genesis of ideas and events of the past few years relating to the discovery of noble gas compounds. With respect to skepticism, however, we are apparently at cross purposes. The "need for skepticism" to which Abelson referred in his editorial (*1*), which I quoted, applies to earlier scientific work over a much longer time span, not to the relatively recent work initiated through Bartlett's discovery in 1962. The techniques of producing, handling, and reacting fluorine were available in a number of laboratories abroad and in this country, including the one from which I write [through the work of Bigelow (*2*) and his co-workers] by the early 1930's, and during World War II many other laboratories acquired these techniques. Thus, Abelson's statement that "For perhaps 15 years, at least a million scientists all over the world have been blind to a potential opportunity to make this important discovery" appears to me to be essentially valid and the need for healthy skepticism in the face of entrenched dogma ever present.

As a physical chemist, but not as a physicist, after my amateur's venture in the sociology of science I appreciate a statement in Ziman's review (*3*) of Frauenfelder's book *The Mössbauer Effect*. In criticizing Frauenfelder's account (*4*) of the history of Josephson's early work relating to the effect, Ziman concludes that "all this goes to show that history is much too exact a science for a physicist."

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3. J. M. Ziman, *Phil. Mag.* **7**, 1973 (1962).
4. H. Frauenfelder, *The Mössbauer Effect* (Benjamin, New York, 1962), p. 64.

Impurities in "Pure" Biochemicals

It is common to assume that commercially available biochemicals are sufficiently pure for most chemical studies. However, it is necessary to exercise caution when these materials are used in high concentration, because then trace impurities may be present in amounts permitting some biological activity. I wish to point out that a few biochemicals which I have recently used and which are in the realm of everyday materials possess impurities which are not easily detectable by ordinary chemical means but which manifest themselves by their biological effect.

Deoxyuridine, deoxycytidine, and preparations of fluorouracil deoxyriboside (FUDR) made prior to 1962 contain an impurity which is presumably a thymine derivative. Twenty micrograms per milliliter of each of these substances will (i) inhibit death from lack of thymine in thymine-requiring bacteria and (ii) support their growth to a titer of about 10^7 bacteria per milliliter. Ten micrograms of each of two of them added together has the same effect. The level of contamination is therefore about 0.05 percent by weight. The contaminant cannot be seen as a spot on a chromatogram, but if these materials are purified by chromatography in ammonia-butanol, all activity supporting growth and inhibiting death from lack of thymine

is lost. Some of these compounds were labeled "chromatographically homogeneous" by manufacturers; others were listed as "99%." It should be pointed out that at least one manufacturer sells a special grade labeled "thymine-free." This indicates the vast difference between "chromatographically pure" and "purified by chromatography."

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Archives

Apropos of your recent discussions of the MURA problems (31 Jan., p. 450), I have resurrected the attached document.

AIUR never had the prestige of AMSOC, but it flourished at MIT shortly before World War II and briefly thereafter. Publication dates were irregular, and I think that this was the last. The occasion was the receipt of a letter of intent from Brookhaven for the design of the present Cambridge accelerator. I thought it might have at least historical interest for your readers.

PROCEEDINGS OF THE AMERICAN INSTITUTE FOR USELESS RESEARCH
FEBRUARY 3, 1953

Progress in the design of charged particle accelerators during the past two decades, has been marked by an exponential growth in the energies about to be achieved, of approximately 2.5 db per year. It is well established that the cost of the equipment increases at least linearly with the energy. Thoughtful scientists have been increasingly uneasy about the future because the national income has increased at the rate of only 0.5 db per year over the same period. This leads to the conclusion that there is danger the ultimate expected particle energies will be determined by the national income instead of scientifically acceptable natural laws.

Happily, the initial results of a long-range program sponsored by the Institute lay these fears to rest. Professor L. S. Coupling* reports that there is a limit to earthbound particle energies of 3×10^{15} ev. The result follows since particle energy

$$E = \frac{eb\,R\,E_0}{m_0\,c}$$

Professor Coupling assumes an iron core machine with a practically attainable flux density $B \approx 2$ webers/m^2 and a maximum radius of curvature equal to the earth's, $R \approx 6.4 \times 10^6$ meters.

At the going rate of 10^3 ev/$, the ultimate proton accelerator will cost about $3000 billion, which is only 10 db above present national income. Thus we can easily afford the machine by spreading the payments over 10 years, and a joint effort with Russia would make it even easier.

In addition to these welcome results, Professor Coupling finds that, by a fortunate coincidence, the recently established limiting rate of energy increase of 10^5 ev/cm means that the accelerator can be built to have a length very close to 36×10^6 meters, and will therefore just nicely go around the world once. Besides the economy of having the target and the injection system at the same latitude and longitude, he feels the scientific world will be esthetically gratified at this particularly elegant result. He suggests that the instrument be named the "Circumtron," although other members of the Institute feel that a Greek-derived name is more in keeping with the elegance of it, and prefer "Perimetron" or "Peritron."

* No relation to the notorious science fiction writer, J. J. Coupling.

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One-Tailed Test of Trivia

Mario Bunge is certainly correct in saying (27 Mar., p. 1394) that in research a clear statement of the problem is essential—not only to the reader of the final report but also to the researcher himself, since a careful statement of the problem often suggests the best way to attack it. But I should like to add a cautionary note to his statement that "scientific research does not begin with gathering data but with posing problems." Unfortunately, there are those who go farther and say that the gathering of data must be preceded by a specific "experimental hypothesis."

Many subjects of research involve steady accumulation of data even when the causal relations are so obscure or complex that elaborate hypotheses in advance are impossible, for example in the behavioral sciences or in meteorology. The proposition that one must always have a detailed hypothesis at the beginning and cling doggedly to it until the final bit of the Latin square falls into place is, in my opinion, responsible for many of the trivia that are cluttering our journals more and more. It also leads to that greatest abomination of all, the so-called "one-tailed test of significance," a ploy in which the researcher claims partial precognition and informs us that he knew in advance that treatment X could only produce either an increase in Y or no change, never a decrease.

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