Letters

Moondoggle

One notes increasing congressional resistance against requests to raise the level of federal spending for "research." It seems high time that scientists speak out more frankly about the inordinate waste of federal funds in certain parts of what Congress vaguely regards as the scientific community. In my own field of the atmospheric sciences I have been shocked by details of some of the high-budget, low-caliber proposals recently submitted to federal agencies by science-and-technology corporations that have burgeoned in the recent years of scientific affluence. Scientists primarily concerned with sound and promising research rather than with bigger contracts and fatter budgets must speak out more frankly and critically about each new scientific boondoggle as it comes along, lest support of legitimate and important scientific research soon suffer from indiscriminate congressional reaction. Congress cannot be expected to be capable of easily distinguishing the good from the bad (until it's too late). Congressmen see only the huge volume of federal funds they are being asked to pour into what they lump together as "research and development." They are growing worried, as they should. Consider the following illustrative case. (On a separate enclosure, not for publication, appear the names of the individuals and organizations referred to.)

Last week the director of the space-sciences division of a large West Coast aircraft company telephoned to invite me to serve as a consultant on a "small team of knowledgeable scientists" it is assembling. The object: This company had been invited by a Washington space-oriented agency to bid on a contract to do a 6-month study of scientific missions for a lunar-exploration project. The very idea of regarding as scientific research anything that was planned by a firm which happened to be successful bidder on a contract to do a 6-month study of "scientific missions" would seem unbelievably ludicrous were it not all too ordinary a part of the present-day picture of Big Science.

My expression of surprise that a specialist in cloud physics should be considered a prospective member of a team to suggest ideas for the bid on this lunar study brought the reply that the company thought a program of observations of terrestrial clouds by moon-based scientists would perhaps be a good thing to propose. My jaundiced reaction only led my caller to ask next what I thought about the chances of getting either Dr. X or Dr. Y to help in writing up this lunar cloud-observing portion of the bid. This query clearly revealed the director's complete ignorance of the scientific field involved, for it was necessary to point out to him that Dr. X is primarily expert in the physics of the solar corona and that Dr. Y is a geophysicist primarily interested in problems of the earth's interior! I was next asked if I could suggest anyone else as a possibility; and so on finally to his desperate query as to whether any West Coast schools might have anyone who would be likely to be interested in clouds.

The truly distressing part of that telephone conversation is that it is by no means unrepresentative of what is going on all over the country now that big science means big money. Poorly conceived and poorly executed marginally scientific activities are swallowing ever larger portions of federal funds for what passes, at least in Congress and in many agency annual reports, as "research." Those activities lead chiefly to expensively multigraphed project reports containing almost nothing that adds to the stock of scientific knowledge. If such free-wheeling and overfunded activities carried out in the name of "science" are not more openly criticized, the entire scientific community will inevitably suffer.

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Skepticism and the Noble Gases

The closed-shell, closed-mind dogma has undoubtedly been a stumbling block in the development of the chemistry of the noble gases, and in his account Gross (*1*) has provided a timely appreciation of academic skepticism. Yet in certain respects his remarks fall wide of the actual historical situation.

Immediately after the discovery of the noble gases, many extensive attempts to effect reaction of these gases with other chemicals met with failure. Similar, but sporadic, work over the past 70 years had firmly established a paradoxical situation, in which the six elements, helium, neon, argon, krypton, xenon, and radon possessed no chemistry. This followed despite chemical forecasts, notably those of Pauling, suggesting several fluorides and salts of xenon. To level the charge of skepticism at chemists for not undertaking more experiments in the face of overwhelming evidence, much of it obtained by eminently capable chemists, is quite unfair. To carry Gross's suggestion to its logical conclusion, it would be necessary to indefinitely pursue any series of experiments to no purposeful result despite contrary evidence. A degree of skepticism is valuable, but too much is, of course, ridiculous.

Gross's point concerning the restriction of academic freedom among scientists "under present conditions of highly organized and programmed scientific endeavor" has already been taken up by Claassen (*2*). The fact that 17 scientists can be switched to a collective study of any problem is surely as important as academic freedom, and such staff mobility must have greatly facilitated the preparation of xenon (IV) fluoride and other noble gas compounds. An even better example of the principle of collective study is the recent report in *Physical Review Letters*, by no less than 33 authors, on the omega-minus particle.

Gross implies that the success at the Argonne National Laboratory was due to skepticism concerning the many previous negative results with the noble gases. This surely cannot be true, since Bartlett had pioneered the way to removing the existing skepticism, if any, by the facile realization of xenon hexafluoroplatinate (V), as described in his publication of June 1962 (*3*). Any well-equipped laboratory with experience of fluorine manipulation could have repeated and extended Bartlett's 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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References

1. P. M. Gross, *Science* **143**, 13 (1964).
2. H. H. Claassen, *ibid.* **143**, 917 (1964).
3. N. Bartlett, *Proc. Chem. Soc.* **1962**, 218 (1962).
4. H. H. Claassen, H. Selig, J. G. Malm, *J. Am. Chem. Soc.* **84**, 3593 (1962).
5. R. Hoppe, H. Mattauch, K. M. Rödder, W. Dähne, *Z. Anorg. Allgem. Chem.* **324**, 214 (1963).

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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References

1. P. H. Abelson, *Science* **138**, **75** (1962).
2. See for example J. H. Pearson, L. B. Cook, W. T. Miller, Jr., *J. Am. Chem. Soc.* **55**, 4614 (1933).
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