(for example, *1*, *4*), and no logically defensible system of weighting has yet been advanced by the proponents of the idea that some characters are a priori "more important" or "more basic" than others. There is no reason why information from the genetic system cannot be used in numerical comparisons. However, at this stage it must be asked, In what way is degree of chromosome pairing "more important" than degree of hairiness? It is known that, in diverse organisms, the first may be controlled by a single gene (*5*), as may the latter. But inasmuch as single genes, as well as balanced polygenic systems, may affect gross morphological characters we find it hard to see how such evidence may be employed in a system of weighting. Are we to imply from Webster's letter that the "sound theoretical basis" for weighting of characters is the "experience of most competent systematics"?

Few taxonomists (classical or numerical) have attempted to remove the environmental component and deal only with the genetic variation in their material, for obvious practical reasons. Webster seems to feel that taxonomy ideally would deal only with additive genetic variance, but there is some theoretical question as to whether this would be desirable even if it were practical.

In no place do we suggest a " 'non-Euclidean' theory of classification," although the idea may have merit. We have suggested that the present strong interlocking of taxonomy and evolutionary theory may inhibit the development of a "non-Euclidean" theory of evolution. This does not mean that we decry the existence of taxonomy or repudiate the present theory of evolution. We would not, however, wish to be placed in the position of having to affirm a "belief" in evolution.

Today biological evidence seems overwhelmingly in favor of the neo-Darwinist view of evolution. Therefore it is especially important for us continually to re-examine its most fundamental tenets. In our article we wished merely to point out that certain problems might be viewed in different perspective. We had no intention of retroactively supplanting one approach with another, even if that were in some way possible. When it appears that a road is blocked because the cart is before the horse, there are two possible courses of action. One is to blow up the horse and cart. The other is to send an exploring party up a nearby path while the road is being cleared. We wished to suggest the second alternative.

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A Grain of Skepticism

"What is really needed is more of that healthy skepticism which generates the key idea . . ." [*Science* **138**, 75 (1962)].

Allow me to apply a germ of skepticism to this statement. At just what point did skepticism generate a key idea in the xenon tetrafluoride synthesis case?

I suspect that Neil Bartlett would report that skepticism as such had nothing to do with it. How was the key idea generated? It would be interesting—and surely it is important—to know.

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. . . one must agree that "the essential ingredient in discovering xenon tetrafluoride was not money or equipment," but one must conclude that an essential ingredient was people—17 of them.

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. . . seventeen scientists signed their names to an article that required only "a few hours of effort and a germ of skepticism." I am sure that the salaries for 17 scientists come to a very much greater figure than many elaborate pieces of scientific equipment.

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I suppose that I am not alone in feeling that the editorial "The need for skepticism" points a finger at me, along

with the rest of the million scientists referred to.

Much as I agree with the motivation for this editorial, I am afraid I must disagree with its content pretty severely. The "germ of skepticism" certainly existed in at least two groups referred to by the authors of the report in question. In fact, Yost and Kaye tried the specific elements xenon and fluorine. The fact that they happened to do the wrong experiment was just a break of the game. In my own notebook, in early February 1956, I jotted down "Krypton Fluoride—does it exist?" This was not a speculation out of the blue, but grew out of some thinking I had been doing on electron affinities in connection with my work on static electrification. It seemed clear that the electron affinity of fluorine might be great enough to pull an electron out of a stable rare gas of large diameter. In the same vein, I had considered that cesium helide might exist, but only in a metastable form, since the helium negative ion does not exist in the ground state. I even suggested to one of my colleagues in molecular spectroscopy that he might look for these two substances.

Surely dozens, if not hundreds, of my colleagues in various fields have had similar ideas. Our reasons for not dropping everything to search for these new compounds were not that we lacked the idea or were bound by dogma but that we had only finite resources and had to devote our efforts to what seemed promising and feasible in terms of our experience and commitments. Any of us has an idea or two every day that we should like to follow up, but cannot because of practical limitations.

Abelson evidently had tongue in cheek when he said that it would take only a few hours of effort to perform the experiment. Even if the experimenter knew exactly what to do, he could hardly assemble in a few hours an experimental setup that would permit him to heat elemental fluorine to 400°C! In fact, one might make the point that it was availability of facilities for handling difficult chemicals that made it possible for the Argonne group to carry out their experiments, rather than an excess of imagination. This is all to the good, of course, and I often point out to my students that in going to government laboratories they will have unique facilities.

The Argonne group, together with Neil Bartlett, is certainly to be congratulated for excellent work on a stimulating idea. I don't feel that it is fair to

indict all the rest of us because we did not drop everything and turn our attention to this problem, or to underrate the investment in facilities necessary to carry out the experiments under discussion.

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Perhaps I underestimated the experimental difficulties others might find in conducting the reaction of fluorine and xenon and can speak best for myself. I conducted experiments involving fluorine at temperatures above 400°C during the days when experimenters generated their own gas, and I personally observed that an excellent containing vessel can be made from nickel. For the last two years xenon and fluorine gas have been on hand in my laboratory. We have excellent shop facilities at the Geophysical Laboratory [Washington, D.C.], and the fabrication of necessary experimental equipment merely required a request on my part. Thus, all that was needed for me to make the discovery was a dose of the medicine I have prescribed—a grain of skepticism.—P.H.A.

Dictionaries and Language

Although I greatly admire Warren Weaver, and have for a long time, I feel I must oppose the unrealistic viewpoint he expresses in regard to language [*Science* **137**, 1025 (1962)].

He is much mistaken, I believe, in making analogous use of the particular biological fact he cited in order to indicate how we should communicate as human beings. It is fascinating, but not relevant, that genetic communication is so precise and generally so devoid of error that it rarely allows the introduction of deviation or mutation.

With men, living in the complexities of modern society, communication cannot possibly be so stablized, so simplified, and so free of ambiguity—at least, not until there is complete, global cultural homogeneity, with universality of thinking, language, and responses (and probably not even then).

In our society we speak as we live: according to our roles. Our roles are many, and the ways we use speech are as numerous. One may be a physicist, gardner, husband, father, lover, do-it-yourselfer (don't flip your lid, sir!),