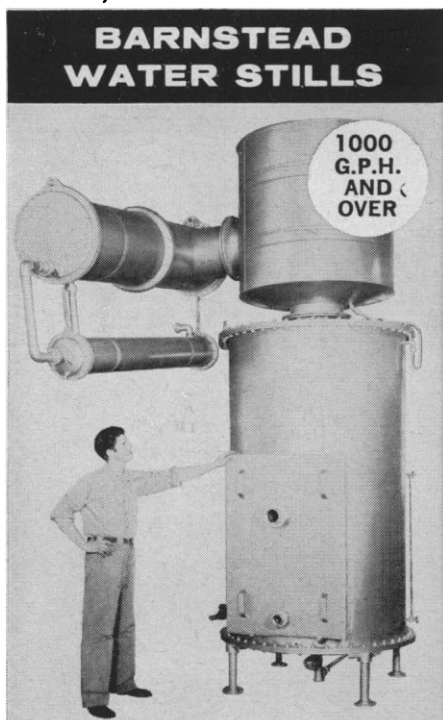




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would have to be interpreted in doubtful cases; ultimately it must be the scientist concerned who decides whether work he is about to undertake is sound intellectually and morally, and we had better select our scientists so that we can entrust them with such decisions. On occasion, a scientist may feel the need to show the rest of us his work as a source of excitement, delight, or inspiration, but the justification of what he is doing—its agreement with a given set of standards, scientific and moral—rests in itself; the record is all that is needed, and nothing else will do. Calling one's results relevant, sensitive, specific, precise, significant, and scientifically valid adds nothing to them.

STEVEN E. ROSS

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San Francisco, California

More about Nomenclature

I have just read K. E. Boulding's letter in [*Science* 131, 874 (1960)]. I don't think that it will be necessary to waste any space on the proposed astronomical classification since, like his "Bimbambim" it probably won't be used by anybody.

But I do wish to protest three statements: (i) that names like selenium, tellurium, or, for that matter, Rhynchocephalia are "unscientific"; the history of science happens to be a science too; (ii) that it is "fortunate" that most scientists are not acquainted with the "dead" languages from which nomenclature is drawn. I, for one, consider this most unfortunate, provided it were true; (iii) that the letter *x* cannot be used to begin a syllable. Boulding may have heard of St. Xavier at one time. To be more personal about it, my younger daughter is named Xenia, and even in grade school her classmates had no trouble learning the proper pronunciation, Ksay-niya.

WILLY LEY

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Jackson Heights, New York

As an old S-F fan I am flattered by Willy Ley's attention to my excursion into the grain-of-truth-among-chaff business. But the history of science is not science but History, an appalling mishmash of unrepeatable accidents, quite unfit company for the respectable readers of this journal. Selenium has nothing to do with the Greeks, the moon, or the metal, just as (to cover his second point) irony has nothing to do with Fe. And in a world in which so much information has to be carried in such little skulls, there is a real problem of Economy in language.

But I am sorry about *x*. It has always

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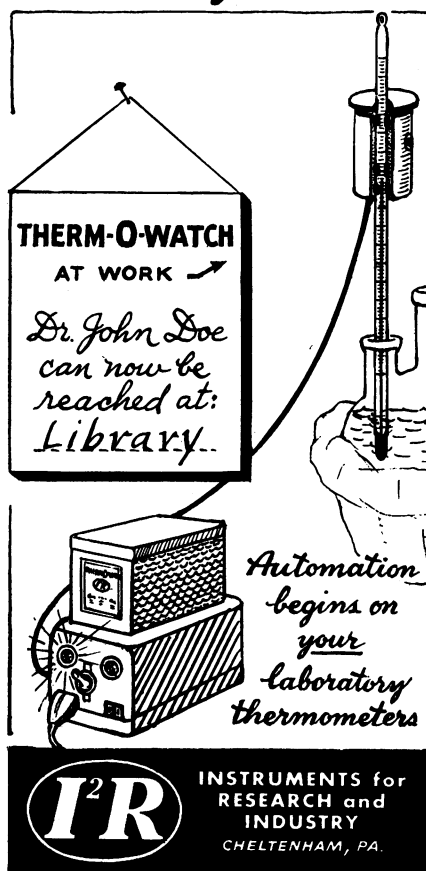


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been my favorite letter (I was practically raised on Oxo), and Xenia is a lovely name and a nice little town. But I did only need 19 consonants, and arbitrariness pursues us even across the savannahs of the blue; it is History, not Science, which gives us 360 degrees, from the Babylonians to the Bahai. But x has glories more renowned than numerology, for what is more splendid than the Unknown? And what more glorious symbol is there for that infinite pursuit of the unknown which is the Being of Science and the End of History?

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Genetical Theory

Lederberg's remarks in his Nobel address [*Science* 131, 269 (1960)] demonstrate his commitment to the deoxyribonucleic acid hypothesis and show how a dominant theory enables its proponent to ignore other points of view. A section on the origin of life contains no references to Oparin [*The Origin of Life on the Earth* (Academic Press, New York, 1957)], Miller [*Ann. N.Y. Acad. Sci.* 69, 260 (1957)], or Fox [AAAS General Symposium (26 Dec. 1959)]. Pirie [*Intern. Council Sci. Unions Rev.* 1, 40 (1959)] is described as (of all things!) an "agnostic." The numerous experiments reported from this laboratory [*Proc. Intern. Genet. Symposium No. 42* (1957)], which demonstrate that various carbohydrates will transform a recessive gene into a dominant gene, are disregarded.

Is it proper to ignore the views and the supporting evidence of others in full knowledge? It has become the current practice of proponents of a genetical theory not to comment on work incompatible with the theory. Sinoto's [*Proc. Intern. Congr. Genet. 10th Congr.* (1958), vol. 2, p. 262] recent confirmation of Lysenko's original experiments (which have not been considered seriously by most Western geneticists because of their conflict with Mendelism) is a case in point. It is now apparent that a considerable revision of views (both by East and West) must occur before a comprehensive genetical theory can be achieved. If current reviewers of genetics stated the limitations under which they were writing, it would explain their failure to refute excluded items. Otherwise they arbitrarily narrow the extent of the scientific enterprise.

CARL C. LINDEGREN
Biological Research Laboratory,
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Carbondale

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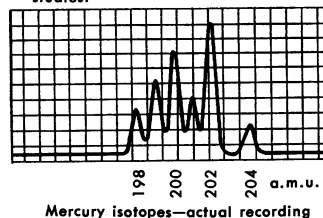
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