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THIS WEEK

RESEARCH NOTES

ARTICLES

DEPARTMENTS

COVER: Computer terminals, scattered throughout an office building, often provide access to valuable information. Protecting such systems from criminal acts or errors is becoming a major concern. See p. 12. (Photo courtesy of Steelcase, Inc.)

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LETTERS

Resisting seductive results

An article discussing Venera 13 and 14 measurements of atmospheric composition on Venus (SN: 5/29/82, p. 358) says in part that "the recent Venera results put new limits on the proportions of krypton and xenon in the Venus atmosphere, indicating the amount of both gases to lie between 30 and 100 parts per billion."

In fact these determinations are very instructive not only in their own right but because they tend to resolve a serious disagreement between mass spectrometers flown on the Pioneer Venus large probe and Veneras 11 and 12 in 1978. The Venera experimenters then reported abundance of ^{84}Kr as 600 + 200 ppb (total Kr 1000 ppb) while the Pioneer Venus group put ^{84}Kr at 25 ppb (total Kr 46 ppb with an upper limit at 69 ppb). Pioneer Venus found that there was surely less than 120 ppb of xenon and very probably less than 40 ppb[1,2]. Veneras 11 and 12 did not attempt to measure the xenon mixing ratio. Thus the recent Venera 13 and 14 results hardly set *new* limits on xenon and krypton, but they *do* nicely confirm the PV results.

This episode illustrates how important it is that no single set of observations of the properties of a planet (or any other object) be taken as definitive until it has been subject to the traditional rigors of scientific scrutiny, challenge and confirmation. The Venera 11 and 12 results for krypton were seductive because they implied that the ratio of the abundance of nonradiogenic argon to krypton was about the same on Venus as it is on the earth. If they had not been challenged by the PV measurements, theorists would have been seriously misled about an important property of planetary atmospheres relating to the mechanism by which the planets were formed. The story of Venus's krypton is an excellent argument for the existence of several vigorous, independent space science programs. If investigations conducted from space vehicles are to be worthy of being characterized as scientific, they will have to follow the vigorous ground rules developed for all scientific disciplines. This means that they must be confirmed independently — whether they be obtained by the United States (for example on Mars) or by the USSR (for example on Venus).

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[1]Donahue *et al.,* Geophys. Res. Lett., *8,* 513-516, 1981.
[2]Istomin *et al.,* paper delivered at COSPAR, Budapest, 1980, and published in Space Research XX.

Lead results: No justification

Thank you for the opportunity to comment on your summary of the EPA hearings regarding the proposal to increase gasoline lead (SN: 4/24/82, p. 278).

I testified that if there are intellectual and behavioral effects of low-level lead exposure they are minimal. This was based on a very careful review of existing studies (including my own) and the methodological weaknesses in each.

In the study you mention, Herbert Needleman controlled for parental intelligence and other confounding variables in some, but not all, of these analyses. (The graphs provided in support of a dose-response effect for behavior ratings could very well reflect father occupation, father education and parent I.Q. rather than lead.) Even when statistical controls of confounding are used they necessarily undercorrect; this is important when effects are small, as they are in these studies. The study also suffers from differential subject loss in the high and low lead groups. Positive results are reported with unstandardized experimental procedures but not with the important standardized measures of school performance. The number of subjects is very small relative to the number of variables. Most importantly, his study is a prime example of a common problem—the use of independent univariate statistical tests for a large number of variables without studywise adjustments of the alpha level. Simply put, there is no statistical justification for the conclusions drawn from that study.

Drs. Needleman and Groth have not recognized the extent to which these features of the research design affect the results. They persist in using these small, probably invalid, "statistically significant" results as the basis of sweeping conclusions about the effects of lead. This is not an easy topic on which to do research. It is crucial that we consider carefully the limitations and the directions of the biases in our studies and that the recognition of these helps shape the inferences we draw.

The children who are identified as being most at risk for lead effects are the children who suffer *real* effects from poverty, substandard housing, poor nutrition, limitations in the abilities, knowledge and maturity of parents, exposure to alcoholism, illicit polydrug use, and stress and violence in the home. When we study this population of children, the faint possibility of a low level lead effect seems trivial. (We should, of course, continue our vigilance for high level lead exposure resulting, usually, from paint in older homes.)

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Configuration vs. rotation

"Amino acids: Fixed in Space?" (SN: 5/8/82, p. 310) contained one major flaw. The "L" statement is a structural or configurational statement concerning the chiral carbon in the amino acids. [A "chiral carbon" is one to which four different groups are bonded.] It does *not* refer to the direction of rotation of plane polarized light. The "D" statement likewise covers the mirror image configuration of an "L" structure but it does *not* represent a right-handed rotation.

Some L amino acids have + [right-handed] rotation while others have − [left-handed] rotations. I have listed several examples. L-Arganine, $\alpha = +12.5$; L-Asparagine, $\alpha = -5.42$; L-Glutamine, $\alpha = +32.5$; L-Histidine, $\alpha = -39.7$.

I hope that this clarifies the D-L configuration statements as being separate from the specific rotation statements.

Nicholas L. Reuter
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Erroneous age difference

I enjoyed your article dealing with iridium anomalies and impacts (SN: 5/22/82, p. 340). However, I note that the difference in age between the North American microtektite layer and the Eocene/Oligocene boundary is 2 million years—not 200 million years.

Billy P. Glass
Newark, Del.