

general lack of communication and all too often a lack of organization. Most conservation organizations have no threat which they can use as a control or a temporary deterrent. There is no single government agency in which pros and cons may be weighed and appropriate decisions made. In essence, we usually find the developers pitted against the conservationists, and too frequently of late the developers are appealing to undefined "recreation" to gain support for their causes. All too often their ideas of recreation are little concerned with conservation or necessary control.

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Channeling of Funds

In his article "Federal money and university research" (21 Jan., p. 285) Don K. Price supports a greater channeling of research funds through universities as opposed to direct grants to investigators. It is true that inadequate central facilities are often a drag on research. However, as a working scientist, allow me to caution against channeling more money than necessary through the universities.

Logic makes clear that it is the scientists—not the universities—who have the greatest drive and incentive to create, and it is the scientists who have the greater knowledge of how to produce. It should therefore be the scientists—not the universities—who use the research funds most effectively. Experience amply confirms this expectation.

When funding must be centralized, every effort should be made to do it through groups of scientists. In cases where that is impractical, the scientists should always have a dominant voice in questions of efficient spending if not always in questions of fair allocation.

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Sophistication

Sophistication has entered the scientific vocabulary as a "must" word. More and more the "deployers" of sophisticated techniques and sophisticated hardware and software are becoming segregated as a scientific élite. The neo-

phyte, not to mention candidates for scientific obsolescence, needs a guide to quick sophistication.

Reference to dictionaries shows that the authorities are in complete agreement. Sophistication is the employment of sophistry; the process of investing with specious fallacies or of misleading by means of these; falsification; quibbling; disingenuous alteration or perversion of something; cunning; trickery; baseness; artificiality; dishonesty; adulteration with a foreign or inferior substance; the state of being spoiled or corrupted; fraudulent and guileful.

Additional guidelines may be drawn from literature: "But the age of chivalry is gone, that of sophisters, economists and calculators has succeeded" (Burke); "I love not a sophisticated truth, with an allay of lye in't" (Dryden); "He is fluent and sophisticate—a sure token of inferior wisdom"; "I laugh at the lore and the pride of man, at the sophist school and the learned clan" (Emerson); "A sophisticated rhetorician, inebriated with the exuberance of his own verbosity and gifted with an egotistical imagination that can at all times command an interminable and inconsistent series of arguments to malign an opponent and to glorify himself" (Disraeli, on Gladstone).

In moments of despair, if any, the fledgling sophisticate may take heart: "Destroy his fib, or sophistry in vain The creature's at his dirty work again" (Pope).

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Priority and Recognition

Page's editorial on "priority" (7 Jan., p. 33) . . . brings to mind some odd and interesting inequities in the citing of references, questionable practices in which many of us engage unconsciously and perhaps at times consciously:

1) The limitation of references to writings by investigators associated with the "schools of thought" that appeal to the author and the ignoring of relevant work of equal or greater merit by workers believed to be allied with other "schools of thought."

2) Selective reference to the work of colleagues who are friends of the author to the exclusion of contributions of others although they may be of equal merit.

3) Complete omission of references, probably in an effort to give the impression of considerable originality. This results only in giving an impression of naiveté or egocentricity; curiously, however, some experienced investigators engage in this practice.

4) Citation of references of secondary importance instead of the more basic publications of the investigator who is being recognized, or reference to a minor point in a publication instead of its major theme. Sometimes this is done with the intent to avoid granting priority, or to diminish the importance of a colleague's contribution. It constitutes a grudging recognition or a reluctant fulfilling of a scientific obligation.

5) Omission of references that contradict or fail to support the views of the author although including them would be quite pertinent.

6) Inclusion of references to especially well-known investigators or to friends although these may be irrelevant or only tangentially related to the subject under discussion. An insecure author may use this device for psychological support.

7) Inaccuracies in citation of the views of colleagues as a result of copying the inaccuracies, perhaps knowingly but usually unwittingly, from another publication in which the errors were made. Such errors can be reiterated endlessly as a result.

8) Reference to the views or findings of others without citing the sources in an attempt to imply that the points are being offered for the first time as an outgrowth of the author's experience. When this is done the intent is often deliberate.

These practices require more careful attention by all authors. Our human traits place limits on us, but by striving and goodwill we can elevate further the standards of scientists and scientific writing.

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Pages suggests that the chief reason for disputes about priority of discovery is "uncertainties concerning publication. What constitutes a definitive publication? . . . What constitutes acceptance for publication?"

If "Who thought of it first?" is the question at issue, dates of receipt and acceptance by a journal may have some

historical interest; a dated entry in a researcher's daybook, or in a letter to a friend, or in a quarterly report to one's director might have even more interest.

In taxonomy, priority of actual *publication* is significant, since it is a criterion for the adoption of names and other nomenclatural actions. The criteria of publication, as defined by the International Code of Zoological Nomenclature, are that a work "be issued for the purpose of scientific, public, permanent record" and that "it be obtainable by purchase or free distribution." These are objective criteria, for, while the exact date of publication may be difficult to determine, it is a point of fact, actually or potentially ascertainable, and concerning which evidence can be gathered. These might well serve as criteria for priority in other fields as well. The emphasis is on public issuance of the information and the time it becomes available to the scientific community for permanent record, without favor, classified distribution, or other limitation.

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. . . I would like to comment on Page's mild criticism of Osler's stand on priority. First, a technicality; Osler did not, as Page suggests, opine "that the credit goes to the man who convinces the world, not to the one to whom the idea first occurred. . . ." Osler quoted these words in his address on "The first printed documents relating to modern surgical anesthesia" [*Proc. Roy. Soc. Med.* **11**, 65 (1917-18)], crediting them to their author, Francis Darwin. Osler then applied the thought to Morton, observing that "Morton convinced the world; the credit is his." It is probably unfair to extend Osler's remarks outside the particular context of the arguments over priority in surgical anesthesia. And within this context, it seems to me, Osler's position is unassailable. For, as he pointed out, surgical anesthesia did not exist prior to 16 October 1846, despite the fact that Horace Wells and Crawford Long had both successfully anesthetized a small number of patients.

Wells attempted a public trial of nitrous oxide anesthesia at the Massachusetts General Hospital, which was a tragic failure. Long, unexplain-

ably, was not sufficiently impressed with the merits of anesthesia either to use it extensively in his own practice or to attempt to extend its use either by demonstrating it to his colleagues or by publication. Surely there are two requirements involved in priority: first, that one make a discovery, and second, that one communicate it to the scientific community. Page's understanding of this is implicit in his editorial, for three of his four concluding suggestions relate to the form of publication.

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More about Car Safety

. . . A simple solution to the automobile-safety problem [see Letters, 21 Jan., p. 277] would be to give complete publicity to all accidents, fatal and nonfatal. Practically every accident of consequence is a matter of record, and the record includes the make and year of manufacture of the cars involved. A safety factor could readily be established by dividing the total number of accidents in each make of car by the number of fatalities resulting. To be fair to manufacturers (even though they may not be being fair to the public), weight should be given to the number of cars of a given make on the road and to their age. Certainly publication of the fact that fatalities were particularly high in certain cars would compel their manufacturers to take notice. The ratio of total number of accidents to total number of cars of a given make in service could also be published. This would give a good indication of which cars were accident-prone because of faulty design.

It is an odd fact that, while insurance companies have all these figures, they will not release them. I have tried to obtain such statistics from insurance men, many of them personal friends, but a wall of silence is erected the moment the subject is mentioned.

It is high time some organization came forward to do battle with the agencies that seem bent on keeping facts about automobile safety from the public. Why not the American Medical Association? . . .

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In commenting (Letters, 21 Jan., p. 279) on drivers' ineptitude as being the major cause of automobile crashes, Dickinson misses the principal point concerning the need for safe automobiles. It is because of this very ineptitude—or the occasional psychic lapse which occurs in most people at one time or another—that automobiles need to be made safer. Countless examples could be given of safety measures that have been put into use in all areas of human behavior in order to protect not only the inept from their own acts but also the innocent from the actions of the inept.

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. . . Any rear-engined car, from the small Renaults and Simcas to the Corvair, can be driven safely by people of average ability who take the trouble to become aware of the peculiarities of handling associated with rear-engined vehicles and make adequate compensation in their driving habits for these characteristics.

Domestic cars in general seem far behind such vehicles as the Citroen DS/ID series of cars in overall design with respect to safety. For example, the Citroens have extra-capacity, premium-grade tires; a balanced braking system including a device which apportions the load between the front and rear brakes to prevent lockup; a body designed to preserve the driver's vision if either the hood or trunk lid should open; a nonlethal steering column; and other safety features.

In all fairness it should be noted that the handling and cornering qualities of our domestic vehicles have greatly improved recently; cars like the Chrysler-built compacts, with small torsion-bar suspension, offer quality available only in exotic imports around 10 years ago.

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. . . A Senate subcommittee headed by Senator Ribicoff revealed that the American Motors Corporation cars and General Motors Cadillac were the only cars made in 1965 with true dual brakes. During the Denver meeting of the AAAS, I had the opportunity to question the director of research of General Motors about this. His reply was that the public would not be will-