"publicly" by antivivisectionists and others in our society who either have not known or have not cared about the scientific aspects of the problem. I regret that Ansevin is apparently unaware of the very grave problems that the scientific community is even today facing as a result of several congressional bills regarding animal experimentation. She certainly seems unaware of the concerted efforts of the various scientific societies in response to these bills. For some sense of this problem I refer her to the *A.I.B.S. Bulletin* of February 1961. In my opinion it would have been better not to "publicly" raise this complex problem in this manner at this time.

Regarding the experiment itself, if precedent may be taken as a defense, we find ourselves on solid ground. The bibliography on sleep deprivation experiments runs into the hundreds. Such experiments extend at least from 1891—from an early experiment by Manasseina on the exhaustion of young pups by sleep deprivation—to a recent Russian experiment in 1961, by Feldman, involving prolonged sleep deprivation in cats and dogs. The treadmill apparatus used in our experiment with rats was described in 1946 by Bunch and Licklider, in the *Journal of Comparative and Physiological Psychology*, and an "improved" version for mice was described by Kavanau in March 1962 in the *Journal of Applied Physiology*. Certainly these precedents should place our experiment at least within the general professional ethic.

As for the animals themselves, considerable care was taken in dealing with them. Evidence of this is the fact that only one animal died in the 28-day experiments, and that this death resulted from a pulmonary condition which could have occurred under circumstances independent of the experiment. The remaining animals were carefully watched, often at the cost of considerable discomfort and occasional "exhaustion" on the part of the experimeters. Finally, the animals were carefully tested for several months after their experience on the wheel, and no evidence of permanent damage was found by comparison with the control group. I do not, of course, have measures to indicate whether "pitiful states of exhaustion" occurred, or whether the animals were forced to counteract a "sensation of drowning."

However, Ansevin's letter poses two more general propositions that cannot be so specifically dealt with. It is suggested (i) that we refrain from performing "extreme" experiments, and (ii) that when animals *are* used in experiments that involve "acute suffering" "clear and important justification" be provided. Clearly, the words *extreme, acute suffering*, and *clear and important justification* are highly judgmental and value-laden terms. *Extreme* may be defined as any condition exceeding that occurring to an "average" animal at an "average" time; *acute suffering*, as any condition in which it might be inferred that the subject would not freely volunteer for the condition; *clear and important justification*, as the prospect of completely modifying a theory or saving x number of lives in y time. Clearly, on the basis of such criteria or variations thereof, to provide controls would be impossible and the range of our experiments would be pedestrian; the use of subjects would be governed by whimsy or short-term emotional outbursts. Perhaps even more important, the requirement of "justification" would obliterate basic research. I hope that we may rather continue to be guided in our choice of conditions and use of subjects by the desire to seek systematic relationships in the world about us and integration of these relationships with the theories and accumulated knowledge of our various disciplines. Let us hope that the "ethic" that we are to be governed and judged by in our choice of such conditions or subjects will be that of our peers in the scientific community rather than one derived in the absence of an awareness of the overall issues involved.

Finally, I cannot forbear noting that the "inconsequential" findings which Agnew and I reported have been of such interest to at least a portion of our scientific community as to exhaust our supply of reprints within 6 weeks.

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Wrong Subtitle

Since political scientists have not yet invented a new and more accurate language for their discipline, I think they have an obligation to be as precise and objective as they can in writing plain English. For this reason I trust you will let me note for the record that

the subtitle which *Science* added to my article entitled "The scientific establishment" [**136**, 1099 (1962)] was not written by me, and indeed was one which I had expressly objected to using.

That subtitle, as published, read thus: "The American system gives scientists in government a freedom and influence unmatched in other countries." The statement is vague enough to be defensible, but I do not really believe it is true, and my article made no such sweeping gesture to our national self-esteem. I would not argue with a British scientist that he is less free than his American colleague, and I am not sure that certain Russian scientists have less influence than their counterparts here.

The subtitle which I proposed (and which I am sure was left out by mistake) was this: "Scientists in policy roles help create a constitutional system unlike parliamentary or Marxist models." I was trying to say that scientists in the United States had had an important role in creating a different system, not that the system was better. I happen to like it, but it is a good idea, I think, to analyze objectively a system that we are talking about even if we then wish to judge it according to our prejudices. I have been pleased to note that some of the things in my article which American readers thought were meant to give high praise to the system have been taken by certain British friends as a shameful confession of the political delinquency of the United States.

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We plead guilty to the oversight of not substituting the subtitle suggested by Dr. Price after his article had gone to the printer. Our proofreaders should have caught the mistake in the galley proofs.—ED.

Sand Dune Alignment

G. F. Jordan in his excellent paper on "Large submarine sand waves" [*Science* **136**, 839 (1962)] has quoted me [*ibid.* **132**, 1369 (1960)] as implying that linear sand dunes from the Arabian Peninsula are oriented parallel to the prevailing winds.

Without detracting from the excellence of his paper, I wish to state, for the record, that as far as the Arabian Desert is concerned, under no circumstances can linear dunes be aligned parallel to the prevailing winds. The situation is much more complex than this. Linear dunes are generally formed parallel to a line that may be the resultant of forces exerted by winds from a minimum of three directions, only one of which, usually a mild or moderate wind, is apt to be parallel to the axes of the dunes.

In the Rub' al Khali, linear dunes originate as fields of transverse crescentic dunes, which, in the course of a long time, evolve into elongated, linear groups, which may in turn become partly or entirely linear in shape. Relief is built up by seasonal storm winds from opposite to adjacent quarters and the lineation is accentuated by moderate winds parallel to the axis, usually from one direction.

The curved linear sand waves shown in Jordan's Fig. 7, bear some similarity to desert dune patterns but appear to be oriented 180° out of phase. For desert dunes, the arcs are always *upwind* and the concavities are *downwind.*

The analogy of sand dunes to waves is unfortunate. Dunes, contrary to commonly held notions, are immobile, thus can hardly be considered as wave forms. Sand migrates; dunes do not. The distinction is made obscurely by R. A. Bagnold in his book, *The Physics of Blown Sand and Desert Dunes.*

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My statement about extensive linear dunes and their alignment parallel to "prevailing winds" should have been attributed to Kadar, whom I cited, and to others, including Bagnold, cited by him, who described Libyan dunes, not to Holm, who described the Arabian dunes. The latter dunes appear to be parallel to "reversing winds," as indicated in Holm's paper by his description of the wind regime in Rub' al Khali and by lineations in his Fig. 1.

As for the curvature of crestlines on Cultivator Shoal in my Fig. 7 in relation to cross-sectional asymmetry (Profile 2), it does appear that conditions are contrary to those in the encircled area to the west (Profile 7) and in dune patterns described by Holm and others. Further field investigations of the environment here are certainly needed.

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