ket? Scientists do have a direct interest in the social environment. It seems to me that the editor has taken a very responsible position with respect to the scientific community. For his breadth of view I wish to commend him.

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If I understand Morse's argument correctly he has worked himself into the extraordinary position of asserting that anyone who urges Congress to find out the facts of a situation before trying to regulate it is attacking rationality! As I see it, the issue posed by the "Truth in Lending Act" (S. 1740) is not one of rationality versus irrationality but one of true rationality versus pseudorationality. In pure mathematics or logic the test of truth is consistency alone. In mathematics or logic applied to the real world we must add the test of relevance. It is not really rational to transfer principles bodily from mathematics into politics without assembling and analyzing whatever evidence we can obtain as to their relevance.

The merchants and consumers whom Senator Douglas would regulate are not intellectual constructs or systems of equations limited to a few arbitrarily chosen variables. They are flesh-and-blood people whose welfare is at stake. They differ widely in ability, shrewdness, diligence, integrity, intelligence, education, persistence, and purpose. The specific physical, social, and commercial environments in which they live are spread over a wide spectrum. I therefore have no apology to offer for saying that before Congress enacts this law it should do a better job than I believe it has done thus far in deciding what will happen to whom after it has acted. Nor do I see any real conflict between expressing concern as to "how many win and lose" and efforts to help consumers be more rational in their buying. Morse merely assumes that enactment of S. 1740 would be a "step that encourages rational decisions." Legislation should be based upon knowledge, not assumptions.

Morse is correct when he says that in my illustrations I have arbitrarily held P and t constant and so made r equal I in the formula $I = Prt$. He neglects to say, however, that I have done this only to point out that even in the world of pure logic the assumption that one must always compare rates in order to be rational is not valid. I do not know and I believe no one else knows, as a matter of fact based upon empirical evidence, the proportion of purchases in which consumers would get from absolute numbers all the information they need for rational comparison of the bargains offered them by different sellers.

Morse apparently has no doubts as to his own knowledge of the facts. He says that introducing many variations of P and t would have made my case "realistic," more in accord with what is "experienced in real life." In practice, I suspect, most individual consumers when they make specific installment purchases have to choose not from all the mathematical possibilities but from quite narrow assortments of alternatives as to goods, sellers, and terms available in the markets open to them. Whether the system now used "is too complex for the consumer to make rational choices" is a question of fact that can be answered only by going into the field and seeing what consumers do. So is the question of whether working with formulas for "true" interest will increase or decrease the complexity of the problems faced by real consumers in the real world.

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Ancient Agriculture in the Negev

The editor of *Science* has kindly consented to the publication of a letter not to exceed 600 words and focusing sharply on material introduced by Evenari *et al.* but not mentioned in my earlier communication (*1*). I have learned too late that the policy of *Science* allows only one letter of criticism and the author's rebuttal; hence I must reserve my detailed reply to Evenari *et al.* for another forum.

The present issue centers on the following statement by Evenari *et al.*: "Finally, in objecting to our theory Mayerson refers to unpublished information . . . of a series of experiments carried out by the Hebrew University (by us) and the Soil Conservation Service. These experiments, Mayerson maintains, showed that 'undisturbed hammada gave much more runoff than the adjoining piece of ground that was bared of its upper cover. The exact percentages have not been published, but they range from 100%–200%'. . . . This statement is false."

Let me state simply and directly that

I have never cited this information in objection to, or in refutation of, the efficient-runoff (soil-crusting) theory of Evenari *et al.*, nor have I made any attempt whatsoever to evaluate the information. My actual words regarding the experiments were these (*2*): "The results, of course, are tentative and we await confirmation and elaboration by the sponsors of the experiments. Yet, had these experiments proved that cleared *hammada* did in fact increase runoff, they still would not have proved conclusively that the *teleilât* were the result of a conscious effort on the part of the farmer to increase runoff. They would only have proved that it was *scientifically demonstrable* to increase rates of runoff by clearing stones from the *hammada*."

In other words, my interest focused solely and exclusively upon whether these experiments, successful or not, could in any way prove that the ancient farmer stripped slopes of their stone cover in order to enhance crusting and thereby increase rates of runoff. I believe they did not and will not.

The article in which the foregoing statement appeared was entitled, "The ancient agricultural remains of the Central Negeb: Methodology and dating criteria," and the report of the soil-crusting experiments by the Hebrew University and the Soil Conservation Service was cited to support a methodological position which I have steadfastly maintained and which I believe is grounded in scientific logic: namely, that observations on the function of ancient agricultural installations which cannot be supported by ancient or modern analogies must be regarded as speculative. It is from this position alone that I object to the soil-crusting theory of Evenari *et al.*, for not one of their citations (*3*) provides the slightest support for the theory.

This was the point I wished to make with respect to the experiments undertaken by the Hebrew University—no other. And as for my own view on the stone heaps (*teleilât el 'anab*), I am not so wedded to my theory—which I only maintain is more plausible than others—that I would not divorce myself from it if sound and substantive evidence were adduced in support of the efficient-runoff (soil-crusting) theory or any other theory. But above all, let evidence and testimony, not invective and detraction, prevail. *Absit invidia.*

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References and Notes

1. *Science* **134**, 1751 (1961).
2. *Bull. Am. Schools Oriental Res. No. 160* (1960), p. 27.
3. *Science* **133**, 979, ref. 36 (1961). The citation of B. Hellström has no bearing whatsoever on the soil-crusting theory, and the installations he describes do not in the slightest resemble the stone heaps of the Central Negev.

Evenari informs us that he sees no reason for making additional comments on this subject.—Ed.

Center for Retired Scientists

It is pleasant to read that C. W. Weiant proposes a "center for retired scientists" [*Science* **135**, 961 (1962)]. There are a number of suitable locations in Mexico and Central America. As an alternative to Weiant's Jalapa. I would like to suggest Tapachula, which lacks the "*Nortes*" so numerous in Jalapa.

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