Letters

The Florida Torreya: Efforts To Preserve It

The letter by R. K. Godfrey and H. Kurz [*Science* **136**, 900 (1962)] on the Florida torreya has created widespread interest, and the following account will explain more clearly the efforts that the Florida Board of Parks and Historic Memorials has taken since it became evident that some unknown disease was attacking the *Torreya taxifolia* at Torreya State Park.

We in the Florida Park Service noted some 8 years ago that the *Torreya taxifolia* at Torreya State Park, was decreasing in numbers. The Florida Forest Service, at our request, sent in dendrologists to advise us what steps should be taken to attempt to correct this situation. Plots surrounding trees have been cut down in order to give the Torreya more air and sunlight, various fertilizers have been applied, and the results have been remarkably negligible. Meanwhile, over these years, samples of roots, stems, and also foliage have been collected by or sent to various academic departments of botany and bacteriology, both within Florida and out of the state. To date, this blight has not been indentified.

Meanwhile, in order to maintain the species, seeds were taken from the female tree at Killearn Gardens and planted. We now have at Killearn Gardens a beautiful stand of 114 Torreya trees in an open field. Other seeds from this same tree have been raised at Torreya State Park by Superintendent Homer Barber, who recently advised me that the 125 seedlings which he transplanted into open areas at Torreya State Park are now about 6 inches high and to date do not show any evidence of blight. Again, at Killearn Gardens, we have an additional 28 trees which are growing in another locality within Killearn Gardens State Park. This year we will again harvest seeds at Killearn and Torreya state parks.

If the trees as raised from seed and now transplanted continue to thrive as indicated, it is conceivable that within a very few years a sufficient number of female trees will bear a sufficient quantity of seeds to make it possible to distribute the seeds to other areas for those who wish to raise the Torreya for scientific purposes.

As I write this, one of the botanists of the Florida Park Service has just taken another collection of specimens to Erdman West at the University of Florida in Gainesville. West is actively pursuing research on this Torreya blight.

W. A. COLDWELL
Florida Board of Parks and Historic Memorials, Tallahassee

On Planarian Behavior

Best's reply (*1*) to Davenport's letter (*2*) concerning the Best and Rubinstein experiment on delayed feeding in planarians (*3*) requires additional comment. Davenport correctly objects that no control for manipulation or handling was employed for the unfamiliarized animals; the familiarized animals were given five transfers prior to the feeding period (home bowl to pipet, pipet to test receptacle for familiarization period, test receptacle to home bowl, home bowl to pipet, and pipet back to test receptacle for feeding period), while the unfamiliarized animals were given two transfers (home bowl to pipet and pipet to test receptacle for feeding). Best states that Davenport's objection is logically correct, but of no practical consequence. He notes that the way in which familiarized animals were transferred minimized manipulation and handling, and that even if there should be a residual effect of such handling, the transfer from home bowl to test receptacle for the feeding period, common to both groups, would completely mask the effect.

Two points need to be made. First, the logical and practical importance of Davenport's objection can be seen more easily if we speak of stimulus changes, defined as any modification of the external stimulation impinging on the animals, rather than manipulation or handling, terms with uncertain meaning in this context. Best and Rubinstein did not control for number of stimulus changes before feeding; as described above, the familiarized animals were exposed to five, the unfamiliarized animals to two. Second, Best's statement that the residual effect, if any, from such stimulus changes (transfers) would be masked by the common change at the start of the feeding period is presumably a statement of opinion. It is difficult to accept opinion as a substitute for controlled experimentation. In the absence of evidence bearing on the existence of a residual effect and on the susceptibility of this effect to masking (and Best cites no evidence on these), no person can do better than express opinion, and this is precisely why the experiment should have controlled for the possibility of a residual effect not masked by the common transfer.

Elementary (*4*) and definitive (*5*, pp. 35–36, 89–90, 136–137) treatments of behavioral experimentation stress that a meaningful comparison between groups can be made only when a single factor is varied while others are controlled. In this experiment, the groups were different on two factors: number of stimulus changes prior to feeding and exposure or nonexposure to the test receptacle prior to feeding. The conclusion that differences in feeding latency were produced by the second of these factors may or may not be correct. The differences might have been produced by the first factor or the two factors working together (see *5*, chap. 5, for examples). It is not incumbent upon other scientists to suggest ways in which uncontrolled factors might produce the observed behavior, as Davenport attempts to do; rather, if the experimenters wish their conclusion to become a part of science, it is their responsibility to rule out by suitable control procedures the possibility that such factors could have an effect, for example, through processes we do not as yet know about.

A suitable control procedure in this case is to treat unfamiliarized animals in the same way as familiarized animals, with the exception that the receptacle in which they would be placed initially should be different from the test receptacle. The number of stimulus changes then would be the same for both groups, and the obtained differences in feeding latency, if found under

these conditions, could be attributed directly to the similarity of the receptacle in which animals were placed initially to the test receptacle used for the feeding period. This would constitute a demonstration of the delayed feeding effect.

ROBERT H. DUFORT
Department of Psychology,
Wake Forest College,
Winston-Salem, North Carolina

References

1. J. B. Best, *Science* **137**, 454 (1962).
2. D. Davenport, *ibid.* **137**, 452 (1962).
3. J. B. Best and I. Rubinstein, *ibid.* **135**, 916 (1962).
4. G. H. Zimny, *Method in Experimental Psychology* (Ronald, New York, 1961), chap. 3.
5. B. J. Underwood, *Psychological Research* (Appleton-Century-Crofts, New York, 1957).

Social Science Research

In a recent issue of *Science* [**136**, 509 (1962)] there appeared a joint review by Kenneth Boulding of Clarence Ayres's *Toward a Reasonable Society* and Marshall Dimock's *The New American Political Economy*. Boulding dismisses Dimock's book as a study in the pathology of rhetoric. He is somewhat kinder to Ayres, whom he classifies as the outstanding representative of the school of institutional economics. Then he goes on, "For all the enormous difference in quality in these two works . . . [each] represents, in a sense, a personal philosophy of society. . . . But [this type of intellectual activity] does not have that peculiar property of securely based information and carefully tested prediction which is the identifying mark of the scientific process." Then follows this patronizing comment: "Political Non-Science is not necessarily nonsense, though it seems to have a bias in that direction. It should not, however, be mistaken for that *political science based on quantitative data and testable theory, which is now in the making*" (italics mine).

May I remind Boulding of Aristotle's dictum that each field carries a method appropriate to the material it is examining. Paul Lazarsfeld is much more modest in his claims. He refers to the hostile outsiders who ask, "What has social research all added up to in the last fifty years? Is there any sociological finding that has not been anticipated by philosophers or novelists? The answer has to be qualified. True, it is unlikely that any surprising 'discoveries' will be made for quite some time to come."

I am certain that the work of Dimock and Ayres will survive Boulding's review. What is disturbing is the implication that the methods of the physical sciences are the sole key to unraveling the mysteries of the social disciplines. This is not an entirely new point of view. Herbert Spencer was able to gain a widespread audience for nonsense disguised as sociology by trading on the language of Darwin. Just as physics-thinking dominates our intellectual climate today so Darwinian thinking dominated the late-19th century cultural climate.

May I suggest that the social researchers who are engaged in quantitative research be somewhat less aggressive in proclaiming their virtues until they are able to come up with some insights not previously arrived at by other methods.

It is distressing that this quasi-scientific group, despite the poverty of the results thus far gleaned from their labors, demand control of all research in the social disciplines.

There is room for both traditions in our intellectual investigations. It is unfortunate that the physics-oriented group seem to have the inside track where funds from federal sources and from the private philanthropic foundations are concerned.

However, in the area of social research, despite all the financing and all the grants, we have yet to see men of the stature of Veblen or Commons or Mitchell emerge.

The whole field of social studies is in a serious state of flux. If social studies are to receive federal support, then the criterion of choice should be broader than the National Science Foundation's narrowly conceived ideas of what basic social science research is. The kind of intellectual authoritarian arrogance portrayed is doing well enough without additional federal funds to encourage it.

WILLIAM GOMBERG
Wharton School of Finance and Commerce, University of Pennsylvania, Philadelphia

Gomberg is reading things into my review which I did not say. I did not say, nor do I believe, that the methods of the physical sciences are the sole key to unraveling the mysteries of the social disciplines. I have always maintained that the methods of the humanities gave us true knowledge and important knowledge, and I have main-

(Continued on page 456)