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.

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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