References

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An animal trained in a choice situation learns not only which alternative is rewarded, but other things as well. To say that after a series of reversals it is unable to remember at the beginning of any session which alternative was rewarded in the preceding session is not to say that it is "effectively naive," because there is no reason to believe that adjustment to the unvarying features of the training situation is impaired progressively by reversal. In one of the papers cited by Wiener and Huppert, I have emphasized the need to distinguish between the general effects of practice (known since the turn of the century) which may contribute to improvement of performance in reversal experiments and the effects which may be specific to reversal training (*1*, pp. 407–409). Simply to compare performance in reversal 0 (the original problem) with performance in some later reversal is to confound these effects. The value of the 2-day design is that nonspecific factors may be controlled by comparing reversal and nonreversal performance at various stages of training (*2*).

Perhaps it will be helpful to look at the pigeon data in a somewhat different way. One of the curves plotted in Fig. 1 shows the probability of error on the first ten trials of reversal 0. (There were 80 trials on each problem, 40 per day, with positive and negative colors reversed every 2 days.) The animals began with a slightly greater-than-chance tendency to choose the unrewarded color, but a preference for the rewarded color was established quickly, and the new preference was retained until the following day (the first nonreversal day), as shown by the low probability of error on the early trials of that day (curve NR-1). There was good retention also on the 3rd day of the experiment (the first reversal day), as shown by the high probability of error on the early trials of that day (curve R-1). After 120 such problems, however, retention from day to day was rather poor (*3*). Both the reversal (R-120) and the nonreversal (NR-120) curves begin at about the chance level, and they fall at much the same rate in subsequent trials. Reversal performance improves progressively over a series of reversals, but nonreversal performance deteriorates. The convergence may be traced to forgetting, which is measured directly in such an experiment.

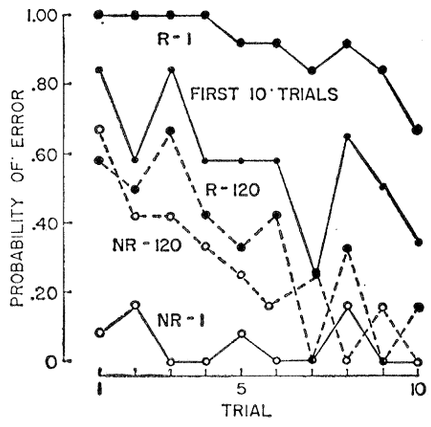

Fig. 1. Probability of error per trial plotted for the first ten trials of selected days (R, reversal; NR, nonreversal).

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References and Notes

1. M. E. Bitterman, *Amer. Psychologist* **20**, 396 (1965).
2. R. C. Gonzalez, E. R. Behrend, M. E. Bitterman, *Science* **158**, 519 (1967).
3. The training of the animals was carried beyond the point described in the original report by Miss Alice Schade, who provided the later data.

4. Supported by contract Nonr 2829(01) with ONR.

4 March 1968

Stone Tools and Woodworking

Comparatively recent experiments in the manufacture of wooden implements with flint tools have shown results rather similar to those in (*1*).

Breasted (*2*) reported that with a set of stone axes a Danish woodsman was able in 10 working hours to fell 26 pine trees 20 cm in diameter and hew them into logs. The entire work of hewing the planks and timbers and building a house was then done by the same man in 81 days.

The archeologists Troel Smith (*3*) and Jorgensen, together with two professional lumberjacks, using several hafted axes fitted with authentic Stone Age blades, found that the usual technique of chopping trees (as with steel axes) shattered the edges of the delicate flint blades and broke some of them in two. They soon discovered that, by chipping away at the tree with short quick strokes using mainly wrist and elbow, even with unresharpened blades from antiquity they could fell oak trees larger than 30 cm in diameter in 30 minutes.

Pont (*4*) states that with a good, grooved, ground ax, hafted and ready for use, he felled a 7.5-cm-diameter tree in 10 minutes. It took only 4.5 hours to make the ax, while a good arrowhead with serrated edge required only 1.5 minutes.

The speed with which flints have been made for flintlock pistols is quite extraordinary. Clarke reports (*5*) that, in England, as recently as 1868, men were able to prepare 300 flints an hour or 5000 to 7000 a day! Some men tallied 200,000 in a single week. He says that the accumulation of debris within 200 years was "almost beyond belief."

In view of the further discovery (*1*) relating to fragments and broken tools due to faulty handling, it seems not unlikely, as Lowie (*6*) proposed, that some supposed eoliths may well be nothing more than rejects or waste rather than evidence of poor skill in manufacture. Indeed, Breuil (*7*) found that weather conditions could significantly affect the end product—one individual might produce very different types of tools on different days; on one very cold day his co-worker, trying to make an Acheulian hand ax, ended up with a large Clactonian flake! Interpretation of the true significance of a flint tool or weapon can be difficult at times.

Years ago Dawson (*8*) noted that certain hollow-ground gouging chisels found in Europe were presumed to have been used for hollowing canoes. But he observed that the same basic tools were very commonly found among North American Indians who used birch-bark canoes clearly not requiring such tools for manufacture. The use to which the Indians put the tools was quite different: to tap maple trees! Dawson suggested that the use may have been the same in Europe.

Some very special kinds of arrowheads first appeared to be examples of poor craftsmanship; it is now realized that they were deliberately spiraled. Tylor (*9*) proposed that the spiraling was a kind of rifling to give the arrow truer flight and perhaps even to eliminate the need for feathers. But the spiraling does something more: conventional Indian arrowheads, fitted to proper shafts and shot from 34-kg bows, pass right through the usual backstop; the spiral head does not, its energy being absorbed by the corkscrewing of the

head into the backstop—which seems to me to have been the true purpose of the shape. While the spiraling does not rotate the arrow quickly enough in flight to eliminate the need for feathers, it does cause embedment when the arrow hits. An embedded arrow is less easily removed than one that has passed through.

There is no question that flint can make remarkably effective tools. Because flint tools were much sharper, Mexican barbers continued to shave with them in preference to the softer metals when the Spaniards first arrived; for the same reason many primitive peoples prefer them for circumcision. Leakey (*10*) himself, using a single Aurignacian backed blade about 5 cm long and estimated to be 25,000 years old, was able to skin and dress a Thompson's gazelle (about the size of a goat) in just under 20 minutes; the blade had certainly kept its cutting edge. Stone Age man may have been better equipped than we suppose; his tools and weapons were cheap, sharp, and very enduring.

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6. R. Lowie, *Science* **100**, 321 (1944).
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8. J. W. Dawson, *Fossil Men and their Modern Representatives* (Hodder and Stoughton, London, 1883), pp. 132, 133.
9. E. B. Tylor, *Anthropology* (Hill, New York, 1904), p. 155.
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13 February 1968

Tree Ring Indices and Statistics

A report by Haugen, "Tree ring indices: A circumpolar comparison" (*1*), contains several examples of statistical naiveté in his analyses of time series. From the data he presents, I would judge that he has established the existence of a circumpolar synchrony of tree rings with about the same precision that Cole established the "Biological clock in the unicorn" (*2*). The parallels between these two papers are quite striking. First, they find moving averages the solution to perplexing variation. Haugen perpetuates uncritically Adamenko's practice of using a 30-year running mean although, as Cole recognized, "Slutzky [*3*] showed in 1927 that such smoothing would actually create cycles in random data." Second, the tree ring indices of two of the series were "slipped" by a 25-year period—although this lag for the Labrador series was unsupported by any logical extrapolation of Adamenko's tentative reason. By similar logic Cole found a 1-hour lag in the unicorn's physiology attributable to the delayed rising of the moon.

Finally, Haugen grossly overstates the significance of the correlations he computed by applying criteria of significance that are applicable only to paired data in which each pair is independent of the remaining pairs. Data comprised of 30-year running averages certainly cannot be judged against this standard unless the pairs are limited to points in time separated by at least 30 years. Thus, Haugen overstates the degrees of freedom by a factor of about 30. Judged on this basis, the 21 correlations listed in Haugen's Table 2 would just about match Cole's admonition about granting the certificate of significance to the one in 20.

I find Cole's conclusion can be applied to Haugen's paper so precisely that even the figure reference stands unchanged: "A rhythm as definite as that in Fig. 1 could easily be shown to be highly correlated with environmental fluctuations, but the nature of the material employed in this experiment seems to preclude any such causal relationships."

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3. E. Slutzky, *Econometrica* **5**, 105 (1937). (This was Cole's reference *4*.)

2 January 1968

The analogies Stage has drawn between the papers by Cole and myself are entertaining but not valid. First, Cole did not "find moving averages the solution to perplexing variation," as Stage has stated. Cole lagged five repetitions of his "hourly" data by one, then added and averaged the results bringing his desired pattern "clearly into focus." A three-point moving average was subsequently applied to this already clear pattern. Second, in Cole's unicorn example, time was treated as a continuum, defined by an arbitrary unit of time, the hour, which Cole clearly distinguished from studies involving year-to-year variations where time can legitimately be treated as a discontinuous variable. Also, Stage refers to the Slutzky effect mentioned by Cole. Quasi-oscillations induced by this effect could indeed influence the apparent smoothness of the running mean values, but their influence on the amplitude and period of the major oscillations interpreted in the circumpolar comparison would be negligible. Further, induced quasi-oscillations would not tend to produce a synchrony among the indices. The major oscillations apparent in the running means I presented were also apparent in the original data.

Table 1. Correlations of interregional tree ring data; *n* refres to pairs of data.

Regions	Inclusive dates	*n*	*r*	*P*
Alaska–Urals	1780–1959	6	.86	.05
Urals–Scandinavia	1790–1939	5	.91	.05
Alaska–Scandinavia	1700–1939	8	.72	.05

Regarding Stage's second point, I compared the Labrador series directly with the Scandinavian series, which it appeared to resemble most closely. I gave no physical interpretation of this and would consider speculative any extrapolation of Adamenko's tentative reasoning.

Stage's criticism of the significance of the correlations presented, however, is justified; but the correlation coefficients remain valid as a measure of covariation between the series. Following Stage's suggestion, using *n* pairs of data taken at 30-year intervals from the final date of each series, I find the correlations (*r*) and their significance levels (*P*) shown in Table 1. The Labrador series is too short to attain significance on this basis.

I thank Stage for pointing out an important error in the significance levels originally stated. The above treatment indicates, however, that a correlation between the indices at an acceptable level of significance exists, and the main conclusion of the paper that "there is a significant degree of similarity in the major oscillations of tree ring indices representative of the circumpolar area" remains valid.

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