last halves of the acquisition series, but these failed to show any significant shift of latency. There was a nonsignificant overall tendency for latency to decrease from first to last half, but these scores were found to be extremely variable.

Figure 3 shows examples of actual responses selected from various stages of CR development. The responses range from a relatively ineffectual response in terms of puff avoidance (Fig. 3, top left) to complete avoidance of the puff and absence of UCR (Fig. 3, bottom right).

These responses also illustrate an aspect of the latency data which became apparent in examining the records, namely that the latency function fails to reflect the adaptive change for the reason that responses may differ in amplitude or form in such a way as to offset the effect of any increase or decrease in latency. Indeed, a given response may be appropriately placed, in relation to the UCS, either by increasing or decreasing its latency, depending upon its amplitude and rise time. Thus a complex interaction between time and amplitude seems required for efficient adaptive responses. The exact mechanisms underlying increasing efficiency presumably depend on feedback concerning latency, form, and amplitude from previous responses.

Several further characteristics of the ratio score are of interest. Appreciable variation in the pattern of CR development was reflected in these scores. As an alternative estimate of the change across trials, slopes representing the linear component of regression upon trial number were computed for each subject individually. Of these, 38 were positive, indicating that increased efficiency was the modal pattern. These slopes ranged from .0048 to .0824, of which 15 were significant at the .01-percent level or better. It should be noted that these slopes were used as an approximate estimate of individual performance, but that many individuals showed nonlinear patterns. Among these was an appreciable number who appeared to achieve, and then to lose, the effective response, although as shown by the analysis reported above the overall trend was toward successively better response levels.

The failure of CR *frequency* to reflect the effects of CS-UCS interval, whereas the ratio was highly successful in this respect, carries the implication that the two measures are not closely related, and this was in fact the case. The correlation between average ratio and number of CR's was .205 (not significant); thus the measures are virtually independent.

The data further show that the behavior of the ratio measure is not simply accounted for in terms of amplitude or magnitude measures of the CR; nor is it due to decreases in UCR amplitude.

Our measure of conditioned response efficiency is capable of reflecting adaptive changes in the eyelid CR which are obscured in the conventional measures traditionally in use. It is useful to regard the conditioned response as performing a part of the work of the reflex on which it is based; the proposed measure may be designated as the "work ratio" of the conditioned response.

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References and Notes

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10. The calculation of the split-half reliability followed the procedure Hilgard and Campbell (*4*) in order to afford a comparison with the very few published estimates of reliability. Average scores for the first and last quarter were compared with the middle half; no correction for attenuation was employed. The value reported is that for continuous reinforcement, the condition obtaining in the earlier report.
11. Only reinforced trials are included in these curves, since, for the partial reinforcement condition, the ratio score does not apply to trials on which the UCS was omitted. The first trial block is based on the scores of seven subjects, there being no responses recorded in this block for the 400-msec interval. From trial block 2 onward, the number of subjects responding increased from 21 to 46, divided fairly evenly between the two groups.
12. The financial support of the Medical Research Council and the encouragement of H. J. Eysenck are gratefully acknowledged.

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Convection Plumes and Insects

In "Convection plumes from *Ulmus americana* L." [*Science* **148**, 392 (1965)] Peterson and Damman make a good case for the opinion that the observed plumes were convection columns (attended by condensation) above treetops.

My wife and I observed the same phenomenon on 20 June 1964 while driving on the Capitol Beltway up the Maryland slope of the Potomac from the Woodrow Wilson Bridge. The time was just after sunset, the terrain below a swampy flood-plain, the sky was clear, and the visible columns, which extended for 2 or 3 meters above each treetop, wavered slightly from side to side in the still air. The occurrence was so striking—the first I had observed in 40 years as a weather observer and meteorologist—that we mentioned it to friends who lived nearby. They assured us that the columns were swarms of insects or gnats which collected above the treetops during summer evenings and became visible just after sunset against the bright northwest sky.

On further examination I concluded that this was the obvious explanation. The columns were too dark and wavered too much to be moisture columns. Insects were not present at ground level because (like fireflies) they had all congregated in the warmer air at treetop level, where, perhaps, the convective columns bore them up.

Damman and Peterson's statement that "there were no local concentrations of smoke or insects in the air that could have contributed to the phenomenon" is therefore subject to question, especially in view of the season and topography, even though their meteorological data and the explanation of the convective columns may be valid. The fact that the columns contain condensed water vapor from transpiration should also be verified before the phenomenon (as explained) is considered to be an established fact. Even though rare, such conditions must be frequent enough to permit further verification under similar conditions next summer.

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