were observed, along with the phenomenon of lower atmospheric clouds being projected on the glow stratum (*4*). The glow was not visible from 10 to 15 November from Puebla, Mexico, even though the weather was satisfactory. On returning to Mazatlan on the evening of 16 November, we again saw the glow stratum from the same vicinity along the flight path, but with maximum intensity shifted northward. The layer was even more brilliant and showed distinctly the ripples and modulations in intensity we had suspected on 9 November.

We began a systematic patrol for the enhanced glow stratum from our home east of Tucson after our return from Mexico, but saw nothing of the nature of what we had just seen from Mexico, even though the weather was ideal. Just at sunset on the evening of 21 November we saw a bluish-white band low in the western sky, at a time when the sky would have been too bright to distinguish the stratum produced by the Agung event, and we started a sequence of camera pictures. Intense coloration appeared on this band 15 to 45 minutes after sunset, typical of solar illumination of the volcanic ash layer reported after Krakatoa (*1*) and with the patchy striated bands and ripple structure reported by Volz (*3*). The strong coloration was limited to the ashen band, the sky above it being quite bluish, indicating that the aerosol layer in general had not yet been perturbed.

During the day of 22 November the principal portion of the ash layer passed over Tucson, drifting eastward at about 40 km/hour. The layer was visible all day as a smoky-white haze layer with striated bands and ripple structure extending from northeast to southwest. Included in the layer was a circumsolar silvery-blue disk with a reddish edge and a radius of about 27 degrees. This silvery disk apparently was a faint indication of "Bishop's ring," discovered by S. E. Bishop from Hawaii after the Krakatoa event (*1*). The twilight coloration was actually inhibited by self-absorption of the stratum, parts of which cast greatly elongated shadows on the ash layer. The time of glow set on this night was about 45 minutes after local sunset, but on subsequent nights increased to 47 minutes. The altitude for the layer and aerosol stratum was therefore 19 $\pm$ 2 km. The cover photograph was made from a color slide of the heavily striated glow stratum after sunset on 22 November 1974.

The major portion of the layer was distinctly east of Tucson on the morning of 23 November, with only the slightest visible trace of the silvery layer during midday. This day, like the preceding ones, was sparkling clear otherwise. The evening twilight glow, however, was spectacular, indicating that the ash-aerosol stratum had not ceased flowing over southern Arizona. From 24 to 28 November the stratum continued to be visible all day only on the southern horizon, which indicated that the flow was mainly over northern Mexico. The sunset skies showed brilliant sunset glows of a rather uniform pellucid nature, which grew in intensity for about a week after the last sighting of the daylight ash stratum; this may indicate that the enhanced aerosol layer is somewhat different from the ash cloud itself, consistent with the hypothesis (*5*) that the aerosol enhancement is a photochemical smog from the reaction of SO_2 with O_3 to form sulfate particles.

The reappearance of the ashen skies on 22 to 30 December 1974, distinctly weaker than on 21 to 28 November, would indicate the encircling of the earth by the initial cloud. This time of reappearance is compatible with a mean eastward drift velocity of about 40 km/hour, the approximate velocity we noted from angular drift when the cloud was overhead on 22 November.

The opacity of the stratum, its detailed structure, and its small extent of about 500 km indicated that its point of origin was not far from where we first observed it in Mexico. Examination of satellite infrared observations eliminated any source in the eastern Pacific, such as the Revilla Gigedo Islands. The only major nearby eruption was that of Volcan de Fuego in Guatemala on 10 to 23 October, and this is the probable source of the stratospheric event. The 4- to 6-week delay in appearance of the main cloud over the United States is explained satisfactorily by its probable trajectory in the upper air. The ash cloud from the eruption flowed westward from Guatemala, depositing ash as far as 200 km up the west coast of Mexico before moving over the Pacific (*6*). Some of the material went as far west as Hawaii before being acquired by the eastward stratospheric flow over the United States and Mexico (*7*). Lidar measurements have confirmed the existence and altitude of this dust-aerosol cloud (*8*).

The continued strength of the ash layer after its second passage over the United States indicates that the sunset effects will be observable until early spring, when the seasonal minimum begins, and will probably resume their beauty in the fall of 1975. With so much interest centered on man-caused perturbations to the atmospheric aerosols and the ozonosphere, perhaps we are fortunate to have this very large natural perturbation to study.

ADEN B. MEINEL
MARJORIE P. MEINEL
Optical Sciences Center, University of Arizona, Tucson 85721

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Self-Control of Occipital Theta Activity and Task Performance

Beatty *et al.* (*1*) claim to have demonstrated a "lawful relationship between operantly regulated cortical activity and behavior in man." There are, however, a number of aspects of this experiment which merit closer attention.

The use of the concept "operant" must be questioned. Lynch and Paskewitz (*2*), in a review of brain wave feedback experiments, feel that there is often insufficient evidence to justify the use of this term, particularly in the absence of a consideration of mediating mechanisms, and they question whether it will ever be possible to demonstrate true operant learning of brain wave activity in man. It seems preferable, therefore, to think in terms of self-control or feedback control rather than operant control.

It is not clear whether Beatty's experiment does in fact demonstrate self-control of theta activity. It is unfortunate that a noncontingent control group was not included in the experi-

ment, because Cleeland *et al.* (*3*) have shown, in an experiment on alpha control, greater changes in alpha abundance during noncontingent than contingent feedback trials. However, others have not confirmed this (*4*).

Inspection of Beatty's data reveals that although his conclusion that theta suppression has occurred can be supported, the same is not true of theta augmentation. The difference between the regulated and the control group is only significant at $P < .10$, a confidence level that would be rejected by most workers; in addition, the theta ratios for both regulated and control conditions are identical for the final 30-minute period of the 120-minute vigilance task.

Lynch and Paskewitz (*2*) question whether the discrimination learning paradigm is appropriate for the demonstration of self-control in feedback experiments of this nature. They point out, in a discussion of alpha control experiments, that such a procedure may indicate only that subjects are able to bring into play blocking influences; that is, apparent control may arise as a result of manipulation of an attentional variable. Theta activity is an indicator of states of reduced arousal and attention, and one would expect that a task such as asking an individual to manipulate a tone would maintain attention (and thus decrease theta activity).

The relationship between performance and theta activity demonstrated by Beatty *et al.* is also unclear. The absence of the end spurt in the electroencephalographically regulated vigilance sessions is interesting, as it occurs in both the control sessions. Also, the performance of the theta augment group for the last half-hour of the vigilance task was much poorer during the regulated session than the nonregulated session, despite the presence of identical theta ratios for the two conditions during that period.

It would seem that a causal relationship between theta activity and vigilance decrement is claimed by the authors. In the absence of controls for noncontingent feedback and distraction effect of the added task, one can go no further than claiming a correlational relationship.

PAUL WILLIAMS
Department of Psychological Medicine, University Hospital of Wales, Cardiff CF4 4XN

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We (*1*) have used the term "operant control" as it adequately characterizes the methodology of the experiment. We considered but rejected the term "feedback control," which suggests a strained analogy with system control theory. Williams' term "self-control" we find philosophically difficult and unenlightening.

Second, we did not design our experiment to demonstrate "true operant learning" of a brain rhythm and did not characterize our results as such. We agree that a wide variety of control procedures would be necessary before any such claim could be made. However, we doubt that a yoked, noncontingent control group would be useful even in that context (*2*).

Third, a bidirectional training design in which one group is trained to increase and the other to decrease a variable is indeed capable of demonstrating experimental control of the variable (*3*), but does not of necessity identify the mechanism by which that control is accomplished. However, successful discriminant training precludes interpretations of the data that attribute changes in the dependent variables to factors shared by both groups. Thus, while it might be reasonable for Williams to "expect that a task such as asking an individual to manipulate a tone would maintain attention (and thus decrease theta activity)," evidence for increased behavioral attention and decreased theta activity occurred only in the theta suppress group; in the theta augment group, which was also required to manipulate the same tone, exactly the reverse pattern of results was obtained. Nonspecific factors, such as the presence of an electroencephalographic (EEG) regulation task, cannot account for the observed differences in EEG activity or performance efficiency between experimental and control conditions in both the theta suppress and theta augment groups.

Fourth, the effect of the experimental procedures in the theta augmentation condition appears to be genuine. We agree that a single significance test that indicates a probability between .10 and .05 in itself constitutes only weak support for the observed hypothesis. However, in the case of the observed EEG changes in the theta augment group, the direction of the effect was as expected and its magnitude was comparable with that observed in the larger theta suppress group; for these reasons we trusted the conclusion that our experimental procedures produced the expected increase in theta activity in the theta augment group. Subsequent large-sample replications in our laboratory have confirmed that the reinforcement of high theta ratios significantly increases theta activity.

Fifth, with regard to the observation that mean performance of the theta augment group was differentiated between conditions in the final 30 minutes of the test whereas the theta ratio was not, we can only share Williams' puzzlement, point out that physiological indices of arousal do not account for all behavioral variance in monotonous monitoring tasks (*4*), and report that this dissociation of regulated theta activity and performance has not reappeared in our large-sample replication.

Finally, we believe that we have demonstrated a "lawful"—that is, orderly—"relationship between operantly regulated cortical phenomena and performance," as we originally stated. We agree that our experiment does not establish a causal relationship, nor was it designed to do so. However, neither the "absence of controls for noncontingent feedback nor the distraction effect of the added task"—an effect which was controlled in our experimental design —bear upon the issue of causality, as Williams suggests.

JACKSON BEATTY
Department of Psychology, University of California, Los Angeles 90024

JAMES F. O'HANLON
Human Factors Research, Inc., Goleta, California 93017

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