experiment was 32. These results are apparently attributable to observation of a lever-pressing response, rather than to any facilitation offered by the presence of two cats in the cage, since cats in group 2 (crosses) failed to press the lever even once during the entire experiment. The group 2 situation actually seems to inhibit lever-pressing, but as a function of observation learning. Conventionally shaped animals (group 3) acquired the lever-pressing response more slowly than the observers, with two animals failing to make a single lever press throughout the 6-day training period (open circles). A two-tailed *t*-test comparing group 1 and group 3 for total number of failures to perform during the 6-day period was significant ($P < 0.01$). Moreover, the level of stimulus discrimination was significantly higher in the observer group, as measured by the number of spontaneous lever presses performed by criterion animals in both groups. Specifically, on the sixth day, the operant group had a mean number of 89 intertrial lever presses, as compared to an observer mean of 28 intertrial presses.

It should be stressed that several of our observer cats, in both the approach and avoidance situations, performed correctly at the first opportunity and committed few or no errors while reaching criterion. Some of these animals behaved as though they "knew" what they were doing. Thus, learning mechanisms seem to exist which are capable of integrating diverse perceived stimuli into a meaningful whole, without direct reinforcement and without overt performance of the response.

Many physiological theories of learning assume that learning is a gradual phenomenon, requiring repeated reinforced performance of a response, and consisting of the establishment of neural pathways that connect brain regions receptive to the sensory stimulus to areas which mediate the behavioral response. Such learning theories have been derived from the facts of classical or instrumental conditioning. Yet, numerous experiments in latent learning (*1*) and observational or vicarious process learning (*2–5*) demonstrate that learning can take place without reinforcement and with little or no performance of the response which is required. Perhaps because of its theoretical significance, controversy has existed regarding the mediating mechanisms (*3, 4, 6, 7*) and reproducibility (*4, 5*) of observational learning. In part, this may be due to different definitions of observational learning which have led to a variety of experimental designs using different species.

Undoubtedly, conditioning techniques have been of great utility in the quantitative study of learning. Yet the impressive speed and efficiency of observational learning, contrasted with the potentially catastrophic slowness and need for repetition which often characterize conventional conditioning, suggest that the latter may well be a phenomenon of limited relevance, utilizing relatively unnatural mechanisms. Observational learning may be the primary method of acquiring language, ideas, and social habits in man, and such learning may also play an important role in the adaptation and survival of lower organisms (*7*). Thus, the behavioral and physiological study of more natural learning situations may be essential for adequate understanding of learning mechanisms.

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2,5-Dimethoxy-4-Methyl-Amphetamine: New Hallucinogenic Drug

The report (*1*) on the effects of a hallucinogen, 2,5-dimethoxy-4-methylamphetamine (STP, or DOM), due to a number of methodological inadequacies can be accepted only as an interesting collection of observations and not as a definitive assessment.

That suggestion or expectation in a subject affects the outcome of an experiment is common knowledge, particularly where hallucinations are induced (for example, in sensory deprivation experiments). Snyder, Faillace, and Hollister state "subjects were told that they would receive a drug . . . presumably a hallucinogen . . . ." These instructions are somewhat more than a suggestion that hallucinations were to be expected. Hallucinogens have achieved great notoriety, and their psychological effects are widely known. It is probable that suggestion influenced the results ascribed to DOM.

The authors quote one subject who compared the supposed effects of DOM to a "halfway decent pot experience." Even minimal prior drug experience might have been an additional uncontrolled factor. The report points out that sensitization to the effects of DOM may be a result of previous drug experiences. Without an adequate control the effects of suggestion and of past drug use in producing DOM hallucinations cannot be determined.

The method of obtaining reports of symptoms is somewhat suspect. Self-reports and questionnaires in general are of dubious validity and reliability, and answers may be easily faked. The physiological signs reported are apparently objective enough, but cannot be presumed to be due solely to the effect of the drug and not to fatigue, stress, or the attention given to the subjects by the experimenters. The authors did not indicate whether reported deviations differed significantly from baseline measurements of healthy, normal persons. Quantitative estimates of the relative potencies of DOM, lysergic acid diethylamide (LSD), and mescaline are therefore unwarranted.

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Our report (*1*) describing the psychological and physiological effects of the hallucinogen 2,5-dimethoxy-4-methylamphetamine (DOM or STP) has been criticized by Cabe on several counts. Cabe maintains that instructing subjects that they would receive a hallucinogen may have accounted for the hallucinogenic effects of DOM. Ethical considerations require that subjects be adequately informed before obtaining their consent in such experiments. Moreover, our report (*1*) clearly demonstrates that suggestibility could not have accounted for the hallucinogenic effects observed. All subjects received the same instructions but were given doses of DOM varying from 2.0 to 14.0 mg. If the hallucinogenic effects were "suggested" to the subjects there should have been no correlation between dose and effect. Since we found a clear-cut and well-graded relationship between dose and response, the effects obtained cannot be ascribed primarily to suggestibility. In a subsequent study (*2*), we have administered low, subhallucinogenic doses of DOM or placebo to normal control volunteers in a double blind experimental design. The mild euphoria produced by these low doses of DOM was readily distinguished from placebo.

We are fully aware of and have studied (*3*) the effect of setting and suggestibility on responses to hallucinogenic drugs. Nonetheless, in several studies of the psychological effects of hallucinogenic drugs (*4–6*) we observed that suggestibility plays a lesser role than armchair reasoning would suppose. In comparing LSD with epinephrine (*5*), we found little resemblance between the clinical effects despite similar instructions preceding each trial. In a similar blind experimental design we compared a presumed psychotomimetic (3,4-dimethoxyphenylethylamine) with mescaline and placebo and found that only mescaline produced hallucinogenic effects (*6*).

Since we did not specify exactly what psychological testing procedures were used, Cabe cannot be objecting to the particular tests we employed. In determining the effects of hallucinogenic drugs, we can ask the subject to describe his experience, direct him to perform a specified task, or observe him. All three methods were used in our study (*1*). Since a clearly graded dose-response effect was obtained in both the self-reports and the psychological tests, it is unlikely that the results we obtained by these procedures could be ascribed to "faking," as Cabe suggests.

Cabe criticizes our use of subjects with some limited experience of marijuana. In our report (*1*), we indicated that "applicants with a history of frequent use of marijuana or other mental stimulants were rejected." Unfortunately, obtaining subjects fully naive to drugs is difficult. Since experienced drug users usually excel in their ability to discriminate between the effects of different agents, one could argue that such individuals might be the best subjects for studies such as ours (*1*).

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Chromosome Damage by LSD

Loughman, Sargent, and Israelstam (*1*) have confirmed earlier reports that LSD (lysergic acid diethylamide) consumption is correlated with chromosome damage in vivo. However, the authors misconstrued their results and interpreted them as indicating no evidence of damage.

An analysis of their report must proceed on the assumption that the previous studies of LSD were validly done. Therefore, all statistical tests should ask, "What is the probability that the data accidentally confirm that LSD users have chromosome damage?" In fact, Loughman *et al.* do confirm the observation of chromosome damage among LSD users, and analysis of their data indicates very small probability that their confirmation is due to an accident of sampling. In their own analysis, Loughman *et al.* used a method that asked whether LSD users differed from their one control (a nonuser of LSD). (They did not realize how sensational it would be if they had found that LSD significantly protects against chromosome damage.) In other words, to decide whether damage occurs in users, the statistical tests should be one-tailed, not two-tailed.

Their paper mentions three distinct types of changes that are indicative of chromosome damage. (i) They found that only 12 of the 112 cells of the control (10.7 percent) did not have the normal chromosome number of 46, but that 45 of 245 cells of LSD users (18.4 percent) had other than 46 chromosomes. By Fisher's exact method, the probability that this is an accidental confirmation of damage by LSD is .044, which is statistically significant evidence that the drug causes chromosome damage. (ii) Loughman *et al.* "occasionally . . . found large cells with multiple micronuclei" in cultures from LSD users, but not in cultures from the control. From their data, it is uncertain whether the observed ratio of cells from these LSD users to those from their control was about 2:1 or 6:1. If 2:1, only eight of these highly abnormal cells would give a statistically significant confirmation of damage, but, if 6 : 1, they would have had to see 20 cells for it to be statistically significant. In either case, their observations confirm the previous observations of chromosome damage in LSD users. (iii) Chromosome aberrations were seen three times in 697 cells of LSD users, but not in any of 112 cells of the control. The exact probability for this distribution is .64, which is not small. However, this is the expected result if LSD is associated with chromosome damage.

Thus, Loughman *et al.* observed three types of abnormalities associated with chromosome damage, and each was more severe among LSD users than in the control. It is impossible to make an overall estimate of the probability that these confirmations of previous reports could be due to chance alone, but the combined probability is likely to be very low, which would make the confirmation highly significant.

Loughman *et al.* stated, "We conclude from our work that LSD . . . has not been shown to damage the chromosomes of human peripheral blood lymphocytes in vivo." On the contrary,