with the resulting drug profiles, unless they were replicated over a reasonable sample of animals. Certainly, from an interpretational point of view, it would be important to determine, for instance, whether the performance of a 'highly efficient' rat was affected by a particular drug in the same way as that of a relatively inefficient animal (even though both might be stable responders). The converse of this question (same efficiency level, but different stabilities or training periods) is also an important issue.

Shock further complicates any interpretation of the data. The number of shocks an animal receives in a given session will be solely determined by the 'efficiency' of the animal's responding. A drug which disrupts efficient responding may do so in one of two main ways—either by slowing down responding (thereby increasing the number of late responses) or by increasing the response rate (with more premature responding and fewer late responses). In the former case the animal will receive more shocks, and in the latter situation fewer shocks. As the session continues, we are therefore uncertain as to whether the animal's subsequent performance is being controlled by the drug or by the differential number of shocks administered. Indeed, it seems likely that these two factors will interact in a way which the Smythies' procedure cannot hope to describe. Furthermore, those drugs having analgesic effects will affect shock-avoidance performance in rather different ways, depending upon the degree of analgesia induced.

One way to control for some of the drug-shock interaction effects would be to eliminate the shock altogether from the saline/drug test sessions. In the case of reasonably efficient rats, their performance is almost wholly controlled by the threat of shock, rather than by the shock itself; and therefore switching off the shock will make very little difference to their performance (provided they have undergone substantial training with shock). It seems rather surprising that neither Bovet and Gatti nor Smythies *et al.* appear to have utilized this control.

To conclude: (i) it should be possible, with different time periods, etc., to make Smythies' technique more efficient in terms of training time per animal; (ii) to control for drug-shock interactions, non-shock test sessions should be run; (iii) it seems probable that either simple Sidman schedules or basic discriminated avoidance programmes would give similar results, which would then be easier to interpret (in the sense that learning strategies could be more precisely specified).

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Dear Sir,

Our paper was not originally intended to answer any of Lowe and Williams' theoretical interests. Initially, they have intimated that our method may be quite useful from a pharmacological point of view in differentiating between drug effects. This in fact is our only avowed interest, and we ask nothing more from the behavioural baseline which we have chosen. They then suggest that this particular technique would make interpretation of the drug action rather suspect. However, as is evident in the theoretical disposition of our paper, we are not concerned with explaining *why* a drug disrupts behaviour in a particular way or *why* certain sensory or behavioural mechanisms are altered in specific ways by certain compounds. The essential pragmatism of our work is geared to categorizing drugs into different classes based on behavioural effects which they may exhibit in common.

The psychopharmacologist using behaviour as a measure of drug effects is always in a dilemma. If he is interested in what effect the drug has on behaviour he must design a test with only one dependent variable according to the recommendations of Lowe and Williams. On the other hand if he is seeking to develop a test to categorize a new compound into one of a number of possible drug classes he will need to develop a test with several dependent variables so that a complex and informative drug 'profile' may be obtained. It is very difficult to try and combine these objectives, as Lowe and Williams ask us to do. Bovet and Gatti (1963) used this test for the purpose of drug screening, and we have developed it for this sole purpose. Thus our criterion was merely that the test should allow us to say whether a new drug synthesized was likely to be an hallucinogen, or to have an amphetamine-like action, or to be inactive. For this purpose no schedule less complex than a discriminative Sidman avoidance schedule will suffice —even though, as Lowe and Williams rightly point

out, it makes a behavioural interpretation of what the rats might have been attending to more difficult. But we were not concerned with this problem, and those who are should design procedures for their own purposes.

Lowe and Williams do not appear to have read our previous papers in great enough detail, for several of their criticisms are answered therein. The name given to the schedule is based on that used by Bovet and Gatti (1963) where they describe the derivation as follows: 'On the basis of Sidman's (1955) discriminated avoidance, a technique was developed.' The parameters of the schedule are given in detail in Smythies *et al.* (1967a, 1968), yet Lowe and Williams say that the light does not come on during the first 10 seconds, when in fact it does not come on during the first 20 seconds.

In addition they say that the Sidman procedure is further disrupted by the fact that a shock is given every 10 seconds if the rat fails to respond within 30 seconds. The time between these repeated shocks is referred to as the 'shock-shock' interval in Sidman avoidance, and is naturally a part of the discriminated Sidman avoidance which we describe. Our schedule is merely Sidman avoidance with the addition of a discriminative stimulus which is turned on 10 seconds before shock and remains on during the shock-shock interval.

Initially the animal 'may learn at least two "tricks",' as they suggest, i.e., pressing the lever in darkness or when the light is on. However, these responses are surely compatible since both reset the cycle to zero and therefore postpone shock. It is also compatible for the animal to learn that the light is a 'danger' signal and the dark period is 'safe' and this learning becomes apparent as the number of bar presses made in the dark period falls off. With extended training the animal produces a delicate temporal discrimination within the light period, for the later he leaves his response (within 30 seconds) the fewer responses he has to make. But, if he cuts it too fine, the risk of a slight miscalculation leading to shock becomes unacceptably high.

The criteria involved in training are also given elsewhere, but a simple examination of figures 4 or 5A in this *Journal* (Smythies *et al.*, 1969) shows the high level of efficient responses emitted by the animals with a corresponding low number of premature and late responses. The rats are trained until they emit a minimum of 85 per cent efficient responses, i.e. responses made in the presence of the light before shock. Normally they reach a stable day-to-day baseline level at a higher level of efficiency. They never reach complete efficiency, i.e. 100 per cent efficient responses, but does any avoidance schedule afford such a level? (The animals may however take no shocks in a particular session, which might be classified as 100 per cent efficiency by other researchers). By a stable day-to-day base-line we refer to a profile similar to that shown in figure 5A (Smythies *et al.*, 1969) with very little variation in response distribution between days, and with most of the efficient responses falling in the second half of the light-shock interval. Only after the animals have met these criteria do we begin drug administrations. On the average it takes between 60 and 80 sessions of training for an animal to achieve these levels of efficiency and stability. It is very rare for animals to differ appreciably in the amount of training time which is required to reach both the high level of efficient responding and the stable response distribution from day to day.

As Lowe and Williams say, we use each animal as his own control, and, as stated above, efficiency levels etc. are virtually equal between and within animals. Although only two or four animals are used in each of our drug studies, all of the animals used over the past five years have been tested with mescaline or some other standard hallucinogen and all have given similar profiles. We do, however, feel that two to four animals constitute a reasonable sample, and as Boren (1966) writes, 'A common feature of the operant approach involves the intensive study of individual subjects. The emphasis is upon close observation and firm experimental control of the individual subject.' All of these factors are assiduously incorporated in our experimental technique.

We have never tested any compounds on animals whose efficiency levels are lower than 85 per cent because, as Takaori *et al.* (1969) have shown, drugs have a differential effect on 'good and bad-performing animals' in an avoidance situation.

Lowe and Williams suggest eliminating shock for some of the sessions in order to control for some of the drug-shock interactions because with, '. . . reasonably efficient rats, their performance is almost wholly controlled by the threat of shock . . .' and that '. . . switching off the shock will make very little difference to their performance . . .'. What about extinction? Examination of the cumulative records in figure 1 (Smythies *et al.*, 1967) shows clearly that as the mescaline effect wears off the animal begins to take fewer shocks. If the shock had been turned off for this session, we feel sure that the animal's behaviour would be different in that he might not return to bar pressing when he 'discovers' that shock is no longer being delivered. Lowe and Williams must be aware of the resourceful behaviour of rats in devising methods of avoiding shock, and that they take full advantage of any opportunity. To our knowledge none

of the compounds which we have tested are known to induce analgesia, therefore, their point on that subject is irrelevant to our work.

We feel that a test such as ours can only be evaluated in terms of its results and their correlation with known human data. Shulgin (1970) has confirmed our human predictions (Smythies *et al.*, 1967b) for the activity of the amphetamine series, and Snyder *et al.* (1967) have produced data identical to ours (Beaton *et al.*, 1969) for humans with DOM. Webster (1971) has replicated our baseline profiles for rats and has recently completed a collaborative study on the effects of d-amphetamine on guinea pigs and rats (Beaton *et al.*, 1971). The results of this study replicated our previous findings across species.

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THE MENSTRUAL CYCLE AND SUICIDE

Dear Sir,

In a review of the literature on the menstrual cycle and suicide, we found nine papers that reported the number of female suicides who were pregnant. After excluding women reported to be under 11 or over 50 years of age, pre-menarchal, post-menopausal or status post-hysterectomy, we found that 93 of 685 (13·5 per cent) female suicides of childbearing age were pregnant at the time of their death.

Date	Author	N-Total	N-Pregnant
1900	Heller	57	7
1905	Pilcz	211	56
1905	Ollendorf	49	1
1927	Sachwitz	46	6
1931	Elo	165	18
1933	Babin	20	3
1936	Krugelstein	107	0
1962	Jannone	8	0
1962	Ribeiro	22	2
Totals		685	93

We believe these results may be of interest to psychiatrists reviewing requests for therapeutic abortion.

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