

# An experimental test of the hypothesis of formative causation

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## INTRODUCTION

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### 1. INTRODUCTION

The hypothesis of formative causation, which I first proposed in 1981 (SHELDRAKE, 1981) postulates that organisms are subject to an influence from previous similar organisms by a process called morphic resonance. Through morphic resonance, each member of a species draws upon, and in turn contributes to, a pooled or collective memory. Thus, for example, if animals learn a new skill in one place, similar animals raised under similar conditions should subsequently tend to learn the same thing more readily all over the world. Likewise, people should tend to learn more readily what others have already learnt, even in the absence of any known means of connection or communication. In the human realm, this hypothesis resembles C.G. Jung's postulate of the collective unconscious (SHELDRAKE, 1988). The hypothesis also applies in the chemical and physical realms, and predicts, for example, that crystals of new compounds should become easier to crystallize all over the world the more often they are made. There is already circumstantial evidence that this actually happens (SHELDRAKE, 1981; 1988).

The hypothesis of formative causation raises many theoretical and philosophical questions, which I have discussed in detail in my books (SHELDRAKE, 1981; 1988; 1990), but as a scientific hypothesis, its value has to be assessed by empirical tests. Most experimental tests of this hypothesis to date have involved human learning, and results so far have supported it (SHELDRAKE, 1986; 1988; MAHLBERG, 1987; ERTEL, 1992).

When I first proposed the hypothesis of formative causation in 1981, it aroused considerable controversy, and was attacked in an editorial in *Nature* entitled "A Book for Burning?" (ANON, 1981). As a result of this attack, Steven Rose of the Biology Department at the Open University in Britain, wrote to me offering facilities in his laboratory for testing the hypothesis in the learning of animals. We discussed this possibility soon afterwards, but for various practical reasons, nothing came of it. In 1988, as a result of an article I wrote on morphic resonance in *The Guardian*, a British newspaper, Rose wrote an attack on the concept and publicly repeated his offer to test this "seemingly absurd hypothesis" in his laboratory (ROSE, 1988). This time, it was possible to take up the offer. Funding was available, and a summer student, Ms Amanda Harrison, was appointed to carry out the experiment in the summer of 1990. She knew nothing of morphic resonance, and was deliberately not informed of the hypothesis being tested until the experiments were completed. Thus the experiment described below was performed blind. The design was agreed in advance by Rose and myself, and we both recorded our predictions before the experiment began. Rose predicted that the experiment would show no morphic resonance effects; I predicted that it would.

### 2. EXPERIMENTAL DESIGN

The research of Rose and his group is largely centred on biochemical changes in the brains of chicks following various kinds of learning. In designing an experimental test of morphic resonance together, Rose and I decided that it would be best to use a technique routinely employed in his laboratory, namely a form of learning involving conditioned aversion. Day-old chicks peck at small bright objects placed in their immediate environment, and they normally do so with little hesitation. But if they peck something distasteful, such as a bead coated with a bitter substance, then they show a strong tendency to avoid pecking it again, even many hours later. They are not averse to pecking beads of different colours, showing that this response involves a specific kind of learning, known in the literature as "one-trial passive avoidance learning" (CHERKIN, 1969). A similar response occurs even with tasteless beads if, after pecking, the chicks are made temporarily sick by an injection of lithium chloride. Thereafter, they show a strong tendency to avoid those beads. In Rose's laboratory this behavioural response is regularly used in experiments on learning and memory. The chicks are exposed to a stimulus, such as a shiny chromium-plated bead or small coloured light. Most peck at it quickly. Half an hour later they are injected with lithium chloride; they are then tested with the same stimulus four hours later, and also with a control stimulus to which they have not been made averse. Under these conditions, most chicks show a strong aversion to the test stimulus but not to the control stimulus. Control chicks injected with saline solution do not show any comparable specific aversion.

In the present experiment, this kind of aversive response was brought about in chicks exposed to a small yellow light, a stimulus not previously used in experiments of this kind. Control chicks pecked at a chromium-plated bead. Exactly the same experiment was performed with fresh batches of day-old chicks for 37 days, spread over ten weeks. According to the hypothesis of formative causation, subsequent batches of chicks should have a progressive tendency to avoid pecking the yellow light. They should be influenced by the experience of previous similar chicks, even though they had never met them, and without any possibility of communication by conventionally-recognized means. This effect should happen as a result of cumulative morphic resonance from the previous chicks that had developed an aversion to the yellow light. This increasing aversion in naive chicks should be measurable in terms of an increase in the time-lag between being exposed to the stimulus and pecking at it, or in other words an

increasing latency. Meanwhile control birds should show no such tendency to avoid pecking chrome beads. In addition, the increasing aversion to pecking the yellow light should also be apparent in control chicks after being injected with saline solution. Rose is very sceptical about the hypothesis of formative causation, and made the common-sense prediction that there would be no progressive aversion to pecking the yellow light. The predictions as recorded in advance were as follows:

**Rose:** "No secular trends apparent; latencies to peck the illuminated bead after ten weeks are no different from those on week 1, and the differences between latencies for illuminated and chrome beads, if they occur, are also unchanged."

**Sheldrake:** "There is a secular trend; the latency to first peck of the illuminated bead increases by the tenth week, while the latency to peck the chrome bead remains unchanged."

Rose and I both embarked on this research in the conviction that a scientific hypothesis, however radical, can and should be tested by experiment. We both hoped for a clear-cut result, and our original intention was to publish the results of this experiment jointly. However, we disagreed over the interpretation of the data, and Rose decided he did not want to publish the results as we had planned. I am therefore writing this paper without Rose, but inviting him to comment on it.

### 3. MATERIALS AND METHODS

The general procedures follow those used by Rose and his colleagues, as described by BARBER et al. (1989), using chicks of the domestic hen, *Gallus gallus domesticus*. Ross Chunky chicks of both sexes were hatched in the laboratory and held in a communal incubator until they were 24 to 36 hours old, when they were transferred to metal pens, lined with coloured cardboard, at the beginning of each experiment, and were kept under red lights. Two chicks were placed in each pen to minimize isolation stress. One of each pair was marked with a spot of dye so that the two could be distinguished. They were allowed to acclimatize for 1-1.5 hr before being presented with a 4 mm chromium-plated bead or a yellow light-emitting diode (LED) to initiate pecking activity. The chromium-plated bead and diode were on hand-held rods, and the LED was powered by a 9V battery.

Both chicks in a given pen were exposed to the same stimulus, either the control or LED, and the assignment of treatments to pens was randomized. In the initial training period, each chick was exposed to the control or LED for a period of 30s, and the number of seconds taken to the first peck was recorded with a stopwatch. This figure is referred to as the latency. The number of pecks was also recorded. Chicks that did not peck at all within the test period were recorded as having a latency of 30s, and these "untrained" chicks were dropped from the experiment, although they were left in their pens.

Half an hour after the training period, each trained bird was injected intraperitoneally with 0.1 ml of 0.9% NaCl in the case of control birds, or 1.0M LiCl in 0.9% NaCl for the birds trained with the LED. These injections were carried out by Rose or one of his colleagues while the student, Amanda Harrison, was not in the room. She was thus unaware of the treatments administered to the birds and was working "blind". Three hours after these injections, each bird was tested sequentially by Harrison with both stimuli, first the chrome bead and then the yellow LED. The latency to first peck, up to a maximum of 30s, was recorded. Exactly the same procedure was followed every day for 37 days. The experiment was carried out on Tuesdays to Fridays inclusive in the period from 20 July to 21 September 1990. On all days except Fridays between 10 to 18 test birds were used, together with similar numbers of controls; on Fridays about half as many birds were used.

There were considerable fluctuations from day to day in the average latency of chicks when pecking at the control and test beads. Variations in general levels of activity are well known to researchers on chicks; the day-old birds may be sluggish one day, while those hatching a few days later may be hyperactive. Such fluctuations are associated with changes in the weather, particularly with the barometric pressure around the twelfth day of incubation (BATESON, 1974). In order to smooth out such fluctuations, and also to compensate for variation in sample size from day to day, the data for successive three-day periods were pooled (days 7-9, 10-12, and so on. The last sample included data for the last four days). The data were analysed in several different ways:

1. By the standard method used by Rose and his colleagues (e.g. BARBER et al 1989). The number of chicks not pecking at the stimulus within the first ten seconds was divided by the total number tested, giving a proportional measure of aversion.
2. By calculating the mean latency.
3. By calculating latencies in terms of trimmed means. This procedure, sometimes referred to as Winsorizing, involves omitting the upper and lower 15% of the observations in any given sample and provides a more robust statistical measure than straightforward means (HOWELL, 1987).

Changes over time were analysed by computer using a linear regression program.

### 4. RESULTS

#### *The effects of practice by the experimenter*

The results clearly reveal how Harrison improved as an experimenter through practice. This is hardly surprising; she was learning on the job, and had never worked with chicks before. This experimenter effect is evident in the data shown in Fig. 1. Both test and control birds were trained more successfully as time went on; in other words, a higher proportion of the birds pecked at the yellow light or chrome bead within the initial 30s training period. This effect was greatest with the yellow light over the first six days, and corresponds to the fact that Harrison initially experienced greater difficulty learning to manipulate the rod with the yellow LED than the rod with the chromium plated bead. The former was larger and more bulky.

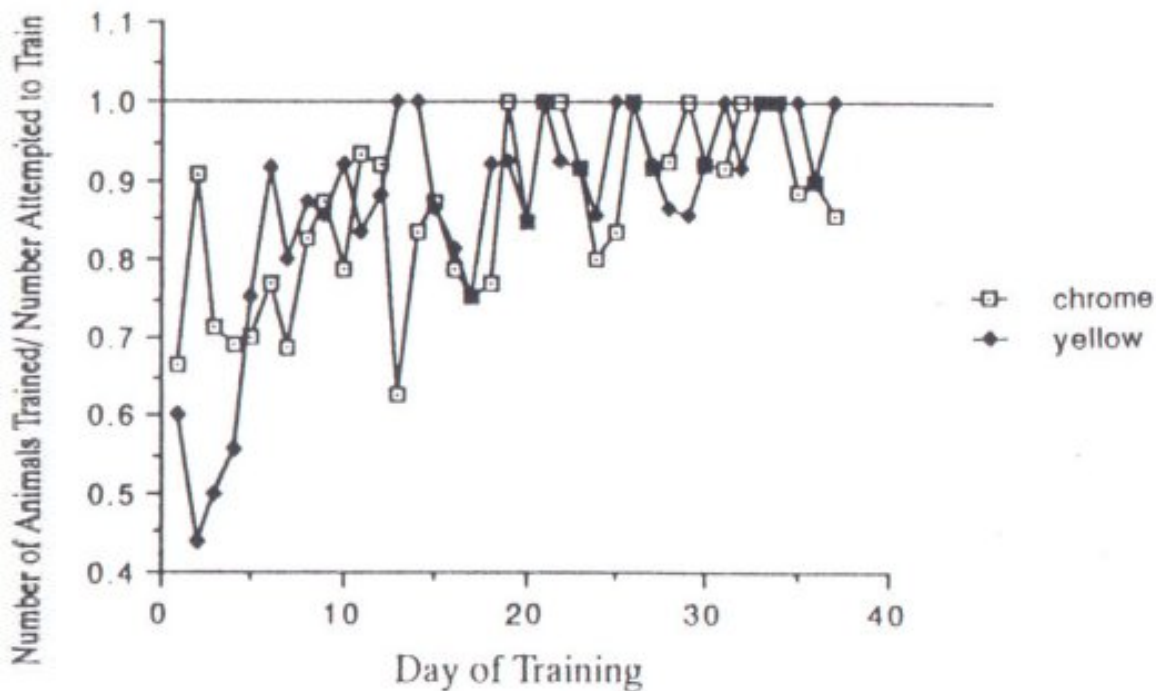


Fig. 1 - Proportion of chicks successfully trained on the 37 days of the experiment (number of chicks that pecked at the stimulus within 30s divided by the total number tested). A value of 1.0 meant that all chicks pecked within 30s.

For the analysis of overall trends, it seemed best to exclude the initial period when this experimenter practice effect was so pronounced, since it overshadowed the differential pattern of change in response to the control and test stimuli. I therefore omitted the data for the first six days from the analysis. (The overall pattern of results described below remained essentially the same if the initial three to twelve days were excluded; the exact number of days omitted was not critical within this range.)

#### *Changes in initial latencies*

Unexpectedly, there was a change in latency in the control birds as time went on (Fig. 2). They showed a clear tendency to peck sooner at the chromium-plated beads as the experiment progressed; the mean latency declined from about thirteen to eight seconds. (This effect was statistically significant at the 0.02 level using mean latencies; by the proportional method,  $p=0.09$ .)

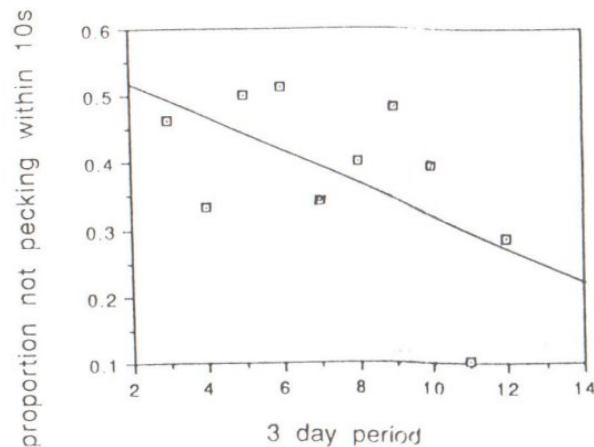


Fig. 2 - Proportion of naive chicks in successive three-day periods that did not peck within the first 10s at the control stimulus, the chromium-plated bead during the initial training procedure ( $p=0.091$ ).

Although neither Rose nor I had anticipated such a trend in the control data, we agree that it was probably due to an experimenter practice effect (cf. Fig. 1); as Harrison became more experienced at testing the chicks, they tended to peck more readily at the chromium-plated bead. By contrast, in the test birds there was no such pattern of declining latency. Indeed, using the proportional method of analysis of Rose and his colleagues, there was a tendency for latencies to increase (Fig. 3).

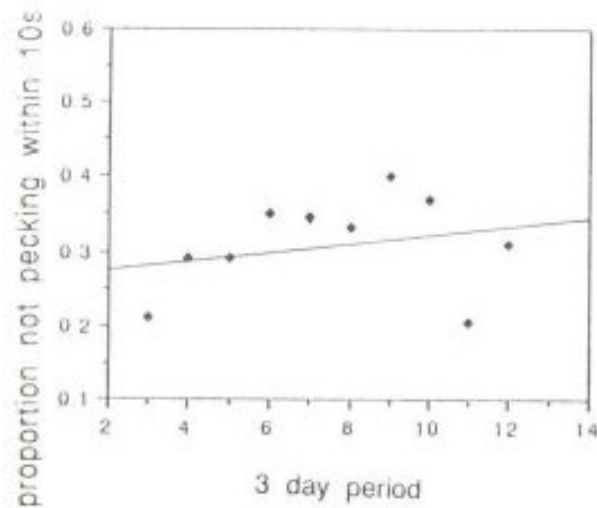


Fig. 3 - Proportion of naive chicks in successive three-day periods that did not peck within the first 10s at the test stimulus, the yellow LED, during the initial training procedure ( $p=0.610$ ).

The crucial question in the present experiment is whether there is a progressive difference between the behaviour of the control and test birds. This difference can be seen by subtracting the average latency for control birds from the average latency for test birds in each period (Fig 4). This procedure enables fluctuations due to changes in atmospheric pressure and other environmental factors to be controlled for. The results show a statistically significant increase in the latency with the test beads relative to the controls ( $p=0.009$  using Rose's proportional criterion;  $p=0.007$  using mean latencies;  $p=0.008$  using trimmed means).

Since this is the crucial effect on which the interpretation of this experiment depends, I have examined several other ways of analysing the results. First, the statistical significance of this effect was calculated using pooled data for one-, two- and four-day periods, in order to ensure that the results shown in Fig. 4 were not an artefact that arose from using three-days means. All these methods showed a similar increase in latency in test birds relative to controls. Using mean latencies, the statistical significances for one-, two-, three- and four-day means were  $p=0.008$ ,  $p=0.011$ ,  $p=0.007$  and  $p=0.004$  respectively.

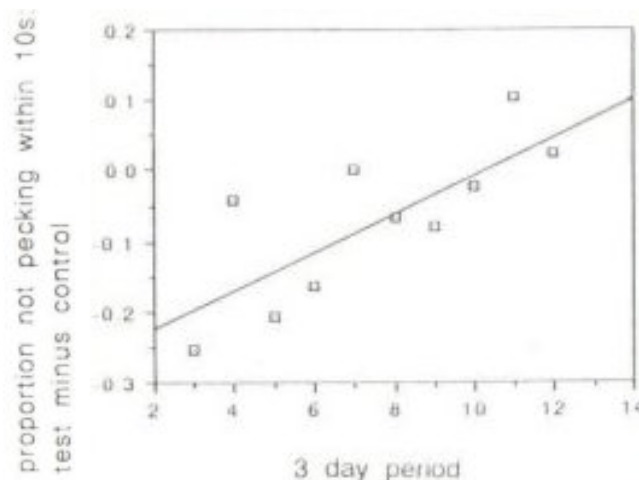


Fig. 4 - The secular trend in latency of naive test chicks in the initial training procedure relative to controls. For each three-day period the proportion with a latency of 10s or more with the control bead (shown in Fig. 2) was subtracted from the proportion with a latent of 10s or more with the test bead (shown in Fig. 3). The regression was significant at  $p=0.009$ .

Second, the effect of comparing the test and control data in different ways was examined. If instead of subtracting the control (C) from the test (T) mean latencies (T-G, as shown in Fig. 4), the test latencies were divided by the control (T/C), the overall pattern was the same, and the regression was again highly significant ( $p=0.005$ ). Expressing the test latencies as a proportion of the total (T/T+C) again gave very similar results ( $p=0.006$ ).

#### *Changes in latencies in control birds after injection with saline solution*

As in previous research by Rose and his colleagues, chicks that pecked the LED and then were made sick by an injection of LiCl developed a high degree of aversion to the test stimulus; most of the birds would not peck the yellow light at all when tested after the injection, although they readily pecked at the control stimulus, the chromium-plated bead. The control chicks after pecking the chromium-plated bead received an injection of saline solution, and they were likewise tested with both chromium-plated and yellow beads thereafter. The data for these post-injection tests are quite independent from the comparison of initial test and control latencies discussed in the previous section, and provide another way of looking for possible morphic resonance effects.

When the control birds were tested with the control stimulus after the saline injection, there was a tendency for latencies to decline over the course of the experiment (Fig. 5). By contrast, there was a tendency for latencies with the test stimulus to increase (Fig. 5).

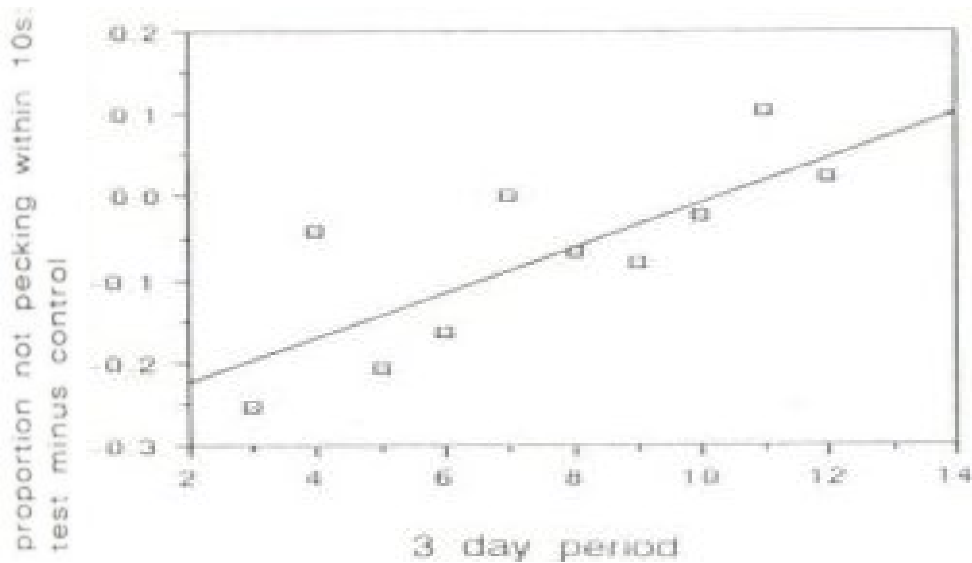


Fig. 5 - The mean latency of control chicks tested with the chromium-plated bead and the yellow light three hours after injection with saline solution. Data from successive three-day periods are shown ( $p = 0.169$  for the chromium-plated bead;  $p = 0.121$  for the yellow light).

As in the case of the initial training period (Fig. 4), there was an increase in the test latencies relative to the controls (Fig. 6). This was statistically significant at the  $p = 0.02$  level when mean latencies were considered, as shown in Fig. 6. However, by the method of trimmed means the significance was only  $p = 0.075$ ; and by the proportional method  $p = 0.096$ . When the data were analysed on a daily basis, rather than with three-day pooled data, the statistical significances were  $p = 0.057$  for mean latencies,  $p = 0.075$  for trimmed means and  $p = 0.015$  for the proportional method. Thus the general pattern of results for the post-injection control birds agrees with that for the initial training period, although the effect is less robust statistically.

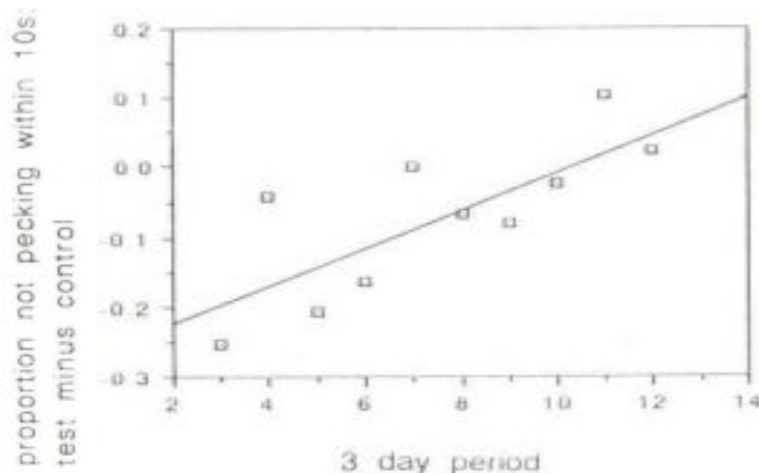


Fig. 6 - The secular trend in the latency of control chicks with the test stimulus relative to the control stimulus, when tested three hours after injection with saline solution. For each three-day period the mean latency with the control stimulus was subtracted from the mean latency with the test stimulus. The regression was significant at  $p = 0.022$ .

## 5. DISCUSSION

The increased latency in test chicks relative to controls (Fig. 4 and 6) is just the kind of effect that would be expected on the basis of morphic resonance. The difference in latency between test and control chicks increased as time went on, and the morphic resonance explanation for this would be that successive batches of chicks showed a progressive tendency to become more averse to the yellow LED because previous chicks (whom they had never met) had been made averse to it. In other words, this looks like the kind of collective memory effect predicted by the hypothesis of formative causation.

I predicted that there would be a secular trend, with latency of birds exposed to the test stimulus progressively increasing, while the latency with the control stimulus stayed the same. This prediction was correct in that there was a secular trend, but incorrect in that the latency with the control stimulus tended to decrease. I should have worded my prediction more carefully, emphasising that what was important was an increasing difference between the response to test and control stimulus.

Rose's prediction was that no secular trends would be apparent; that latencies to peck the illuminated bead after ten weeks would be no different from those at the beginning of the experiment, and that the differences between latencies for illuminated and chrome beads, if they occurred, would also be unchanged. This prediction was incorrect in that secular trends were apparent, and the differences between latencies for illuminated and control beads increased.

Neither Rose nor I anticipated that the latency of the control birds would show a progressive decline, and we agree that this is

probably due to an experimenter practice affect. From the point of view of the hypothesis of formative causation, the important finding is that in the test birds, the experimenter practice effect was counteracted by some other influence which tended to increase the latency to peck the yellow light, an influence that could well have been due to morphic resonance. From the point of view of the hypothesis of formative causation, the results of this experiment are encouraging. But of course further research will be needed to find out if this apparent morphic resonance effect is repeatable. For example, the same experiment could be done again, but with the control and test stimuli reversed: i.e. using the yellow LED as control and the chrome bead as test stimulus. If there is again a progressive increase in aversion with the test stimulus relative to the control, this should eliminate most conceivable *ad hoc* objections to the apparent morphic resonance effect shown in the present experiment.

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