

REPLY TO SILBERBERG AND ZIRIAX

WILLIAM VAUGHAN, JR.

HARVARD UNIVERSITY

Silberberg and Ziriix (1985) report that a modification of Vaughan's (1981) procedure produces results inconsistent with melioration (the position advocated by Vaughan) but consistent with a process they term molecular maximizing. Here it is argued that the theory of molecular maximization is not sufficiently unambiguous that researchers other than the developers can test its predictions, and that in any case none of the data presented by Silberberg and Ziriix are both clearly consistent with molecular maximization and inconsistent with melioration.

Key words: matching, maximizing, melioration, choice, key peck, pigeons

In a recent paper, Silberberg and Ziriix (1985) reported the results of an experiment which extends one that I had previously reported (Vaughan, 1981). According to Silberberg and Ziriix, although my results were inconsistent with regard to both global maximization and a process of matching, and consistent with melioration, those results did not discriminate between the latter and a process operating on a different level of analysis, which they term molecular maximization. Silberberg and Ziriix claim, however, that their experiment does just that, and that their results are consistent with molecular maximization but not with melioration. Although I am prepared to acknowledge that behavior is presumably orderly at their level of analysis, I try to demonstrate here that Silberberg and Ziriix (1985) have not made a persuasive case in support of their claim.

Rather than discussing melioration in detail here (see Herrnstein & Vaughan, 1980), my focus will be on molecular maximization. The two issues of importance are (a) What is molecular maximization? In particular, is the theory sufficiently clear that a researcher not involved with its development could make relatively unambiguous assertions regarding which predictions flow from it? (b) Do the data that Silberberg and Ziriix (1985) present tend both to support molecular maximization and to go against melioration? Only if both (a)

and (b) can be answered in the affirmative are there grounds for preferring molecular maximization to melioration as an account of the data. If it should turn out the data are either inconsistent or ambiguous with respect to molecular maximization, or ambiguous with respect to melioration, then no strong conclusion can be drawn about the relative merits of the two accounts. Data are relevant in debate concerning two theories only if those data are simultaneously consistent with one theory and inconsistent with the other.

MOLECULAR MAXIMIZATION:
THE THEORY

First, what is molecular maximization? Silberberg and Ziriix (1985) mention both molar maximization and momentary maximization (the theory that a changeover occurs from one alternative schedule to the other when the latter has a higher probability of reinforcement than the former), and go on to say:

... we favor a third maximizing account called *molecular maximizing* (see Silberberg & Ziriix, 1982). Molecular maximizing is based on the proposition that animals allocate the times they spend responding to the schedules, as opposed to the responses themselves, such that they maximize the reinforcement rates these interchangeover times (ICTs) produce (see Silberberg & Ziriix, 1982). (p. 84)

This quote suggests that a more definitive account of molecular maximization is to be found in the 1982 paper. In that earlier paper, Silberberg and Ziriix initially introduce molecular maximization largely on the basis of two figures. In the first (Figure 5-2), data from an

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earlier experiment are shown: Given *conc* VI 60 VI 180-s schedules, the conditional probability of a changeover from each schedule is shown as a function of time on that schedule. On the VI 60-s schedule, for example, there is a mode in the function around 2 s (mean data), and the function shows a gradual decrease for longer times. On the VI 180-s schedule, the mean data show a higher mode around 1 s, and a somewhat greater rate of decrease for longer times. (It should be mentioned that although the plots are of conditional probabilities, in the text it sometimes seems as if they are actually raw distributions of times on the schedules. They state, for example, "these data, plotted so as to present the conditional probability of a changeover as a function of time since the last changeover, show that there is a modal time for staying on a key before switching" (p. 140). Strictly speaking, these data show that there is a modal time for the *probability* of making a changeover, and an inference to the actual distributions of times is required before it can be said that there is a modal *time* for staying on a key before switching. However, the two modes were presumably close to each other.)

In their next figure (Figure 5-3), data from a simulation are plotted: the probability of reinforcement for different interchangeover times as a function of interchangeover time length. Interchangeover times (ICTs) and the probabilities of reinforcement for them were defined as follows (see p. 140 of their 1982 paper). Suppose that in a simulation there were 15 visits to a side lasting 1 s in duration, 10 visits to the side 2 s in duration, and 5 visits to the side 3 s in duration. All three of these classes provide opportunities for a 1-s visit, so there are 30 (that is, 15 plus 10 plus 5) 1-s interchangeover times. Only the 2- and 3-s visits contribute to the 2-s class, so there are 15 cases of 2-s visits, and finally only 5 cases of 3-s visits. In order to obtain a probability of reinforcement for each of these classes, the number of reinforcements occurring during, say, visits of 2 s are divided by the number of 2-s cases (15 in the example). It is not made clear from the text whether a reinforcer occurring 1 s into a 2-s visit is attributed to the 1-s class or the 2-s class. Figure 5-3 of the paper shows that the probability of reinforcement on the VI 60-s schedule begins somewhat elevated and then declines slightly and remains

approximately constant, whereas that for the VI 180-s schedule begins much higher and drops much more quickly, reaching approximately zero at 4 s.

Given these two figures, Silberberg and Zirriax (1982) then present the central idea of molecular maximization:

A comparison of the slopes of this figure's curves [Figure 5-3] with those from Figure 5-2 underscores an interesting correspondence: The probability of an ICT (Figure 5-2) on a key appears to follow the local probability of reinforcement on a key (Figure 5-3). If appearance conforms with reality, this is a new demonstration of the molecular control of choice: Birds terminate lean-key runs immediately because the local probability of reinforcement for longer runs is nil; conversely, the conditional probability of an ICT on the rich key drops slowly with time in the presence of that key because the probability of reinforcement for longer times also drops slowly. (pp. 140-143)

According to the earlier theory of momentary maximization (e.g., Shimp, 1966), a changeover occurs if the probability of reinforcement on the other side was greater than on the side being visited. Molecular maximization, according to the above description, differs in that it holds that the function showing the conditional probability of a changeover from a side will be similar to the function showing the probability of reinforcement for ICTs to that side.

There is, however, something paradoxical about this theory. Consider the above statement that "the conditional probability of an ICT on the rich key drops slowly with time in the presence of that key because the probability of reinforcement for longer times also drops slowly." The conditional probability of an ICT is in fact the probability of leaving the key concerned. One would expect that any maximizing strategy would be reflected in a higher, rather than a lower, probability of leaving as the probability of reinforcement decreased. The congruence between the two functions above might therefore even be termed *molecular minimization*: When the probability of reinforcement on a key is high, so is the probability of leaving, and vice versa. This suggests that, even though conditional distributions were shown and discussed as such, Silberberg and Zirriax really had in mind the *raw* distributions of times on a key for their

Figure 5-2, in which case they might have concluded that there are more ICTs of various lengths spent on a side when those ICTs are reinforced more, and vice versa. This interpretation is consistent with the quote from their 1985 paper near the beginning of this section. If this is the correct interpretation, however, then neither Figure 5-2 nor any of the other data presented in the 1982 paper directly support the theory, because they are all plots of conditional probabilities. The raw distributions from which those shown in Figure 5-2 were calculated would, in general, have peaks slightly to the left of those shown, and would drop more steeply to the right. These raw distributions are probably in qualitative agreement with their theory for the 180-s key, but would be too peaked for the 60-s key.

Consider this next quote from the 1985 paper regarding the theory: "Molecular maximizing occurs when animals allocate their time to the momentarily better schedule as based upon each schedule's reinforcement probabilities over the next few seconds. Paradoxically, this emphasis on local reinforcement rates can lead a molecular-maximizing bird to an overall decrease in reinforcement rates in Vaughan's procedure" (p. 85). The first sentence seems to imply a close similarity between molecular maximization and momentary maximization, with the former looking ahead (to speak loosely) the next few seconds, and the latter only to the next peck. Here, but not in any of the earlier quotes, it seems that a comparison of both schedules comes into play in the decision of whether to continue on the current schedule or switch to the other one. Perhaps such a process is mathematically equivalent to a similarity between the distribution of ICTs and the probability of reinforcement for those ICTs, but the issue does not seem to have been addressed.

Silberberg and Ziriix (1985) go on to discuss why a molecular-maximizing bird would behave as my birds did in the second condition of my 1981 experiment. There, given that relative time on the right was somewhat greater than .25 as measured over a 4-min period, during the next 4 min the right would pay off better than the left. If relative right time fell between .75 and .875, both schedules would pay off equally, and if it was greater than .875, the left would pay off better than the right. They say "... a molecular maximizer should

increase its right-key allocations in response to that key's momentarily higher reinforcement rate. When relative right-key allocations are in the range predicted by melioration (0.75–0.875), they should stabilize. Only in that range will the relative rate maximize local reinforcement probabilities, because shifts from this relative time-allocation range are met by shifts in local reinforcement rates opposed to the direction of change in choice allocation" (p. 85). This statement seems consistent with the last: When the right key is better than the left, more time will come to be spent on the right.

In the next paragraph, they say that the 4 min over which I measured relative right time allocation is "... well beyond extant demonstrations of pigeon memory for prior choices and their consequences ... a pigeon cannot maximize its choice allocations when it cannot recall these key events ..." (p. 85). They go on to discuss short-duration preference timers, and in their experiments settle on a 6-s measurement period. The implication is that pigeons in a choice situation are under the control of events only 6 s or so in the past. If this is so, it would seem that a bird in my experiment, confronted with the right key paying off better than the left for 4 min, would shift entirely to the right early in that period. Such a shift would in turn result in a measurement of somewhere around 95% of the time on the right at the end of 4 min, which would cause the left key to be better than the right for the next 4 min (causing the next measurement to be around 5% on the right). For each of my birds, however, once it began shifting toward the right the process took on the order of 5 *days* before reaching the range predicted by melioration (Vaughan, 1981, Figure 2). It seems, then, that Silberberg and Ziriix have made some implicit assumption that allows them to make the correct prediction regarding my birds.

There is one more place where the theory of molecular maximization is spelled out. Silberberg and Ziriix (1985) present the results of a simulation intended to show that a bird behaving according to the theory generates data similar to the experimental results (discussed further below) that they found. In the simulation, "stat" birds began by responding 10 times on a side once per second, doing the same thing on the other side, and so on. In one condition the schedules employed 6-s mea-

surement periods, and in the other 4-min periods. When a reinforcer on, say, side A was received, the time on that side preceding the reinforcer was stored, until a total of 20 times-to-reinforcement had been stored. Then the birds began spending amounts of time on a side that corresponded to the oldest time on the *other* side which produced reinforcement. Receipt of reinforcement on a side was entered into the memory for time to reinforcement. The logic of this procedure is as follows:

In this simulation, molecular-maximizing behavior was simulated by taking the CO-to-rf [changeover to reinforcement] delays generated by one schedule and reproducing them as ICTs on the other. To illustrate how this simple rule can produce molecular maximizing, consider its application when concurrent VI 1-min VI 2-min schedules are used on Vaughan's procedure. The distribution of CO-to-rf delays for the VI 1-min schedule should be, on average, half those for the VI 2-min schedule. By using each schedule's CO-to-rf delay distribution to define ICTs to the alternate schedule, the ICTs emitted should produce time-based matching. Because matching on these schedules is compatible with maximization (see Rachlin et al., 1976), this algorithm can be fairly described as maximizing; and because individual, brief CO-to-rf delays were used to define time allocation, this algorithm can also be labeled as molecular. (Silberberg & Ziriax, 1985, pp. 94-95)

There are actually a number of points that need to be made about this simulation. First, whereas molecular maximization was first described in terms of the similarity between the distribution of ICTs and their reinforcement on each of two sides (Silberberg & Ziriax, 1982), here it seems to be that the distribution of ICTs to one side should be similar to the distribution of reinforced ICTs to the *other* side. Apart from the fact that such a switch seems to give the desired result, its rationale is unclear. Second, the procedure is illustrated by means of the operation of concurrent VI 1-min VI 2-min schedules on my procedure. In that procedure, however, a schedule only operated while time was being spent there. With the schedules under discussion, that procedure would presumably give exclusive preference for the VI 1-min schedule. Silberberg and Ziriax say, however, that "the ICTs emitted should produce time-based matching." Although exclusive preference is consistent with matching, it is clear that that is not what they

had in mind. They next argue that their procedure can be called a form of maximization, because matching on concurrent VI 1-min VI 2-min is compatible with maximization, and refer to Rachlin, Green, Kagel, and Battalio (1976). But Rachlin et al. were advocating *molar* maximization, a process which Silberberg and Ziriax earlier acknowledged as distinct from molecular maximization (inasmuch as birds on my procedure are said to show molecular maximization but did not maximize globally). It is thus unclear why the use of the term by Rachlin et al. is called upon in the present context.

In summary, Silberberg and Ziriax seem to be using at least three distinct interpretations of molecular maximization. One interpretation is based on the similarity between the probability of reinforcement for different ICTs and either the distribution of ICTs or the conditional probability of an ICT. The second interpretation is similar to momentary maximization, except that several seconds on a side, rather than a single peck, is the behavioral variable controlled by the schedules. The third interpretation is based on a similarity between reinforced ICTs and the distribution of ICTs on the other side, and is only described under conditions that seem to contain contradictory elements. In addition, some additional assumption seems necessary before any of these processes will reproduce the behavior exhibited by my birds (Vaughan, 1981). Unless the theory of molecular maximization is rendered less ambiguous, it will be impossible for other investigators to test its implications experimentally.

MOLECULAR MAXIMIZATION: THE DATA

Silberberg and Ziriax (1985) ran a number of conditions in which a 6-s measurement period was used to determine the rate at which each of two schedules would operate, and other conditions in which a 4-min measurement period was used. The results are summarized in four figures. In their Figure 2, log ratios of both pecks and time are plotted against the log ratio of obtained reinforcers separately for the 6-s and 4-min conditions. Although approximate matching was found with the 4-min procedure, rather extreme undermatching was found with the 6-s procedure. In their Figure

3, the difference in local rates of reinforcement was plotted as a function of the log ratio of obtained reinforcers. In the 4-min conditions those differences tended to be small, whereas in the 6-s conditions they were often quite large. In their Figure 4 the distribution of times spent on the right for one of the 6-s conditions was plotted for each bird, and the average for the birds in the corresponding 4-min condition was plotted. Whereas 3 of the 4 birds in the 6-s condition showed large modes at the ends of the distribution, the 4-min condition produced a single large mode near the middle. And in their Figure 5, for the same conditions shown in Figure 4, the median relative time on the right which followed each possible relative time on the right was plotted. That is, during every 6-s period, for example, some proportion of time was spent on the right. These were then followed by some proportion of time on the right during the next 6-s (or 4-min) period; the data plots showed which proportions tended to follow. The functions for the 6-s condition tended to fall from the upper left to the lower right of the figure, whereas those for the 4-min condition occupied only the central part of the figure (on both the abscissa and ordinate).

As mentioned above, these data can be construed as favoring molecular maximization over melioration only if they are both unambiguously consistent with molecular maximization and inconsistent with melioration. Consider now Figures 2 and 3. Each shows that matching is better in the case of the 4-min measurement period than in the case of the 6-s measurement period (equality of local rates of reinforcement is equivalent mathematically to time-based matching). However, nowhere is it shown (or even proposed) that molecular maximization produces undermatching or unequal local rates of reinforcement under the conditions studied. In fact the theory was proposed as an account of molar *matching*, so at least on the face of it the deviations would seem to be evidence against both theories in question. In any case, it seems those deviations can say nothing about the relative merits of molecular maximization versus melioration, the issue of concern here.

In the next figure (Silberberg & Ziriix, 1985, Figure 4), it can be seen that the distributions of relative right times in one of the 6-s conditions show modes at the extremes,

whereas the average distribution for the corresponding 4-min condition only shows a mode at the center. Of this Silberberg and Ziriix say:

Except for [pigeon] B1 in Figure 4, we see from the U-shaped distribution of local time allocations that birds with the 6-s preference timers tended to have modes at extreme left- and right-key allocations. This outcome is important because even if matching and melioration had been descriptively adequate at a molar level in Figures 2 and 3, they would still have rendered a false portrayal of the molecular processes of choice: By aggregating behavior over large blocks of time, the matching and meliorating equations deny the relevance of a local process in choice; yet, the operation of such a process is clearly evidenced in the bimodal distributions of Figure 4.

Although these bimodal functions are not predicted by matching and melioration theories, they are predicted by molecular maximizing because the highest reinforcement rates . . . were available only at the extreme local time allocations, where the modes were found. (pp. 90-91)

However, there is another possible interpretation of the differences between the functions, which has to do with the measurement periods in and of themselves, independent of what behavior was occurring. Suppose a bird in a concurrent experiment were spending somewhere between 10 and 20 s on each side before switching to the other. If relative time on the right were measured every 6 s, many of those measurement periods would coincide entirely with a visit to the right, and many would coincide with a visit to the left. Only some fraction would include times spent on both sides. If, simultaneously, a 4-min measurement period were in effect, at the end of each period it would always be the case that somewhere around half the time had been spent on the right. It is thus theoretically possible to generate different plots of the distributions of times on the right, given identical behavior, simply by using different measurement periods. This possible influence of the measurement period on the functions obtained is not addressed by Silberberg and Ziriix.

As part of an ongoing experiment, data were collected to address the two possible interpretations of Figure 4. Four pigeons were exposed to *conc* VI 120 VI 120-s schedules, with 1-s

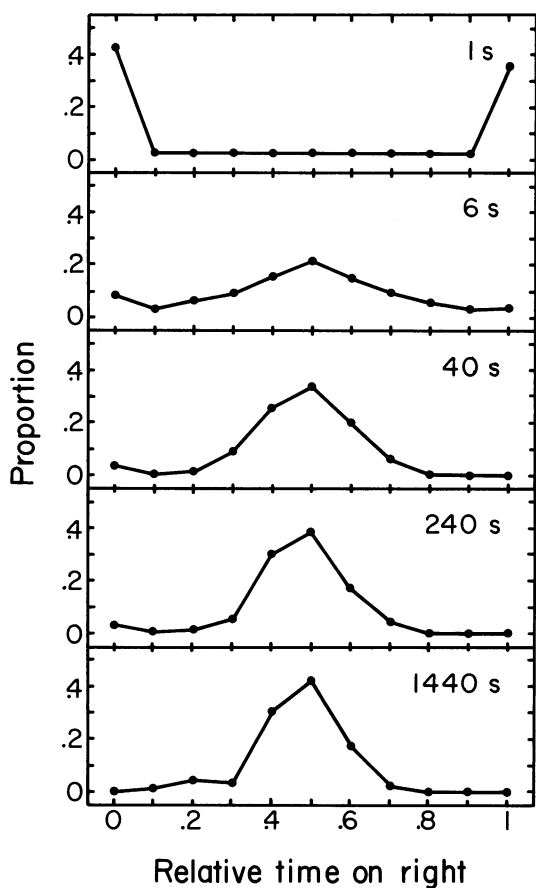


Fig. 1. Normalized distributions of relative times on the right, as a function of the measurement period: 1 s (top), 6 s, 40 s, 240 s, and 1,440 s (bottom). These distributions are averages based on data from 4 birds, all of whom showed the same general patterns.

changeover delays and 2.5-s reinforcement durations, for 27 sessions. Behavior on such schedules should have no particular relation to either a 6-s or a 4-min measurement period. Independent of the schedules, each peck started a timer, which accumulated time (in 0.10-s increments) either until 2 s passed without a response, reinforcement occurred (in which cases the timer stopped), or the other side was pecked (in which case the other timer started, and the first stopped). Every second, relative time on the right was recorded.

Following those sessions, the data for each of the 4 birds were concatenated, so as to create in effect four long sessions (it turns out to make little difference whether each session is analyzed alone, as was also done, or all are done together). The distributions of relative-right times were then plotted, based on five different

measurement periods: 1, 6, 40, 240, and 1,440 s. (Each of these is approximately six times the next smaller, and the 6-s and 240-s measurement periods are flanked by other periods.) There were thus approximately six times as many 1-s as 6-s points, and so on through 1,440 s. If the sessions had not been concatenated the number of 1,440-s measurements would have been much smaller, due to sessions not being an integral number of 1,440-s periods.

After distributions were generated and normalized for each bird, they were averaged across birds for each of the measurement periods. Figure 1 shows the result. For the 1-s measurement period (Figure 1, top), there are extreme modes at each end of the distribution. This would be expected because most of the 1-s measurements consisted entirely of time on the left or time on the right, and only some small proportion occurred at transitions. For the 6-s measurement period (next panel down), there is a mode at .5, and the mode on the right is gone. This general pattern becomes more extreme as the length of the measurement periods is increased, in a manner similar (albeit not identical) to that shown by Silberberg and Ziriax (1985, Figure 4). Here, however, the distributions are all based on the *same* behavior. The conclusion drawn by Silberberg and Ziriax with regard to their Figure 4 is thus, I suggest, moot.

Although not stated explicitly, it is clear from the text (e.g., the quote above discussing the bimodal functions) that molecular maximization predicts that in the 6-s conditions, birds would stay on a side until about the time the preference timer timed out, and then switch to the other side so as to track the higher rate of reinforcement. In their Figure 5, Silberberg and Ziriax showed that for 3 of 4 birds, a small relative right-key time tended to be followed by a large relative right-key time (based on medians), and vice versa, with intermediate points following intermediate. Of this figure they say:

There we see strong evidence of behavioral patterning. This sequencing of time allocations is exactly what would be expected by maximizing of local reinforcement likelihoods, because maximizing reinforcement with this study's choice contingencies requires strict alternation of time allocations from one schedule to the other over successive 6-s periods. Figure 5 makes

clear that animals reasonably approximated this strategy. (p. 91)

However, the data shown in Figure 5 are in fact quite weak in terms of sequences of times on the two sides. Taking Bird B2 as an example, one possible trajectory is shown in the present Figure 2. Beginning at *a*, a relative right time of 1 produces a subsequent time of .32. At *b*, a relative right time of .32 produces one of .64. At *c*, a relative right time of .64 produces one of .38. And at *d*, a relative right time of .38 produces one of .59, and so on. This sequence is not a very close approximation of a strategy of strict alternation coinciding with the 6-s timer. In addition, molecular maximization is a theory about each visit to a side, not about some average measure of visit durations, so something more than medians should have been plotted in their Figure 5.

The two triangles (at coordinates 1,0 and 0,1) show the prediction of strict alternation as discussed by Silberberg and Ziriax: Following a relative right time of 1, the next one should have a value of 0, and vice versa. (This applies to every visit, and not just to medians of visit times.) Judging from the experimental procedure (see Figure 1, top, of Silberberg & Ziriax), the theory further predicts that for all relative times less than .25 a relative right time of 1 should follow, and for all times greater than .75 a relative time of 0 should follow. However, none of the data points in Silberberg and Ziriax's Figure 5 are consistent with such a function (although some proportion of the original data may be).

In summary, none of the data presented by Silberberg and Ziriax (1985) in any of their figures is simultaneously consistent with molecular maximization and inconsistent with melioration. Although the undermatching of their Figure 2 is perhaps inconsistent with melioration, it was not shown to be consistent with molecular maximization; a similar conclusion applies to the deviations from equal local rates of reinforcement shown in their Figure 3. (I say "perhaps" because they did not change conditions based upon stability criteria, but at experimenter convenience, and hence stability may not have always been attained. Melioration does not specify how fast stability will be approached, but only that when it is reached local rates of reinforcement, for example, will be equal.) It is probable that the

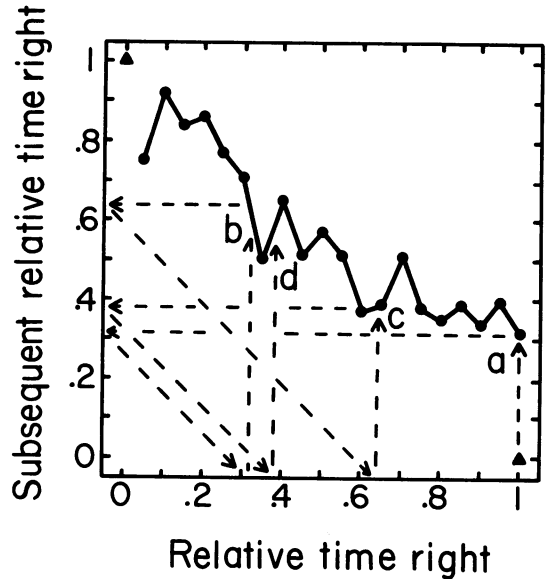


Fig. 2. Replotting of data for Bird B2 from Figure 5 of Silberberg and Ziriax (1985). Dashed lines show one possible trajectory of relative times on the right over a number of 6-s intervals. Triangles correspond to the prediction of molecular maximization.

differences in the distributions shown in their Figure 4 are at least in part a result of the different measurement periods rather than of differences in behavior, and their Figure 5 does not support the strong generalization which they draw from it.

CONCLUSION

Although there is other evidence (e.g., some contained in Table 2 of Silberberg & Ziriax, 1985), it may not be necessary to go any further in defending the assertion that Silberberg and Ziriax have neither presented a relatively unambiguous theory nor have they provided empirical data that are both consistent with that theory and inconsistent with melioration. Although melioration would have been brought into question only if both questions (a) and (b) asked above had been answered in the affirmative, here both have been answered in the negative. I have no doubt that behavior is orderly even at the level of interresponse times, but Silberberg and Ziriax have not persuaded me that molecular maximization is presently a viable account of behavior at their level of analysis.

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