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IDENTITIES AND THEIR USES: RESPONSE TO "RATE, PROBABILITY AND MATCHING" BY RACHLIN AND LOCEY

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Some of the differences between Rachlin & Locey (2010) and Thorne (2010) are due to what assumptions are taken as givens versus questionable. Most of the other apparent differences are largely linguistic and can be resolved by agreeing on terminology. The identity combining both rate and probability of reinforcement is general, has already revealed order not previously noted, even in its currently simple form, and provides a way of incorporating additional variables known to influence and characterize behavior.

Key words: identity, tautology, law, matching, reinforcement probability, reinforcement rate, running rate, postreinforcement pause, bout

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Rachlin & Locey's (2010) first sentence says that Herrnstein's (1961) relationship and their Equations 1 and 1a are identities rather than empirical findings. I disagree. The single-schedule arithmetically forced identity is

$$\text{Response Rate} = \frac{\text{Rate of Reinforcement}}{\text{Probability of Reinforcement}} = \frac{\text{R}}{\text{p}} = \text{B}$$

hence the two-schedule "identities" become

$$\frac{B_1}{B_1 + B_2} = \frac{R_1\, p_2}{R_1\, p_2 + R_2\, p_1} \qquad (3)$$

$$\frac{B_1}{B_2} = \frac{R_1\, p_2}{R_2\, p_1} = \left(\frac{p_2}{p_1}\right)\left(\frac{R_1}{R_2}\right) \qquad (3a)$$

which will hold for concurrent, multiple, and chain schedules with fixed or variable ratio or interval components, whether matching occurs or not. Their Equations 1 and 1a can be viewed in two ways: (1) as an "empirical" finding that may hold in some conditions but not others (i.e. when matching does indeed occur, but not with the more frequent case of undermatching or behavioral contrast), or (2) as a "theoretical" model or proposed law (possibly thought to underlie the apparent exceptions but concealed by other considerations, such as hypothetical reinforcers, or other parameters). That Rachlin and Locey take the latter view is suggested by the second half of their sentence saying that "the empirical interest in matching lies not in whether organisms match (that they do is our underlying assumption)". That assumption is not one I make.

The same sentence references Rachlin (1971)—an article that is topically relevant and which I regret not having cited. In it Rachlin used relative time allocation rather than response allocation but either will suffice. He argued that the concept of reinforcer "value" is tautologous, from the standpoint of being based on preexisting assumptions that are not themselves falsifiable, and concluded that matching is too. Given those assumptions, I would agree, and Equation 3 of that article leads, not surprisingly, to Rachlin & Locey's (2010) Equation 1a. It will also reduce to the same equation if his "other" parameter X is replaced with the probability of reinforcement p.

The disagreement above (and some others below) are due to the ambiguities in language and its usage. In everyday parlance we usually consider the words *tautology*, *circularity*, and *identity* to be synonymous, with little confusion. In the present context it is necessary to distinguish between a logical tautology and a mathematical identity; and between the theoretical, empirical, and definitional.

Rachlin & Locey (2010) take issue with my statement that rate matching and probability equality are confounded correlates of equal

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potential significance, saying, ''But they are more than that; they are one and the same thing''. I see no error in my statement and would still say it the same way (possibly deleting ''potential''). We are in agreement here but just describing it differently. I agree that they are one and the same thing, in the sense that if one holds then so must the other, as Revusky (1963) pointed out. That was my point and message. However, I do not see how being the same means ''they are more than that'' versus ''merely that''. They continue similarly with, ''To say that when matching does not occur it is due to differences in the two probabilities is to say that when matching does not occur it is due to the fact that matching does not occur'' (but see Equations 3 and 3a above). Both statements are true. I consider this as something of a semantic quibble (perhaps on both sides) again arising from the inadequacies of languages when dealing with circularity, simultaneity, mutual dependence, etc. The ambiguity here may be in whether ''due to'' is taken as causal versus connected and inseparable. Alternative wordings might be ''…must necessarily be contingent on the two probabilities of reinforcement being unequal'', or perhaps less arguably ''…must necessarily be accompanied by unequal probabilities of reinforcement''. Mathematics is clearer, sometimes.

Rachlin & Locey (2010) repeatedly make reference to feedback functions. It is a term I deliberately avoided in the short Identities paper since the topic is a large one leading to extended discussion. I will make only a few selected points about it here.

The behavioral identity $B = R / p$ is in itself a definition of the feedback function for ratio schedules (and is often expressed as such after rearrangement and inverting p). Less obvious is that it is also and simultaneously the feedback function for interval schedules *if* the interreinforcement intervals are timed from the availability (set up) of the previous reinforcement instead of its delivery (the distinction disappears in ratio schedules). This also makes the mean Obtained and Programmed values the same (provided that a few minor details are observed). The common practice of timing intervals from delivery rather than availability is partly an historical accident due more to Ralph Gerbrands than to Skinner, and the cost of stepping relays to temporarily hold any uncollected reinforers until delivery—not a problem with today's computers. Ferster and Skinner (1957) used both procedures but, for FI, preferred the now traditional one so that ''none of the intervals will be less than the designated fixed interval. Some will be larger, however … within a second or two of the designated fixed interval''. Today we are more concerned with the Obtained versus Programmed disparity on VI, and quantifying those cumulative ''second or two'' differences with derived feedback functions has occupied many researchers' thought and time. Which of the two interval programming procedures best models which conditions in the working world or natural world has not been explored.

None of the above is to imply that feedback functions are not important, they definitely are (Baum, 1973, 1992), and deliberately manipulating them can be informative (e.g. McDowell & Wixted, 1986; Soto, McDowell & Dallery, 2006).

Rachlin & Locey (2010) frequently use the word ''simpler'' and say I argued that that was an advantage of the ''probabilistic'' formulation over the rate form. I did not, nor did I even use the word. When applied to Baum's generalized power law expression the probability and rate formulations are equally simple. Both describe the data well, with interesting differences which I pointed out. I do not know whether the function $p = \alpha + \beta\ r$ would be considered ''a lot simpler'' than the ''more complicated'' $b = r / (\alpha + \beta\ r)$, having ''greater complexity''. Both contain the same number of free variables, but with the second having one more operator (division). Although I appreciate simplicity, other things being equal, what I would argue is that the identity-based formulations are more ''general''—applying to both rates *and* probabilities, and to the different conditions listed under Equation 3a above; while also being theory free. Neither did I say that a model based on reinforcement probability was ''better'' than a model based on its rate. Using either variable alone simply hides the other in its parameters.

Rachlin & Locey's (2010) interpretation of my post hoc VR that ''would have'' produced exactly the same results as a single session's programmed VI assumes I would generate that VR from the identity of Equation 7. That is not the case. Equation 7 is based on overall means.

An arithmetic, geometric or other VR with the same mean would not produce the same result. The ''equivalent'' VR would consist of the ordered response counts that the animal actually emitted for each of the ordered interreinforcement intervals in that session, had they been programmed instead. This is contrived because it is impossible to realize unless the experimenter is prescient. Or so it would seem. Rather interesting, however, is what happens if this sort of procedure is applied to a subject ''yoked to itself''. McDowell & Wixted (1986) and Baum (1993) did essentially that—synthesizing an ''equivalent'' VI from the subject's previously programmed VR.

In the first study the response rates and patterns generated by the two different schedules were nearly identical. The second study showed higher absolute response rates under VR but similar response rate patterns across a wide range of reinforcement rates, with the two approaching coincidence at the higher values.

This contrived example was just one of several positing that rate of reinforcement and probability of reinforcement both affect behavior simultaneously and continuously (we can argue over unit size and integration times), that both variables operate in single schedules and in choice, whether one of them is the experimenter's explicit independent (Zeiler's 1977 ''direct'') variable or not; and that attending to only one of them may be concealing useful information.

Referring to Figure 2, Rachlin & Locey (2010) say the data points ''are remarkably well fit by straight lines (almost) going through the origin''. They then assume that ''the y-intercepts are essentially zero'' thus reducing to $R/B = \beta(R/T)$, where β is the slope, telling us that ''response rates are constant. But…response rates are not constant'' (they later say ''the Catania-Reynolds plots show, over a wide range of obtained reinforcer rates, response rates…are nearly constant''). This puzzles me since I said, ''With the units used here the absolute values for α are small. However, α cannot be zero, or the animal would always respond at its asymptotic response rate regardless of reinforcement rate.'' Note that ''small'' is relative, and a small value can matter—particularly when it's in the denominator. We seem to be saying the same thing, and differing only on whether the empirically fitted nonzero intercepts are somehow really zero.

Regarding some of their other implications and speculations: Similar results were found for the other 3 of the 6 pigeons tested, although bird 121 (which was unrepresentative of the other 5 birds in Catania and Reynolds, 1968, Figure 1) yielded an r^2 of only 0.784 for the probability plot versus 0.537 for the rate plot (the other two being 0.971 versus 0.876 and 0.995 versus 0.874). Only 2 of the 6 birds showed a few data points ''bunched up around the origin''. Enlarging the plots, including or excluding them, or plotting the two separately did not reveal any ''obscured'' deviation from the straight line.

I agree that the points on the left of Figure 2 ''are remarkably well fitted by straight lines,'' both visually and in terms of their r^2s. This was determined by Catania and Reynolds' (1968) data, which others can check, and does not appear to be forced by anything that I can identify. I did not know in advance whether the result would turn out to be nonlinear, roughly linear, or noticeably linear, and I too was surprised by the excellent fit. This seems to merit speculative interpretation, one possibility being that obtained probability of reinforcement may be a controlling or modulating variable even on interval schedules.

I do not say that VI schedules are really VR schedules in disguise, or vice versa. We all know that ratio and interval schedules are different and produce different behavioral results, the most consistent being higher and lower response rates, respectively. But they have more in common. If the schedules are variable rather than fixed both produce steadier response rates. Whether fixed or variable both result in a noticeable postreinforcement pause, which lengthens roughly in proportion to the schedule value, followed by a ''run''. Both can produce inverted U- shaped functions at middle values. When both become ''lean,'' steady responding becomes increasingly erratic (typically called strain in one case, or bouts and pauses in the other). This suggests that the many of the same variables are operating in both.

Like the behavioral identity, many of the laws of physics are also circular defining equations, but have proven quite useful. Rachlin (1971) said much the same using The First Law of Thermodynamics as an example, and adding that apparent departures from such laws lead us to look further. On this we agree completely. I will use the example of Ohm's Law $E = I * R$, where apparent variation led to the discovery that $R = c * Length/Area$ with a different c for each material, then that the battery must have an internal resistance, then that R varied as a function of ambient temperature, then that temperature also increased with $I^2 R$ self heating, making the Law no longer linear. I ended by saying that the behavioral identity should increase in usefulness as we incorporate additional equations relating its three variables to themselves or other variables, and that these equations might be empirical, theoretical or mathematically definitional. Although most recent attempts have been theoretical, my own preference is the definitional. How does that apply here or for the future? I was hoping to treat that in a follow-up article but it may be necessary and more timely to introduce it here. The first step might be to parse mean response rate B/T in the identity, or its IRT, into the mean postreinforcement pause (PRP) and mean ''running'' IRT, which are algebraically related. A later step might be to further parse the PRP into reinforcement duration and the post-eating or pre-responding pause; and/or parsing mean running rate into bouts and pauses (see Nevin & Baum, 1980; Rachlin, 1978; Shull, Gaynor & Grimes, 2001). This approach would be a departure or shift from most current practice, not only in being atheoretical but also resulting in equations that would include several dependent and independent variables at once. In the behavioral world of continuous interaction and feedback this in/dependent variable distinction is rather artificial anyway (and I suspect in fields like, say, Magnetohydrodynamics). The possibility of partitioning and including reinforcement rate and probability; reinforcement duration or density; the postreinforcement pause; inter- and intrabout rates, durations and transition probabilities; and other variables into a unified framework and algebraic identity might be nontraditional, but consistent with the suggestion in the abstract and closing paragraph of Baum (1993).

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