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Bryan Klapes Date

Testing the Validity of a Matching Law-Based Estimation of Punishment Sensitivity

By

Bryan Klapes
Master of Arts

Psychology

Jack J McDowell, Ph.D.
Advisor

Darryl Neill, Ph.D.
Committee Member

Michael T. Treadway, Ph.D.
Committee Member

Accepted:

Lisa A. Tedesco, Ph.D.
Dean of the James T. Laney School of Graduate Studies

Date

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Bryan Klapes
B.S., Emory University, 2013

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An abstract of
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Abstract

Testing the Validity of a Matching Law-Based Estimation of Punishment Sensitivity

By Bryan Klapes

An individual's sensitivity to punishing stimuli has the potential to be used as an objective assessment of his or her level of depressive symptomology. Quantification of loss aversion (Kahneman & Tversky, 1984) was the leading approach to ascertaining this estimation. However, violations of the gain-loss separability (GLS; Kahneman & Tversky, 1992) axiom of Prospect Theory have arisen in the literature, leading to an opening for other approaches to take hold as the best approach to estimating punishment sensitivity. A matching law-based (Hernnstein, 1970) approach to punishment sensitivity estimation was developed by Rasmussen & Newland (2008). However, fits of the generalized matching law (GML; Baum, 1974b) to data acquired from their "punished" conditions were below the field's customary 85% variance accounted for (%VAF) threshold for good fits. Methodological alterations to the study design were employed in the present project in an attempt to obtain better fits using Rasmussen & Newland's approach. None of these manipulations resulted in fits of the GML to the punished conditions that consistently exceeded the %VAF threshold, and hence their method is not likely to be useful. A general increase in our foundational knowledge about contingent punishment may be necessary before a valid idiographic approach to estimating one's punishment sensitivity can be developed.

Keywords: matching law, loss aversion, punishment

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Table of Contents

Testing the Validity of a Matching Law-Based Estimation of Punishment Sensitivity

The relatively high validity and reliability of the learned helplessness hypothesis has made it a popular animal model of depression (Sherman, Sacquitne & Petty, 1982; Vollmayr & Henn, 2001). The learned helplessness hypothesis postulates that an organism exposed to non-elicited and uncontrolled punishment will have lasting changes in its motivational, cognitive and emotional states (Maier & Seligman, 1976). These deviations from normality in "helpless" organisms fit well with conclusions made about the vicissitudes of cognitive-emotional and motivational states in depressed humans (Sullivan & Conway, 1989; Treadway, Bossaller, Shelton & Zald, 2012). The study of contingent punishment on behavior can also be helpful in understanding the effect of aversive stimuli on human affect.

The ethical issues related to the study of punishment are substantial, leading many individuals to advocate for the avoidance of these practices whenever possible (e.g., Sidman, 2001; Skinner, 1953). However, not attempting to understand the effect of contingent punishment on human behavior would be foolish; even after eliminating all socially-derived punishment, our everyday experiences are still governed by punishment contingencies:

> Whenever we interact with the physical word, there are many punishing contingencies awaiting us. … The degree to which these punishing contingencies are actually applied can be seen in the initial efforts of the young child in learning to walk and to run. So powerful are these potential punishing contingencies that they exist even when we sleep. The response of rolling off a bed is punished immediately and severely by collision with the floor below. Elimination of punishing contingencies by the physical world would appear to require elimination of all behavior that involves interaction with the physical world.
> (Azrin & Holz, 1966, p. 438)

The absence or reduction of interaction with the physical world has also been linked with negative affect (Lewinsohn & Libet, 1972). With this in mind, the fact that behavioral activation (BA) therapy (Lewinsohn, Biglan, & Zeiss, 1976), which aims to increase the opportunities for one to acquire positive reinforcement by increasing one's behavioral output, has become one of the most successful behavioral therapies for depression is unsurprising (Cuijpers, van Straten, & Warmerdam, 2007; Lejuez, Hopko, Acierno, Daughters, & Pagoto; 2011).

One could argue that BA's effectiveness comes from an alleviation of the heightened sensitivity to punishment that is seen in depressed individuals (Eshel & Roiser, 2010; Murphy, Michael, Robbins, & Sahakian, 2003; Taylor Tavares, Clark, Furey, Williams, Sahakian, & Drevets, 2008). By encouraging clients to perform tasks that they may be perceiving as more costly (or punishing) than reinforcing (e.g., a depressed client states that going to a movie with friends will require more effort than enjoyment), clinicians are effectively using an exposure-based technique to reduce the client's perceived punishing qualities of the event. Similar to the physiological changes seen in systematic desensitization (Wolpe, 1961) or exposure therapy for phobias (Watson, Gaind, & Marks, 1972), repeated exposures to these stimuli may produce a global change in the depressed individual's perception of punishing events. Thus, an objective measurement of sensitivity to punishment could be a vital tool in both assessing depressive symptomology (Elliot, Sahakian, Herrod, Robbins, & Paykel, 1997) and determining the efficacy of BA and other behavior-based therapies.

Prospect Theory and Loss Aversion

Prospect Theory (PT) was developed by Daniel Kahneman and Amos Tversky as a means of understanding behavior (1979). The basic tenet of PT is that the decision to emit a behavior is assessed using both gains and losses associated with the performance of that behavior. This concept was originally proposed as an alternative to Expected Utility Theory (von Neumann & Morenstern, 1944), which states that the decision to emit a behavior is assessed from the cumulative value of all assets or losses associated with the performance of that behavior. Since its conception, PT has generated vast amounts of literature, including publications that are almost completely devoted to its study (e.g., *The Journal of Risk and Uncertainty*). In 2002, Kahneman and (posthumously) Tversky won the Nobel Memorial Prize in Economics for the theory.

Prospect Theory is considered to be "reference-dependent." That is, as individual's point of reference changes, often times he or she will choose alternatives differently (Tversky & Kahneman, 1992). A major aspect of PT's reference-dependent nature is the asymmetrical effect of gains and losses on behavior. This phenomenon is called "loss aversion," which refers to the fact that organisms are more sensitive to losses than gains (Kahneman & Tversky, 1984). An estimate of loss aversion, λ, can be quantified by using a "mixed gamble" procedure. This procedure implements differing probabilities and magnitudes of gains and losses on one or more alternatives (e.g., Abdellaoui, Bleichrodt, & Paraschiv, 2007; Schmidt & Traub, 2002; Tom, Fox, Trepel, & Poldrack, 2007). Studies utilizing mixed gambles have shown that the extent to which one is loss averse varies on an individual basis; however, looking across studies, a loss of the objectively equivalent value of a gain is generally valued about two times as large in subjective magnitude (Sokol-Hessner et al., 2009).

An important axiom of PT and loss aversion is "gain-loss separability" (GLS; Tversky & Kahneman, 1992). Wu & Markle (2008) wished to test the assumption of GLS using a "double-matching" experiment. In the experiment, participants were asked to pick between two mixed gambles. The mixed gambles were then split into separate choices; participants were asked to pick between only the gain components of the mixed gambles, and then the loss components, individually. Wu & Markle found that when split, the mixed gamble that was originally (albeit only slightly) preferred was now much less preferred. GLS violations have continued to be found in the literature (for a review, see Por & Budescu, 2013), calling into question the theoretical validity of loss aversion estimation techniques and even PT as a whole. To provide a more theoretically-sound approach to assessing one's sensitivity to punishment, one could potentially look toward the field of experimental behavior analysis and the matching law.

A Brief History of Experimental Behavior Analysis and the Matching Law

The etiology of the scientific study of "operant" or "instrumental" conditioning parallels that of "classical" conditioning. Similar to the profound effect that Ivan Pavlov's seminal research (1906) pertaining to the conditioned salvation of canines had on John Watson's reflexology (1913), Edward Lee Thorndike's formulation of the "Law of Effect" (1898, 1911) had vast implications on the field of experimental behavior analysis (see Chance, 1999, for a comprehensive review). Thorndike's lasting legacy on the field, however, is generally overshadowed by that of his most famous intellectual descendent, B.F. Skinner.

Skinner (1938) posited that when an organism is allowed to freely emit or not emit behaviors, studying the rate of responding by the organism would be the most fruitful type of behavior analysis. An operant chamber, which sometimes is referred to as a "Skinner box" due to his popularization of the device, allows a researcher to provide continuous reinforcement to the organism, which in turn will result in continuous emission of a target behavior. Skinner and Charles Ferster (1957) provided a broad review of the many different types of reinforcement schedules that could be used with the operant chamber. In that review, the authors first discussed concurrent schedules, or the simultaneous presentation of two schedules of reinforcement. Concurrent schedules of reinforcement have garnered significant research attention due to their propensity to study choice and preference (Catania, 1966; Findley, 1958). Behaviors maintained by concurrent variable-interval (VI) schedules are sensitive to reinforcement rate; as such, these types of schedules are used most frequently in studies of continuous choice (Pierce & Cheney, 2013).

Herrnstein (1961) published a prominent study that changed the course of the field of experimental behavior analysis. Using a concurrent schedule experimental design, he studied pigeons' behavior in continuous choice paradigms. However, instead of the typical data analytic strategies of the day, Herrnstein decided to analyze the birds' relative acquired reinforcement and allocated responses. By plotting these proportions against each other, Herrnstein found that the relative rate of acquired reinforcement on one alternative was roughly equal to the relative rate of allocated responses on the same alternative. A mathematical equation, later termed the matching law (Herrnstein, 1970), can be formulated to represent this discovery:

$$\frac{B_1}{B_1 + B_2} = \frac{r_1}{r_1 + r_2}\ , \tag{1}$$

where B_1 = number of behaviors performed on side 1, B_2 = number of behaviors performed on side 2, r_1 = number of reinforcers acquired on side 1, and r_2 = number of reinforcers acquired on side 2. A set of simple algebraic manipulations will transform the proportional form (Equation 1) into a ratio form:

$$\frac{B_1}{B_2} = \frac{r_1}{r_2}\ . \tag{2}$$

This relationship between an organism's responses and acquired reinforcement does not solely pertain to pigeons' key pecking behavior in a specific laboratory paradigm; the matching "law" was deemed as such because it describes many organisms' behavior cross-situationally (de Villiers, 1977). For example, in addition to countless laboratory experiments involving pigeons and rats, matching has also been observed in naturalistic settings by non-captive birds (Baum, 1974a). Matching has also been extended to human behavior, both in the laboratory (e.g., Bradshaw, Szabadi & Bevans, 1976) and in naturalistic settings (e.g., Conger & Killeen, 1974; McDowell & Caron, 2010a, 2010b).

Despite providing a compelling account of individual behavior for a variety of organismic and environmental properties, Equations 1 and 2 only factor in the rate of reinforcement. Other attributes of the reinforcers (e.g., magnitude or duration) are also important in determining the allocation of an organism's behavior (Catania, 1963; Neuringer, 1967). Rachlin (1971) and Killeen (1972) showed that other reinforcer attributes can be included in the Equation 2 in a multiplicative fashion:

$$\frac{B_1}{B_2} = \left(\frac{r_1}{r_2}\right)\left(\frac{m_1}{m_2}\right)\left(\frac{i_1}{i_2}\right)\left(\frac{x_1}{x_2}\right), \quad (3)$$

where m_1 = magnitude of the reinforcer on side 1, m_2 = magnitude of the reinforcer on side 2, i_1 = immediacy of reinforcement on side 1, i_2 = immediacy of reinforcement on side 2, x_1 = reinforcement attributes affecting behavior other than rate, amount, and immediacy on side 1, and x_2 = reinforcement attributes affecting behavior other than rate, amount, and immediacy on side 2. Empirically, results from Schneider's (1973) analysis of reinforcer magnitude and Torodov's (1973) analysis of reinforcer duration suggested that a power function more appropriately described the data:

$$\frac{B_1}{B_2} = \left(\frac{r_1}{r_2}\right)^{a_r}\left(\frac{m_1}{m_2}\right)^{a_m}\left(\frac{i_1}{i_2}\right)^{a_i}\left(\frac{x_1}{x_2}\right)^{a_x}, \quad (4)$$

where a_r, a_m, a_i, and a_x are empirically estimated values for rate, magnitude, duration, and other reinforcer properties, respectively.

Due to the non-operationalized manner by which x_1 and x_2 are determined, m_1, m_2, i_1, and i_2 could be encapsulated into x_1 and x_2 in order to retain reinforcement rate as the sole predicting variable of behavior (i.e., in line with Equation 2). Substituting a "bias parameter" (*b*; Baum, 1974b) for the newly determined $\frac{x_1}{x_2}^{a_x}$ that subsumes both $\frac{m_1}{m_2}^{a_m}$ and $\frac{i_1}{i_2}^{a_i}$, a new version of Equation 2 would be:

$$\frac{B_1}{B_2} = b\left(\frac{r_1}{r_2}\right)^a . \tag{5}$$

Since a_r is the only exponent left over from Equation 4, its subscript is now unnecessary in Equation 5.

With the incorporation of exponents into the equations, one can see that Equations 4 and 5 are no longer linear. Although this form is not imperative for analysis, linearized models have historically been preferred within the literature on the matching law. By taking the logarithm of each side, Equation 5 becomes linear again (i.e., transformation into a log-log model):

$$\log\left(\frac{B_1}{B_2}\right) = a\log\left(\frac{r_1}{r_2}\right) + \log(b) . \tag{6}$$

Equations 5 and 6 have widespread empirical support over the classic forms (Baum, 1979; Wearden & Burgess, 1982). "Both logic and data indicate that classic matching theory…is false. At best, the classic theory can be considered a special case of the modern theory, with limited applicability" (McDowell, 2005, p. 123). McDowell's (2013) comprehensive review of the two versions of matching theory provides a compelling account of the modern version's dominance over the classic version.

Using Matching Law Equations to Estimate Punishment Sensitivity

Environmental factors other than reinforcement properties have an effect on an organism's behavior. Punishment, or the implementation of a stimulus that decreases operant behavior, has been studied intensively in experimental behavior analysis. Dinsmoor (1952) was the first to study the behavioral effects of a schedule of punishment that was superimposed on a variable-interval reinforcement schedule. His rationale for using this type of analysis makes intuitive sense: "[o]utside of the walls of the psychologist's laboratory, it seems to me that punishment is more typically added to than substituted for positive reward or reinforcement" (p. 27). Rats were trained to press a bar for a food reward, and then were run in alternating sessions where the subject was either "safe" (i.e., in the same environment as the training sessions) or was punished for every response (i.e., fixed-ratio, FR, schedule of 1). Azrin, Holz, and Hake (1963) followed up on this study, looking at the effect of implementing FR schedules of punishment with larger values. They found a significant suppression of behavior even when implementing FR 1000 schedules (i.e., every 1000th peck was punished).

Although punishment has been shown to be more impactful when presented on fixed-ratio schedules (Pierce & Cheney, 2013), to assimilate punishment rates into the matching law, punishment should be operating under the same schedule type as reinforcement: variable-interval. Filby and Appel (1966) were the first to implement variable-interval punishment schedules in the presence of variable-interval reinforcement. Their finding that behavior maintained by richer reinforcement schedules showed less suppression in the presence of punishment than leaner reinforcement schedules was corroborated by Church and Raymond (1967).

Rasmussen & Newland (2008) used matching law equations to study the asymmetry between reinforcement and punishment, an endeavor that has typically only been performed by social psychologists (Baumeister, Bratslavsky, Finkenauer & Vohs, 2001; Mummendey & Otten, 1998; Taylor, 1991). Their method appears to rely heavily upon past attempts to scale stimuli against one another within a matching law framework. Miller (1976) was the first to do such an experiment; he ran birds on three sets of five concurrent VI schedules of reinforcement using different types of grain as the reinforcer on each alternative. He then fit Equation 6 to all three sets of data, and used the *b* parameters calculated from each fit to scale the "qualities" of the different types of grain:

$$b = \frac{Q_g}{Q_r}, \tag{7}$$

where Q_g and Q_r were estimates of the "qualities" for the two types of grain used in each condition. Dallery, McDowell, & Soto (2004) performed a similar study using linear system theory (McDowell, Bass, & Kessel, 1993) with rats, by modifying the amount of sucrose that was contained in the food pellets used as reinforcers.

Rasmussen & Newland (2008) ran human participants on a computer program that simulated an operant chamber, with the acquisition and loss of $0.04 acting as reinforcement and punishment, respectively. Each participant ran on two sets of concurrent VI schedules of reinforcement: a baseline set (i.e., one with no punishment implemented) and one with the same VI schedule pairings, but with a VI schedule of punishment superimposed on one alternative. Equation 6 was fitted to the data, and *b* parameters were estimated for similarities or differences between the two conditions. Although not explicitly stated as such, Rasmussen & Newland used Miller's analytic method scaled the "qualities" of the alternative in both conditions:

$$b_{baseline} = \frac{Q_{1unpunished}}{Q_2} \tag{7.1}$$

and

$$b_{punished} = \frac{Q_{1punished}}{Q_2} \;. \tag{7.2}$$

Thus, to ascertain a value that has a theoretical similarity to λ, one would have to look at the relative increase of Q_1 in the baseline condition compared to the punished condition:

$$\lambda = \frac{Q_{1unpunished}}{Q_{1punished}} = \frac{b_{baseline}}{b_{punished}}. \tag{8}$$

Rasmussen & Newland found that the average *b* for the punished conditions was approximately three times less than the average *b* calculated from the baseline conditions. They concluded that "losing a penny is three times more punishing than earning that same penny is reinforcing" (pg. 165).

Despite providing a compelling alternative to PT's approach of using mixed gambles to estimating λ, a major issue is present in Rasmussen & Newland's (2008) study. Four of the 5 fits of Equation 6 to the data acquired from the punishment condition had a percent variance accounted for (%VAF) that were poor relative to the literature (~85%). Two possible explanations exist for these poor fits. First, applying single alternative punishment to a concurrent schedule could inherently disrupt the mechanisms by which an organism matches its behavior to reinforcement. Second, only three, instead of the usual five, schedules pairings were used in the experiment (VI 12s-60s, VI 20s-20s, VI 60s-12s). This small number of schedule pairings may not provide a wide enough range of values for appropriate fits. Replicating

Rasmussen and Newland's study with more schedules could determine if this is a valid approach to estimating sensitivity to punishment.

General Method

Three experiments were conducted over an approximately five month period (October 2015 to March 2016).

Participants

A total of 33 Emory University undergraduate students participated in the experiments. These students signed up for the study using a SONA Systems database as a course requirement for their Introductory Psychology classes.

Informed Consent. At the beginning of each study, each participant was given an explanation that delineated the general principles, risks, benefits, and methods to retract one's consent for the study. At the conclusion of this explanation, they were given a consent-to-participate form to sign. All studies were under "exempt" status for Emory University's Institutional Review Board, due to the non-sensitive nature of the data that were being acquired.

Procedure

Different instructions were given for each experiment. All versions of the instructions can be found in Appendix A.

Apparatus. All experiments described here were run using a computerized operant device developed within the Laboratory for Computational and Mathematical Behavior Analysis (PI: Jack J McDowell, PhD). The device is rooted in work done by researchers assessing rapid-acquisition of operant behavior in humans (Krägeloh, Zapanta, Shephard, & Landon, 2010; Lie, Harper, & Hunt, 2009), but implements very rich schedules of reinforcement (as in Popa, 2013). It incorporates aspects of previous mechanical (Bradshaw, Szabadi, & Bevan, 1976) and computerized (e.g., Madden & Perrone, 1999) apparatuses, as well as novel additions, to improve upon the procedure developed and described by Popa. These additions resulted in fits of Equation 6 to the participants' data that accounted for a very large proportion of the variance (median = 94%; unpublished data). Additionally, estimates of a (0.65 ± 0.02) and b (0.99 ± 0.02) obtained from these fits conformed to those found in previous studies on human operant behavior (Kollins, Newland, & Critchfield, 1997).

In tasks utilizing this device, participants are asked to use the keyboard of an off-the-shelf laptop computer to acquire as many points as possible. These points are essentially tokens of social reinforcement, given weight by a leaderboard that displayed the number of points previous participants had acquired during their testing sessions. Pressing the space bar would be considered a behavior, while pressing the "ctrl" key switched between the two alternatives (Figure 1). Experimental data was sent to an XML database and consolidated into an Excel Spreadsheet for analysis. Modifications to the device's code was performed by the author, with the assistance of Olivia L. Calvin, in order to answer the specific research question for each experiment.

Experimental variables and design. The acquisition of points acted as reinforcement. In all experiments, reinforcement was administered according to random-interval (RI) schedules, with mean intervals varying between 1.0 *s* and 3.0 *s* for all experiments. The loss of points acted as punishment. In all cases, punishment was also administered according to RI schedules, with mean intervals that varied for each experiment. The RI schedules of punishment were always superimposed on an RI schedule of reinforcement. The mean intervals of the punishment RI schedules varied between 1.25 *s* and 6.0 *s*.

Experiments consisted of either 9, 18, or 27 components of two concurrently available schedules of reinforcement. Some components also had punishment schedules superimposed on one alternative. Each component lasted 200 *s* (as in Popa, 2013), but were broken down into ten 20 *s* segments. The average values for the experimental variables (i.e., behavior, acquired reinforcers, acquired punishers) for these ten 20 *s* segments were calculated for each component. These averages were ultimately used as the data for each component in the experiments.

Experiment 1

The first experiment was performed to validate that the loss of points in the aforementioned operant device was an effective punisher. As such, this experiment attempted to replicate the findings of Critchfield, Paletz, MacAlleese, & Newland's (2003) Experiment 1.

Method

Participants. A total of 21 Emory University undergraduate students were recruited to participate. Four participants dropped out before the experiment was complete. Due to experimenter error, two participants' (S032 and S041) data were improperly recorded and therefore unanalyzable. Thus, 15 participants were run in Experiment 1. The sample was 60% female and had an average age of 19.3 ± 0.2 years. The distribution of race/ethnicity was 80% Asian and 20% White.

Experimental variables and design. Participants completed nine components of concurrently-available schedules of reinforcement, where the reinforcement rate was equal on both alternatives. Components were presented in three blocks, each of which was associated with a different RI value of reinforcement (Table 1). To start each block, a baseline component was implemented, where there was no superimposed punishment schedule. For the second component of each block, an RI schedule of punishment with a value of either 2 (50%; P50) or 1.25 (80%; P80) times greater than the RI value of the reinforcement schedules was randomly assigned and superimposed on one of the alternatives. The other punishment schedule was then presented on the same alternative for the third component of the block. This process was iterated for each block, switching between the two alternatives for the implementation of the superimposed punishment schedule (i.e., participants experienced punishing stimuli on both alternatives during the experiment). In addition to simply validating that point loss was an effective punisher, which could be shown by presenting just two components (a baseline condition and a punished condition), schedules of consequences with varying RI values were employed to assess the relationship between consequence rate and behavior.

Data analytic strategy. The sample-wide mean, total behavioral output in each of the three blocks (i.e., for each reinforcement rate) was calculated to assess the reinforcement rate dependency of behavior. The difference in behavioral output, on both the punished and unpunished alternatives, between baseline and punishment conditions for each individual was calculated to assess behavioral change in the presence of punishment. These differences were calculated for both P50 and P80 components of each block to assess punishment rate dependency on behavior.

Results and Discussion

Figure 1 displays the mean change in behavior from the baseline component on the unpunished (top panel) and punished (bottom panel) alternatives in the punishment components. In all blocks, there was the expected reallocation of behavior from the punished alternative to the unpunished alternative, for both the P50 and P80 components. However, this reallocation, in contrast to Critchfield *et al.*'s (2003) results, was not related to the rate of punishment. There were no significant differences in the change of unpunished or punished behavior between P50 and P80 components (grey and black bars in Figure 1, respectively) in any of the blocks. Additionally, behavior did not seem to be dependent upon the reinforcement rate, as the blocks for RI 1 (118.4 ± 31.1), RI 2 (122.0 ± 30.8), and RI 3 (126.5 ± 37.0) were not significantly different from each other in terms of behavioral output. Nevertheless, the results indicate that point-loss is an effective punisher in this procedure.

Experiment 2

The second experiment attempted to use a modified version of Rasmussen & Newland's (2008) study design to acquire good fits of Equation 6 to data collected from the punishment conditions.

Method

Participants. The same 17 participants from Experiment 1 were run in Experiment 2. The issues that caused the two participants' data to be improperly collected in Experiment 1 was rectified for Experiment 2. Thus, these two participants were included in these analyses. The sample was 64% female and had an average age of 19.2 ± 0.2 years. The distribution of race/ethnicity was 76.5% Asian and 23.5% White.

Experimental variables and design. Participants completed two sets of nine components of concurrently available schedules of reinforcement. Components were presented in blocks of three, with 90 second breaks in between. The same pairings of the RI reinforcement schedules were presented in both sets, but in a randomized order (Table 2). One set had no punishment schedule superimposed (baseline condition), while the other had a punishment schedule superimposed on one alternative (punished condition). The RI value of that punishment schedule was 1.5 times that of the reinforcement schedule upon which it was superimposed. Ten of the participants had the punishment schedule superimposed on one alternative, while the remaining 7 had the punishment schedule superimposed on the other.

Data analytic strategy. The mean behavior and acquired reinforcement within a component were calculated for each alternative. Equation 6 was fitted to the data from both the baseline and punished conditions. If a participant did not acquire any reinforcement on one alternative during a component, then a mean of 0 would be present for acquired reinforcement on that alternative. Due to Equation 6 being a log-log model, any data points of 0 for acquired reinforcement will lead to indeterminate estimations of behavior allocation. As such, any component that had a mean of 0 acquired reinforcers on an alternative was discarded from the analysis. Thus, Equation 6 was fitted only to conditions that had at least 5 analyzable components.

Fits were assessed for the percent of variance accounted for (%VAF), *a* and *b* parameter estimates, and number of components that were used for the fit. To validate the Rasmussen & Newland (2008) approach to estimating punishment sensitivity, fits in the punishment condition should have %VAFs that exceeded 85% and *b* parameter estimates that were much lower than the *b* calculated from fits to the baseline condition.

Results

The *a*, *b*, %VAF, and number of analyzable schedules for the fits of Equation 6 to the baseline and punished conditions for each participant can be found in Table 4. Figure 3 displays box-and-whisker plots for the number of analyzable components for each condition across all participants. Eleven of the 17 participants had at least 5 analyzable components for the baseline condition, with the median number of analyzable schedules being 7. However, only 8 of the 17 participants (4 from each of the punished alternatives) had at least 5 analyzable components for the punished condition (median = 5).

Participants showed good conformance of behavior to the matching law in the baseline condition. The average *a* estimate (0.70 ± 0.08) conformed to the results of past human operant studies (Kollins et al., 1997) and *b* estimates (1.05 ± 0.08) were generally centered around unity. Eight of the 11 fits exceeded the 85% threshold for %VAF.

Figure 4 displays box-and-whisker plots for the %VAF of Equation 6 fits to the data in both conditions. The median %VAF for the baseline condition (91.9%) greatly exceeded the median %VAF for the punished condition (73.7%). Most of the participants had poor fits to the punished condition. Of the eight participants who had enough analyzable components in the punishment condition to have Equation 6 fit to the data, only two of them had fits that exceeded the %VAF threshold of 85%. As such, only two matching law-based λ estimates (i.e., the quantification of loss aversion),

$$\lambda = \frac{b_{baseline}}{b_{punished}}, \tag{8}$$

were able to be calculated for the cohort (2.86 for S044 and 2.84 for S046).

Discussion

The results of Experiment 2 suggest that Rasmussen & Newland's (2008) approach to measuring punishment sensitivity is not a good one. The addition of more schedule pairings did not alleviate the issue of poor fits of Equation 6 to the punishment conditions; adding six more schedule pairings still only resulted in 12% (2 of 17) of those who participated in the study having a reasonable and reliable (by matching law standards) λ estimation. However, it should be noted that these estimations (median of 2.85) were close to those attained in Rasmussen & Newland's study (~3.00). These estimations are slightly larger than the generally accepted value of λ in mixed gamble procedures.

The lack of analyzable schedules in the punishment conditions could be a potential confounder that is causing the poor fits. This aspect of the experiment was apparently not an issue for Rasmussen & Newland (2008). Looking at the data, it becomes apparent that individuals "figure out" which of the two alternatives was punished and avoid it entirely; as the experiment progresses, it becomes more likely that the participant emits no behavior on the punished alternative. One way to rectify this issue is to alternate the punished alternative (as was performed in Experiment 1).

Experiment 3

The third experiment was performed to modify and improve upon the replication attempt made in Experiment 2. The goal was to eliminate the potential confounding effect of the large number of unanalyzable components in the punished condition.

Method

Participants. A total of 18 Emory University undergraduate students were recruited to perform in Experiment 3. Two participants dropped out before the experiment concluded. Thus, 16 participants were run for Experiment 3. The sample was 69% female with an average

age of 18.7 ± 0.5 years. The distribution of race/ethnicity was 19% African-American, 25% Asian, 37% Hispanic, and 19% White.

Experimental variables and design. Participants completed the same set of nine components for a baseline condition as performed in Experiment 2. However, instead of completing the same set with a superimposed punishment schedule for the punished condition, participants completed a set of 18 components. In nine of these components, the punishment schedule was superimposed on one alternative. In the remaining nine, the punishment schedule was superimposed on the other alternative. These components were randomly presented within the punishment condition (see Table 3). The RI value of punishment schedules was again 1.5 times that of the reinforcement schedule upon which it was superimposed. As in Experiments 1 and 2, components were presented in blocks of 3, with 90 second breaks in between each block.

Data analytic strategy. The mean behavior and acquired reinforcement within a component was calculated for each alternative. Equation 6 was fitted to the data from the baseline condition. For the punishment conditions, Equation 6 was fitted to the data from components that had the punishment schedule superimposed on the same alternative. Thus, there were two punishment condition fits for each participant. Only conditions that had at least 5 analyzable components were able to have Equation 6 fitted to the data.

Fits were assessed for the percent of variance accounted for (%VAF), *a* and *b* parameter estimates, and number of components that were used for the fit. To validate the Rasmussen & Newland (2008) approach to estimating punishment sensitivity, fits in the punishment condition should have %VAF that exceeded 85% and *b* parameter estimates that were much lower than the *b* calculated from fits to the baseline condition.

Results

The *a*, *b*, %VAF, and number of analyzable schedules for the fits of Equation 6 to the baseline and punished conditions for each participant can be found in Table 5. As with Figure 3 for Experiment 2, Figure 5 displays box-and-whisker plots for the number of analyzable components for each condition across all participants. All 16 of the participants had at least 5 analyzable components for the baseline condition, with the median number of analyzable schedules being 8. Twenty-seven of the 32 punishment conditions (15 fits from the condition where alternative 1 was the punished alternative, and 12 from the condition where alternative 2 was the punished alternative) had at least 5 analyzable components for fits of Equation 6 to the data (median = 6).

Participants, like in Experiment 2, showed good conformance of behavior to the matching law in the baseline condition. The mean of *a* estimates (0.61 ± 0.08) was slightly lower than that in Experiment 2, but the estimates were still within a reasonable range for humans (Kollins et al., 1997). Estimates of *b* (1.03 ± 0.06) were again generally centered around unity. Eleven of the 16 fits exceeded the 85% threshold for %VAF.

Figure 6 displays box-and-whisker plots for the %VAF of Equation 6 fits to the data in both conditions. Following the pattern seen in Experiment 2, the median %VAF for the baseline condition (92.0%) exceeded the median %VAF for the punished condition (67.0%). The vast majority of fits to the punished condition were poor; of the 27 punished conditions that had enough analyzable components, only 7 had fits that exceeded the %VAF threshold of 85%. Additionally, of the 7 punishment condition fits that exceeded the 85% VAF threshold, only three corresponding baseline condition fits also exceeded the 85% VAF threshold. The λ

estimates from these fits were much lower than those attained from "appropriate" fits in Experiment 2 (0.11, 1.64, and 1.95 for S057.1, S071.2, and S072.1, respectively).

Discussion

The modification of having both alternatives punished at some point during the session for the punished conditions helped resolve the issue of non-analyzable components. However, rectifying this issue did not increase Equation 6's goodness-of-fit to the data. Although slightly more fits exceeded the 85% threshold (21.9%) in this replication than in Experiment 2, this number is still too low for a clinician or scientist to have full confidence in the approach's ability to obtain a reliable λ estimation for a particular individual. Further, it should be noted that the "valid" estimations derived in Experiment 3 were much lower than those obtained in Rasmussen & Newland's (2008) study.

General Discussion

The results of the experiments indicate that Rasmussen & Newland's (2008) approach is not a theoretically-sound alternative to PT's mixed gamble procedures for estimating punishment sensitivity. The baseline and punishment conditions that exceeded the %VAF threshold did yield λ values that corresponded with population-wide λ values seen in the loss aversion literature (median of 1.92). However, only 5 (10.2%) of the 49 potential estimations could be deemed as "valid." Thus, the idiographic utility of this method is suspect at best. Nevertheless, the GLS assumption violation leaves an opening for new approaches to the estimation of punishment sensitivity to be created and validated. Rasmussen & Newland's approach is only one of many potential ways to use matching law equations to estimate punishment sensitivity.

Using Matching Law-based Models of Punishment to Estimate Punishment Sensitivity

The direct-suppression model of punishment, first termed as such by Critchfield *et al.* (2003), is a matching law-based model of punishment. Estes (1969) and de Villiers & Millenson (1972) initially discussed the direct-suppression model's main theoretical position (but without a formal mathematical expression): punishment directly subtracts from the reinforcing value of the alternative upon which it is placed. Farley and Fantino (1978) originally present the direct-suppression model in mathematical form as:

$$\frac{B_1}{B_1 + B_2} = \frac{r_1 - cp_1}{r_1 - cp_1 + r_2 - cp_2}, \tag{1a}$$

where p_1 = number of punishers acquired on side 1, and p_2 = number of punishers acquired on side 2.

The coefficient, c, of the acquired punishment rates was a conversion factor from the actual foot-shock punisher implemented in Farley & Fantino's (1978) study to "negative food units." To retain the direct-suppression model's theoretical validity, c cannot be less than 0 (de Villiers, 1980; Farley, 1980); if c were a negative value, then the model would be stating that punishment is directly *additive* to the reinforcing value of the alternative upon which it is placed. In addition, another restriction needs to be implemented on c in order to not have a negative predicted value for behavior allocation. When $r_1 / p_1 > r_2 / p_2$, then c must either be less than r_2 / p_2 or greater than r_1 / p_1. Conversely, when $r_2 / p_2 > r_1 / p_1$, then c must either be less than r_1 / p_1 or greater than r_2 / p_2.

Although not explicitly expressed by Farley & Fantino (1978), de Villiers (1980), or Farley (1980), c has theoretical roots in the matching law framework. Equation 4 can be used as a template to create a direct-suppression model that accounts for reinforcement and punishment magnitude, as well as rate:

$$\frac{B_1}{B_2} = \frac{r_1^{a_r}(m_r)^{a_{mr}} - p_1^{a_r}(m_p)^{a_{mp}}}{r_2^{a_r}(m_r)^{a_{mr}} - p_2^{a_r}(m_p)^{a_{mp}}}\left(\frac{x_1}{x_2}\right)^{a_x}, \tag{4a}$$

where, assuming that the same reinforcers and punishers are being used on the two alternatives, m_r = effective magnitude of reinforcement, m_p = effective magnitude of punishment, and each type of variable (i.e., reinforcement rate, punishment rate, reinforcement magnitude, and punishment magnitude) has its own exponent. Equations 4a can be further reduced by multiplying the numerator and denominator by $\frac{1}{m_r^{a_{m_r}}}$ (and substituting *b* for $\frac{x_1}{x_2}^{a_x}$) to obtain

$$\frac{B_1}{B_2} = b\frac{r_1^{a_r} - p_1^{a_p}\left(\frac{m_p^{a_{m_p}}}{m_r^{a_{m_r}}}\right)}{r_2^{a_r} - p_2^{a_p}\left(\frac{m_p^{a_{m_p}}}{m_r^{a_{m_r}}}\right)}. \tag{4a}$$

One can observe that the substitution of *c* for $\left(\frac{m_p^{a_{m_p}}}{m_r^{a_{m_r}}}\right)$ results in a "modernized" version of the direct-suppression model that incorporates *c*:

$$\frac{B_1}{B_2} = b\frac{r_1^{a_r} - cp_1^{a_p}}{r_2^{a_r} - cp_2^{a_p}}. \tag{5a}$$

If this theoretical derivation is correct, then *c* should be the perfect estimator of one's sensitivity to punishment. If *c* were equal to the ratio of the punishment and reinforcement magnitudes, then deviations from 1 would indicate differences in effective magnitude of the two stimuli. All previous estimates of *c* have been obtained by fitting Equations 1a (Farley & Fantino, 1978; de Villiers, 1980). The values of all estimates thus far have been below 1, indicating greater reinforcement magnitude compared to punishment magnitude. According to

PT's loss aversion, and general common sense, all *c* estimates should be above 1 (i.e., effective magnitude of punishment is greater than effective magnitude of reinforcement).

Three possible explanations exist for this discrepancy. First, it is possible that fitting Equation 1a, which does not incorporate important contemporary alterations to matching law equations (i.e., *b* and exponents), to the data is yielding poor estimates of *c*. Fitting an up-to-date version of the direct-suppression model (i.e., Equation 5a) may rectify this issue. Second, in past calculations of *c,* the reinforcing and punishing stimuli were qualitatively different (food pellets and shocks, respectively). Thus, if an extremely mild shock was used and the grain was very reinforcing to the bird, then it may have been that the pigeon's perception of reinforcement magnitude was truly greater than its subjective experience of the punisher's magnitude. Using objectively equivalent reinforcing and punishing stimuli (e.g., amount of money accrued as reinforcement is the same as the amount of money taken away as punishment) could potentially resolve this concern. Third, the direct-suppression model is generally touted as the superior model of contingent punishment. However, only one experiment (Critchfield et al., 2003) quantitatively compared the direct-suppression model to its main competitor, the competitive-suppression model (Deluty, 1976; Deluty & Church, 1978). Skepticism regarding the direct-suppression model's superiority has led researchers to call for a continuation of punishment-model development (e.g., Critchfield et al., 2003). New theories and models of punishment may provide a better explanation of behavior under punishing contingencies and, subsequently, better ways of estimating one's sensitivity to punishment.

Conceptualizing "Punishment Sensitivity" as "Sensitivity to Aversive Stimuli"

Rasmussen & Newland's (2008) approach to punishment sensitivity estimation relies heavily upon the assumption that loss aversion is due to an asymmetrical relationship between reinforcement and punishment. Instead, it may be the case that loss aversion is more closely related to an individuals' propensity to perform escape or avoidance behavior in the presence of an aversive stimulus (i.e., negative reinforcement). Miller's (1976) scaling procedure could be used to assess the difference between negative and positive reinforcement. Instead of different types of grain, researchers could establish negative and positive reinforcement contingencies on the two alternatives. One would assume that if *b* is encapsulating all aspects of reinforcing attributes other than rate, then fitting Equation 6 to the data would result in

$$b = \frac{Q_{neg}}{Q_{pos}}. \tag{7.3}$$

Unfortunately, experiments utilizing this type of experiment have found no systematic bias towards the alternative with negative reinforcement (Magoon & Critchfield, 2008; Ruddle, Bradshaw, Szabadi, 1981; Ruddle, Bradshaw, Szabadi, & Foster, 1982).

Past attempts at scaling negative and positive reinforcement have relied on the manipulation of reinforcement rate. Due to loss aversion being related to asymmetry in perceived magnitude of gains and losses, manipulating reinforcement magnitude may be more appropriate. Previous studies using varied magnitudes of positive reinforcement (Cording, McLean, & Grace, 2011; Landon, Davison, Elliffe, 2002) lend support to the reduction and transformation of Equation 4 into a generalized matching equation (Equation 6) with magnitudes as the sole predictor variable, rather than rate:

$$\log\left(\frac{B_1}{B_2}\right) = a_m \log\left(\frac{m_1}{m_2}\right) + \log(b). \tag{9}$$

Thus, modifying past experimental designs (e.g., Magoon & Critchfield, 2008; Ruddle et al., 1982) by keeping reinforcement rate constant and varying reinforcement magnitude may result in the systematic bias predicted by loss aversion.

Another possibility would be to run the same sets of concurrently-available reinforcement schedules with varied magnitudes for both positive and negative reinforcement. Then, by fitting Equation 9 to the two sets of data, one would have an estimate of a_m for both positive and negative reinforcement. Estimates of a values have been touted as approximate measures of how much "control" a variable has over choice (Landon, Davison, & Elliffe, 2002). Thus, a scaling of the two a_m values may lead to a good estimation of the differential impact of negative and positive reinforcement on behavior:

$$\lambda = \frac{a_{m1}}{a_{m2}}, \tag{10}$$

where a_{m1} and a_{m2} were equivalent to the a_m estimates calculated from fits of Equation 9 to data from the negative and positive reinforcement schedules, respectively.

Conclusion

Rasmussen & Newland's (2008) approach to scaling punishment and reinforcement is the first and only matching law-based approach to estimating punishment sensitivity. Ultimately, the attempts here to rectify methodological concerns did not alleviate the presented issues of validity. This result was not a complete surprise; without a foundational understanding of punishment as a whole, how are we to expect to parameterize one's sensitivity to it? Punishment theory and model development appear to be the best potential avenue of exploration.

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Appendix A

Instructions for Experiment 1

The following instructions for the “computer game” were given to every participant after he or she had finished the informed consent process:

> As discussed in the informed consent, thank you again for choosing to participate in this study. This task investigates how people behave in changing environments. Your goal is to earn as many points as possible. You can earn or lose points by pressing the space bar on the keyboard. The center button that says ‘START’ on it is currently grey, but it will change to either blue or yellow when the experiment begins. The rate at which you can gain or lose points is different depending on whether this button is blue or yellow. You can switch between blue or yellow by pressing the ‘ctrl’ button on the keyboard.
>
> Your mission is to figure out how to earn as many points as possible. Visual and auditory cues will help you with this task. Every time that you earn a point, the green light to the left of the center button will flash and a ‘ding’ will sound. Every time you lose a point, the red light to the right of the center button will flash and a ‘womp’ will sound.
>
> Today’s session will consist of three parts. You will receive a forced 90-second break between the sessions. You may take longer than the required time, if you wish. When you are finished taking your break, use the trackpad to left-click ‘Ok’ on the message prompt, and then left-click again on the screen to begin acquiring points again.
>
> Lastly, you’ll notice a leaderboard on the right-hand side of the screen. If you score enough points, then a three character expression of your choice will be placed on this board for other participants to see. Once you begin, please focus on the task and refrain from other activities, such as using your cell phone.
>
> Do you have any questions?

Instructions for Experiment 2

Participants who completed Experiment 1 came back to the laboratory to complete Experiment 2. Thus, the instructions for Experiment 2 were catered to the previous knowledge the participants had about the apparatus and study design:

> Similar to the last time you were here, your goal is to earn as many points as possible by using the keyboard. Pressing the space bar allows you to earn or loss points, while pressing the ‘ctrl’ key allows you to switch the center button between blue and yellow. Remember, the rate at which you gain or lose points is different depending on whether this button is blue or yellow.
>
> Visual and auditory cues will again help you with this task. In addition to the green and red lights, and the ‘ding’ and ‘womp’ sounds from last time, you can see that there are also lights at the top of the screen. As these lights change, the effects of blue and yellow also change.
>
> Today’s session will consist of two parts. You will have two 90 second breaks during each of these two parts. You may take longer than the required time, if you wish. When you are finished taking your break, use the trackpad to left-click ‘Ok’ on the message prompt, and then left-click again on the screen to begin acquiring points again. Once you have finished with part one, please alert me and I will set up part 2 for you. Upon completion of part 2, we will talk about the experiment and I will ask you to fill out a demographic information sheet.
>
> You’ll again notice a leaderboard on the right-hand side of the screen. If you score enough points, then a three character expression of your choice will be placed on this board for other participants to see. Once you begin, please focus on the task and refrain from other activities, such as using your cell phone.
>
> Do you have any questions?

Instructions for Experiment 3

The data from Experiment 3 were collected over two sessions. During the first session, participants first performed the “baseline” condition and were given the following instructions:

> As discussed in the informed consent, thank you again for choosing to participate in this study. This task investigates how people behave in changing environments. Your goal is to earn as many points as possible. You can earn points by pressing the space bar on the keyboard. The center button that says ‘START’ on it is currently grey, but it will change to either blue or yellow when the experiment begins. The rate at which you can gain points is different depending on whether this button is blue or yellow. You can switch between blue or yellow by pressing the ‘ctrl’ button on the keyboard.
>
> Your mission is to figure out how to earn as many points as possible. Visual and auditory cues will help you with this task. Every time that you earn a point, the green light to the left of the center button will flash and a ‘ding’ will sound. You

> will also notice lights at the top of the screen. As these lights change, the effects of blue and yellow also change.
>
> Today's session will consist of two parts. You will receive two, forced 90-second break during each session. You may take longer than the required time, if you wish. When you are finished taking your break, use the trackpad to left-click 'Ok' on the message prompt, and then left-click again on the screen to begin acquiring points again. When you are finished with this part, alert of your completion, and we will move on to the next part.
>
> Lastly, you'll notice a leaderboard on the right-hand side of the screen. If you score enough points, then a three character expression of your choice will be placed on this board for other participants to see. Once you begin, please focus on the task and refrain from other activities, such as using your cell phone.
>
> Do you have any questions?

Upon completing the first part, the experimenter set up Part 2, which ascertained data for another experiment not presented here. Instructions are based on the vernacular used by Bradshaw, Szabadi, & Bevan (1978), and were stated as follows:

> Thank you for completing the first part! We will call that part the 'good part.' We will now move onto the 'bad part.'
>
> In this part, in addition to gaining you points, sometimes pressing the space bar will make you lose points. Every time you lose a point, the red light to the right of the center button will flash and a 'womp' will sound. As with the last part, earning points will be associated with the green light flashing and the 'ding' sound.
>
> You will again receive two, forced 90-second break during each session. You may take longer than the required time, if you wish. When you are finished taking your break, use the trackpad to left-click 'Ok' on the message prompt, and then left-click again on the screen to begin acquiring points again. When you are finished this part, then you have finished today's portion of the study.

Participants returned to the laboratory for the second session, and performed the "punished" conditions. They were read the following instructions:

> Similar to the last time you were here, your goal is to earn as many points as possible by using the keyboard. Pressing the space bar allows you to earn or loss

points, while pressing the ‘ctrl’ key allows you to switch the center button between blue and yellow. Remember, the rate at which you gain or lose points is different depending on whether this button is blue or yellow.

Visual and auditory cues will again help you with this task. The green and red lights, the ‘ding’ and ‘womp’ sounds, and the lights at the top of the screen are all present during this session, just like the last one.

Today’s session will consist of one, very long ‘bad’ part. You will have four 90 second breaks during the session. You may take longer than the required time, if you wish. When you are finished taking your break, use the trackpad to left-click “Ok” on the message prompt, and then left-click again on the screen to begin acquiring points again. Once you have finished, we will talk about the experiment.

You’ll again notice a leaderboard on the right-hand side of the screen. If you score enough points, then a three character expression of your choice will be placed on this board for other participants to see. Once you begin, please focus on the task and refrain from other activities, such as using your phone.

Do you have any questions?

Tables

Table 1. Experimental design of Experiment 1

	Reinforcement RI Value		*Punishment RI Value*			
Block	*r_1 (s)*	*r_2 (s)*	*p_1 (s)*	*p_2 (s)*	*Duration (s)*	*Presentation*
1	1.0	1.0	-	-	200	Always First
	1.0	1.0	-	2.0	200	Random Order
	1.0	1.0	-	1.25	200	
		Break			90	
2	2.0	2.0	-	-	200	Always First
	2.0	2.0	4.0	-	200	Random Order
	2.0	2.0	2.5	-	200	
		Break			90	
3	3.0	3.0	-	-	200	Always First
	3.0	3.0	-	6.0	200	Random Order
	3.0	3.0	-	3.75	200	

Note. Half of the participants experienced two blocks of superimposed punishment on alternative 1, while the other half experienced two blocks of superimposed punishment on alternative 2 (i.e., p_1 and p_2 were alternated).

Table 2. Experimental design of Experiment 2

Condition	*Reinforcement RI Value*		*Punishment RI Value*			
Block	*r_1 (s)*	*r_2 (s)*	*p_1 (s)*	*p_2 (s)*	*Duration (s)*	*Presentation*
Baseline						
Acquisition	0.7	0.7	-	-	60	Always First
1	1.0	3.0	-	-	200	
	1.25	2.75	-	-	200	
	1.5	2.5	-	-	200	
	Break				90	
2	1.75	2.25	-	-	200	Random Order
	2.0	2.0	-	-	200	
	2.25	1.75	-	-	200	
	Break				90	
3	2.5	1.5	-	-	200	
	2.75	1.25	-	-	200	
	3.0	1.0	-	-	200	
Extinction	-	-	-	-	30	Always Last
Punished						
Acquisition	0.7	0.7	-	-	60	Always First
1	1.0	3.0	1.5	-	200	
	1.25	2.75	1.875	-	200	Random Order
	1.5	2.5	2.25	-	200	
	Break				90	
2	1.75	2.25	2.625	-	200	

	2.0	2.0	3.0	-	200	
	2.25	1.75	3.375	-	200	
	Break				90	
3	2.5	1.5	3.75	-	200	
	2.75	1.25	4.125	-	200	
	3.0	1.0	4.5	-	200	
Extinction	-	-	-	-	30	Always Last

Note. For punished conditions, half of the participants experienced superimposed punishment on alternative 1, while the other half experienced superimposed punishment on alternative 2 (i.e., p_1 and p_2 were alternated).

Table 3. Experimental design of Experiment 3

Condition	*Reinforcement RI Value*		*Punishment RI Value*			
Block	r_1 *(s)*	r_2 *(s)*	p_1 *(s)*	p_2 *(s)*	*Duration (s)*	*Presentation*
Baseline						
Acquisition	0.7	0.7	-	-	60	Always First
1	1.0	3.0	-	-	200	
	1.25	2.75	-	-	200	
	1.5	2.5	-	-	200	
	Break				90	
2	1.75	2.25	-	-	200	
	2.0	2.0	-	-	200	Random Order
	2.25	1.75	-	-	200	
	Break				90	
3	2.5	1.5	-	-	200	
	2.75	1.25	-	-	200	
	3.0	1.0	-	-	200	
Extinction	-	-	-	-	30	Always Last
Punished						
Acquisition	0.7	0.7	-	-	60	Always First
1	1.0	3.0	1.5	-	200	
	1.25	2.75	1.875	-	200	
	1.5	2.5	2.25	-	200	Random Order
	Break				90	
2	1.75	2.25	2.625	-	200	
	2.0	2.0	3.0	-	200	

	2.25	1.75	3.375	-	200	
	Break				90	
3	2.5	1.5	3.75	-	200	
	2.75	1.25	4.125	-	200	
	3.0	1.0	4.5	-	200	
	Break				90	
4	1.0	3.0	-	1.5	200	
	1.25	2.75	-	1.875	200	
	1.5	2.5	-	2.25	200	
	Break				90	
5	1.75	2.25	-	2.625	200	
	2.0	2.0	-	3.0	200	
	2.25	1.75	-	3.375	200	
	Break				90	
6	2.5	1.5	-	3.75	200	
	2.75	1.25	-	4.125	200	
	3.0	1.0	-	4.5	200	
Extinction	-	-	-	-	30	Always Last

Table 4. Fits of Equation 6 to participant data, Experiment 2

Participant				
Condition	*a*	*b*	*%VAF*	*Analyzable Schedules*
S028				
Baseline	-	-	-	3
Punished	-	-	-	0
S029				
Baseline	0.78	1.14	84.1	6
Punished	0.73	0.37	77.8	6
S030				
Baseline	-	-	-	3
Punished	-	-	-	2
S031				
Baseline	-	-	-	3
Punished	-	-	-	2
S031				
Baseline	0.89	1.27	91.2	7
Punished	-	-	-	1
S032				
Baseline	0.70	0.80	82.5	8
Punished	0.65	0.26	28	5
S034				
Baseline	-	-	-	4
Punished	-	-	-	3
S035				

Baseline	0.74	1.15	91.9	9
Punished	0.65	0.26	63.2	5
S038				
Baseline	-	-	-	2
Punished	-	-	-	2
S039				
Baseline	0.42	1.05	83.4	7
Punished	0.73	0.56	39.6	6
S040				
Baseline	0.66	1.17	96.6	8
Punished	0.45	0.10	73.7	6
S041				
Baseline	0.80	1.10	99.1	7
Punished	0.14	0.03	4	6
S043				
Baseline	-	-	-	1
Punished	-	-	-	2
S044				
Baseline	0.83	0.94	97.9	9
Punished	0.73	0.33	98.3	6
S046				
Baseline	0.59	1.02	91	9
Punished	0.73	0.36	86.9	8
S048				
Baseline	0.63	0.94	97.8	6

Punished	-	-	-	3
S049				
Baseline	0.71	1.01	98.8	8
Punished	0.68	0.28	75.7	6
S050				
Baseline	-	-	-	2
Punished	-	-	-	0

Table 5. Fits of Equation 6 to participant data, Experiment 3

Participant Condition	*a*	*b*	*%VAF*	*Analyzable Schedules*
S056				
Baseline	0.54	0.90	44.9	6
Punished (Side 1)	1.03	1.31	98.7	6
Punished (Side 2)	0.52	0.33	58.1	6
S057				
Baseline	0.72	1.07	95.7	9
Punished (Side 1)	0.79	9.56	95.7	7
Punished (Side 2)	1.19	1.57	46.7	5
S058				
Baseline	0.68	1.02	99.3	9
Punished (Side 1)	-0.11	0.01	0.8	7
Punished (Side 2)	0.37	0.06	22.2	5
S060				
Baseline	0.64	1.12	37.7	8
Punished (Side 1)	0.72	0.37	93.8	6
Punished (Side 2)	-	-	-	3
S061				
Baseline	0.65	1.07	97.7	8
Punished (Side 1)	0.37	0.09	36.7	5
Punished (Side 2)	-	-	-	4
S063				
Baseline	0.67	0.94	98.1	8

Punished (Side 1)	0.64	0.33	64	9
Punished (Side 2)	0.60	0.23	60.6	7
S064				
Baseline	0.71	0.90	96	9
Punished (Side 1)	0.60	0.24	68.9	7
Punished (Side 2)	0.26	0.08	44.8	6
S065				
Baseline	0.65	1.06	97.3	8
Punished (Side 1)	0.98	0.98	67	5
Punished (Side 2)	0.03	0.01	0.01	5
S066				
Baseline	0.59	0.96	82	5
Punished (Side 1)	0.41	0.08	21.8	5
Punished (Side 2)	-	-	-	2
S067				
Baseline	0.33	1.03	90.1	9
Punished (Side 1)	2.06	27.9	41.5	7
Punished (Side 2)	0.94	0.95	71.5	8
S068				
Baseline	0.71	1.01	90.1	7
Punished (Side 1)	-	-	-	4
Punished (Side 2)	-	-	-	3
S069				
Baseline	0.12	0.94	49.5	8
Punished (Side 1)	0.84	0.78	78.4	9

Punished (Side 2)	0.48	0.25	45.2	9
S070				
Baseline	0.63	1.14	93.8	9
Punished (Side 1)	0.79	0.67	82.8	9
Punished (Side 2)	1.00	1.13	54.7	9
S071				
Baseline	0.56	0.93	98.0	9
Punished (Side 1)	1.01	0.80	70.5	6
Punished (Side 2)	0.89	0.58	85.7	6
S072				
Baseline	0.56	0.93	98.0	9
Punished (Side 1)	0.45	0.48	86.0	8
Punished (Side 2)	0.70	0.52	73.5	9
S073				
Baseline	0.82	1.36	82.6	8
Punished (Side 1)	1.29	1.50	86.1	5
Punished (Side 2)	1.37	3.82	96.8	6

Figure Captions

Figure 1. The design of the apparatus used for these experiments draws heavily from the subjects' response panel used by Bradshaw, Szabadi & Bevan (1976) during their human operant studies of the late 1970s. 1) Each schedule was associated with a discriminative stimulus (white light). During the acquisition phase, which aims to acquaint the participant to the task by presenting reinforcement schedules with RI means of 0.7 seconds (as in Popa, 2013), all lights were illuminated. 2) An odometer-styled counter holds the total points accrued by the participant during the session. 3) Green and red lights were briefly illuminated when reward and punishment were delivered. Auditory cues, onomatopoeically defined as "ding" and "womp," were associated with reward and punishment, respectively. 4) The larger button is the operandum and is operated by pressing the "space" bar on the computer keyboard. It is initially colored grey with the word "START" on it, changes between blue (alternative 1) or yellow (alternative 2) with the activation of the Findley (1958) key. 5) The aforementioned Findley (1958) key is activated by pressing the "ctrl" button.

Figure 2. Mean change in behavior on the unpunished (top panel) and punished (bottom panel) alternatives in each block of Experiment 1. Black bars represent the P50 conditions, where the RI value of the superimposed punishment schedule was twice that of the RI value of reinforcement schedule, and the P85 conditions, where the RI value of the superimposed punishment schedule was 1.25 times greater than the RI value of the reinforcement schedule.

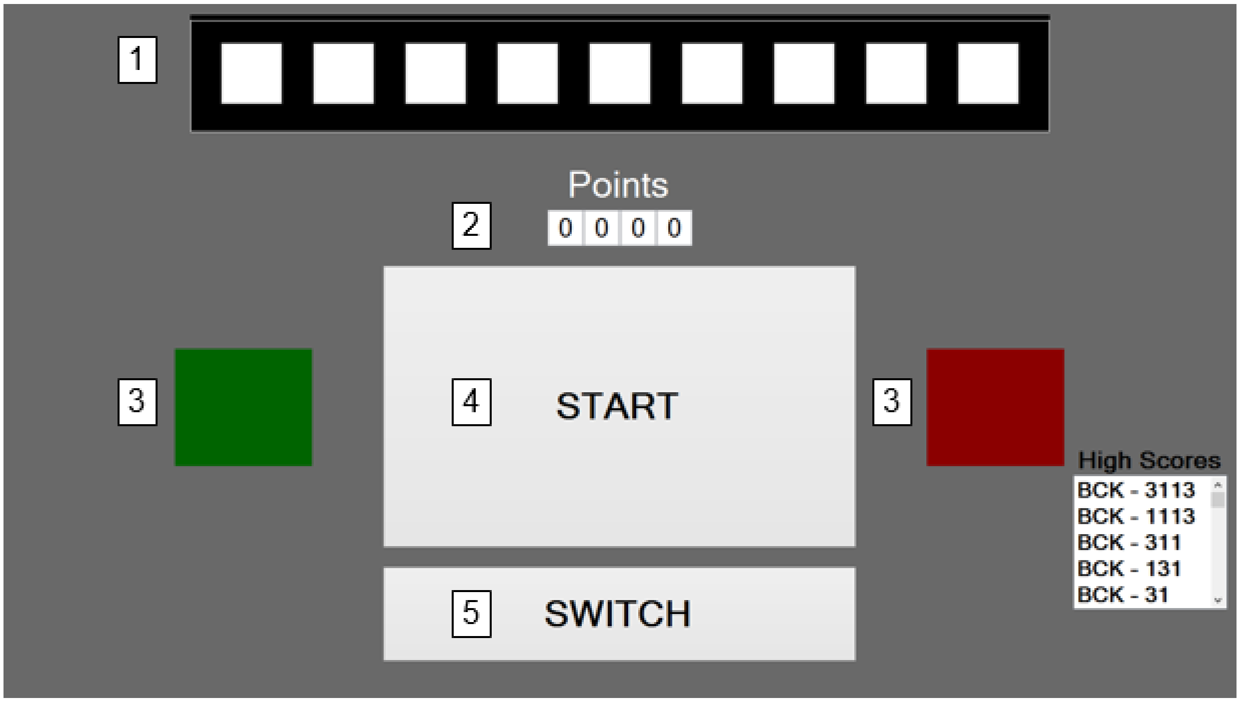
Figure 3. Box-and-whisker plots of the number of analyzable schedules in the baseline (left) and punished (right) conditions (Experiment 2).

Figure 4. Box-and-whisker plots of the %VAF values for fits of Equation 6 to the data acquired from baseline (left) and punished (right) conditions (Experiment 2).

Figure 5. Box-and-whisker plots of the number of analyzable schedules in the baseline (left) and punished (right) conditions (Experiment 3).

Figure 6. Box-and-whisker plots of the %VAF values for fits of Equation 6 to the data acquired from baseline (left) and punished (right) conditions (Experiment 3).

Figure 1 Klapes



Klapes Figure 2

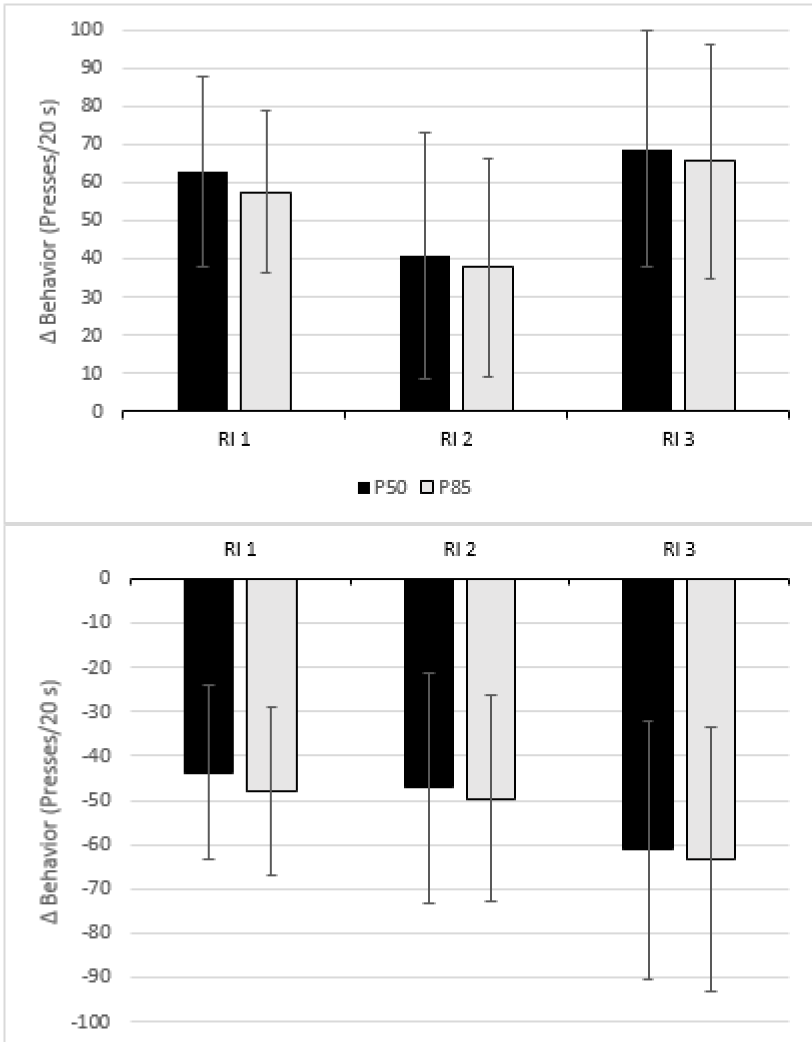


Figure 3 Klapes

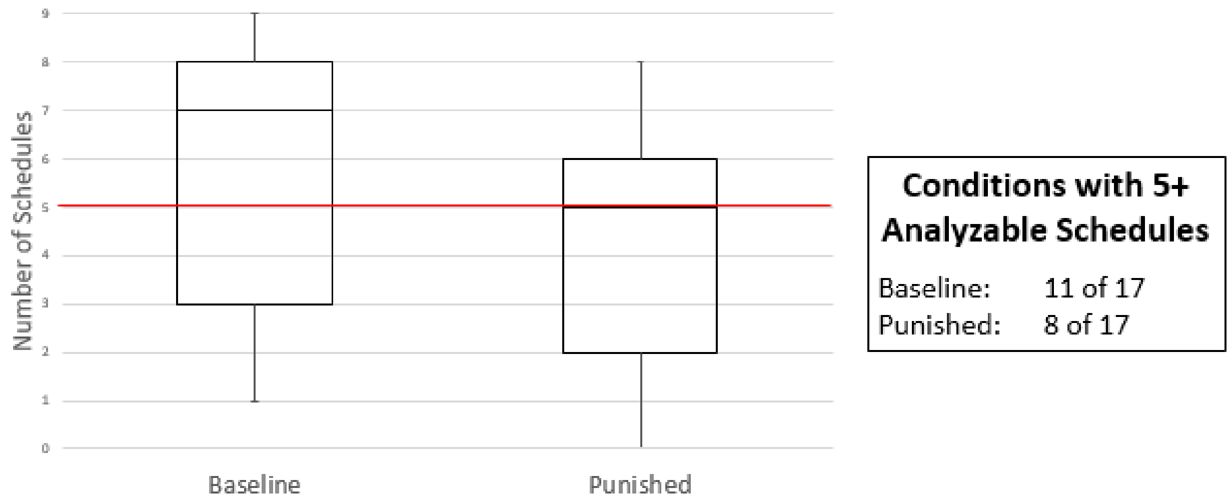


Figure 4 Klapes

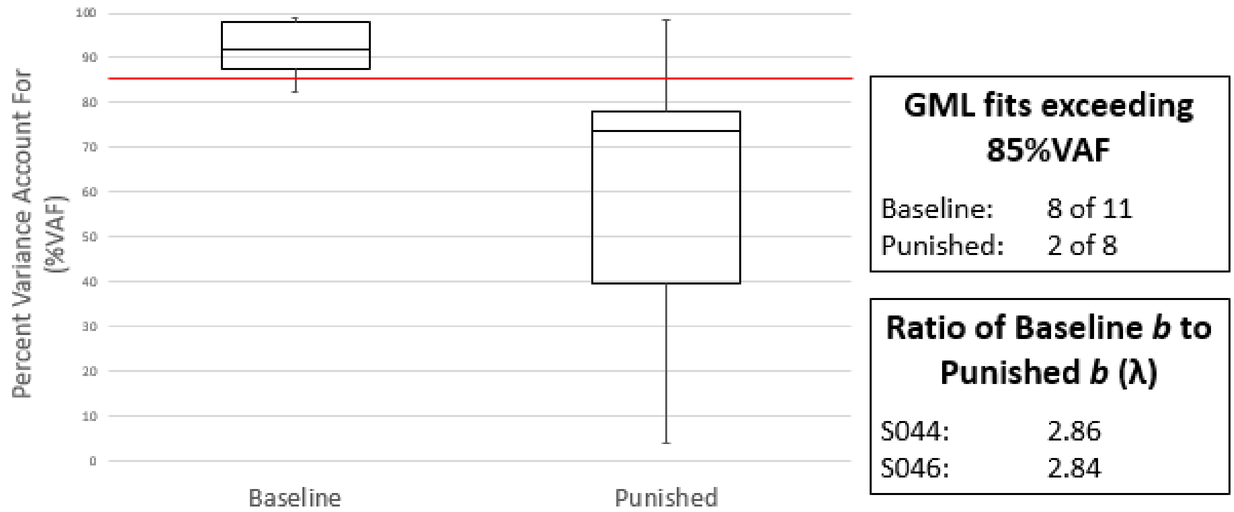


Figure 5 Klapes

Figure 6 Klapes