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PUNISHMENT IN HUMAN CHOICE: DIRECT OR COMPETITIVE SUPPRESSION?

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This investigation compared the predictions of two models describing the integration of reinforcement and punishment effects in operant choice. Deluty's (1976) competitive-suppression model (conceptually related to two-factor punishment theories) and de Villiers' (1980) direct-suppression model (conceptually related to one-factor punishment theories) have been tested previously in nonhumans but not at the individual level in humans. Mouse clicking by college students was maintained in a two-alternative concurrent schedule of variable-interval money reinforcement. Punishment consisted of variable-interval money losses. Experiment 1 verified that money loss was an effective punisher in this context. Experiment 2 consisted of qualitative model comparisons similar to those used in previous studies involving nonhumans. Following a no-punishment baseline, punishment was superimposed upon both response alternatives. Under schedule values for which the direct-suppression model, but not the competitive-suppression model, predicted distinct shifts from baseline performance, or vice versa, 12 of 14 individual-subject functions, generated by 7 subjects, supported the direct-suppression model. When the punishment models were converted to the form of the generalized matching law, least-squares linear regression fits for a direct-suppression model were superior to those of a competitive-suppression model for 6 of 7 subjects. In Experiment 3, a more thorough quantitative test of the modified models, fits for a direct-suppression model were superior in 11 of 13 cases. These results correspond well to those of investigations conducted with nonhumans and provide the first individual-subject evidence that a direct-suppression model, evaluated both qualitatively and quantitatively, describes human punishment better than a competitive-suppression model. We discuss implications for developing better punishment models and future investigations of punishment in human choice.

Key words: punishment, concurrent schedules, one factor theory, two factor theory, mouse click, humans

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There is widespread agreement in the social sciences that behavior is influenced by its benefits and costs (e.g., Akers, 1994; Davison, 1991; Ehrlich, 1996; Eysenck, 1967; Gray, Stafford, & Tallman, 1991; Kahneman & Tversky, 1979; Leung, 1995; Lohman, 1997; Neilson, 1998), but the specifics of the interaction are rarely addressed with much precision. Within operant psychology, it is axiomatic to assume that behavior is jointly determined by reinforcement, which increases behavior frequency, and punishment, which decreases behavior frequency, but exactly how the two combine to influence behavior output often remains unstated (e.g., Skinner, 1953; see also Lerman & Vorndran, 2002).

Explicit attention to the reinforcement-punishment interaction can be found in two variations on Herrnstein's (1970) matching law

$$\frac{B_x}{B_x + B_y} = \frac{R_X}{R_x + R_y} \tag{1}$$

in which the B terms refer to response rates for concurrently-available response options x and y, and the R terms refer to rates of reinforcement for those alternatives. In the tradition of single-factor theories of punishment

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(e.g., Rachlin & Herrnstein, 1969; Thorndike, 1911), de Villiers (1980) proposed that punishers (P terms below) directly reduce the strength of reinforced responding:

$$\frac{B_x}{B_x + B_y} = \frac{(R_X - P_x)}{(R_x - P_x) + (R_y - P_y)}. \quad (2)$$

The de Villiers model, thus, can be termed a *direct-suppression* model.

By contrast, Deluty (1976) proposed a model that, in the tradition of two-factor theories of punishment (e.g., Bolles, 1967; Dinsmoor, 1954; Estes, 1944; Mowrer, 1947; Rescorla & Solomon, 1967), assumes that punishment of one behavior increases the relative value of reinforcement for other behaviors. Deluty's model, which can be termed a *competitive-suppression* model, assumes that punishers for alternative x supplement the reinforcement rate for alternative y, and vice versa:

$$\frac{B_x}{B_x + B_y} = \frac{(R_X + P_y)}{(R_x + P_y) + (R_y + P_x)}. \quad (3)$$

Only a few experiments have compared the predictions of these models. Before examining those experiments, it is important to note that neither Equation 2 nor Equation 3 is based on the contemporary standard for describing concurrent schedule performance, the generalized matching law (Baum, 1974, 1979), in which relative behavior and consequence rates are expressed as logarithmically-transformed ratios rather than as proportions and are modulated by two fitted parameters. The generalized matching law is superior to Equation 1 in accounting for systematic deviations from strict matching (predominantly undermatching; e.g., Baum, 1979; Kollins, Newland, & Critchfield, 1997), but it creates certain ambiguities for punishment models that we will address later in this report.

Because models based on Equation 1 do not accommodate the systematic deviations from matching that characterize most concurrent schedule performances, Equations 2 and 3 can be compared in terms of qualitative (directional), but not quantitative (point), predictions. de Villiers (1980, 1982) described an approach in which the logic of mathematical inequalities is applied to yield qualitative predictions. Consider a case in which the same rate of punishment is applied to two response alternatives, x and y, with unequal reinforcement rates (i.e., $R_x > R_y$). According to Equation 2, R_y is discounted proportionally more than R_x, leading to increased preference for R_x. According to Equation 3, R_x and R_y are augmented equally in absolute terms, thus becoming more similar than during baseline, which leads to reduced preference for R_x. In this fashion, therefore, the models can be compared without reference to point predictions.

Only two investigations (de Villiers, 1980; Farley, 1980) have unambiguously compared the models represented by Equations 2 and 3. The limited evidence from these studies, evaluated using de Villiers' (1980, 1982) qualitative approach, strongly favors the direct-suppression model of Equation 2. Only Deluty (1976, 1982; see also Deluty & Church, 1978) has claimed empirical support for the competitive-suppression model, but without directly comparing models. In fact, Equations 2 and 3 make nearly identical predictions under the schedule values employed by Deluty (e.g., de Villiers, 1980). Thus the Deluty studies do not provide a meaningful test of the models.

All of the aforementioned data were obtained from nonhumans, and it is important to determine the extent to which animal-based principles apply to human behavior. For example, it is interesting to note that Skinner's (e.g., 1953) influential writings about punishment in human affairs appear to endorse a competitive-suppression perspective. Another reason to be interested in human outcomes concerns a limitation of studies with nonhumans. Equations 2 and 3 assign equal weights to reinforcers and punishers, implying that reinforcers and punishers have equal impact upon behavior, but this is a dubious assumption when qualitatively different events are employed as reinforcers (e.g., food) and punishers (e.g., electric shock). In studies with nonhumans, therefore, the predictions of the two models are blurred by unknown functional magnitudes of the consequences (Farley & Fantino, 1978). In human studies, however, it is possible to program money-gain reinforcers and money-loss punishers at values nominally consistent with the equal-magnitude assumptions of Equations 2 and 3.

Apparently only one published study

Table 1

Session duration, changeover delay (COD), and details of compensation in Experiments 2 and 3; see text for additional information about money contingencies. Values varied across subjects in Experiment 1 (see Appendix A). Base pay refers to payment for attending experimental sessions.

Experiment	Part	Session (min)	COD (s)	Base pay per hour	Consequences (cents) Reinforcers	Punishers
2	A	10	0.5	$1.50	3	3
	B	10	0.5	$1.50	3	3
	C	8	2.0	—	7	7
3	A	8	2.0	—	8	8
	B	15	0.5	$1.50	2	2

(Bradshaw, Szabadi, & Bevan, 1979) has examined the effects of punishment in human free-operant choice. Variable-ratio money-loss punishment was superimposed upon one response option in a concurrent variable-interval (VI) VI schedule of money-gain reinforcement, but models like Equations 2 and 3 were not applied to the data, and the published report lacks information (e.g., obtained rates of consequences) necessary to reanalyze the results in these terms. An additional study employed punishment in a group-comparison design involving discrete-trial choice procedures (Gray et al., 1991). Subjects participated for 50 trials in one of several conditions (N = 5 per group) across which the probability and magnitude of reinforcement and punishment were varied. Because each individual participated in only one condition, models similar to Equations 2 and 3 were fitted to group-aggregate functions. A variant of the direct-suppression model provided a better fit to the group functions than a variant of the competitive-suppression model. Whether the same would be true for individual functions is not known.

It remains to be determined, therefore, whether animal-based models of punishment in choice adequately describe individual human behavior. The present investigation sought to generate new human data relevant to direct-suppression and competitive-suppression punishment models, and was designed to incorporate parallels with the procedures of studies conducted with nonhumans. In particular, the present investigation employed free-operant procedures (unlike Gray et al., 1991), a changeover delay that applied to reinforcement and punishment schedules alike (unlike Bradshaw et al., 1979, and Gray et al.), and VI punishment (unlike Bradshaw et al.). The present study retained one important feature of previous human studies by using money-based reinforcers and punishers of equal magnitude.

The general experimental strategy was as follows. In the first experiment, a brief manipulation check was conducted to verify that that point loss functioned as punishment. Experiment 2 compared Equations 2 and 3 using the qualitative approach of de Villiers (1980, 1982), and incorporated a first attempt to compare direct-suppression and competitive-suppression models based on the generalized matching law. Experiment 3 generated data sets better suited to evaluating models based on the generalized matching law.

GENERAL METHOD

This research was conducted over approximately a 7-year period at two institutions, resulting in two types of procedural differences across studies that are summarized in Table 1. First, session durations became shorter across experiments (the studies were not conducted in the order reported here) as we learned that workable data could be obtained in briefer observation periods. Second, details of subject compensation varied according to the dictates of local institutional review boards and local economies. In particular, the value (in cents) of reinforcers and punishers varied across studies to assure that total earnings approximated the federal minimum wage.

Subjects and Apparatus

Undergraduate students (subjects numbered 500 and above at Illinois State University, the rest at Auburn University) volunteered for research on ''Choice and Problem-Solving.'' Potential volunteers were initially contacted by telephone after responding to recruitment flyers. They provided informed consent after visiting the laboratory and completing a brief sample session with the experimental task. Subjects agreed to participate for a minimum of 20 hr and a maximum of 40 hr. The actual duration of each subject's participation was influenced by how quickly stable data were obtained during the experimental sessions and by the vagaries of participant and university schedules.

Subjects were asked to leave personal belongings such as watches, calculators, and backpacks outside the workroom during experimental sessions. Each subject worked alone in an office-sized room containing a chair, a table supporting a 14-in. (35.56-cm) computer monitor on which stimuli were presented, and a computer mouse that was used to register responses. An IBM®-compatible computer in an adjacent room controlled experimental events and collected the data via a custom program written in QuickBasic©.

Procedure

Typically, subjects visited the laboratory 3 to 5 days per week and completed between 6 and 12 sessions per visit. For session durations see Table 1 and Appendix A. Subjects could take breaks between sessions.

Experimental task. The concurrent-schedules task was based on that of Madden and Perone (1999). Sessions began with the display of a message stating, ''Click here to begin.'' Clicking the message caused it to be replaced by two white rectangles separated by a thin black line. Each rectangle occupied approximately one half of the screen except for the top 2.5 cm, which remained black. An arrow-shaped cursor indicated the virtual position of the mouse at all times during the session. In the center of each white rectangle was a colored target approximately 0.5 cm square. Clicking either target once set both targets into motion. Targets moved about 0.5 cm per second in a randomly-determined direction. Clicks within the borders of a target registered responses upon which the reinforcement and punishment schedules were based. Clicks elsewhere were ineffective and were not counted.

Normally, a money counter, located in the center of the top, black region of the screen, displayed total session earnings in numerals about 1 cm high. To the left and right of this central counter were additional counters, in numerals about 0.5 cm high, displaying money outcomes specific to schedules operating in the two white regions. On each side, counters were labeled, "Money earned this side = __"; and ''Money lost this side = __.'' All counters registered zero at the start of each session, and the appropriate counters incremented or decremented with the occurrence of each reinforcer or punisher. The omission of the money counters served as the experimental manipulation in a portion of Experiment 2.

Schedules and consequences. Reinforcers (money gains) and punishers (money losses) occurred according to independent VI schedules arranged using constant-probability distributions (Fleshler & Hoffman, 1962). Within a condition, reinforcement schedules made available approximately 360 total reinforcers per hour of session time, based on programmed schedule values. The aggregate punishment rate depended on the design of the individual experiments.

Within a screen location (side), reinforcement and punishment VI schedules operated conjointly. Across screen locations, schedules operated concurrently. A changeover delay (COD) precluded the adventitious reinforcement or punishment of switching sides. During the COD, the VI schedules and the session clock were suspended, and responses, although recorded, were ineffective. The COD was as specified in Table 1, with one exception. For S272 (Experiment 2, Parts A and B), preference for the richer of two VI schedules was not evident during the early stages of the initial experimental condition. Consequently, the COD was gradually increased to 6 s until preference became apparent, and thereafter the COD remained constant throughout this subject's participation.

If a reinforcer or punisher became available on one side while responding took place on the other, the relevant VI timer was suspended until a changeover occurred and the COD was completed. Thereafter, the first response produced the consequence and restarted the VI timer. If a reinforcer and punisher both became available on one side while responding took place on the other side, the consequence that became available first was delivered contingent upon the first response after a changeover and COD. The second consequence was delivered contingent upon the next response on the same side. If a reinforcer and punisher for one response option became available simultaneously, the order of delivery was determined randomly.

Reinforcers and punishers were signaled by a 1-s flashing alternation (0.25 s per flash) of the most-recently clicked target with a message indicating the amount of money gain (e.g., "+3¢," printed in black) or loss (e.g., "−3¢," printed in red). During feedback messages, the cursor disappeared from the screen, the VI and session clocks were suspended, and mouse clicks were ineffective.

Experimental designs. All experiments incorporated no-punishment baselines in which VI reinforcement schedules operated for both response alternatives. Associated with each baseline were at least two additional conditions in which punishment was superimposed upon both of the response alternatives.

Discriminative stimuli. The schedules assigned to each response alternative operated in a single screen location (left or right), as described above, and the moving target associated with each location was distinctly colored. Target colors remained constant within a condition. Across conditions, target colors were assigned, without replacement, from a pool incorporating 16 different hues, then were recycled as necessary to produce a unique pair of colors in all conditions.

Stability criteria. A condition was terminated when one of three criteria was met: (a) Visual inspection of graphed data revealed no systematic trends in either response or time allocation proportions, and, for both proportions, over four consecutive sessions, the difference in means between the first and second pair of sessions differed by no more than 10% of the four-session mean; (b) all response and time proportions in four consecutive sessions were less than 0.1 or greater than 0.9, suggesting floor or ceiling effects; or (c) stability was not achieved according to the above criteria within 15 sessions. In this last eventuality, the condition already in progress continued through the end of the day's visit to the laboratory, and a new condition began at the start of the next visit.

Instructions. The informed consent agreement stated that:

> The researchers hope to learn about how individuals make choices based on their experience in ambiguous situations. You will view information on a computer screen and make decisions by pressing buttons on a mouse. The decisions you make sometimes will result in money rewards or penalties. You will not be given extensive instructions but rather will be asked to learn from experience as you work.

At the start of the first session, subjects were told which mouse button to press and were given the following instructions:

> You will see that the screen is divided into two separate sections, one on the left and one on the right. Two different colored squares move about on the screen, but each will stay within its respective section. With the mouse, you may click either square as much or as little as you like. Money from both squares counts toward your overall earnings. The squares pay off differently. It is up to you to decide when, and how often, to click each square. Try to earn as much money as you can.

Data Reduction and Presentation

For all experiments, we present terminal data, defined as the mean of the final four sessions in a condition. Because response-allocation and time-allocation outcomes were quite similar, for economy of presentation, graphic displays and model tests focus on response allocation. Time allocation data are presented in the appendixes.

Model predictions were based on obtained rates of reinforcement and punishment for individual subjects. Occasionally, obtained punishment rates in a condition equaled or exceeded obtained reinforcement rates for a response option. Such outcomes create calculation problems for direct-suppression models (e.g., see Davison & McCarthy, 1988; Gray et al., 1991), a matter that we will address in the General Discussion. For analyses based on Equation 2 (see Experiment 1), high punishment rates may lead to predictions of preference greater than 1.0 or less than zero. In such cases, for purposes of graphic display, the prediction was considered to be the ceiling (1) or floor (0) of the measurement scale. In Experiments 2 and 3, punishment models were modified to take the form of the generalized matching law, which utilizes the natural logarithm of consequence and behavior ratios. In direct-suppression model analyses, high punishment rates may yield undefined predictions because only positive ratio values can be so transformed. Conditions in which punishment rate equaled or exceeded reinforcement rate were excluded from analyses based on the generalized matching law.

EXPERIMENT 1

This brief study served as a manipulation check for subsequent experiments. In particular, in Experiment 2 programmed rates of reinforcement and punishment were employed for which, according to de Villiers' (1980) qualitative approach to model comparison, only one punishment model predicted a distinct shift from baseline choice patterns. Thus the absence of a preference shift could reflect either a failure of the model under consideration or simply the use of ineffective punishers. Although the loss of points worth money has proven to be an effective consequence in other studies of aversive control, most such studies have involved negative reinforcement (Crosbie, 1998). Thus this study was undertaken to verify that money loss would serve as a punisher in the context of the present procedures.

Method

Eight individuals who also participated in Experiment 2 or 3 served as subjects. Session duration, changeover delay, and details of money contingencies are shown for each subject in Appendix A. The 6 subjects in Part A completed a no-punishment baseline followed by punishment of one response option. The baseline involved a 1:1 reinforcement ratio (VI 20 s VI 20 s), and VI 40-s punishment (50% of reinforcement rate) was applied to the response option that generated the higher response rate during baseline. The purpose of Part A was to verify that money loss reduced rates of the behavior on which it was contingent. The 2 subjects in Part B completed a VI 2-s VI 20-s baseline plus conditions in which VI 40-s punishment and VI 25-s punishment (50% and 80% of reinforcement rate, respectively) were applied to the preferred response option. The purpose of Part B was to verify that effects of punishment were frequency dependent.

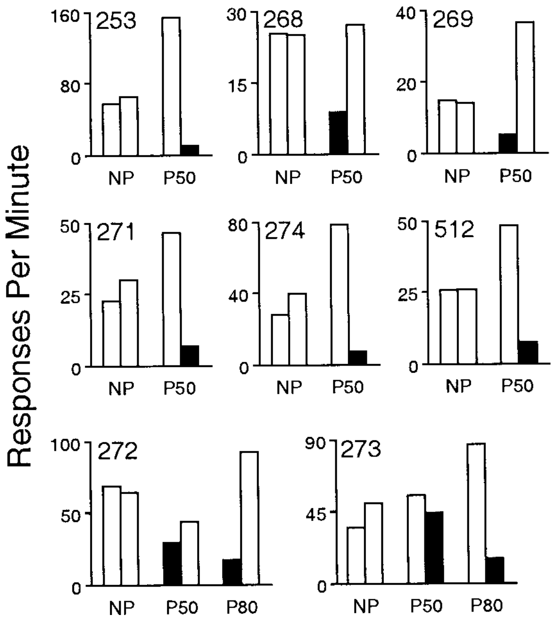

Fig. 1. Experiment 1: Response rates during a no-punishment baseline (NP) and with punishment (P) superimposed on one of the response options. Punishment rate was either 50% or 80% of reinforcement rate, based on programmed schedule values. The left and right bar of each adjacent pair represent the left and right response alternative, respectively. For punishment conditions, the punished alternative is shaded. Note that ordinate scaling differs across subjects.

Results and Discussion

Appendix A lists obtained rates of reinforcement and punishment. The top two rows of Figure 1 show response rates for the left and right screen locations (shown as adjacent bars) in the baseline and 50% punishment conditions of Part A. In all cases, money loss decreased response rate for the alternative on which it was contingent. The bottom row of Figure 1 shows analogous data for Part B. Again, punishment decreased response rates in all instances, and the magnitude of effect depended on punishment frequency. Across Parts A and B, in all but one instance (S272, 50% punishment), contingent money loss led to response rate increases for the unpunished alternative, replicating a common finding (e.g., Azrin & Holz, 1966; Bradshaw et al., 1979). Overall, the results of manipulation check indicated that response-contingent money-loss functioned as punishment in the context of the present procedures.

EXPERIMENT 2

This experiment promoted qualitative comparisons of the predictions of Equations 2 and 3. Part A was designed as a test of the direct-suppression model of Equation 2. Following a no-punishment baseline, punishment was superimposed on both response alternatives at a rate equal to 50% and 100% of the reinforcement rate of the leaner schedule. Under these conditions, Equation 2 predicts a preference shift toward the rich alternative, and the competitive-suppression model of Equation 3 predicts relatively little preference shift. Part B was designed as a test of the competitive-suppression model of Equation 3. Following a no-punishment baseline, punishment was superimposed on both response alternatives at a rate proportional to the programmed reinforcement rate. Under the programmed contingencies, Equation 3 predicts decreased preference for the reinforcement-rich alternative, while the direct-suppression model of Equation 2 predicts no change in preference

Part C was conducted in recognition of the fact that experimental procedures for humans are never identical to those for nonhumans (e.g., Baron, Perone, & Galizio, 1991). Unlike in nonhuman experiments, in Parts A and B money counters were displayed during experimental sessions, including a central counter showing net session earnings and pairs of counters showing money gained and money lost for each of the two response alternatives. We were concerned that by making aggregate gains and losses explicit, these counters would impose a subtractive logic that, although consistent with predictions of the direct-suppression model, would be idiosyncratic to this particular computer work environment (that is, discriminative control exerted by the counters might overwhelm control by the consequences). To determine whether money counters were integral to effects in Parts A and B, Part C replicated the schedule values of Part A in the presence and absence of money counters.

METHOD

Eight students volunteered to participate. One was dropped from the study after failing to show consistent preference for the richer source of reinforcement during 13 hr of exposure to a no-punishment baseline. No data are presented for this subject. Five subjects completed both Part A and Part B. For 2 of these subjects (S271 and S274), Part A came first, and for the other 3 (S269, S272, and S273), Part B came first. Two other subjects (S500 and S501) completed Part C. Table 2 shows the schedule values. The screen location to which the richer schedule was assigned was randomly determined for each condition.

Part A consisted of a no-punishment baseline (2:1 reinforcement ratio) and two conditions in which punishment was applied to both response options. Across conditions, punishment for both response options was programmed at a rate equal to 50% and 100% of the programmed reinforcement rate for the leaner reinforcement schedule. For example, given baseline reinforcement schedules of VI 15 s and VI 30 s, the 50% condition would yield punishment schedules of VI 60 s for both alternatives.

Part B consisted of a no-punishment baseline (5:1 reinforcement ratio) and three punishment conditions in which punishment was applied to both response options. Across conditions, punishment was programmed at a rate equal to 25%, 50%, and 75% of the reinforcement rate for each response option. For example, given baseline reinforcement schedules of VI 12 s and VI 60 s, the 50% condition would yield punishment schedules of VI 24 s and VI 120 s, respectively.

In Part C, the design and procedures of Part A were replicated; once with money counters present as during Part A, and once with the money counters absent. S500 completed the counter conditions first; S501 completed the no-counter conditions first. When money counters were absent, the top portion of the screen where counters normally would be displayed remained black. Prior to the first session of the no-counters phase, subjects were told, "The computer will indicate each time you gain or lose money. Your total earnings during the session will not be shown on your screen, although the computer will keep

Table 2

Experiments 2 and 3: Variable-interval schedule values (s). Note that in Experiment 3 each punishment condition was preceded by a baseline condition using the same reinforcement schedule values. Within experiments, the sequence of conditions varied across subjects (see Appendixes B and C).

			Reinforcement		Punishment	
Experiment	Part	Condition	Rich	Lean	Rich	Lean
2	A, C	2:1 baseline	15	30	—	—
		2:1 50% punishment	15	30	60	60
		2:1 100% punishment	15	30	30	30
	B	5:1 baseline	12	60	—	—
		5:1 25% punishment	12	60	48	240
		5:1 50% punishment	12	60	24	120
		5:1 75% punishment	12	60	16	80
3	A	2:1	15	30	85	85
		3:1	13	40	85	85
		4:1	12	50	85	85
		5:1	12	60	85	85
		7:1	11	80	85	85
	B	3:2	17	25	34	50
		2:1	15	30	30	60
		3:1	13	40	26	80
		4:1	12	50	24	100
		5:1	12	60	24	120
		9:1	11	100	22	200
		17:1	10	180	20	360

track of this. You may always ask how much money you have made after any session.'' Neither subject inquired about session totals. Prior to the first session of the counters phase, subjects were told, ''The computer will indicate each time you gain or lose money and display your total earnings during the session on your screen.''

Results

Following the lead of de Villiers (1980, 1982), emphasis was placed on evaluating the qualitative predictions of the models. For all subjects, response proportions during baseline were lower than predicted by Equation 1, suggesting the commonly-reported human tendency toward undermatching (Kollins et al., 1997). To facilitate visual comparison of predicted versus obtained response patterns, in Figures 2 and 3 the predictions of Equations 2 and 3 are plotted against the left ordinate of each panel, and obtained response proportions are plotted against the right ordinate, which is offset vertically to bring obtained baseline proportions into correspondence with model predictions. Appendix B shows the obtained reinforcement, punishment, and response rates upon which the analyses shown in Figures 2 and 3 were based.

For Part A, the test of the direct-suppression model, Figure 2 (top, right panel) shows the model predictions based on programmed schedule values. Visual inspection of the top five subject panels suggests that, for all subjects, as predicted by the direct-suppression model of Equation 2, rich-side preference increased as punishment rose from 50% to 100% of the lean-side reinforcement rate.

For Part B, the test of the competitive-suppression model, Figure 3 (top, right panel) shows the model predictions based on programmed schedule values. Visual inspection of the remainder of Figure 3 suggests that the performance of 3 of 5 subjects mirrored the predictions of the direct-suppression model. For S271, performance was roughly intermediate to prediction of the two models. For S269, based on programmed rates of consequences, both models predicted a punishment-related decrease in rich-side preference. These subjects may be viewed as uninformative for model-comparison purposes.

Figure 2 (bottom four panels) summarizes

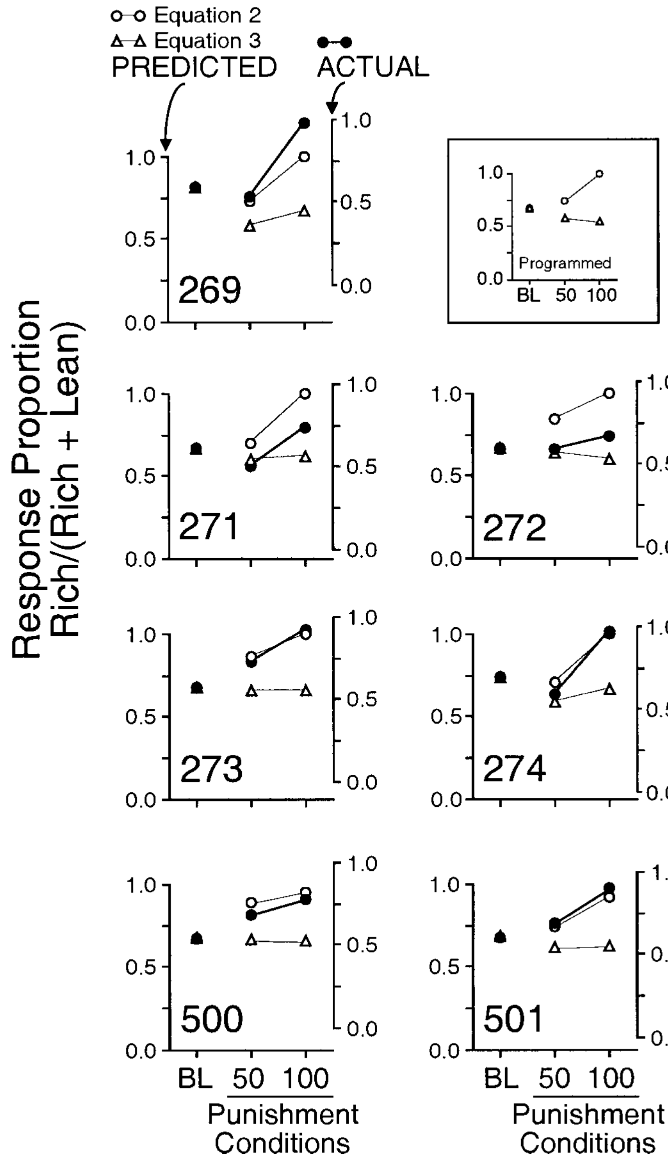

Fig. 2. Experiment 2, Parts A and C: Response proportions (right ordinate) and predictions of Equations 2 and 3 based on obtained rates of reinforcement and punishment (left ordinate) for individual subjects. Punishment rate for both response alternatives was a percentage of the lean-side reinforcement rate. Top, right panel: Predictions based on programmed rates of reinforcement and punishment.

the results from Part C, the counter test. Visual inspection suggests that, for both subjects, as predicted by the direct-suppression model, rich-side preference increased as punishment rose from 50% to 100% of the lean-side reinforcement rate. This was true regardless of the presence or absence of money counters. Although Part C involved only 2 subjects and a limited (A-B) experimental design, the data suggest that results of previous experiments were not an artifact of the screen display.

Discussion

Punishment was superimposed upon human concurrent schedule performance to produce 14 individual-subject functions potentially relevant to the predictions of the di-

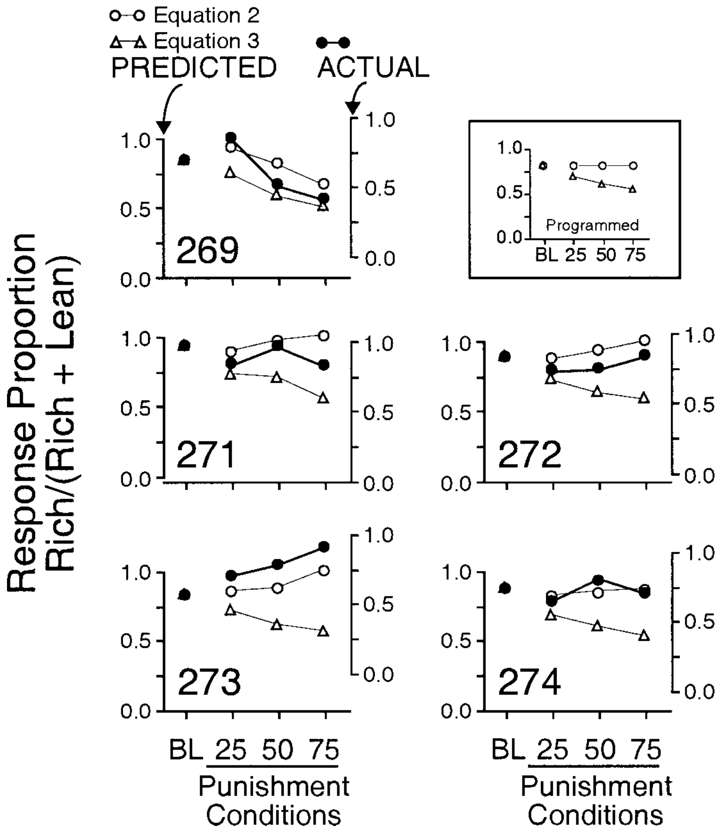

Fig. 3. Experiment 2, Part B: Response proportions (right ordinate) and predictions of Equations 2 and 3 based on obtained rates of reinforcement and punishment (left ordinate) for individual subjects. Punishment rate for each response alternative was a percentage of the reinforcement rate for that alternative. The top, right panel shows model predictions based on programmed rates of reinforcement and punishment.

rect-suppression (Equation 2) and competitive-suppression (Equation 3) punishment models. In each of these cases, based on programmed rates of reinforcement and punishment, one of the punishment models predicted a distinct shift in preference while the other predicted little or no change from baseline. Visual inspection revealed 12 outcomes that were consistent with Equation 2 predictions and no outcomes that were clearly consistent with Equation 3 predictions. Two cases were ambiguous with respect to model predictions.

These findings join with those of studies conducted with individual nonhumans in supporting the qualitative predictions of Equation 2 over those of Equation 3 (de Villiers, 1980; Farley, 1980). Some previous studies have applied punishment to human choice, but the punishment models under consideration here were tested with group-aggregate data (Gray et al., 1991) or were not evaluated at all (Bradshaw et al., 1979). The present investigation compared punishment models at the level of individual subjects and improved on previous studies by incorporating features that made the comparisons easier to interpret (free-operant procedures, COD, interval-schedule punishment). Overall, these results point to human punishment as a process involving direct suppression, as implied by one-factor punishment theories.

Like those from earlier studies of nonhumans, the present results must be interpreted cautiously because neither of the punishment models under consideration allowed formal consideration of deviations from perfect matching that occur routinely in all species (Baum, 1979; Kollins et al., 1997). The problem is evident in Figures 2 and 3 in which

Table 3

Experiment 2: Model parameters and percentage of variance accounted for (%VAC) in fitting Equations 4, 5, and 6 to data from punishment conditions. Data from Experiment 1 were included in the analysis when available.

	Equation 5			Equation 6			Equation 4		
Subject	*a*	log *b*	%VAC	*a*	log *b*	%VAC	*a*	log *b*	%VAC
269	.76	−.15	90.8	1.36	.06	82.5	.67	−.14	79.6
271	1.15	−.40	79.4	1.59	.12	24.2	1.18	−.14	83.3
272	.43	−.01	95.3	1.02	.06	92.2	.71	−.14	98.9
273	.91	−.21	85.7	1.08	.12	29.0	.27	.31	15.7
274	1.41	−.49	52.8	1.33	.14	53.3	.20	.37	1.5
500	.39	−.11	83.2	1.21	−.01	87.8	1.13	−.21	92.1
501	.75	.02	98.4	2.55	.07	82.6	.76	.22	90.5

ordinates were offset to adjust for baseline performances not precisely in accord with the predictions of perfect matching. Without this affordance, the putative direct-suppression effects of punishment would be difficult to discern through visual inspection.

The generalized matching law (Baum, 1974)

$$\log\left(\frac{B_x}{B_y}\right) = a \log\left(\frac{R_x}{R_y}\right) + \log b \qquad (4)$$

accounts for deviations from perfect matching through two fitted parameters: *a* (slope) serves as an estimate of sensitivity to different frequencies of reinforcement for the two response alternatives, and log *b* (intercept) serves as an estimate of bias for one response alternative. It is a simple matter to convert the punishment models to this form, yielding a direct-suppression model

$$\log\left(\frac{B_x}{B_y}\right) = a \log\left(\frac{R_x - P_x}{R_y - P_y}\right) + \log b \qquad (5)$$

and a competitive-suppression model

$$\log\left(\frac{B_x}{B_y}\right) = a \log\left(\frac{R_x + P_y}{R_y + P_x}\right) + \log b. \qquad (6)$$

Although models similar to Equation 5 have been proposed to account for travel costs in foraging analogs involving concurrent schedules (under the assumption that travel results in lost reinforcers; Baum, 1982, Davison, 1991), punishment effects on individual behavior apparently have not been evaluated using models based on the generalized matching law. At least three approaches to model comparisons can be imagined.

One method of evaluating punishment models would be to compare the percentage of variance accounted for (%VAC) when competing models are fit to data from punishment conditions (excluding baseline conditions). Unfortunately, as long as the functions from punishment conditions are linear when plotted on logarithmic coordinates, the generalized matching law (Equation 4) will describe the data well without reference to punishment, and it makes no sense to render punishment irrelevant in studies of punishment. Thus both Equations 5 and 6, that contain the same fitted parameters, would be expected to provide good fits to data from punishment conditions. Table 3 summarizes the least-squares linear-regression fits of Equations 4, 5, and 6 to the punishment-data conditions. As expected, all three models provided an acceptable account in the majority of cases, and the model providing the best fit varied across subjects.

A second approach is suggested by previous work on travel costs in foraging. Baum (1982) and Davison (1991; Davison & McCarthy, 1988) assumed that travel costs—explicitly equated with punishment by Baum—leave the fitted parameters of the generalized matching law unaltered. If the same can be assumed of punishment, and if the purpose of any punishment model is to account for the effects of reinforcement and punishment within a single mathematical expression, then Equations 5 and 6 may be compared in terms of their capacity to integrate the data from punishment and no-punishment conditions. To create Figure 4, baseline and punishment data for each of the 7 participants in both

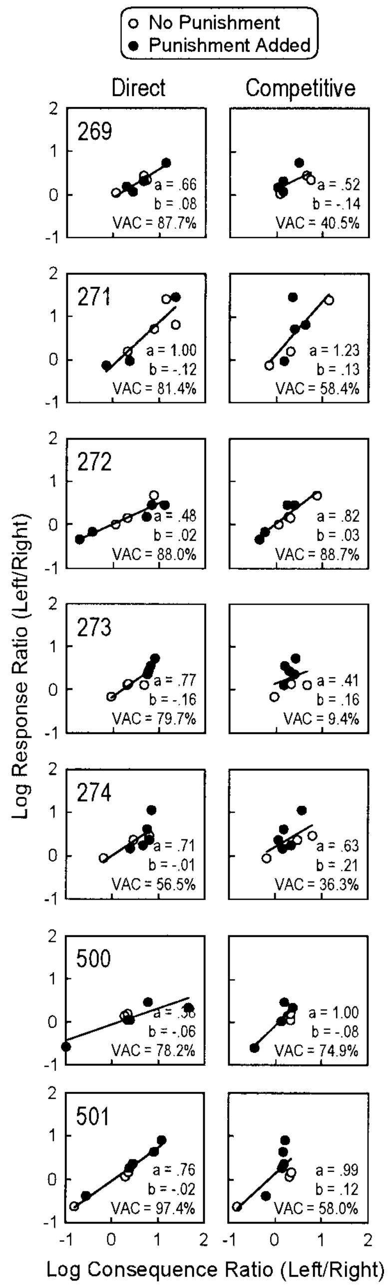

Fig. 4. Experiment 2: Relationship between log consequence ratio, based on Equations 5 and 6, and log response ratio for individual subjects. Note: *a* and log *b* are fitted parameters of the models. VAC = percentage of variance accounted for.

parts of Experiment 2 were pooled with data from Experiment 1 (if available). Figure 4 shows the least-squares linear-regression fits of Equations 5 and 6 to these data. In six of seven cases, the direct-suppression model of Equation 5 accounted for more variance than the competitive-suppression model of Equation 6, although the %VAC was modest in some cases. Overall, analyses based on the generalized matching law concurred with those based on a more qualitative approach.

A third, and more stringent, model test suggested by an anonymous reviewer and by M. Davison (personal communication, May 26, 2002) also assumes that punishment is inert with respect to the fitted parameters of the generalized matching law: Equation 4 may be fit to baseline (no-punishment) data and the resulting slope and bias parameters (*a* and log *b*, respectively) held constant as Equations 5 and 6 are fit to punishment data. For the present data set, this third approach was rejected on practical grounds, as it requires more baseline conditions than were included in the experimental design.

EXPERIMENT 3

Equation 2 was designed as a more thorough test of models in which subjects typically completed more conditions than in Experiment 1, including equal numbers of baseline and punishment conditions, thereby supporting a strategy of model comparison that could not be applied in the first experiment.

METHOD

Seventeen subjects participated, 6 in Part A and 11 in Part B (more subjects were included in Part B because the results were more variable across subjects). Two subjects withdrew from each part of the study before adequate data could be collected, citing boredom or schedule conflicts as the reason for withdrawing. Data are reported for the remaining 4 subjects in Part A and 9 subjects in Part B.

Subjects completed at least 4 two-condition phases, each consisting of a baseline (reinforcement-only) condition plus a punishment condition with identical reinforcement rates. The punishment contingencies of Part A were similar to those of Part A in Experiment 2 in that a constant rate of punishment was applied to both response alternatives across a range of relative reinforcement rates. The punishment contingencies of Part B were similar to those of Part B in Experiment 2 in

Table 4

Experiment 3: Model parameters and percentage of variance accounted for (%VAC) in fitting Equations 4, 5, 6, and 7 to data from punishment conditions. Analyses included subjects who completed at least four punishment conditions in which obtained reinforcement rate exceeded obtained punishment rate.

Subject	Equation 5			Equation 6			Equation 4			Equation 7		
	a	log *b*	%VAC	*a*	log *b*	%VAC	*a*	log *b*	%VAC	*a*	log *b*	%VAC
209	−.21	.04	44.1	−.90	.04	48.2	−.20	.03	42.7	.20	.01	41.4
243	−.19	.13	35.9	−.85	.14	36.6	−.23	.11	48.8	.25	.09	63.6
252	.24	−.06	84.0	1.06	−.04	90.0	.25	−.06	86.2	−.20	.02	67.8
253	1.44	−.11	97.8	6.23	−.04	96.8	1.25	−.07	98.7	−1.21	−.19	98.7
254	.40	.05	83.5	2.86	.04	97.8	.60	.07	95.1	−.70	.07	97.0
265	.64	.08	98.3	3.45	.01	95.1	.68	.05	97.8	−.71	.03	97.3
267	.99	.08	97.7	4.66	.12	94.1	.93	.13	99.0	−.71	−.05	96.3
512	.66	.06	95.2	1.62	.61	96.8	.87	.01	94.9	−1.70	−1.7	95.1
513	.66	.10	92.8	.32	.14	94.5	.74	.05	94.0	−5.87	−.05	97.1
514	.50	.13	64.2	1.09	.10	91.8	.85	.05	85.4	−8.60	−.11	90.9
515	.33	−.14	96.7	.75	−.18	98.6	.44	.14	95.9	−.73	−.27	50.1

that punishment proportional to the reinforcement rate was applied to both alternatives.

For each subject in both parts of the experiment, an attempt was made to complete approximately the same number of phases in which the left and right response options were more frequently reinforced. The number of phases completed varied across subjects depending on individual availability and limitations of academic schedules. Reinforcement and punishment schedule values are shown in Table 2, and the sequence of conditions completed by each individual is shown in Appendix C.

Results and Discussion

Appendix C shows the data on which the three types of model comparisons discussed previously were based. Table 4 (leftmost three sections) shows the fitted parameter values of, and %VAC by, Equations 4, 5, and 6 when fit to data from punishment conditions for individuals who completed at least four such conditions in which obtained reinforcement rate exceeded obtained punishment rate for both response options. The two punishment models and the generalized matching law all provided good accounts of the data for most subjects. Thus, as anticipated (see Experiment 2, Discussion), this type of analysis provides no clear basis for distinguishing between punishment models.

Figure 5 shows the least-squares linear regression fits of Equations 5 (direct-suppression model) and 6 (competitive-suppression model) to the baseline and punishment conditions combined. Recall that this approach is one means of evaluating the capacity of punishment models to integrate reinforcement and punishment conditions in a single expression. In all cases in Part A, and in seven of nine cases in Part B, the direct-suppression model accounted for more variance than the competitive-suppression model. These results generally corroborate those of Experiment 2.

Table 5 summarizes the third type of model test in which Equation 4 was fit to baseline (no-punishment) data, and the resulting slope and bias parameters (*a* and log *b*, respectively) were held constant as Equations 5 and 6 were fit to punishment data. The table shows outcomes for subjects with at least four punishment conditions in which obtained reinforcement rate exceeded obtained punishment rate for both response options. In 3 of 11 cases (S209, S243, and S265), neither the direct-suppression model of Equation 5 nor the competitive-suppression model of Equation 6 accounted well for the punishment data. In all of the remaining eight cases, the direct-suppression model provided a better account than the competitive-suppression model, although the %VAC by the better-fitting model was fairly low (median = 68.5%).

Overall, the data from Experiment 3 may be said to support direct suppression as the mechanism underlying punishment effects, but, unlike in Experiment 2, the competitive-suppression model of Equation 6 provided an

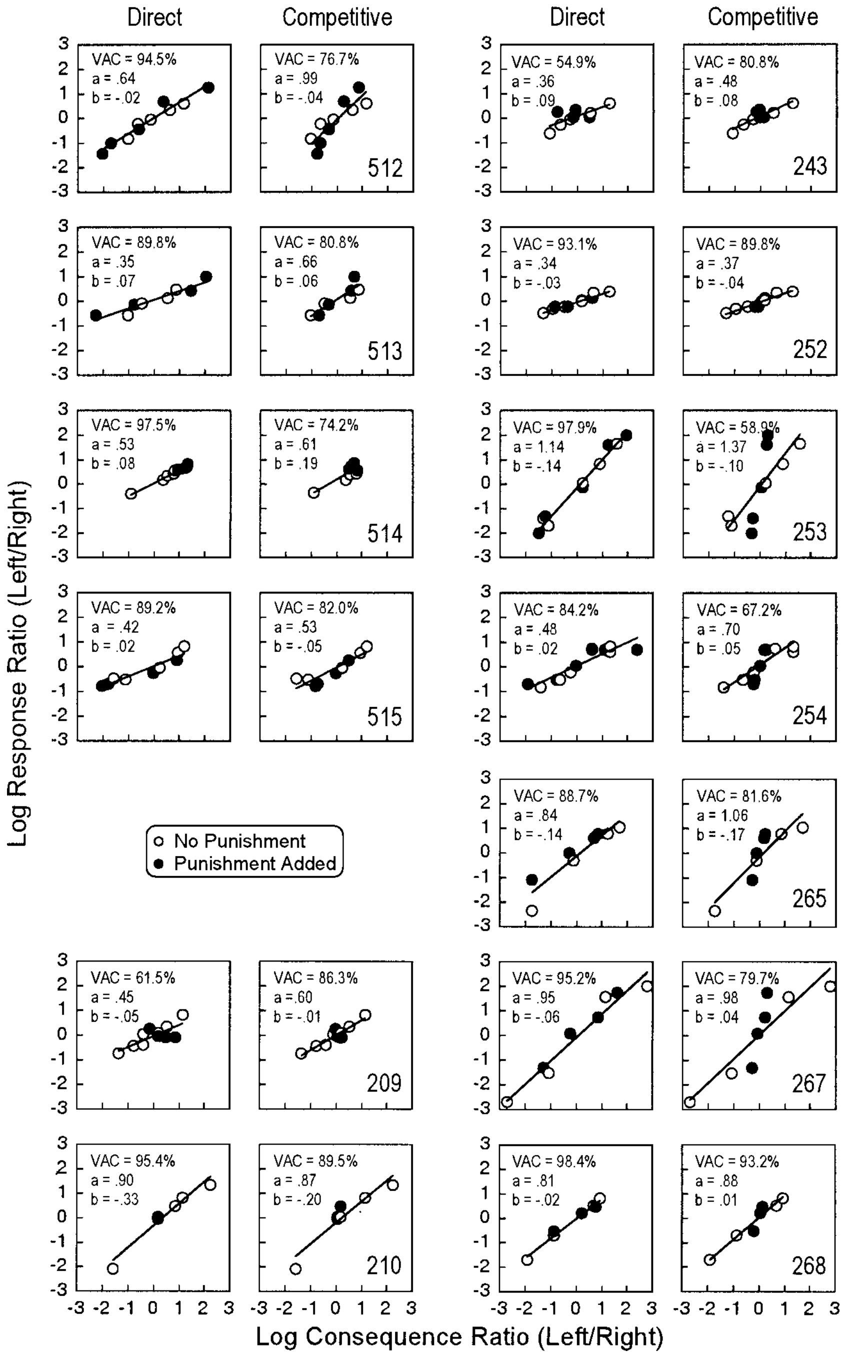

Fig. 5. Experiment 3: Relationship between log consequence ratio, based on Equations 5 and 6, and log response ratio for individual subjects. Note: a and log b are fitted parameters of the models. VAC = percentage of variance accounted for.

Table 5

Experiment 3: Model parameters and percentage of variance accounted for (%VAC) in fitting the generalized matching law to baseline (no-punishment data), and variance in punishment data accounted for by Equations 5 and 6 when parameters were forced to baseline values. Analyses included subjects who completed at least four punishment conditions in which obtained reinforcement rate exceeded obtained punishment rate.

	Equation 4 fit to baseline			Punishment conditions: Variance accounted for by	
Subject	*a*	log *b*	%VAC	Equation 5	Equation 6
S209	.624	.009	97.0	—[a]	—[a]
S243	.488	−.002	98.4	—[a]	—[a]
S252	.361	−.014	96.8	58.1%	27.0%
S253	1.123	−.178	98.1	97.4%	31.7%
S254	.921	−.008	92.1	59.4%	33.3%
S265	1.021	−.389	96.8	26.3%	11.8%
S267	.928	−.180	93.7	91.2%	16.6%
S512	.598	−.021	93.0	94.0%	57.3%
S513	.471	.001	91.6	70.7%	62.9%
S514	.512	.073	97.7	47.3%	—[a]
S515	.470	.092	91.9	68.5%	12.9%

[a] Undefined (negative sum of squares).

obviously superior fit to the data of 2 subjects in one analysis (S209 and S243 in Figure 5). Visual inspection of Figure 5 suggests one possible source of this discrepancy. Whereas the baseline and punishment data from most subjects were readily integrated into a single, positively-sloped linear function, 2 subjects apparently produced negatively-sloped punishment functions. This outcome is reminiscent of a finding reported by Deluty and Church (1978), who exposed rats to unequal, independent, concurrent schedules of response-independent shock. When the rats could select which of these schedules operated, time allocation was an inverse function of shock rate, a pattern well described by a model that can be expressed as

$$\log\left(\frac{B_x}{B_y}\right) = a \log\left(\frac{P_y}{P_x}\right) + \log b. \qquad (7)$$

Table 4 (rightmost section) shows the results of fitting Equation 7 to punishment-condition data for Experiment 2 subjects. Although Equation 7 tended to account for about the same %VAC as Equations 4 through 6 for most subjects, the fitted parameter values verify that S209 and S243 were qualitatively different than other subjects. Because Equation 7 inverts punishment terms (P_y/P_x rather than P_x/P_y), inverse matching to punishment yields a positive value of the slope parameter, *a,* as obtained for S209 and S243. Thus these subjects tended to allocate the bulk of their responding to the option with the lower rate of money loss, even though this was also the option with the lower reinforcement rate and, thus, the lower net money gain. Note that this outcome is consistent with a competitive-suppression view (Equation 6 reduces to Equation 7 when the reinforcement terms are omitted). Equation 7 can be rejected on logical grounds as a simple account of negative slopes generated by other subjects. These subjects allocated the bulk of their responding to the option with the higher rate of money loss (and also the higher rate of reinforcement). Because Experiment 1 showed money loss to function as punishment, it seems likely that for these subjects responding was controlled by net money gain (as assumed in direct-suppression accounts) rather than by punishment rate alone. Taken together, these results suggest the possibility of pronounced individual differences in human punishment effects that merit consideration in future studies.

GENERAL DISCUSSION

The purpose of this investigation was to compare direct-suppression and competitive-suppression models of punishment in choice, and to apply these models for the first time to individual human behavior. Experiment 2 compared models based on Herrnstein's (1970) proportional matching law using a qualitative evaluation procedure that has been employed in all previous model comparisons. A direct-suppression punishment model was superior to a competitive-suppression punishment model in describing 12 of 14 individual-subject functions.

Apparently, no previous report has attempted to update operant punishment models to the form of Baum's (1974) generalized matching law or to compare such models quantitatively (although see Gray et al., 1991). In both Experiments 2 and 3, models based on the generalized matching law were compared by determining how well they integrated data from punishment and no-punishment conditions. A direct-suppression model proved superior to a competitive-suppression model for 17 of 20 individual functions (Figures 4 and 5). In Experiment 3, a more rigorous method of model comparison supported a direct-suppression model in eight of eight interpretable cases (Table 5).

Limited data from previous investigations have suggested the superiority of a direct-suppression punishment model (de Villiers, 1980; Farley, 1980), thereby lending support to the one-factor view of punishment on which Equations 2 and 5 are based (e.g., see de Villiers, 1980, 1982; Farley & Fantino, 1978; Mazur, 1994). This conclusion, heretofore based primarily on studies of nonhumans, can now be provisionally extended to individual human behavior. One reason to regard the present results as provisional lies in the nature of the instructions that, although minimal by most standards, exhorted our subjects to "earn as much money as you can." By possibly focusing attention on net earnings, this phrase might have predisposed subjects toward performances that were consistent with the direct-suppression model. For instance, a subject attending closely to net session earnings might be relatively insensitive to momentary influences such as the transient, punishment-elicited emotional responses that two-factor theories (e.g., Dinsmoor, 1954) hold as the basis of alternative reinforcement effects described in competitive-suppression models. Whether brief instructions heard once at the start of participation can exert such powerful effects across many hours of exposure to changing contingencies can only be revealed by replicating our procedures using different instructions.

The one-factor view of human punishment suggested by the present findings raises interesting questions about prominent interpretive writings that incorporate a two-factor perspective. Skinner (1953), for example, proposed that, consistent with two-factor assumptions, "the most important effect of punishment . . . is to establish aversive conditions which are avoided by any behavior of 'doing something else' " (p. 189). Events that function as punishers often do generate emotional responses (e.g., Axelrod & Apshe, 1983; Taylor, 1991), and casual observation indicated that our subjects sometimes reacted emotionally to point loss. Yet the present findings lend no systematic support to the notion that punishment makes alternative behavior more reinforcing in absolute terms. We suggest, therefore, that although emotional by-products may contribute to troublesome side effects often associated with therapeutic, social, and legal applications of punishment (e.g., Axelrod & Apsche; Gershoff, 2002; Skinner), they bear no necessary relation to the operant response-rate changes that define the operation of punishment.

Although the present findings are broadly consistent with direct suppression of behavior by punishment, several unresolved issues will loom large in the continued evaluation of this view.

Functional Consequence Scaling

Cognitive decision research suggests that equal-sized money gains and losses can have different degrees of impact on choice (e.g., Kahneman & Tversky, 1979), a finding for which, so far, no clear operant parallel apparently exists (e.g., see Lerman & Vorndran, 2002). The present investigation used reinforcers and punishers of equal monetary value to avoid scaling ambiguities that plagued previous investigations with nonhumans in which food and shock, respectively, served as reinforcers and punishers. Nevertheless, the assumption that reinforcers and punishers of nominally equal value exert equal degrees of control over behavior bears formal scrutiny.

The present investigation provided clues that reinforcers and punishers did not always have equal impact upon behavior. First, 2 subjects in Experiment 2 appeared to show inverse matching-to-punishment rates. Thus, under the contingencies employed in Part B of Experiment 2, they preferred the option with the lower rate of reinforcement and, therefore, the lower rate of net money earnings. This outcome makes sense only if, for these individuals, one punisher was more efficacious than one reinforcer. Second, as noted previously, in 14 instances in Experiments 2 and 3, the condition-mean punishment rate equaled or exceeded reinforcement rate for a response option (see Appendixes B and C), resulting in a net gain of zero cents (or less) for that response option according to the subtractive logic of direct-suppression models. In no case, however, did this result in exclusive preference for the other response option, suggesting that punishers may sometimes have had a lower functional value than reinforcers despite their nominally equal magnitude.

Future studies should assess the functional magnitudes of money gain and money loss in the context of human operant experiments because no quantitative choice model can be fully evaluated without knowing the functional magnitudes of the consequences involved (e.g., Herrnstein, 1970). Statistical scaling procedures such as those described by Farley and Fantino (1978) provide one means of accomplishing this. We note, however, that such procedures can be employed only *after* a general form of punishment model (e.g., direct suppression versus competitive suppression) has been adopted (see Baum, 1982; Farley & Fantino). By lending support to direct-suppression models, therefore, the present investigation helps to pave the way for studies of functional punishment value.

It would be surprising if the functional value of money consequences does not vary across individuals. In scaling the functional impact of food reinforcers and shock punishers in pigeons, for example, Farley and Fantino (1978) found different relative values for different subjects. Intersubject differences might be especially pronounced for conditioned consequences (such as money), which acquire their capacity to influence behavior through experience that, in the world outside the laboratory, is unique for each individual (e.g., Lerman & Vorndran, 2002). Unusual preexperimental histories may well have led to the aberrant performances of S209 and S243 in Experiment 2. For this reason, experimentally-created conditioned consequences (e.g., Jackson & Hackenberg, 1996) might provide a better foundation for future investigations.

Model Limitations and Characteristics

Limitations of existing direct-suppression models. Although in the present investigation direct-suppression models (Equations 2 and 5) outperformed competitive-suppression models (Equations 3 and 6), it is unclear whether Equations 2 and 5 form the basis of a *good* punishment model. The present investigation highlights two limitations of existing direct-suppression models. The first limitation is illustrated through several instances in which the response rate for a response option remained greater than zero despite the fact that punishment rate equaled or exceeded reinforcement rate (Appendixes B and C). As noted previously, because of high punishment rates many of these cases are incompatible with qualitative model evaluations based on the proportional matching law (Herrnstein, 1970), and all are incompatible with quantitative model evaluations based on the generalized matching law (Baum, 1974). Our practice was to drop these cases from consideration, but a good punishment model should accommodate them. Pending further model development, these cases can be avoided through scheduling conventions such as the Stubbs-Pliskoff technique for arranging nonindependent concurrent schedules that enforces programmed relative consequence rates (Stubbs & Pliskoff, 1969). Ultimately, however, a general-purpose direct-suppression model is required.

A second limitation of existing direct-suppression models is demonstrated empirically. Even when troublesome cases of high-rate punishment were excluded from analysis, direct-suppression models, although superior to their competitive-suppression counterparts, often accounted for only a modest percentage of variance in individual-subject functions (Tables 3 and 4; Figures 4 and 5). Whether the unexplained variance can be attributed to features of the models, features of the present investigation, or both, remains to be determined. One obvious hypothesis can be immediately ruled out. It might be proposed that emotional responses—unmeasured here but thought to be elicited by aversive events (e.g., Axelrod & Apshe, 1983; Taylor, 1991; Skinner, 1953)—competed with operant processes to create unsystematic noise in the data from punishment conditions. Tables 3 and 4 argue against this possibility by showing that data from punishment conditions, in which these emotional responses should have been elicited, were reasonably orderly. The difficulty seems to lie instead in integrating data from punishment and no-punishment conditions; the very goal that a successful punishment model should achieve.

An additional, and possibly related, concern is whether Equations 2 and 5 promote the most appropriate level of analysis. For example, these models make no reference to the discriminability of the consequences or the stimuli associated with them—factors that are important in concurrent schedule performance involving only reinforcement (Davison & Jenkins, 1985; Davison & McCarthy, 1988; Davison & Nevin, 1999; Madden & Perone, 1999). Additionally, Equations 2 and 5 are molar models, and perhaps punishment effects are better understood on a molecular level of analysis (e.g., Vaughan, 1987). Orderly effects of reinforcement in concurrent schedules have been detected at both levels (e.g., Landon, Davison, & Elliffe, 2002). It makes sense to anticipate parallels in the effects of punishment.

Parameter invariance. Key model tests of the present investigation were predicated on the assumption that punishment leaves the fitted parameters of the generalized matching law unchanged—an assumption for which we know of no direct empirical support. If punishment were found to alter the fitted parameters of the generalized matching law, then the conceptual status of these parameters in punishment models would have to be reconsidered. Ambiguities already exist. Note, for example, that in the present investigation the sensitivity estimates (slopes) derived from the direct suppression model of Equation 5 nearly always were lower than those derived from the competitive suppression model of Equation 6 (see Tables 3 and 4; Figures 4 and 5). This is because, compared to reinforcement-only models (e.g., Equation 4), direct-suppression models tend to expand the range of consequence ratios (e.g., in Part A of Experiments 2 and 3, subtracting a constant from unequal reinforcement values shifts their ratio away from unity). By contrast, competitive-suppression models tend to compress the range of consequence ratios (e.g., in Part A of Experiments 2 and 3, adding a constant to unequal reinforcement values shifts their ratio toward unity). In matching terms, plotting the same set of behavior ratios against different ranges of consequence ratios necessarily yields functions of unequal slope. It is not clear, however, whether is it justified in such cases to conclude that *sensitivity* to consequence differentials is lower for direct-suppression models, or whether it is even permissable to compare slopes generated by qualitatively-different models. Does sensitivity mean the same thing in different models?

Apparently at odds with the parameter-invariance assumption are reports of punishment-related changes in both the sensitivity (Bradshaw et al., 1979) and bias (Bradshaw et al.; McAdie, Foster, & Temple, 1996; McAdie, Foster, Temple, & Matthews, 1993) parameters of the generalized matching law. For present purposes, these reports admittedly are ambiguous. Bradshaw et al. employed ratio punishment schedules, which confound response and consequence rates, and omitted a changeover delay that could have assured independence of concurrent repertoires. McAdie et al. (1993) examined only one baseline and one punishment condition per subject, precluding conclusions about the matching relation. Additionally, punishment in the McAdie et al. investigations consisted of loud noise presented continuously in association with residence at one response option, an arrangement that appears to punish changing over to one alternative rather discrete responses at that alternative.

If punishment *does* alter the free parameters of the generalized matching law, then many complications arise. Although it may be tempting, in the name of parsimony, to simply employ Equation 4 to describe punishment effects, doing so without reference to punishment leaves the model as merely descriptive. To create testable predictions, punishment would have to be integrated directly into the sensitivity or bias parameter of Equation 4 (that is, these parameters would no longer be entirely free). It is not clear how this might be accomplished or what the implications would be for one-factor and two-factor theories that have guided interpretations of punishment for most of the past century.

Because of the potential for parameter invariance, the specific schedule values employed in punishment-model tests may be important in ways not considered when the present investigation was designed. Assume, for instance, that punishing one behavior creates a bias against engaging in that behavior (as suggested by Bradshaw et al., 1979, and McAdie et al., 1993). Applying a constant rate of punishment to two concurrent response options (as in Part A of the present Experiments 2 and 3) would promote competing biases that cancel each other out, leaving models based on the generalized matching law easy to interpret. By contrast, applying unequal punishment schedules to the two response options (as in Part B of the present experiments) would generate competing biases of unequal strength. If raw punishment rates varied not only across response options but also across reinforcement ratios (as in Part B of the present Experiments 2 and 3), unexplained variance would be introduced in the linear functions, and matching models might appear to perform badly. Thus asymmetrical punishment effects on sensitivity and/or bias may help to explain why, in both Experiment 2 and 3 of the present investigation, Part B (in which punishment rate varied) produced less consistent outcomes than Part A (in which punishment rate was constant).

Obviously, new data are needed to shed light on the status of free parameters in punishment models based on the generalized matching law. Straightforward information could be obtained by simply punishing one of two concurrently-available response options across a range of relative reinforcer ratios. To date, however, no study has done so with human subjects (for whom equal-sized reinforcers and punishers presumably can be arranged) while applying standard procedural controls (e.g., COD) associated with concurrent schedule performances. In studies involving simultaneous punishment of concurrent response options, it makes sense to emphasize cases in which a constant rate of punishment is employed for all response options in all conditions, thereby presumably minimizing problems associated with parameter invariance.

Conclusions

The present investigation provides the clearest and most extensive evidence available to date that operant punishment directly suppresses the behavior on which it is contingent. In supporting a direct-suppression account of human punishment, the present findings agree with those of studies involving nonhumans (de Villiers, 1980; Farley, 1980), thus bolstering confidence in the interspecies generality of punishment effects in choice. Aside from the possibility of parameter invariance, the unresolved theoretical and technical issues discussed above do not detract from these contributions. Rather, by improving on and extending previous investigations, the present one helps to bring these issues into focus for future investigations. In apparently supporting a direct-suppression view, the present results raise questions about interpretations of everyday human punishment that stress competitive-suppression mechanisms inspired by two-factor punishment theories (e.g., Skinner, 1953). Replication and extension of the present investigation will prove informative, therefore, in evaluating the validity of these interpretive accounts. Finally, in highlighting some limitations of existing quantitative models of punishment, the present investigation sets the stage for further punishment-model development.

In these ways, the present report demonstrates the value of continuing research on fundamental processes of punishment. Precious little operant punishment research has been published in recent years, especially research involving human subjects (e.g., see Axelrod & Apshe, 1983; Crosbie, 1998; Lerman & Vorndran, 2002). The present results are important, therefore, in adding to this meager data base. Ironically, behavior analysis appears to have largely abandoned research on punishment and other forms of aversive control just as the world outside of behavior analysis has become fascinated by it. This may help to explain the recent proliferation of nonbehavioral theories of aversive control, often guided by nonoperant data (e.g., Carlson & Tamm, 2000; Gehring & Willoughby, 2002; Gershoff, 2002; Taylor, 1991). Some encouragement may be drawn, however, from the fact that the direct-suppression view of punishment supported in the present investigation corresponds to the assumption, made in many fields and psychology subdisciplines, that benefits and costs combine directly to influence behavior (e.g., Ehrlich, 1996; Gray et al., 1991; Kahneman & Tversky, 1979; Leung, 1995; Lohman, 1997; Neilson, 1998). Operant punishment research capitalizing on this common ground thus has the capacity to both advance operant theory and stimulate interdisciplinary discourse.

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APPENDIX A

Experiment 1: Mean obtained rates of responding, reinforcement, and punishment during the final four sessions per condition.

						Consequences per hour				Behavior allocation			
						Reinforcers per hour		Punishers per hour		Responses per minute		Time (s)	
Subject	Session (min)	Changeover delay (s)	Value of consequences in cents	Condition	Session	Left	Right	Left	Right	Left	Right	Left	Right
Part A													
253	15	0.5	2	BL	7	109	112	—	—	58.1	65.4	417	483
				P 50%	10	149	42	0	28	153.0	8.1	858	42
268	15	0.5	2	BL	7	143	138	—	—	25.8	24.8	460	441
				P 50%	4	83	145	50	0	9.5	27.7	221	678
269	10	0.5	3	BL	5	98	84	—	—	15.6	14.1	347	251
				P 50%	7	23	146	80	0	5.2	36.6	69	529
271	10	0.5	3	BL	4	95	135	—	—	22.9	30.2	259	339
				P 50%	4	162	48	0	41	46.7	7.1	512	87
274	10	0.5	3	BL	4	90	141	—	—	28.3	40.4	254	344
				P 50%	4	165	53	0	30	77.5	7.6	522	77
512	8	2.0	8	BL	4	101	103	—	—	26.6	27.0	240	238
				P 50%	4	148	41	0	30	47.8	8.8	402	77
Part B													
272	10	6.0	3	BL	10	116	105	—	—	68.5	64.9	306	293
				P 50%	4	96	114	57	0	32.7	44.8	265	333
				P 80%	6	78	128	54	0	36.4	78.2	194	405
273	10	0.5	3	BL	4	134	149	—	—	35.7	50.3	252	347
				P 50%	4	144	147	0	75	55.1	44.7	333	267
				P 80%	4	159	83	0	63	87.5	16.4	502	98

Note. BL = baseline (no punishment); P = punishment; % = punishment rate as a percentage of reinforcement rate based on programmed schedule values.

APPENDIX B

Experiment 2: Mean obtained rates of responding, reinforcement, and punishment during the final four sessions per condition. ''Rich'' and ''Lean'' refer to programmed reinforcement rates. BL = Baseline (no punishment). In the Condition column of Part A, percentages refer to punishment rate (applied to both response options) as a percentage of the lean-side reinforcement rate. In the Condition column of Part B, percentages refer to punishment rate as a percentage of the reinforcement rate of each response option. See text and Table 2 for details.

				Consequences per hour				Behavior allocation			
				Reinforcers		Punishers		Responses per minute		Time (s)	
Subject	Money Counter?	Order/ Condition	Session	Rich	Lean	Rich	Lean	Rich	Lean	Rich	Lean
Part A											
269	Yes	1 BL	10	180	39	—	—	13.1	8.1	330	269
		2 50%	4	93	24	48	8	23.9	3.8	82	518
		3 100%[a]	7	200	2	102	8	28.6	0.42	9	590
271	Yes	1 BL	4	159	78	—	—	57.4	36.9	355	243
		2 50%	4	168	99	48	48	45.5	46.6	294	306
		3 100%[a]	6	198	72	96	72	64.5	24.3	437	163
272	Yes	1 BL	12	132	65	—	—	28.0	19.6	354	245
		2 50%	6	150	62	47	42	49.0	31.6	345	253
		3 100%[a]	8	152	56	87	62	69.6	34.3	385	213
273	Yes	1 BL	4	185	87	—	—	20.1	15.1	333	266
		2 50%	7	188	66	54	44	26.4	9.9	423	176
		3 100%[a]	7	219	24	107	38	42.1	3.6	540	59
274	Yes	1 BL	4	197	68	—	—	64.2	27.8	413	187
		2 50%	4	159	89	54	45	52.8	36.2	355	245
		3 100%[a]	6	225	8	110	15	55.6	1.8	582	18
Part B											
269	Yes	1 BL	5	204	38	—	—	23.0	10.5	401	198
		2 25%	5	227	14	62	2	27.4	4.9	37	562
		3 50%	12	152	32	89	18	16.8	8.0	274	325
		4 75%	11	81	42	69	36	9.5	6.3	229	370
271	Yes	1 BL	7	272	20	—	—	33.9	1.4	544	27
		2 25%	4	239	30	68	8	29.4	5.9	488	111
		4 50%	5	278	14	110	6	34.8	1.2	570	28
		3 75%[a]	5	239	27	188	27	29.7	6.0	111	488
272	Yes	3 BL	6	223	29	—	—	67.4	13.8	498	102
		1 25%	4	225	30	60	6	80.5	28.2	433	166
		2 50%	6	189	27	93	20	65.3	24.6	163	436
		4 75%[a]	7	239	18	170	27	73.9	14.4	129	469
273	Yes	1 BL	5	219	45	—	—	18.3	14.2	333	266
		3 25%	4	258	44	65	9	24.1	10.6	402	198
		4 50%	4	242	39	132	23	29.7	8.2	447	153
		2 75%[a]	11	264	16	208	16	36.5	3.2	544	55
274	Yes	1 BL	4	260	41	—	—	53.6	18.8	444	155
		2 25%	6	213	44	52	11	31.7	18.3	377	222
		4 50%	4	248	38	137	18	31.8	7.9	477	123
		3 75%	4	218	42	180	36	31.8	13.9	416	182
Part C											
500	Yes	1 BL	9	135	63	—	—	14.3	12.8	250	230
		2 50%	7	177	44	45	41	31.6	14.1	338	143
		3 100%[a]	9	173	32	89	32	21.5	6.6	369	111
	No	5 BL	8	150	80	—	—	26.2	18.5	288	191
		6 50%	8	122	69	50	39	14.6	13.5	249	230
		4 100%	7	173	65	77	49	19.6	6.8	360	119
501	Yes	4 BL	11	159	70	—	—	31.6	21.6	4.57	3.43
		5 50%	5	180	89	54	45	40.2	17.2	5.60	2.39
		6 100%	6	200	49	94	40	44.2	5.5	7.04	0.96

APPENDIX B

(*Continued*)

Subject	Money Counter?	Order/ Condition	Session	Consequences per hour: Reinforcers Rich	Reinforcers Lean	Punishers Rich	Punishers Lean	Behavior allocation: Responses per minute Rich	Responses per minute Lean	Time (s) Rich	Time (s) Lean
	No	3 BL	7	166	84	—	—	23.2	19.8	4.35	3.65
		1 50%	8	171	88	60	41	34.6	18.7	5.21	2.79
		2 100%	9	182	49	99	39	31.6	7.5	6.42	1.58

[a] Conditions in which obtained punishment rate equaled or exceeded obtained reinforcement rate for a response option were excluded from model evaluations summarized in Figure 5.

APPENDIX C

Experiment 3: Mean obtained rates of responding, reinforcement, and punishment during the final four sessions per condition.

			No-Punishment baseline						
	Reinforce-ment ratio			Reinforcers per hour		Responses per minute		Time (s)	
Subject	(L : R)	Phase	Sessions	Left	Right	Left	Right	Left	Right
Part A									
512	7:1	4	5	261	19	44.7	11.0	377	103
	3:1	1	11	176	43	36.3	16.4	318	161
	1:2	3	10	94	133	25.6	30.0	229	250
	1:4	5	4	45	204	20.4	33.0	189	290
	1:5	2	4	24	244	7.3	48.1	65	413
513	7:1	4	4	210	30	18.4	6.3	359	121
	5:1	2	5	141	41	6.1	4.6	276	203
	1:3	1	12	45	133	7.1	8.6	216	263
	1:7	3	4	23	244	6.5	23.5	111	369
514	6:1	4	4	233	38	33.8	12.2	361	118
	4:1	2	7	218	32	39.0	10.6	381	100
	2:1	5	6	158	69	25.2	16.8	293	185
	1:3	3	5	201	60	42.1	17.8	339	141
	1:7[b]	1	8	30	246	16.6	39.6	143	336
515	4:1	3	7	188	21	16.4	4.6	375	104
	3:1	4	6	208	13	18.7	2.8	413	67
	2:1	1	4	114	66	10.7	11.8	263	216
	1:5	5	4	15	193	5.4	18.7	103	376
	1:7	2	4	6	221	6.9	20.3	75	404
Part B									
209	9:1	2	8	284	20	91.8	14.0	771	127
	3:1	4	4	208	64	68.1	33.5	564	334
	3:2	6	4	144	100	53.4	45.4	484	415
	1:2	3	19	76	187	28.0	73.2	255	644
	1:4	1	14	38	235	27.3	77.9	237	662
	1:17[b]	5	7	14	320	15.0	81.6	152	747
210	9:1	4	4	217	15	18.3	3.9	763	136
	3:1	1	11	201	1	14.5	0.2	895	6
	3:2	3	12	124	84	14.9	15.0	483	416
	1:17[a,b]	2	10	0	235	0.1	12.0	2	898
243	17:1[b]	2	4	280	15	38.1	11.8	659	240
	3:1	4	4	188	57	26.2	15.6	545	353
	1:2	3	4	80	148	19.5	22.3	422	476
	1:4	1	10	43	200	15.4	31.0	315	583
	1:9	5	7	21	257	10.1	40.5	456	443
252	17:1[b]	6	5	269	15	33.0	13.7	612	288
	4:1	2	4	199	45	26.6	14.3	603	298
	3:2	5	4	167	114	24.8	23.9	459	441
	1:3	4	6	67	206	17.2	30.0	352	546
	1:9	1	5	28	251	15.8	33.9	317	583
	1:17[b]	3	6	14	288	12.0	37.2	251	648
253	17:1	3	4	332	9	114.0	6.0	860	39
	4:1	5	4	275	34	115.0	20.7	755	144
	2:1	1	4	157	96	68.8	67.6	456	442
	1:3	4	8	22	290	2.9	125.0	24	875
	1:9	2	11	17	303	4.8	98.5	43	856
254	17:1	6	4	304	15	82.8	22.0	727	172
	9:1	3	4	284	14	85.7	14.4	800	98
	3:1	2	5	250	62	124.5	23.2	744	157
	2:3	5	4	92	157	18.8	32.3	257	641
	1:4	4	4	56	255	18.4	61.9	166	732
	1:17	1	6	12	308	16.8	123.0	115	783

APPENDIX C

(*Extended*)

	Punishment superimposed							
	Reinforcers per hour		Punishers per hour		Responses per minute		Time (s)	
Sessions	Left	Right	Left	Right	Left	Right	Left	Right
7	283	6	32	4	54.0	3.1	451	28
5	246	113	34	17	45.4	9.1	397	81
6	71	193	30	32	13.3	36.5	132	347
5	10	255	8	35	2.0	54.0	19	460
8	11	184	8	32	5.0	51.5	44	435
8	246	23	30	21	21.8	2.3	425	55
17	204	30	30	24	15.4	5.9	345	134
7	45	128	28	28	4.2	5.8	201	278
10	24	227	23	29	5.5	20.0	101	381
5	244	30	24	19	42.2	5.93	417	62
5	225	38	34	28	45.7	8.6	407	73
4	193	45	30	26	31.9	7.8	377	102
4	219	36	32	24	43.1	9.4	394	86
10	23	248	26	30	9.5	53.3	78	401
4	203	8	23	11	16.7	3.0	401	78
10	141	30	21	15	12.6	7.3	312	167
4	83	86	24	24	18.8	34.7	167	319
4	13	212	10	28	3.8	19.4	70	409
14	6	244	4	32	3.3	19.8	54	425
4	190	29	99	16	42.8	56.0	391	507
4	190	71	105	43	40.8	53.8	596	303
4	154	107	83	60	25.3	31.2	413	485
6	105	160	53	88	58.7	32.3	564	334
6	55	176	29	107	44.7	42.1	460	439
4	15	220	16	120	58.6	36.9	538	361
4	206	26	118	14	21.1	7.5	659	239
5	114	57	69	28	14.1	12.9	459	441
14	99	73	56	46	10.7	12.3	763	136
8	2	269	2	145	0.3	24.5	14	885
5	155	14	91	14	17.1	21.6	394	476
4	173	65	97	39	18.8	18.3	456	442
4	83	130	50	83	18.2	17.3	478	427
5	53	96	29	67	19.4	10.4	554	346
5	30	170	18	98	26.6	14.4	551	347
4	243	14	133	15	21.3	13.4	556	344
6	214	55	113	30	26.3	20.2	498	403
4	141	100	77	57	14.3	15.5	432	466
5	68	179	36	105	11.4	20.9	343	556
4	27	209	15	119	15.2	24.9	343	557
6	13	188	14	114	13.9	16.2	379	520
5	329	8	166	6	106.0	3.6	839	59
4	281	17	143	9	134.0	4.3	872	28
17	140	88	73	46	40.4	66.2	397	502
8	16	257	10	130	4.1	110.0	59	839
7	9	301	4	142	1.1	116.0	44	855
6	282	14	146	13	60.0	11.8	784	115
4	279	23	139	12	69.3	12.7	792	87
5	241	57	129	30	82.1	16.9	752	147
5	107	143	53	86	36.9	33.5	444	455
15	39	238	21	127	9.9	36.5	124	777
22	14	304	12	160	13.3	71.9	84	814

APPENDIX C

(*Continued*)

			No-Punishment baseline						
	Reinforce-ment ratio			Reinforcers per hour		Responses per minute		Time (s)	
Subject	(L : R)	Phase	Sessions	Left	Right	Left	Right	Left	Right
265	9:1	4	5	246	5	41.0	3.8	820	81
	4:1	2	13	265	35	143.0	25.0	775	124
	1:2	1	4	96	125	107.0	54.0	593	306
	1:17[a]	3	5	0	337	0.7	154.0	4	894
267	17:1	2	5	346	6	104.0	1.4	886	14
	4:1	4	6	278	20	103.3	2.6	875	25
	1:2	1	6	19	224	2.7	92.0	28	871
	1:9[a]	3	4	0	319	0.2	97.9	2	898
268	5:1	4	6	223	26	36.3	5.8	778	121
	4:1	1	5	239	48	104.0	34.8	671	229
	1:3	3	8	29	226	11.4	60.6	147	752
	1:17[b]	2	13	4	351	2.1	110.0	17	882

[a] When, during the terminal sessions of a baseline condition, a response option generated nonzero response rates but insufficient residence time to allow reinforcers to accrue, a reinforcement rate of 0.1 per minute, or 6 per hour, was used during model fits.

[b] Conditions in which obtained punishment rate equaled or exceeded obtained reinforcement rate for a response option were excluded from model evaluations summarized in Figure 6.

APPENDIX C

(*Continued Extended*)

	Punishment superimposed							
	Reinforcers per hour		Punishers per hour		Responses per minute		Time (s)	
Sessions	Left	Right	Left	Right	Left	Right	Left	Right
5	269	16	145	9	108.0	18.9	767	132
5	236	46	126	23	108.0	27.1	716	183
8	72	151	41	81	64.2	65.9	434	467
5	9	328	6	168	10.3	130.0	68	831
4	341	6	166	2	93.5	4.8	860	39
8	233	42	117	26	70.5	34.6	600	299
7	98	171	54	95	48.8	39.4	490	410
8	11	304	3	150	3.5	90.0	29	871
6	112	18	76	12	8.1	3.1	658	241
4	97	44	69	27	21.9	34.1	356	543
4	23	154	14	86	3.5	12.7	198	701
6	4	181	5	110	1.4	19.8	61	837