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The Analysis of Gambling Behavior (AGB) is a peer-reviewed publication that contains original general interest and discipline specific articles related to the scientific study of gambling

Content of the Analysis of Gambling Behavior

The Analysis of Gambling Behavior (AGB) contains general interest and discipline specific articles related to the scientific study of gambling. Articles appropriate for the journal include a) full-length research articles, b) research reports, c) clinical demonstrations, d) technical articles, and e) book reviews. Each category is detailed below along with submission guidelines:

Research Articles – a manuscript of full length (20-30 double-spaced pages approximately), which may contain multiple experiments, and are original contributions to the published literature on gambling.

Research Reports – a manuscript of reduced length (no more than 10 double-spaced pages and a single figure or table page), which may be less experimentally rigorous than a Research Article, a replication of or failure to replicate a prior published article, or pilot data that demonstrates a clear relationship between independent and dependent variable(s). The Results and Discussion sections of Reports should be combined.

Clinical Demonstrations – a manuscript of reduced length (no more than 8 double-spaced pages and a single figure or table page) which lack the rigor of a true experimental design, yet do demonstrate behavior change of persons with gambling disorders under clinical care. This manuscript should contain an Introduction, Methods/Treatments, Results, and Discussion sections. The Results and Discussion sections of Clinical Demonstrations should be combined.

Technical Article – a manuscript of either full or reduced length, depending on necessity, that describes either a new technology available that would be of interest to researchers or a task-analysis style description of how to utilize existing technology for the conducting of research. Examples of appropriate topics may include, but are not limited to, the rewiring of a slot machine for the collection of data or controlling of win/losses, how to use computer software to simulate a casino game, or the way in which neuroimaging devices may interfaced with an experimental apparatus.

Book Review – a review of a contemporary book related to gambling not more than three years after the publication data of the book to be reviewed. The review should be no more than 15 doubled-spaced pages in length.

ANALYSIS OF GAMBLING BEHAVIOR

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Contents

Research Articles

Research Reports

Guest Reviewers

DOES PROVIDING ACCURATE INFORMATION ABOUT SLOT MACHINES ALTER HOW PARTICIPANTS PLAY THEM?

Jeffrey N. Weatherly and Ellen Meier
University of North Dakota

It is a commonly held belief that irrational thoughts held by gamblers can promote gambling behavior and ultimately pathological gambling. Some evidence exists to support this view, but little experimental work demonstrates that confronting these beliefs will lead to a decrease in gambling behavior. Eighteen non-pathological participants were given the option to play a slot machine for money. After gambling in two sessions, they were given accurate information about the independence of turns programmed by a slot machine, the negative rate of return of a slot machine over time, or both. Participants were then given the option to gamble in two subsequent sessions. Results showed that the introduction of the accurate information significantly decreased gambling, but did not eliminate it. Furthermore, no significant differences were observed across groups that received the different types of information. The results support the idea that gambling behavior is at least partially rule governed, but also indicate that information alone is unlikely to get individuals to stop gambling.
Keywords: Rule-governed behavior; Slot Machine; Gambling

Within the United States, gambling is a very popular activity. Nearly every state has some form of legalized gambling (MacLin, Dixon, & Hayes, 1999) and estimates suggest that over 90% of the population will engage in some type of gambling behavior within their lifetime (Petry, 2005). Although this behavior can be entertaining, it leads to serious problems for some. Petry (2005), for instance, estimated that between 1 – 3% of the population suffers from pathological gambling.

Although the percentage of individuals who suffer from gambling problems is quite small compared to the percentage of individuals who gamble without such problems, the absolute number of people who suffer from pathological gambling is not. Given the large number of people who suffer, it behooves the field to try to determine why these individuals come to display problem behavior (while other gamblers do not). The research literature on gambling is relatively large, suggesting that researchers have not ignored the study of gambling. However, no universally accepted explanation of pathological gambling currently exists (see Petry, 2005 for a review).

Perhaps the most popular approach to understanding and treating pathological gambling currently comes from the cognitive perspective. This approach espouses that pathological gamblers operate under false or faulty beliefs that lead them down the road to pathology (e.g., see Ladouceur, Sylvain, Boutin, & Doucet, 2002). These fallacious thought patterns can include the illusion of control (i.e., the idea that the person's actions influence the outcome of the game when in fact they do not; Langer, 1975), the failure to understand the independence of outcomes (i.e., the fact that, in most games of chance,

Address Correspondence to:
Jeffrey N. Weatherly, Ph.D.
Department of Psychology
University of North Dakota
Grand Forks, ND 58202-8380
Phone: (701) 777-3470
Fax: (701) 777-3454
E-mail: jeffrey_weatherly@und.nodak.edu

the outcome of any one play is independent of the outcome of the previous or subsequent play), and the failure to recognize the games' negative rate of return (i.e., nearly every game of chance programs a long-term rate of return below 100%, meaning that the longer one plays, the more likely it becomes that one will lose money). Theoretically, people who operate under these fallacies are prone to become pathological gamblers. To successfully treat that pathology, one must eliminate or alter these fallacious thoughts.

From a behavior-analytic perspective, the underlying assumption of this view is that gambling behavior is largely rule governed. Although behavior analysis has long treated gambling behavior as being under the control of contingency-driven factors (see Weatherly & Dixon, 2007 for a discussion), an increasing number of behavior analysts are suggesting that verbal behavior plays a key role in the maintenance of gambling behavior (e.g., Dixon & Delaney, 2006; Dymond & Whelan, 2007; Weatherly & Dixon, 2007). This view has some support. For instance, Dixon (2000) was able to demonstrate that the behavior of roulette players could be altered by the introduction of inaccurate instructions even after the players had come into contact with the programmed contingencies of the game. Dixon, Hayes, and Aban (2000) demonstrated that the best predictor of when participants ceased gambling was the instructions the participants were provided, not the outcomes (e.g., winning or losing) the participants experienced while playing. More recently, Derevensky, Gupta, and Baboushkin (2007) were able to demonstrate that different winning contingencies altered children's' reported cognitions about gambling. That study focused on how risk taking affected cognitions, however, not how cognitions affected gambling behavior.

These demonstrations are informative, but they are not abundant in the literature. Furthermore, as pointed out by Petry (2005), although it is possible to demonstrate that pathological gamblers hold irrational beliefs about the game of chance they might be playing, it is also the case that non-pathological gamblers hold similar beliefs. Thus, these irrational rules may be necessary for the disorder, but they do not appear to be sufficient for it.

More germane to the current investigation is whether or not providing accurate information or rules will benefit the gambler. That is, both Dixon (2000) and Dixon et al. (2000) demonstrated that the introduction of inaccurate rules altered the gambling behavior of the participants. Neither study showed that participants' behavior could be altered by accurate rules. This point is an important one because it represents the foundation of the cognitive approach for the treatment of pathological gambling (e.g., Ladouceur et al., 2002). Namely, if one can get the pathological gambler to follow accurate rules, not inaccurate ones, then the factor leading to the pathology should be eliminated (but see Petry, 2005).

For the present study, we recruited non-pathological individuals to play a slot machine in four different sessions. In the first two sessions, the participants were allowed to play (or not play) a slot machine. Prior to the third session, participants were provided with accurate information about slot machines. One group was informed about the independence of outcomes from play to play. Another group was informed of the diminishing returns one can expect when one continues to play the slot machine. The final group received information on both the independence of outcomes and diminishing returns. The participants then played (or did not play) in two additional gambling sessions.

If gambling behavior is largely rule governed, then one would predict that the introduction of this information would lead to a decrease in participants' gambling behavior. If participants' beliefs in dependence of turns or positive outcomes over time differ in how much they control behavior, then one would predict that information countering these beliefs would have a differential effect between groups. Finally, if both beliefs are governing behavior, then one would predict the greatest decrease in gambling behavior for the group that receives information countering both beliefs.

METHOD

Participants

The participants were 18 (8 females, 10 males) individuals who were recruited from the psychology department's participant pool at the University of North Dakota. To participate, individuals had to be 21 years of age or older and score less than 5 on the South Oaks Gambling Screen (SOGS; Lesieur & Blume, 1987). The mean age of the participants was 22.61 (SD=2.20) years. All but one participant was single (or divorced). All participants were Caucasian and all but two reported making $10,000 or less per year in annual income. No participant reported an annual income above $25,000.

Materials and Apparatus

Participants completed a series of forms that included an informed consent form, a demographic questionnaire, the SOGS (Lesieur & Blume, 1987), and the Gambling Functional Assessment (GFA; Dixon & Johnson, 2007). They completed these forms prior to participating in any gambling sessions.

The demographic questionnaire asked for five pieces of information: sex, age, marital status, race/ethnicity, and annual income. Each of these pieces of information was obtained because each is a known risk factor for pathological gambling (see Petry, 2005).

The SOGS is a self-report questionnaire that contains 20 items. The questionnaire asks respondents about their gambling history and is a widely used measure for screening for the possible presence of pathological gambling (Petry, 2005). A score of 5 or above on the SOGS suggests that the respondent may be a pathological gambler. For the present study, potential participants who scored 5 or more on the SOGS were not allowed to participate in the gambling sessions to assure that individuals who might be suffering from pathology were not allowed to engage in their pathology. No participants had to be excluded because of their score on the SOGS.

The GFA (Dixon & Johnson, 2007) is a self-report questionnaire that contains 20 items. The questionnaire asks respondents about the environments in which they gamble so as to potentially identify the consequences that might be maintaining their gambling behavior. The GFA supposedly identifies four possible reinforcing consequences: sensory experiences, escape, attention, and tangible rewards (i.e., money). Five questions are asked pertaining to each consequence, which respondents can score between 0 and 6, making the top score for any category on the GFA 30. The category with the highest score is theorized to be the primary consequence maintaining the individual's gambling.

Gambling sessions took place in a windowless room that contained three slot machines. All participants played the same one machine in each session. It was an IGT "Red, White, and Blue" (wild) machine. The machine allowed the participant to bet up to three coins per spin. The machines were programmed to accept tokens, which participants were informed were worth $0.05 each. Outcomes on the machine were programmed by a computer chip designed to provide an 87% return rate over an indefinite period of time. The machine was equipped with a series of counters (unobservable to the participant) that recorded the number of coins put into the machine and the number of coins dispensed. All "wins" were paid in tokens (vs. being accumulated on the machine as credits) to ensure an accurate count of the number of coins won.

The number of plays (i.e., spins) was not recorded by the slot machine; therefore the researcher monitored this measure manually.

Procedure

All aspects of the procedure were approved by the Institutional Review Board at the University of North Dakota. Participants were run individually. When a participant arrived for the first session of the experiment, the researcher checked his/her identification to ensure the participant was 21 years of age or older. The participant then went through the process of providing informed consent. Next, the participant completed the SOGS, followed by the demographic questionnaire and the GFA. The researcher scored the SOGS while the participant was completing the final two questionnaires to ensure that the participant did not score 5 or more on the SOGS. No participant did. The researcher then seated the participant in front of the slot machine and read him/her the following instructions:

> You will now be given the opportunity to play on a slot machine. You will be given 100 tokens worth 5 cents each. Thus, you are being given $5 to play with. You may bet as many credits per play as the machine allows. Your goal should be to end the session with as many tokens as you can. You may end the session at any time by informing the researcher that you would like to end the session. The session will end when a) you quit playing, b) you run out of tokens, or c) 15 minutes have elapsed. At the end of the experiment you will be paid in cash for the number of tokens you have left or have accumulated. Do you have any questions?

Questions were answered by repeating the above instructions. The researcher then gave the participant a plastic cup that contained 100 tokens and the participant played the slot machine until one of the three criteria for ending the session was met. When participants arrived for the second gambling session, the researcher informed them that the session was the same as the first. The participant was again given 100 tokens and the session proceeded as did the first session.

Prior to the third gambling session, the participant was pseudo-randomly assigned to one of the three groups (n = 6). The groups differed as to the information they received prior to the third session. The pseudo-random nature of the procedure was that we attempted to keep the distribution of females and males similar across groups (i.e., 2, 3, & 3 females in groups 1, 2, & 3, respectively).

Participants in group one (Independence of Turns) were read the following instructions prior to beginning their third session:

> Slot machines are programmed to pay out on what are known as random-ratio schedules, meaning that each play is independent of another. In other words, the outcome of your next play has absolutely no connection to the outcome of the previous or following play. Furthermore, the machine does not "keep track" of how you are playing. Each time you play, the outcome is randomly determined according to a set probability. There is nothing you can do to increase the chances that a winning combination of symbols will fall on the "win" line.

Participants in group two (Diminishing Returns) were read the following instructions prior to their third session:

> Slot machines are programmed to pay back players a certain percentage of the money that they play. For instance, say a machine is programmed to pay back at 98%. That means that, over a long period of time, that machine will return $98 for every $100 that is put into it. Because the payback percentage is always less than 100%, it is never to the player's advantage to play for a long period of time. Furthermore, few slot machines provide a payback percentage as high as 98%. Some may program payback percentages as low as 83% or lower. Because one cannot tell the payback percentage by simply looking at the machine, it may take some time to determine that you are playing a machine with a low payback percentage.

By that point, you have likely lost a lot of money.

Participants in group three (Both) were read the information provided to both groups one and two. Participants in all three groups were then given 100 tokens and the third session proceeded similarly to the first two. When participants returned for their fourth session, they were again given 100 tokens (but were not read additional instructions). At the completion of the fourth gambling session, the researcher summed the total number of credits the participant had accumulated across the four sessions, paid the participant the equivalent in cash, debriefed the participant as to the nature of the study, and dismissed the participant.

Design and Analysis

Two main dependent measures were taken from the gambling sessions. The first was the number of trials (i.e., plays of the slot machine) participants played per session. This dependent variable served as a measure of persistence or duration of play. The second measure was the total number of credits bet per session. This dependent variable served as a measure of risk taking. These two measures are positively, but not perfectly, correlated. That is, because it was possible for participants to bet one, two, or three credits per trial, it was possible for a participant who played half the number of trials played by another participant to bet more credits than that other participant.

The data from individual subjects on these measures were subjected to a three-way (Group by Condition by Session) mixed model analysis of variance (ANOVA). In these analyses, group (Independence of Turns, Diminishing Returns, Both) served as a between-subjects variable. Condition (Baseline vs. Post Treatment) and session (First vs. Second) were repeated measures. Results for these and all following analyses were considered significant a $p<.05$.

Secondary analyses were conducted by correlating participants' scores on the SOGS and GFA with their behavior in the gambling session. Because these scores could not be assigned causal roles and because there was no theoretical reason to believe that they would be correlated with behavior in specific gambling sessions (e.g., session 2), the correlations were calculated using the average number of trials played and credits bet per session across all four gambling sessions. Gender was also correlated with these measures because the literature suggests that females and males differ in terms of their gambling behavior (e.g., prevalence of pathological gambling, types of games of chance they prefer; see Petry, 2005). Furthermore, research from our laboratory suggests that gender differences sometimes (Dannewitz & Weatherly, 2007; Weatherly, Austin, & Farwell, 2007), but not always (e.g., Weatherly, McDougall, & Gillis, 2006), exist. Correlations were determined by calculating Pearson product-moment coefficients.

RESULTS

The ANOVA conducted on the number of trials played yielded a non-significant main effect of group, $F(2, 15) = 0.92$, $p=.421$, Eta Squared = .109, suggesting that the three groups did not differ in the number of trials they played. The main effect of condition was significant, $F(1, 15) = 4.87$, $p=.043$, Eta Squared = .245, indicating that providing information about slot machines altered the number of trials played. The top graph of Figure 1 displays this effect, demonstrating that the information decreased the number of trials participants played. The main effect of session was not significant, $F(1, 15) = 0.52$, $p=.484$, Eta Squared = .033, indicating that the number of trials played did not change significantly between sessions one and two. The interactions between group and condition, $F(2, 15) = 0.08$, $p=.925$, Eta Squared = .010, between group and session, $F(2, 15) =$

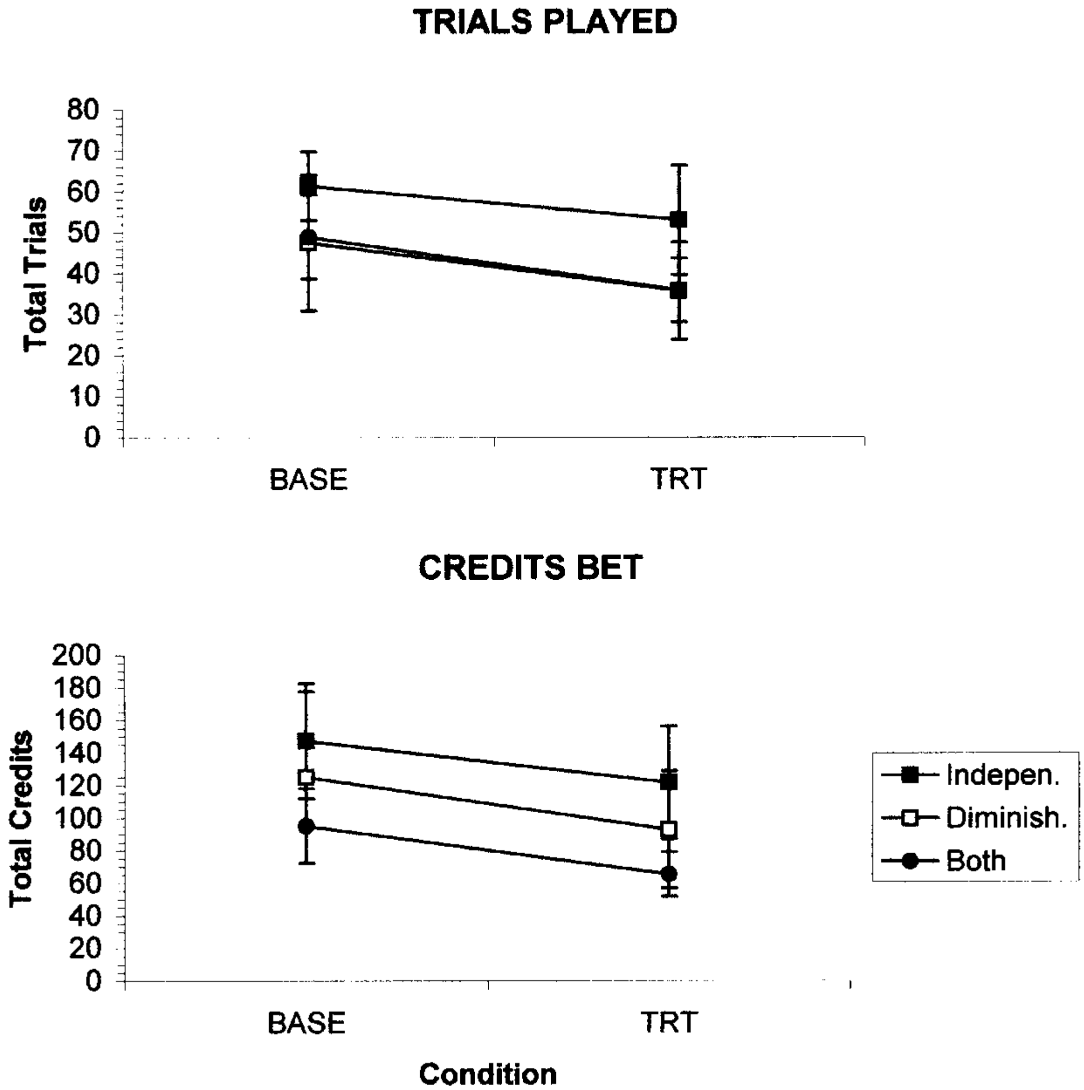

Figure 1. Presented are the number of trials played (top graph) and credits bet (bottom graph) for the mean for all participants in each group in the two sessions before (BASE) and after (TRT) information about slot machines were provided. The error bars represent one standard error of the mean across participants in that particular group in those particular sessions.

.20, p=.820, Eta Squared = .026, between condition and session, $F(1, 15) = 2.60$, p=.128, Eta Squared = .148, and across group, condition, and session, $F(2, 15) = 1.14$, p=.347, Eta Squared = .132, all failed to reach significance, indicating that these measures did not vary systematically as a function of the other(s).

The ANOVA conducted on the number of credits bet also yielded a non-significant main effect of group, $F(2, 15) = 0.78$, p=.478, Eta Squared = .094, suggesting that the three groups did not differ in the number of credits they risked. The main effect of condition was significant, $F(1, 15) = 6.50$, p=.022, Eta Squared = .302, indicating that providing information about slot machines systematically altered the number of credits participants bet. The bottom graph of Figure 1 displays this effect, again demonstrating that providing the information decreased participants' gambling. The main effect of session was not significant

Table 1
Presented are the Pearson product-moment coefficients between SOGS score, gender, GFA scores, and the mean of the two dependent measures from across the four gambling sessions.

	SOGS	Gender	GFA Sensory	GFA Escape	GFA Attention.	GFA Tang.	Trials Played	Credits Bet
SOGS	1.00	-.090	.420	.332	.270	.540*	.355	.023
Gender		1.00	-.356	-.340	-.451	-.368	-.396	-.575*
GFA Sensory			1.00	.788**	.781**	.617**	.678**	.654**
GFA Escape				1.00	.412	.344	.482*	.466
GFA Attent.					1.00	.768**	.524*	.595**
GFA Tang.						1.00	.610**	.510*
Trials Played							1.00	.850**
Credits Bet								1.00

* *p*<.05
** *p*<.01

F(1, 15) = 0.46, *p*=.507, Eta Squared = .030, indicating that the number of credits bet did not change significantly between sessions one and two. The interactions between group and condition, *F*(2, 15) = 0.03, *p*=.973, Eta Squared = .004, between group and session, *F*(2, 15) = .92, *p*=.420, Eta Squared = .109, between condition and session, *F*(1, 15) = 3.49, *p*=.082, Eta Squared = .189, and across group, condition, and session, *F*(2, 15) = 0.88, *p*=.436, Eta Squared = .105, all failed to reach significance, indicating that these measures did not vary systematically as a function of the other(s).

Table 1 presents the correlations between the SOGS scores, participants' gender, GFA scores, and the behavioral measures from the gambling sessions. Participants' SOGS scores were significantly correlated their score on the "tangible" questions of the GFA, but not with actual gambling behavior. Females tended to bet fewer credits than males, but the correlation between gender and trials played was not significant. Scores on the GFA were nearly all significantly correlated with participants' gambling behavior, and also with other scores on the GFA. As suggested above, the number of trials played and the total number of credits bet per session were significantly correlated.

DISCUSSION

The present experiment was designed to determine whether providing players with accurate information about slot machines would lead to a decrease in their gambling on them. Participants in the present study were provided information about the independence of outcomes, the negative rate of return, or both after playing a slot machine for two sessions. The introduction of this information led to a significant decrease in gambling behavior in the subsequent two sessions. These results therefore support the idea that gambling behavior is at least partially rule governed.

Ladouceur et al. (2002) suggested that two of the primary fallacious thought patterns that lead to pathological gambling are the person's inability to recognize that one outcome of a game of chance (i.e., spin of the reels on a slot machine) is independent of the other outcomes and the person's thinking that, sooner or later, the person must win. Participants in the current study were either provided with information meant to confront one of these beliefs or both. The analyses did not find a main effect of group, indicating that information on one type of fallacy did not influence gambling behavior differently than information on the other type. The results also suggest that there was no cumulative effect of providing information on both types of fallacies. Thus, although the present results support the idea that gambling can be decreased by providing accurate information about these beliefs, it does not provide evidence that one type of information is better than the other or that more information is better than less. In fact, it is quite possible that the introduction of the accurate information served to establish a general rule such as "don't trust slot machines" rather than altering the targeted beliefs (i.e., independence of turns, diminishing returns).

It is also worthy of note that although the introduction of accurate information regarding slot machines significantly decreased gambling behavior, it did not eliminate it. In fact, in the 72 gambling sessions that were conducted, in only one did a participant choose not to gamble and thus keep the $5 she had been staked. Interestingly, this outcome occurred in the second session of the experiment, prior to the introduction of information about slot machines. Thus, the present results suggest that information alone is not enough to get non-pathological gamblers to choose not to gamble. It would seem reasonable to assume that pathological gamblers would be more motivated to gamble than non-pathological gamblers, which would lead one to predict that information alone may have less of an impact on the behavior of pathological gamblers than observed in the present study.

One could potentially argue that the observed decreases in gambling were not due to the presentation of accurate information, but rather represent a systematic decrease in gambling over consecutive sessions (e.g., habituation to the procedure). However, results from the statistical analyses can rule out this possibility. The above analyses failed to produce a main effect of session. This result indicates that gambling did not systematically vary from the first to the second session. Furthermore, none of the possible interactions involving session were significant, indicating that changes from the first to second session were not altered as a function of other variables. Neither result should have been observed if gambling behavior was changing as a function of time.

Another argument could be made that the present results are of limited value because the participants were gambling with money that they had been staked, rather than with their own money. This argument cannot be completely countered and will always be one that can be made against gambling research conducted in a laboratory setting. However, existing research has demonstrated that when people are gifted an item, such as the money staked to them in the current experiment, they treat it as if they owned it (e.g., Kahneman, Knetsch, & Thaler, 1990). Furthermore, research from our laboratory has demonstrated that participants gambling with actual (staked) money gamble more conservatively than when they are playing with credits that have no monetary value (Weatherly & Brandt, 2004; Weatherly & Meier, 2007). These results support the idea that the money staked to participants does have value.

If the present procedure was to be replicated, several variations might be warranted. For instance, the participants were presented with the accurate information only once. Although its effect was still present in the second, post-information session, repeating that information may have had a cumulative effect. Next, it is also possible that, had more than two post-instruction sessions been conducted differences in the impact of the different types of information may have emerged. Furthermore, it is possible that the effect of accurate information is, in fact, short lived. Additional sessions would be required to determine whether or not this possibility is a valid one. Finally, in the present procedure, the researcher was present during the sessions to record the number of trials played. Because this situation occurred in every session, it is not possible to tell the impact of having the researcher present.

In terms of the correlation data in the present study, there were several interesting associations between self reports and actual behavior. The SOGS, which is a widely used but sometimes criticized measure (see Petry, 2005), did not correlate with participants' gambling behavior. It did, however, correlate with another self-report measure, namely the "tangible" consequences category of the GFA. This result is of interest because Weatherly and Dixon (2007) postulated that pathological gambling occurs when money becomes the main reinforcing consequence driving the person's gambling. The present result is consistent with that view.

The fact that scores on the GFA were nearly all significantly correlated with the participants' actual gambling behavior suggests that the GFA has value, perhaps for both research and treatment purposes. However, it is also the case that some of the different consequences the GFA was designed to measure were significantly correlated with the other consequences. This result would suggest that the different categories of the GFA may not in fact be measuring separate factors, a finding that is consistent with recent research on the GFA (Miller, Meier, Muehlenkamp, & Weatherly, in press). Thus, although the screen appears to have value, it would seem that it needs to be honed so that the separate categories are in fact measuring separate contingencies.

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INVESTIGATING ILLUSION OF CONTROL IN EXPERIENCED AND NON-EXPERIENCED GAMBLERS: REPLICATION AND EXTENSION

Lingyuan Wong
California State University, Fresno

Jennifer L. Austin
University of Houston, Clear Lake

The illusion of control is a phenomenon in which one erroneously believes he or she can exert control over the contingencies of chance events. To date, many of the studies investigating this phenomenon as it applies to gambling have used artificial gambling contexts and participants with no history of gambling behavior (i.e., undergraduates). This study replicated the procedures outlined in Dixon, Hayes and Ebbs (1998) using experienced and inexperienced gamblers in a more natural gambling setting. Participants played 20 rounds of a game of roulette in which the default procedure was for the dealer to choose the bets. However, players could choose their own bets by paying extra chips. Results indicated that most participants did not buy control of chip placement, indicating an absence of illusion of control. However, the two participants with the highest scores on the South Oaks Gambling Screen engaged in behaviors consistent with illusion of control across almost every trial.

Keywords: illusion of control, experienced gamblers, non-experienced gamblers

Illusion of control has been defined as an "expectancy of a personal success probability inappropriately higher than the objective probability would warrant" (Langer, 1975, p. 313). When present in gamblers, such faulty beliefs can prompt individuals to wager more money across gambling opportunities (Dixon, Hayes, Rehfeldt, & Ebbs, 1998; Joukhador, Blaszczynski, & Maccallum, 2004) and to engage in riskier betting (Dixon, Hayes, & Ebbs, 1998). Further, such beliefs appear to be the most commonly self-reported heuristic for people who gamble regularly or heavily (Toneatto, Blitz-Miller, Calderwood, Dragonetti, & Tsanos, 1997) and tend to be more prevalent in those with gambling problems (Joukhador et al., 2004; Moore & Ohtsuka, 1999).

Several factors appear to influence whether behaviors consistent with illusions of control actually reveal themselves. Langer's (1975) classic study displayed a range of stimulus situations that might influence engagement in behaviors consistent with a belief that chance events can be personally controlled. Specifically, her analyses suggested

Address Correspondence to:
Jennifer L. Austin, Ph.D., BCBA
Division of Psychology
Forest Hall
University of Glamorgan
Pontypridd
CF37 1DL
United Kingdom
Email: jlaustin@glam.ac.uk

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that illusion of control is more prevalent in situations where one's competitor looks less confident, when the game allows the player a choice, and when the player is familiar with or has practiced the game. Her study also revealed that simply thinking about a game across time can increase the tendency to believe in one's ability to control chance outcomes. It does appear, however, that behaviors associated with illusions of control can be altered. For example, Dixon (2000) demonstrated the malleability of illusion of control behaviors via the provision of accurate (e.g., "it does not make a difference who picks the number") and inaccurate (e.g., "you'll win more if you choose your own numbers") rules. Participants in the study played a series of rounds of roulette and could bet as many chips as they chose on 8:1 bets. However, on some trials the participants were allowed to choose the number, whereas as the number for the remaining trials was selected by the researcher. Each participant was exposed to three conditions: no rules, inaccurate rules, and accurate rules. Results showed that the majority of participants wagered more chips in the no rules and inaccurate rules phases than they wagered when accurate rules were provided. These results suggest that external sources of information potentially can exert a strong effect on illusions of control and the behaviors associated with such beliefs. In fact, Ferland, Ladouceur, and Vitaro (2002) found that adolescents' misconceptions about gambling decreased after viewing an informational video explaining the chance nature of gambling and the uselessness of one's behaviors in controlling gambling outcomes. Lectures and activities designed to further explain the video's points produced even stronger effects on participants' reports of beliefs in illusory control.

One potential hypothesis to explain illusion of control is that people who foster such beliefs are insensitive to probabilities and thus cannot discern when outcomes are related to chance. Koehler, Gibbs, and Hogarth (1994) tested this hypothesis by measuring betting behavior on dice games that involved one chance to bet ("single shot") or multiple opportunities ("multi-shot) on a simple dice game with 2:1 odds. Results showed that when the game consisted of one trial, participants who were allowed to throw the dice themselves bet more than those whose throws were made by the researcher. However, when participants were required to bet over a series of trials, they began to make their bets based on the obvious 50% probability of winning on any given trial. Moreover probability-sensitive behavior occurred regardless of whether participants threw the dice themselves or the throw was controlled by the researcher. These results suggest that although illusion of control might be present initially, repeated trials "shatter" the illusion.

In a related study, Dixon, Hayes, and Ebbs (1998) sought to discern illusory control on risk-taking behaviors across multiple trials of roulette. During the course of each game, the amount of each player's bet was kept constant and was provisionally restricted to corner bets. However, participants could pay an additional chip for the opportunity to choose the number on a corner bet, and an additional chip to place their chips on a lower risk bet. Unlike Koehler et al. (1994), Dixon et al.'s participants repeatedly paid additional chips to gain control of chip placement and lower their risks, suggesting that repeated exposure to chance events does not alter illusions of control. However, it is possible that these differences can be accounted for by differences in the games played. Specifically, Koehler et al. used a relatively simple game where the odds remained at 2:1. Roulette could be considered a more complicated game in which odds vary depending on chip placement, thus making probabilities more difficult to discern. In any event, the conflicting results of the two studies raise interesting questions about the effects of repeated exposure to probabilistic outcomes on illusion of control, as well as the influence of familiarity with the game and consistency of the odds.

Clearly, the extant literature examining illusion of control demonstrates the complexity of this phenomenon and the need for additional research. Such investigations have and likely will continue to shed light on important variables in the treatment of pathological gambling (Petry, 2005). However, a potential problem in much of the research examining the role of illusion of control on gambling behavior is that is relies very heavily on self report measures (e.g., Joukhador et al., 2004; Moore & Ohtsuka, 1999; Strickland, Taylor, Hendon, Provost, & Bizo, 2006; Toneatto et al., 1997) as opposed to direct measures of behavior. There is probably good reason for this. First, one's ethics might be challenged if people with serious gambling problems were allowed to engage in potentially dangerous behavior for the sake of participating in a study. Additionally, because casinos in the United States are required to pay-out at a prespecified regulations and rates, experiments which require altering the pay-out and rules are not permitted on the premises (Weatherly & Phelps, 2006). Though some venues may allow direct observation of consenting participants, this still limits investigations of factors which may directly affect gambling behavior. Given the constraints of examining such behaviors in the environments in which they are likely to occur, researchers have used computer simulations (Haw, 2008; MacLin, Dixon, & Hayes, 1999), which allow flexibility with manipulating the parameters and more precision in gathering behavioral data, such as response latency, decision-making periods, and subjective probability estimates.

Despite a strong reliance on self report measures within the gambling literature, some studies have endeavored to directly assess behaviors consistent with illusion of control (Dixon et al., 1998; Dixon et al., 1998a; Dixon 2000; Dixon et al., 2000; Koehler et al., 1994; Langer, 1975). However, the populations from which these measures are collected are comprised exclusively of convenience samples of college students. It is clear that examination of behavior with this particular population is sometimes valuable. For example, Dixon et al. (1998) stated "No subjects had previous experience playing roulette and therefore were chosen to control for any preconceived strategies of how to best play the game" (p. 960). This statement indicates that some studies may have used such samples deliberately to control for particular confounds. There is no doubt that the use of these populations also might allow researchers to construct and run important pilot studies crucial for informing future research.

Despite the potential advantages of using convenience samples for the study of gambling behavior, it is unclear whether the findings from these studies generalize to actual gamblers. The leap of inferring the behaviors of gamblers from non-gamblers may lead to an inaccurate understanding of important behaviors. Inasmuch as this research may provide a foundation for more effective treatments for pathological gambling, accurate understanding of behavior is imperative.

The purpose of this study was to examine the illusion of control and risk-taking behaviors using participants with and without histories of gambling. Additionally, we sought to systematically replicate the procedures of Dixon et al. (1998) to determine whether results attained with college students generalize to those who gamble regularly. We also examined gambling behaviors under more naturalistic stimulus conditions in an attempt to improve the external generality of the procedures and results.

METHOD

Participants and Setting

Seventy nine potential participants were recruited via advertisements published in the local newspaper, on the premises of a local

Table 1

Participant Demographics

Participant	*Age*	*Sex*	*Years of Experience*	*Is participant a student?*	*SOGS Score*	*Roulette Quiz Score*
E1	27	Female	9	Yes	2	4
E2	54	Female	33	No	0	5
E3	31	Male	16	No	4	4
E4	45	Male	27	No	3	4
N1	22	Female	0	Yes	0	0
N2	22	Female	0	Yes	0	0
N3	27	Male	0	Yes	0	0

university, and through word-of-mouth. Each of the 79 respondents subsequently were mailed a package containing an informed consent form, a questionnaire about gambling experience, a five-question assessment on the rules of roulette, the South Oaks Gambling Screen (SOGS, Lesieur & Blume, 1987), information about the local Gamblers Anonymous chapter, and a stamped return envelope. Twenty nine potential participants returned the required forms and were considered for inclusion in the study.

SOGS scores subsequently were reviewed by the first author to further narrow the participant pool. Out of the pool of 29 potential participants, 7 scored $\geq$5 on the SOGS, indicating a potential risk for pathological gambling. Because inclusion of pathological gamblers would raise ethical concerns (i.e., participation in the study would allow engagement in dangerous behavior) and was not approved by the university's Institutional Review Board (IRB), only respondents with scores $\leq$4 were eligible to participate in this study. Of those who scored $\leq$4 on the SOGS, a score of at least 4 on a 5-item questionnaire regarding rules of roulette play was required for inclusion as an experienced participant. Five respondents met this criterion. Further, a score of 1 or 0 on the questionnaire was required to be classified as a non-experienced participant. Five respondents met this criterion. A follow-up phone call was made to those individuals to provide additional details about participation and to confirm interest. Given the monetary costs associated with conducting the study (i.e., staking participants with real money), only 8 of the 10 potential participants were invited to participate in the study. These participants were selected via a random draw.

Seven of the 8 participants reported to the experiment as requested. The 4 experienced gamblers included 2 men (ages 31 and 45) and 2 women (ages 27 and 54). Three of the experienced gamblers held various job vocations in the community while the fourth was an undergraduate student. All had played the table-top version of American roulette on at least three occasions. The 3 inexperienced participants included a man (age 27) and 2 women (both age 22), all of whom were college post-baccalaureate students. None of these participants had prior experience playing any form of casino-related games. A detailed table of the participants' demographics is provided in Table 1.

The games were held in a classroom at California State University, Fresno. The roulette table was rented from a local company which specialized in hosting casino-themed parties, and a dealer was hired to run the games for experimental sessions. On the day of the study, participants' IDs were verified for their name and age before they were allowed to participate in the study.

They also were assured that all personal information would be kept confidential as specified in their informed consent. All procedures were approved by the university's Institutional Review Board prior to participant selection.

Procedure

Participants played the game with all other participants of similar experience (i.e., all 4 experienced gamblers played during a single session and all 3 inexperienced gamblers played during a single session). Prior to beginning play, participants were staked with 80 chips with a value of $20 (i.e., each chip was worth $.25). The following instructions, modeled after the procedures of Dixon et al. (1998a), were then given verbally by the dealer:

> "This is a fair roulette wheel. It is identical in all ways to the roulette wheel found in the casinos in America. You will be given 80 chips which are equivalent to $20. Each chip is worth 25 cents. You will be playing for 20 rounds, and there is no limit to the amount you can win. Each round will start with a default wager of five chips on 8:1 odds, where I will choose the number to bet on. If you wish to choose your own numbers to bet on, it will cost one extra chip. Though you gain control of placing all your five chips, you still need to stake it on a corner bet. If you wish to make a lower risk bet, that being 2:1 or 1:1 bet only, each additional lower-risk bet will also cost an additional chip, and it will permit all your bets to be placed in areas of lower risks. Hence, if you want to control and reduce the risks, it will cost you two chips. Keep in mind that these additional chips are not applied to your bet. Rather, it will always remain a 5-chip bet; only the numbers chosen or the odds will be different. In addition, the wagered chips cannot be split in to different bet ratios or choose to gain partial control of the chips. In the event of someone ending the game before the 20 rounds are completed, he/she will still have to wait for the other players to complete their game. Remember, each chip is worth 25 cents, and at the end of the game, your remaining chips can be cashed in for money, only if you had wagered on all the 20 rounds. There is no borrowing or lending of chips in this experiment. Do any of you have any questions before we start the game? You can still ask questions about the game when it is in play."

Subsequently, participants' questions were answered. The participants then played 20 games of roulette. To ensure that players knew the option to purchase control or lower risks was available each game, the dealer asked each player individually how they would like to place their bets on each round. At the end of the 20 rounds, each player was paid in cash according to the number of chips he or she had remaining.

Procedural Fidelity

An experienced roulette dealer was employed to ensure the proper procedure of the game was conducted. He was trained to read the above instructions and to carry out the procedures as specified in the instructions (e.g., taking a chip from a participant when the participant decided to purchase control). Subsequently, he was assessed for his adherence by role-playing with the primary experimenter and several research assistants. During these sessions, a trained observer recorded adherence to each step of the procedure on a checklist. The dealer performed all the correct steps on 10 consecutive practice rounds before the start of the study. Subsequently, treatment integrity was assessed for each experimental session. Adherence to the protocol was 100% for every round conducted during the study.

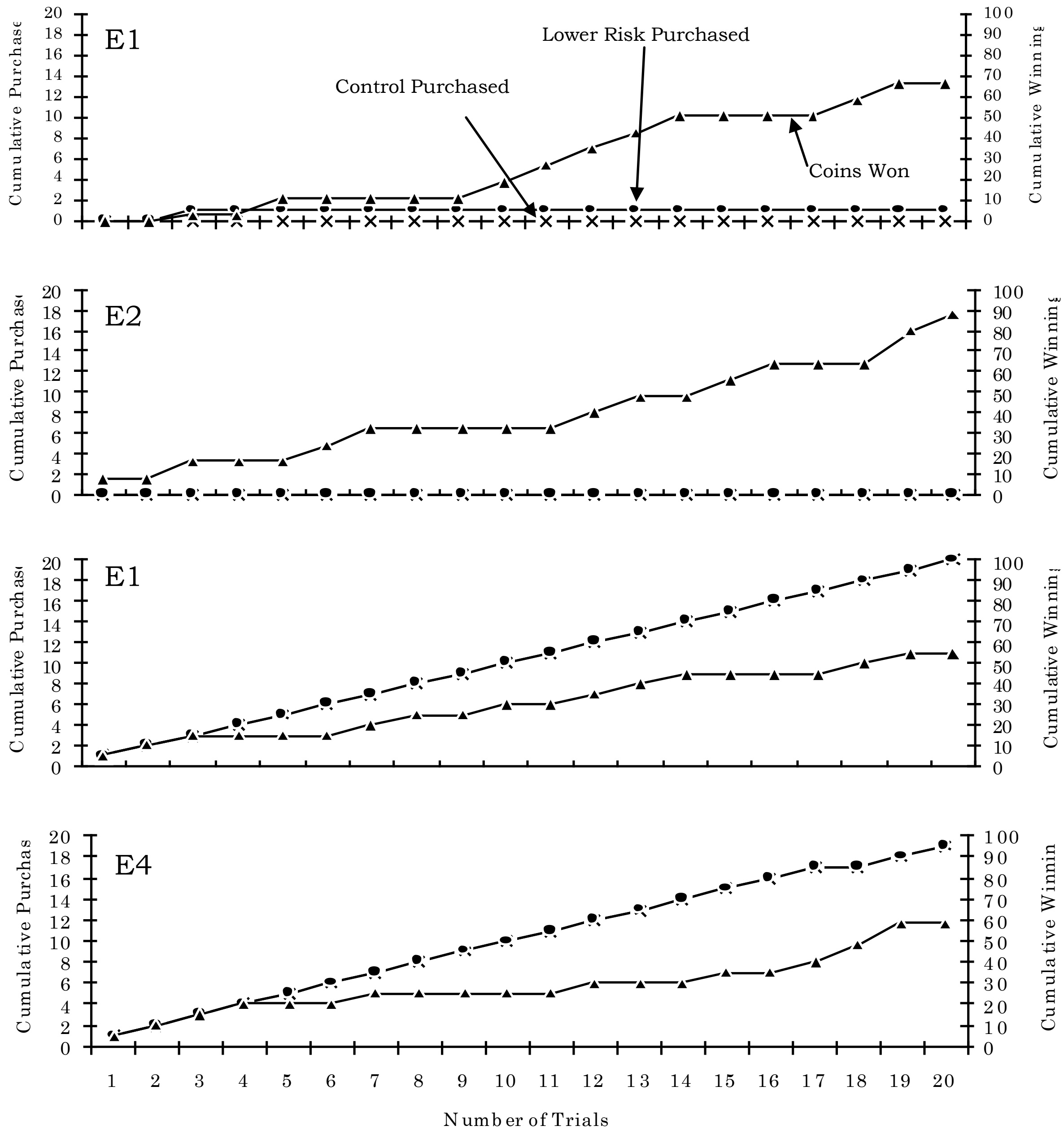

Figure 1. Cumulative winnings in comparison to the cumulative number of trials in which control and decrement of risk were purchased by participants.

Dependent Variables and Measurement

Three primary dependent variables were measured. A *purchase of a decrement in risk* was defined as any trial which a participant paid an extra chip (beyond the five chips allowed for each trial) to have his/her chips placed somewhere other than a corner bet. A *purchase of control* was defined as any round in which a participant paid an extra chip to gain control of the numbers selected for the bet. A *win* was defined as any situation in which the participant was given a payout due to a match between placement of chips and the number selected on the roulette wheel spin. A win was scored (and the number of chips was recorded) even when a participant's total winnings did not exceed the amount wagered for that trial.

A frequency count of the purchase of a decrement in risk and/or control and the outcome of each trial was recorded using a paper-and-pencil data sheet, which also allowed for recording the amount won on each trial. Two video cameras were used to record all sessions. One camera was placed on each side of the roulette table to capture footage from both perspectives.

Interobserver Agreement (IOA)

IOA was assessed for 100% of the experimental sessions and was calculated for each dependent variable by dividing the smaller observed frequency by the larger observed frequency and multiplying by 100% (Bailey & Bostow, 1979; Repp, Deitz, Boles, Deitz, & Repp, 1976). IOA for purchase of decrement of risk averaged 97.5% (range, 95% - 100%). IOA for purchase of control was 100%. IOA for wins averaged 97.5% (range, 95% - 100%).

Self Reports of Winnings and Social Validity

At the end of the study, participants were asked to estimate the total number of trials in which they won and the total number of chips they won across all trials. In addition, a questionnaire was given to each participant at the conclusion of game play to provide an indication of how the setting for the study compared to roulette play at a casino and whether the participants felt their responses during the experimental sessions were similar to those they would have made if they were gambling in a casino.

RESULTS

Table 2 displays a summary of the number of trials in which control and decrement of risk were purchased and the number of chips won for each participant, along with information regarding gambling experience. Figure 1 displays the cumulative winnings in comparison to the cumulative number of trials in which control and decrement of risk was purchased by experienced participants. Participants E1 and E2 (SOGS scores 2 and 0, respectively) never purchased the opportunity to gain control of their chips. However, E1 paid to increase her odds of winning by lowering her risk on one occasion, whereas E2 stayed with the corner bets throughout all 20 trials. The participants' cumulative winnings were 67 chips and 88 chips respectively. Participants E3 and E4 (SOGS scores 4 and 3, respectively) purchased both control and the opportunity to decrease their risk on almost every trial. Their total winnings over 20 trials were 55 chips and 58 chips, respectively.

Figure 2 displays the cumulative winnings in comparison to the cumulative number of trials in which control and decrement of risk was purchased by non-experienced participants. The non-experienced participants bought relatively few opportunities to control the placement of their chips or to improve their odds of winning. N1 and N2 never bought control during the experiment, while N3 did so on only four occasions. However, N1 improved her odds of winning twice, while N2 and N3 maintained their wagers on the corner bets throughout. Their cumulative winnings were 36, 88, and 64 chips, respectively.

An independent samples *t*-test, after adjusting for a significant difference in the homogeneity of variance, revealed that the experienced participants did not purchase significantly more control (m = .49, sd = .5) than the non-experienced participants (m = .07, sd = .25), $t(3.33) = -1.454$, $p = .233$, $d = 1.06$. Similarly, an independent samples t-test, after adjusting for a significant difference in the

Table 2
Total Number of Chips Won, Control and Decrement of Risk Purchased by Each Participant

Participant	Total Chips Won	Control	Risk Decrement	Years of Experience	SOGS Score	Roulette Quiz Score
E1	67	0	1	9	2	4
E2	88	0	0	33	0	5
E3	55	20	20	16	4	4
E4	58	19	19	27	3	4
N1	36	0	2	0	0	0
N2	88	0	0	0	0	0
N3	64	5	0	0	0	0

homogeneity of variance, revealed that the experienced participants also did not purchase significantly more decrement of risk (m = .5, sd = .5) than the non-experienced participants (m = .33, sd = .18), $t(3.08) = -1.687$, p = .188, d = .452. However, Pearson r coefficients revealed that SOGS scores were correlated with purchase of control (r (6) = .843, p = .01) and purchase of risk decrements (r (6) = .887, p = .008).

Self Report and Social Validity

Participants from both groups reported that the dealer performed professionally or very professionally throughout the experiment. All but one of experienced participants indicated that they would make most of the same decisions they made during the experiment at an actual casino, whereas one reported he/she would have made some of the some decisions in an actual casino.

The estimated number of winning trials as indicated by the non-experienced participants ranged between 5 and 12, while the experienced participants ranged from 7 to 13. The actual number of winning trials for the non-experienced participants varied between 6 and 10, and the experienced participants varied between 9 and 11 trials. Thus, both groups appeared relatively accurate in estimating the number of trials in which they won.

Experienced participants estimated winning between 30 and 70 chips, while the inexperienced participants reported winning between 32 and 96 chips. The actual range of number of chips won by the experienced and the inexperienced participants were 55 to 88 and 36 to 88, respectively. By comparison, the non-experienced gamblers better estimated their winnings than the experienced participants.

DISCUSSION

This study examined illusions of control in experienced and inexperienced gamblers using a simulated casino roulette game. Results indicated that the behaviors of the inexperienced participants were relatively uniform throughout the game, and that they rarely purchased control and decrement of risk. Interestingly, two of the experienced participants also displayed the same pattern of behavior, whereas the other two experienced players bought control of chip placement and a decrement of risk on the majority of trials. One purpose of the current study was to assess the generality of Dixon et al.'s (1998)

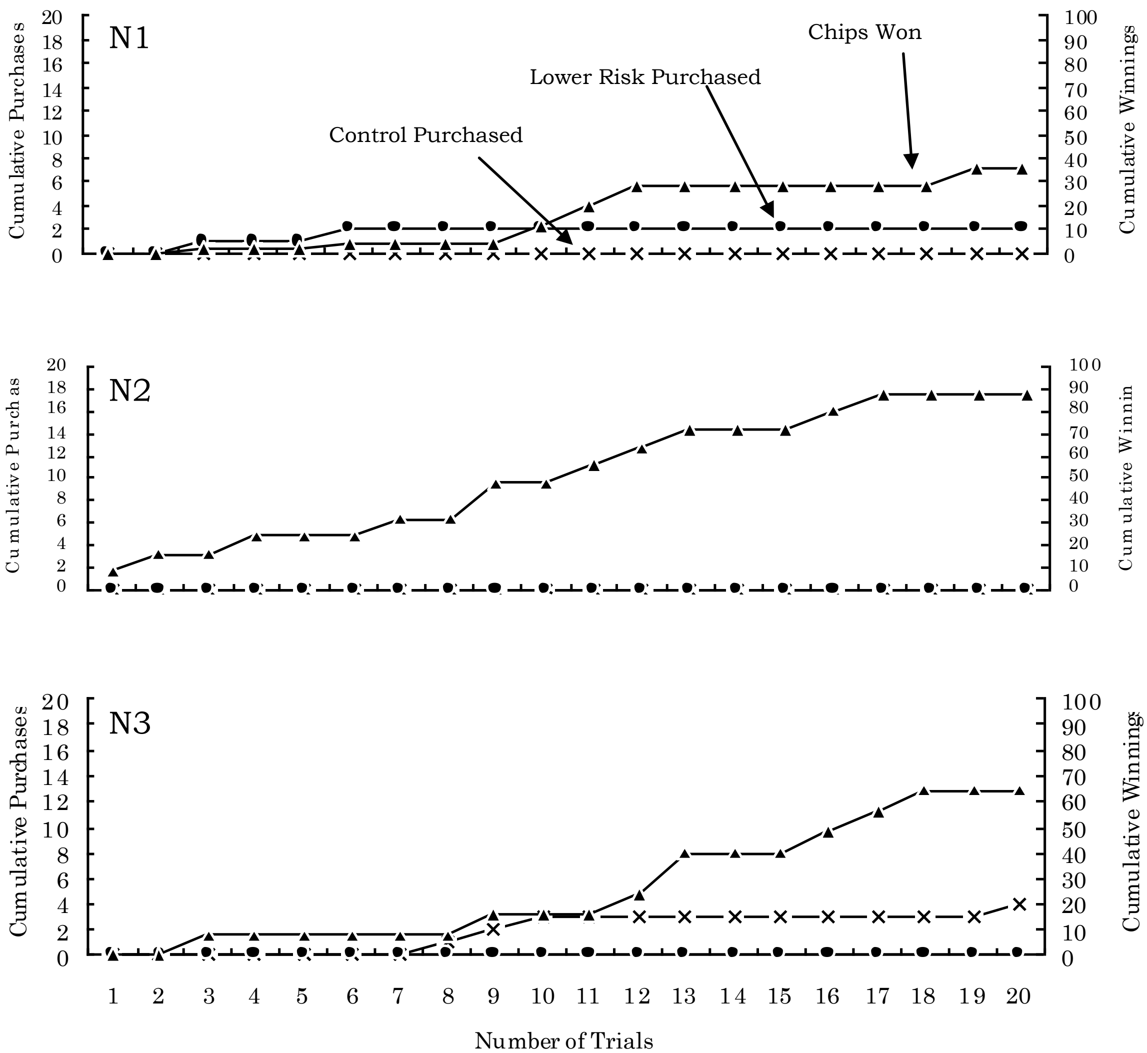

Figure 2. Cumulative winnings in comparison to the cumulative number of trials in which control and decrement of risk were purchased by non-experienced participants.

results to participants with a history of gambling and to measure behavior within a more natural context. To this end, we recruited participants with various histories of gambling from both community and university populations, whereas Dixon et al. focused mainly on undergraduate students who might or might not have had experience gambling (although they did not have experience with roulette). We also attempted to more closely approximate actual casino betting by using a regular roulette table and hiring a professional dealer.

Interestingly, the outcomes of this study differed substantially from those obtained by Dixon et al (1998). Specifically, all of the participants in the prior study bought control of their chips on at least 10 out of 20 trials. Further, 4 out of 5 participants chose to lower their risk on more than half of the trials. In the current study, 5 out of the 7 participants rarely purchased control or decrement of risk. Therefore, the behavior of the majority of the current participants demonstrated responding inconsistent with illusions of control.

It is difficult to determine exactly which variables might have accounted for differences in responding between participants in the two studies or which study represented a more authentic sample of behavior. However, it is imperative to note that the studies differed substantially with regard to stimulus conditions. Dixon et al. (1998) used a graduate research assistant or professor in the role of the dealer, whereas we used a professional dealer. Moreover, payouts in Dixon et al.'s study were in the form of extra course credit, as opposed to the real money used in our study. It is possible that in Dixon et al.'s study, these variables exerted stimulus control over behavior that might not be analogous to typical gambling situations, and produced potential "false positives" of illusions of control. In other words, the participants knew they were in an experiment with someone who had direct influence over their grades; therefore, they might have thought that they needed to continue engaging in behavior (i.e., buying control and risk decrement) to be a "good participant" in the study. It also is unclear as to whether the students who participated in Dixon et al.'s study *needed* extra credit. A better understanding of the motivating operations (Laraway, Snycerski, & Poling, 2003) for the stimuli used as reinforcers would probably assist in understanding gambling behavior, both in Dixon et al.'s study and the current study.

Another difference between the prior and current study was the manner in which participants played the game. Dixon et al.'s (1998) players were run individually, whereas the current study grouped participants according to their level of experience. It is possible that such groupings might have facilitated interaction between the players. For instance, the players might have been influenced by each other's playing strategies based on how much the other players won throughout the game. In fact, N3 mentioned that his purchase of control was somewhat mediated by N2's winnings. On the few occasions when N3 purchased control, he was deliberately trying to follow the placement of N2's chips. Thus, the effects of grouping the participants might have altered some of their responses, whereas Dixon et al. probably provided a better indication of individual responding. However, given that roulette is typically played in groups in most gambling environments, research aimed at understanding the effects of group processes on illusions of control might provide valuable insights into influences on gambling behavior.

Although the failure to replicate Dixon et al.'s (1998) findings raises interesting questions, the current study poses some intriguing findings in its own right. First, although our results were not consistent with Dixon et al.'s, they also were not consistent with of Koehler et al. (1994). Specifically, most participants in the current study never engaged in behaviors consistent with illusions of control, even on the initial trials. These results suggest that our participants were sensitive to the random nature of roulette from the beginning and behaved accordingly.

The striking differences in responding within the experienced group of gamblers were unexpected. Specifically, we anticipated that all the experienced gamblers would be more inclined to demonstrate illusions of control than inexperienced gamblers, given likely histories of reinforcement for engaging in these behaviors. However, it appeared that current (as opposed to remote) reinforcement histories might have exerted substantial influence on behavior. For example, E1 and E2 quickly experienced wins when they let the dealer place their bets at the start of the game, and continued to let the dealer place bets throughout most of the game. Similarly, E3 and E4 experienced wins for buying control and reducing risks early in the game and continued to engage in these behaviors relatively consistently across the study, even when the strategy no longer paid off for them. Given the odds of a fair roulette wheel, any even bets would pay off 47.3% of the time (although each trial is independent from the previous trial). However, E3 and E4 began switching between the colors and later between bets. Further, it appeared that access to a win on a previous trial did not necessarily predict behavior for a subsequent trial. For example, E4 allowed the dealer to place his chips for him on trial 18 and won. Yet on trial 19, E4 purchased both control and risk decrement. These behaviors suggest that both immediate and remote reinforcement contingencies are relevant in predicting gambling behavior. Specifically, it could be that E3's and E4's histories with gambling engendered beliefs about their abilities to control the outcome of the game.

It is interesting to note that the two participants who displayed behaviors consistent with illusion of control (E3 and E4) also had higher scores on the SOGS relative to other players. These findings are consistent with those of Toneatto et al. (1997), who found a significant relationship between SOGS scores and self-reported cognitive distortions. However, this study represents a substantial improvement over prior studies that have compared the beliefs of participants with different gambling histories (e.g., Joukhador et al., 2004; Moore & Ohtsuka, 1999; Strickland et al., 2006; Toneatto et al., 1997), in that we directly observed behaviors indicating illusions of control rather than simply asking participants to report whether they engaged in such behaviors. Although the small sample size limits generality of the findings, it raises interesting questions about differences in the actual behaviors and beliefs of different gambling populations (e.g., non-gamblers, social gamblers, problem gamblers, etc.). Future research should seek to incorporate more direct behavioral measures to discern differential responding among populations. These findings might prove crucial to understanding gambling behavior and assessing the external validity of studies using convenience samples.

Another interesting finding was the positive correlation between SOGS scores and purchase of risk decrement. Whereas paying to control chip placement on an 8:1 bet would not influence winnings, paying to place one's bet on a 2:1 would. Dixon et al. (1998) suggested that both these behaviors are consistent with illusions of control, in that "while responses at these choice points may influence the size of a win or loss, the win or loss itself is randomly set" (p. 960). However, one might also argue that paying to wager on less risky bets represents a greater sensitivity to the *actual* odds of winning and losing. Like Dixon et al., our procedure allowed the subject to purchase control and risk decrement concurrently, so the relative value of each could not be determined. Future research might seek to isolate these variables and assess their relative importance for people with different histories of gambling behavior.

Although the current methodology improved upon that of Dixon et al. (1998), this study is not without its limitations. First, the practical exigencies of conducting the study limited the number of participants we could include. Therefore, it is possible that there were differences between our experienced and inexperienced groups, but the small sample sizes precluded significant findings. Our effect sizes were large for purchase of control (d = 1.06) and medium for decrement of risk (d = .452), which suggests that significant findings might have been obtained had the samples been larger (Hoyle, 1999). However, our results might also have been influenced by the fact that we allowed people with SOGS scores lower than 4 to participate in our study, which might have mitigated differences between players.

Second, although procedures were designed to replicate a casino roulette game as closely as possible, it was clear to participants that they were in a university laboratory participating in an experiment. Therefore, it is possible that the extra stimulus conditions altered typical betting behavior. Almost all the participants overtly wondered about the purpose of the study. In fact, one of the experienced participants even claimed that the study's purpose was to examine his strategy for playing roulette. It is also worth noting that participants were not betting with their own money, and that betting behavior might have been different if their own money was at stake (cf., Weatherly & Brandt, 2004). Despite these limitations, most of the participants reported that they would have placed the same or similar types of bets if they playing roulette in a casino. Given these self reports, it is plausible that the results obtained are accurate reflections of the participant's beliefs about their abilities to control the game, even though evidence of these beliefs was sometimes subtle.

Third, we only assessed illusion of control on the game of roulette. Further replications of this and related research (e.g., Dannewitz & Weatherly, 2007) might address whether illusions of control tend to be more probable with particular games.

A fourth limitation is that we excluded participants with SOGS scores higher than 4. Although it was not our intention to study illusions of control in pathological gamblers relative to non-pathological gamblers, it is not a minor point that individuals with high SOGS scores are more likely to engage in activities that cause difficulties for them and their families. Thus, more research is needed to determine the generality of responding of university undergraduates and "casual" gamblers to those with serious gambling problems.

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DO THE RISK FACTORS FOR PATHOLOGICAL GAMBLING PREDICT TEMPORAL DISCOUNTING?

Jeffrey N. Weatherly, Adam Derenne, and Samantha Chase
University of North Dakota

Weatherly and Dixon (2007) proposed that gambling was related to the increase in how individuals discount delayed (monetary) consequences and that several of the known risk factors for pathological gambling may serve as establishing operations or setting events that lead to such changes. The present study tested these predictions by having participants complete a paper-and-pencil discounting task involving hypothetical monetary consequences and determining whether self-reported measures of the known risk factors would significantly predict participants' rate of discounting. None of the risk factors served as significant predictors of discounting. Interestingly, however, the rate of discounting varied systematically as a function of the number of preference reversals participants displayed at particular delays. The present findings suggest that, if Weatherly and Dixon's proposal is correct, then it likely needs to be assessed using a more diverse sample than college freshmen. The results also suggest that measures of discounting may vary systematically as a function of procedure, which may call for a reevaluation of how discounting data are interpreted.

Keywords: Delay discounting, Gambling, Risk factors

Although many different theories have been forwarded for why people gamble and/or become pathological gamblers (see Petry, 2005, for a review), no universally accepted explanation has yet emerged. Weatherly and Dixon (2007) proposed an integrative behavioral model for gambling based on behavior-analytic principles. Unlike many past behavioral accounts for gambling behavior, the model proposed by Weatherly and Dixon went beyond contingency-driven factors such as intermittent schedules of reinforcement. Rather, the model relied on differences in how gamblers discount delayed consequences, focused on the consequences that maintain gambling, and incorporated verbal behavior.

Address Correspondence to:
Jeffrey N. Weatherly, Ph.D.
Department of Psychology
University of North Dakota
Grand Forks, ND 58202-8380
Phone: (701) 777-3470
Fax: (701) 777-3454
E-mail: jeffrey_weatherly@und.nodak.edu

Delay discounting occurs when the subjective value of a consequence is reduced because it is delayed in time. For instance, when given a choice between receiving some sum of money today and receiving the exact same sum of money one year from today, all but the rare individual would choose to receive the money immediately. Thus, the delay of one year reduces the value of that sum of money below its current value.

Delay discounting has relevance to the study of gambling and gambling problems because research suggests that pathological gamblers discount delayed rewards at a greater rate than do non-pathological gamblers (e.g., Dixon, Marley, & Jacobs, 2003; see Madden et al., 2007, or Petry, 2005, for reviews). In other words, delayed consequences have less control over the behavior of the pathological gambler than of the non-pathological gambler. This finding is consistent with the idea that the factors that control delay discounting may also contribute to the formation of pathological gambling. However, it is also possible that the disorder of pathological gambling precedes changes in how the individual discounts delayed rewards. In other words, although it is possible that how one discounts delayed rewards contributes to pathological gambling, it is also possible that one's experience as a pathological gambler contributes to how one discounts delayed rewards. As is always the case with correlational data, it is also possible that some other, yet unidentified variable could produce both rapid discounting and a tendency toward pathological gambling.

Even if delay discounting contributes to pathological gambling, it is not immediately clear what circumstances would cause a change in how a person discounts delayed rewards and thus makes him or her more susceptible to becoming a pathological gambler. Weatherly and Dixon (2007) proposed a mechanism by suggesting that several of the known risk factors for pathological gambling (i.e., age, gender, socioeconomic status (SES), marital status, ethnic minority status; see Petry, 2005, for a full discussion of the risk factors) may functionally serve as establishing operations (Michael, 1993) or setting events (Kantor & Smith, 1975). These factors may alter the consequences of gambling and change, directly or indirectly, how individuals discount delayed rewards. Weatherly and Dixon further speculated that pathological gambling would be related to a specific consequence of gambling, the attainment of money, more so than other consequences (e.g., attention, sensory experience, escape; see Weatherly & Dixon, 2007, for a complete discussion).

A positive aspect of Weatherly and Dixon's proposal is that it can be tested independently of pathological gambling. If these factors are serving as establishing operations or setting events, then it should be possible to demonstrate that they are related to the rate that individuals discount delayed rewards regardless of whether or not those individuals are pathological gamblers. Furthermore, it should be possible to demonstrate that the rate that individuals discount delayed rewards is related to the consequences that maintain gambling in those individuals.

The present study was an attempt to test these possibilities. Participants were asked to make a series of hypothetical choices between a certain amount of money available immediately and $1,000 available after a delay. Participants' answers were used to calculate how steeply they discounted delayed rewards. Regression analyses were then performed to determine whether the risk factors for gambling were significant predictors of participants' delay discounting. Further analyses were conducted to determine if participants' discounting could predict whether or not participants' gambling behavior was controlled by the attainment of money.

If Weatherly and Dixon's (2007) model is correct, then the risk factors for pathological gambling should be significant predictors of delay discounting and how steeply one discounts delayed rewards should be associated with the monetary consequences of gambling. Furthermore, it should be possible to discover whether one or more of these factors is a greater predictor of differences in delay discounting than are other factors. Should this outcome be true, then researchers and treatment providers alike would have reason to focus their efforts on certain risk factors relative to the others.

METHOD

Participants

The participants were 236 undergraduate students enrolled at the University of North Dakota. Participants were recruited from lower-level psychology courses and received extra course credit for their participation. The demographic information pertaining to the participants can be found in table 1.

Participants were asked to complete a series of questionnaires after providing in

Table 1
Demographic information of the omnibus sample

Gender	101 Males	135 Females		
Age	Mean = 20.89 years (SD = 7.23) Range = 18-67 years			
Ethnicity	9 Hispanic	19 American Indian	1 Asian	1 African American
	2 Native Hawaiian, Pacific Islander		204 Caucasian	
SES	183 <$10,000	15 $10,000-$14,999		13 $15,000-$24,999
	7 $25,000-$34,999	4 $35,000-$49,000		9 $50,000-$74,999
	3 $75,000-$99,999	2 >$100,000		
SOGS	Mean = 1.17 (SD = 2.12)		Range: 0 – 10	
GFA Tangible	Mean = 8.08 (SD = 8.94)		Range: 0 – 25	

formed consent. The first was a demographic questionnaire that ascertained the participant's sex, age, marital status, race/ethnicity, and annual income. These factors were assessed because Weatherly and Dixon (2007) proposed that they are potentially establishing operations or setting events for pathological gambling.

The second measure was the Gambling Functional Assessment (GFA; Dixon & Johnson, 2007). The GFA is a 20-item questionnaire that attempts to assess the consequences that may be maintaining the respondent's gambling behavior. The four potential consequences for gambling are gaining attention, for the sensory experience, a tangible outcome (e.g., winning money), and as an escape. Participants can score between 0 – 30 in each of these categories. Theoretically, the strength of the controlling consequence increases with score and the highest scoring category represents the primary consequence maintaining gambling behavior. The present study focused on participants' score in the tangible category because it is this consequence that Weatherly and Dixon (2007) proposed as being important in the formation and maintenance of pathological gambling.

The third measure was the South Oaks Gambling Screen (SOGS; Lesieur & Blume, 1987). The SOGS is a 20-item questionnaire that attempts to assess the person's history with gambling. It is the most widely used screening measure for pathological gambling (see Petry, 2005). Scores can range from 0 - 20, with a score of 5 or more indicating the potential presence of pathology.

The final measure was a series of hypothetical choices between a certain amount of money available immediately ($1, 50, 100, 250, 500, 750, 900, 950, or 1,000) or $1,000 available after some delay (one week, two weeks, one month, six months, one year, three years, or ten years). Thus, participants made (by circling their preferred option) 63 hypothetical choices. The choices were presented in random order (i.e., the size of the immediate reward and the delay to the $1,000 varied from choice to choice). The choices were presented in list fashion, one after the other, on a total of three sheets of paper.

Analyses

To determine the extent to which individual participants discounted delayed rewards, the point that the participant switched from preferring the immediate reward to the delayed reward was determined for each delay. Because participants were faced with every possible monetary comparison at each different delay presented in random order (vs. presenting the comparisons in linear order at a particular delay until the participant's preference switched and then moving on to the next delay), it was possible for participants to reverse preference more than once at a given delay (i.e., display multiple "changeover" points at a particular delay). Three data sets were therefore created. The first was the sub sample of the 236 participants who only had a single preference reversal or changeover point at each of the seven delays (*n* = 83; 44 female, 39 male). The second was the sub sample of the 236 participants who had displayed multiple changeover points at none or one particular delay (*n* = 141; 77 female, 64 male). At the hypothetical delay for which a participant displayed multiple changeover points, value at that delay was determined by calculating the mean between the two changeover values. The third sub sample was of participants who displayed multiple changeover points at two or fewer delays (*n* = 178; 103 females, 78 males). When multiple changeover points occurred, value was determined as described above. Participants who displayed multiple changeover points at three or more delays (*n* = 58) were ultimately excluded from the analyses because they displayed inconsistency on nearly (or more than) half of the delays.

Each data set was then subjected to two analyses related to delay discounting. In each case, the delays were analyzed in terms of days (see Figure 1). First, the following hyperbolic function was fit to each participant's data:

$$V = A / (1 + kD)$$

In Equation 1, V stands for the subjective monetary value of the delayed reward, A for the amount of the reward, *k* for a free parameter that describes the rate at which discounting occurs, and D for the delay (e.g., Mazur, 1987). For the present study, *k* from Equation 1 was calculated for each participant. Larger values of *k* represent steeper rates of delay discounting. Thus, *k* was used as a dependent measure for participants' rate of discounting.

Equation 1 is theory bound because it makes certain assumptions about the nature of delay discounting (e.g., that discounting follows a hyperbolic function). It is also the case that the distribution of the values of the parameters in Equation 1 is skewed. Thus, a second analysis of discounting was performed. The area under the discounting curve was calculated using the changeover points for each participant (see Myerson, Green, & Warusawitharana, 2001). This measure suffers from neither of the above problems. With this measure, participants who steeply discounted delayed rewards would have smaller values of area under the curve (AUC) than would individuals who did not steeply discount delayed rewards.

Once Equation 1 and the area under the curve were determined for each participant's data, several regression analyses were performed. Specifically, each participant's age, gender, SES (defined by the participant's self report of annual income measured on an ordinal scale), marital status (single, married, divorced, or widow/widower), ethnic minority status (Hispanic/Latino, American Indian, Asian, Black/African American, Native Hawaiian/Other Pacific Islander, or White) and SOGS score were numerically coded and used as predictor variables in a backward regression with either *k* or the area under the curve serving as the dependent variable. This particular regression analysis was chosen because it determines each factor's explanatory power independent of the other factors in the model. These analyses tested the hypothesis that the risk factors for pathological gambling would predict how individuals discount delayed rewards.

Finally, for each data set, participants' *k* or AUC values were used as predictor variables for their cumulative score on the "tangible" questions on the GFA (Dixon & Johnson, 2007). These analyses tested the hypothesis that differences in how individuals discount delayed rewards would be predictive as to whether money served as the maintaining consequence for gambling behavior.

RESULTS

Figure 1 presents the discounting data for the mean of all participants in each of the three groups. The solid line represents the best fit function using Equation 1. The value of *k* for that fit is also presented in each graph. The results of the regression analyses conducted on each data set, for both the value of *k* and the AUC, are presented in Table 2. In no instance in the six analyses did participants' age, gender, SES, marital status, ethnicity, or SOGS score serve as a significant predictor of either *k* or AUC, although in several instances individual factors did approach significance. Furthermore, the total variance accounted for by any individual factor was small, never exceeding 3%.

The *k* and AUC values for each data set presented in Figure 1 were also used as predictor variables for individuals' "tangible" score on the GFA. The results of these tests are presented in Table 3. As can be seen in Table 3, neither *k* nor AUC was a significant predictor of participants' "tangible" GFA score in any analysis. Furthermore, the amount of variance for by either factor was negligible.

DISCUSSION

Weatherly and Dixon (2007) suggested that several of the known risk factors for pathological gambling may be serving as establishing operations or setting events that alter the value of the consequence maintaining gambling (i.e., money). This alteration would lead individuals to discount delayed monetary rewards more steeply than when the risk factors are absent. The present study attempted to test this suggestion by determining whether the risk factors would be significant predictors of how participants discounted delayed monetary rewards. None of the risk factors (nor participants' scores on the SOGS) were predictive of participants' level of discounting.

Weatherly and Dixon (2007) also suggested that pathological gambling would be associated with one type of consequence, money. Given that steep discounting is associated with pathological gambling, the present study tested whether discounting would significantly predict whether participants' gambling was maintained by monetary consequences. Participants' discounting was not predictive of how strongly monetary consequences maintained gambling behavior.

Failing to find that the known risk factors for gambling are predictive of how steeply participants discount delayed rewards runs contrary to the predictions of Weatherly and Dixon (2007). There are several possible explanations for this failure. For instance, the present sample consisted mostly of university freshmen and thus several of the risk factors related to pathological gambling, such as age, marital status, and SES, may have been artificially constrained. Furthermore, because of the population of the upper Midwest of the United States, the present sample may have also provided a limited test of ethnicity.

A remaining possibility is that Weatherly and Dixon's view of the risk factors for pathological gambling as potential establishing operations or setting events is incorrect. For instance, one could argue that establishing operations or setting events operate at the level of individual participants whereas the risk factors for gambling are correlations that exist across a population. Thus, one should not necessarily expect the risk factors to significantly predict individuals' discounting. A full discussion of this issue is beyond the scope of the present paper. However we would argue that such a view diminishes, if not eliminates, the value of risk factors if they can never be

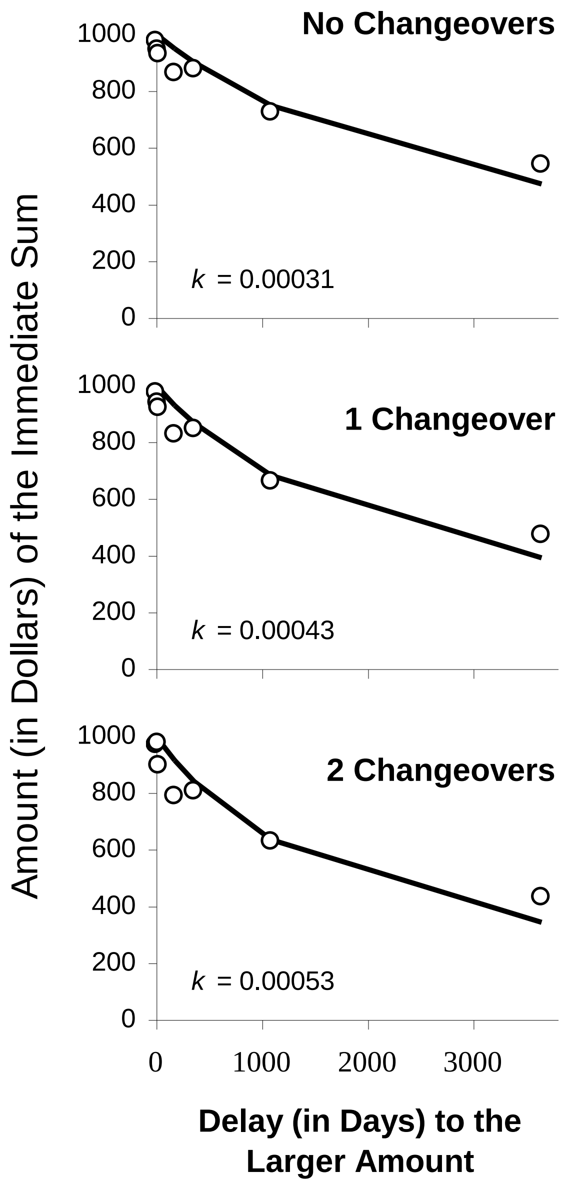

Figure 1. Discount functions for participants dependent on the number of changeovers in responses

used to predict individuals' behavior.

The present data also failed to support Weatherly and Dixon's (2007) prediction that pathological gambling is associated both with steep discounting and to one type of consequence for gambling, money. Again, it is possible that Weatherly and Dixon's proposal was incorrect. It is also possible that the failure to observe this relationship was related to the potential problems with the sample (see above). Another potential reason for this failure is the measure used to determine the consequences maintaining participants' gambling. Although Dixon and Johnson (2007)

Table 2
Results from the regression analyses conducted on the data used to construct Figure 1.

Factor	Coefficient	Beta Weight	*t*	Significance	Semi-Partial R^2
No Changeovers DV = k					
Age	.000	-.009	-.058	.954	.000
Gender	-.002	-.107	-.865	.390	.010
SES	.000	-.035	-.266	.791	.001
Marital Stat.	.000	.018	.003	.910	.000
Ethnicity	.000	.020	.162	.871	.000
SOGS	.000	.038	.318	.752	.001
No Changeovers DV = AUC					
Age	.008	.165	1.045	.300	.013
Gender	.084	.178	1.479	.143	.027
SES	-.013	-.105	-.822	.413	.008
Marital Stat.	-.016	-.093	-.594	.554	.004
Ethnicity	.023	.158	1.325	.189	.022
SOGS	.021	.148	1.248	.216	.019
One or less Changeovers DV = k					
Age	.000	.119	.997	.321	.007
Gender	-.001	-.022	-.248	.805	.018
SES	-.001	-.071	-.708	.480	.006
Marital Stat.	.000	.021	.196	.845	.004
Ethnicity	.000	-.005	-.049	.961	.007
SOGS	-.001	-.056	-.621	.536	.007
One or Less Changeovers DV = AUC					
Age	-.006	-.162	-1.406	.162	.013
Gender	.084	.158	1.886	.061	.024
SES	.002	.015	.152	.879	.000
Marital Stat.	-.004	-.025	-.236	.814	.000
Ethnicity	.022	.137	1.480	.141	.015
SOGS	.013	.100	1.147	.253	.009
Two or Less Changeovers DV = k					
Age	.000	.085	.827	.409	.004
Gender	.000	-.012	-1.56	.876	.000
SES	-.001	-.081	-.901	.369	.005
Marital Stat.	.001	.061	.649	.517	.002
Ethnicity	.000	-.012	-.143	.887	.000
SOGS	.000	-.053	-.675	.500	.003
Two or Less Changeover DV = AUC					
Age	-.004	-.099	-.982	.327	.005
Gender	.074	.136	1.808	.072	.018
SES	.017	.093	1.054	.293	.006
Marital Stat.	-.014	-.078	-.840	.402	.004
Ethnicity	.016	.099	1.157	.249	.007
SOGS	.012	.085	1.101	.272	.007

designed the GFA to measure weather "tangible" outcomes were maintaining gambling behavior, recent evidence suggests that the GFA may identify whether positive reinforcement is maintaining gambling behavior, but may not necessarily accurately delineate between the potential positively reinforcing consequences (e.g., tangible vs. sensory experience; Miller, Meier, Muehlenkamp, & Weatherly, in press).

Table 3
Results from the regression analyses when k or AUC were used to predict participants' "tangible" score on the GFA for each of the three datasets.

Factor	Coefficient	Beta Weight	*t*	Significance	Semi-Partial R^2
No Changeovers					
k	-.29.956	-.035	-.319	.750	.001
AUC	2.040	.054	.488	.627	.003
One or Less Changeovers					
k	54.950	.118	1.405	.162	.014
AUC	-.126	-.004	-.043	-.043	.000
Two or Less Changeovers					
k	56.411	.112	1.515	.132	.013
AUC	.815	.025	.338	.736	.001

The present data also highlight another, unexpected reason why our hypotheses were not supported. Namely, the procedure used in the present study to determine participants' delay discounting did not reliably produce a single preference reversal at each delay. It did, however, produce reliable changes in rates of discounting as a function of the number of multiple preference reversals participants displayed at different delays. This result may constitute the main contribution of the present paper.

Figure 1 demonstrates that how rapidly participants discounted the delayed monetary consequence increased as individuals who displayed multiple changeover points across the seven different tested delays were added to the sample. Because the 83 participants who did not display multiple changeovers are included in the calculations for all three graphs, this increase in discounting is completely due to individuals who had multiple changeovers at one or two delay points. Furthermore, this change in discounting was not trivial. The value of *k* increased 71% from the group displayed in the top graph of Figure 1 to the group displayed in the bottom graph[1].

The delay-discounting task in the present study consisted of 63 choice combinations. These choices were randomly ordered and participants answered all of them. This method was chosen because randomly ordering the choices would theoretically guard against order effects. Doing so also seemed to provide face validity in the sense that individuals are rarely faced with a series of choices that vary systematically along one continuum (e.g., amount) when all other factors remain constant (e.g., delay). Rather, "real life" choices typically different along a number of continuums from choice to choice. However, using the current procedure, the result was that the vast majority of participants displayed multiple preference reversals at one or more delays.

[1] Given the changes in the rate of discounting across the graphs in Figure 1, one could legitimately ask whether participants who displayed no, one, or two multiple preference reversals represented distinct populations. To test this possibility, the analyses outlined in the results were conducted using only those participants who displayed one or two multiple changeovers. These analyses yielded only one major change compared to those presented in the results. Specifically, age and marital status were significant predictors of *k* for those individuals who displayed multiple preference reversals at two (and only two) delays. Discounting tended to be steeper for younger and single participants. The predictive relationship of ethnicity approached, but did not reach, significance (*p*=.054). None of the risk factors were significant predictors of AUC. Furthermore, none of the risk factors were significant predictors of *k* or AUC for those participants who displayed one (and only one) multiple preference reversal.

The procedure used to ascertain participants' rate of delay discounting in the present study is not the only one that has been used. Ostaszewski, Green, and Myerson (1998), for instance, had participants respond to a series of choices at a particular delay with the amount of the immediate option varying systematically in either an ascending or descending sequences. Participants in this study experienced both sequences across the procedure, a practice recommended by some (e.g., Critchfield & Kollins, 2001). Du, Green, and Myerson (2002), on the other hand, used an adjusting procedure in which participants were originally presented with an immediately available amount that was a certain percentage of the delayed amount. Depending on the participant's choice, the next immediately available amount was adjusted upwards or downwards and this process continued until a changeover point was determined for that particular delay.

Both of these techniques make multiple changeover points improbable (although one could argue that a different changeover point could be established for ascending vs. descending sequences or if the adjusting procedure was repeated). However, although these procedures avoid the problem that occurred in the present procedure, they are highly artificial. The systematic nature of presenting the questions creates order effects. In fact, one could argue that the intention is to create an order effect.

However, before one dismisses the changes in the present data as procedural artifacts, it is worthy of noting that an alternative interpretation exists. That is, the individuals who displayed multiple changeovers may not have done so because of the procedure, but rather because these individuals were insensitive to the presented choices relative to individuals who did not display multiple changeovers. Representing discounting for these individuals as a single function may thus be potentially misleading. In other words, these individuals may have had a range of indifference points at each delay, not a single one. This idea is worth exploring in the future. Individuals who display this "range" of indifference may be unique relative to individuals who do not. Furthermore, such an interpretation may alter conclusions that are drawn from studies of delay discounting in general.

A final procedural aspect that requires addressing is the fact that the present procedure, and the procedures used in myriad published studies, asked participants to make hypothetical choices. It is unclear how this fact influences the results. Research from our laboratory (Weatherly & Brandt, 2004; Weatherly & Meier, 2007) has shown that participants in laboratory studies of gambling become more conservative in their gambling as the value of what they are gambling increases. If the same result held true in studies of delay discounting of monetary rewards, then one would expect steeper discounting when hypothetical, rather than "real," choices were required.

The value of the present study may lie in the systematic changes in the main dependent variable as a function of whether a single preference reversal could be identified. Given that researchers have made much ado about the association between delay discounting and pathological gambling, finding such systematic changes is a major concern. Have those associations been based on data sets that contain similar systematic changes? Do procedures designed to avoid these systematic changes result in a valid representation of the individuals' delay discounting? Do multiple changeovers represent ranges of indifference rather than a particular value of a delayed consequence? Do hypothetical choices generalize to actual choices? Does discounting measured in the laboratory accurately predict how the individual actually behaves? These questions, and many additional ones, are worthy of further investigation.

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SLOT-MACHINE PREFERENCES AND SELF-RULES

Terje Fredheim, Kai-Ove Ottersen, & Erik Arntzen
Akershus University College

The present study was a replication and extension of Zlomke and Dixon (2006) investigating the impact of contextually trained discriminations on slot-machine gambling. In each of two experiments, 20 participants were exposed to two concurrently available slot-machines differing only in color. Thus, Experiment 1 was a replication, while in Experiment 2 we included an instruction to ensure that the participants attended to all of the onscreen stimuli. Following a pretest of slot machine preferences, a nonarbitrary relational training and testing procedure was used to establish contextual functions of MORE-THAN and LESS-THAN for two cues. After relational training the participants were exposed to a posttest identical to the pretest. The results of Experiment 1 showed that only a small number of the participants allocated their posttest responses to the slot machine that shared nonarbitrary properties with the contextual cue for MORE-THAN. In Experiment 2, the posttest showed that an increased number of participants who reported having attended to the contextual stimulus increased their preference to gamble on the yellow slot machine.

Keywords: Gambling, slot-machines, non-arbitrary relational training, self-rules, transformation of functions, instructions.

There has been an increase in gambling related problems over the last decade. The literature describes a prevalence of pathological gambling usually between 1-3%, but some studies report prevalence rates up to 10% (e.g., Petry, 2005). Oren and Bakken (2007) found that about 0.7% of people aged between 16 and 75 years in Norway reported gambling problems. However, it is important to be aware that there are no casinos in Norway. Thus, Norwegian gamblers may participate in different betting games hosted by Norsk Tipping, a governmental company that control gambling in Norway. A Norwegian study showed that slot-machines were a highly preferred form of gambling: 61% of the total amount of money spent on gambling was related to slot machines (Oren & Bakken, 2007; Stiftelsestilsynet, 2006).

The behavior analytic approach to understanding gambling is a growing field. Thus, many authors have argued that a behavioral model of gambling would extend and help us to understand variables related to gambling. Furthermore, such an approach would make possible effective treatment for pathological gamblers (Dixon, 2007; Ghezzi, Lyons, Dixon, & Wilson, 2006). There are several variables that seem to be important for the understanding and analysis of gambling behavior. For example, gambling behavior will occasionally lead to reinforcement. A well-known fact is that behavior maintained by intermittent reinforcement is known to have a high, stable response rate and resistance to

Address Correspondence to:
Erik Arntzen
Akershus University College
P.O. Box 423, 2001
Lillstrom, Norway
Email: erik.arntzen@equivalence.net

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extinction (Ferster & Skinner, 1957). Intermittent reinforcement can be one of several reasons why people continue gambling and it seems important to find out why people choose to gamble on specific slot machines or other games. Factors like stimulus control, contextual control by sound, light or colors and verbal behavior must be manipulated and analyzed to see if these factors can control and predict gambling behavior.

Gambling behavior leads to many problems and therefore, it is important to find out more about the variables that lead to or maintain gambling and pathological gambling in order to help people suffering from problems related to gambling. Experiments with people in real gambling environments could, of course, give us relevant knowledge, but it is difficult to conduct experiments with participants' own money, mainly for ethical reasons. With respect to problems with generalization, we might simulate gambling in controlled settings, using technological solutions and artificial reinforcers, even though this is far from a real gambling situation (Weatherly & Meier, 2007; Weatherly & Phelps, 2006). By using recreational gamblers as participants, experiments with simulated gambling have been conducted by some researchers (e.g., Daugherty & MacLin, 2007; Dixon & Schreiber, 2002; MacLin, Dixon, & Hayes, 1999; Weatherly, Austin, & Farwell, 2007).

For instance, Zlomke and Dixon (2006) conducted an experiment showing that slot-machine gambling can come under contextual control by using conditional discrimination training. First, the participants gambled on simulated slot-machines on a PC (MacLin, Dixon, Robinson, & Daugherty, 2006). Nine participants could chose between two concurrently available slot-machines differing only in the colors, yellow and blue. After playing the slot-machines, the participants were trained to choose a comparison stimulus greater than the sample stimulus with a yellow contextual cue present, and to choose a comparison stimulus less than the sample stimulus with a blue contextual cue present. Lastly, the participants were presented with the same simulated slot-machines. The results showed that eight of nine participants allocated most of their responses to the yellow slot machine after conditional discrimination training.

Recently, two studies have tried to replicate Zlomke and Dixon's (2006) findings. The first study by Hoon, Dymond, Jackson, and Dixon (2007) reported mixed success with several variations of the original training procedure. The second study by Hoon, Dymond, Jackson, and Dixon (2008) replicated Zlomke and Dixon (2006), although the change in preferences was not as strong. Despite the small differences in subsequent replications and extensions, Zlomke and Dixon (2006) argued that self-rules acquired through conditional discrimination training can maintain certain responses related to slot machine gambling. Their explanation was related to transformation of functions (see Dymond & Rehfeldt, 2000), which is said to occur when the functions of one stimulus are altered or transformed by virtue of the derived relation between it and another stimulus. The differing procedures employed and results obtained from the Hoon et al. (2007, 2008) studies indicates that more research needs to be conducted to contribute to a better understanding of transformation of functions related to gambling behavior.

The purpose of the current study was to run two experiments with a Norwegian sample of participants by manipulating two contextual cues. In the first experiment, we wanted to replicate and further extend the study of Zlomke and Dixon (2006). In the second experiment, we introduced an instruction to ensure that the participants attended to all the stimuli on the screen.

EXPERIMENT 1
METHOD

Participants

Twelve women and eight men over 18 years old, all students or fulltime workers, participated in this experiment. Everyone reported knowledge of slot machines. None reported any gambling problems. The two first authors recruited participants, and participation was voluntary. Everyone was told that they could withdraw from the experiment whenever they wanted to do so. After the experimental session, participants received a booklet about behavior analysis.

Apparatus and setting

The experimental sessions took place in small rooms (3.5 meters by 4 meters) containing a chair, a desk, office equipment and a computer. Participants were alone in the room during the experiment, but one or both of the two first authors were available for questions in the room next door. A computer controlled presentation of stimuli and data collection. The software program was made by Mark Dixon and coworkers in Microsoft® Visual Basic 6.0, but we used Microsoft® Visual Basic 2008 Express Edition to run it. Three IBM-compatible laptops, one containing an Intel® Pentium® M 1,73 GHz processor and 512 MB RAM, and two containing an Intel® Pentium® 1.66 GHz processor and 512 MB RAM ran the Microsoft Windows XP Professional operating system, version 2002 with Service Pack 2 were used in the experiment.

Procedure

Slot-Machine Task Pretest. The purpose of this pretest was to acquire baseline data on participants' response allocation toward two simulated slot-machines that were equal concerning pay-off probability and reinforcement magnitude, but differed in color. One of the slot-machines was yellow, and the other slot-machine was blue. This phase of the experiment started with the following instructions displayed on the computer screen (the text in Norwegian was available on the table beside the PC):

> On the following screen you will see a button in the middle of the screen. When you click on the button with your mouse, two slot machines will be revealed. Click your mouse on the slot machine you would like to play and earn as many points as possible.

The experimenter answered any questions by repeating the instructions in Norwegian and then left the room. Then, two buttons were presented on the screen. One of the buttons was blue with the text "Slot Machine 1", and the other button was yellow with the text "Slot Machine 2". The buttons were approximately 4 x 8 cm. A mouse click on either button resulted in the presentation of a slot-machine with the same color as the button selected. Each participant started a trial by clicking a button with the text "Spin". Clicking the spin-button resulted in spinning the machine reels for approximately 3s and one credit being subtracted from the participants "cumulative credits" (initially set at 100). Three identical symbols on the payoff line resulted in two credits added to "cumulative credits" and the text "AWESOME… YOU WIN!!" presented on the screen. Any other variation on the pay-off line resulted in removal of the initially bet credit.

A button with the text "Press HERE to continue" was presented on the screen, and by clicking this button trials were repeated as described above. To avoid the possibility for position bias, the blue and yellow buttons were randomly positioned on either side of the screen across trials. In addition, an observer response was instated between all trials, by the presentation of a button with the text "Click here".

Each slot-machine was programmed on a RR schedule of reinforcement with a probability of reinforcement of .5 and the magnitude of reinforcement was held constant. The RR sequence was generated by the program,

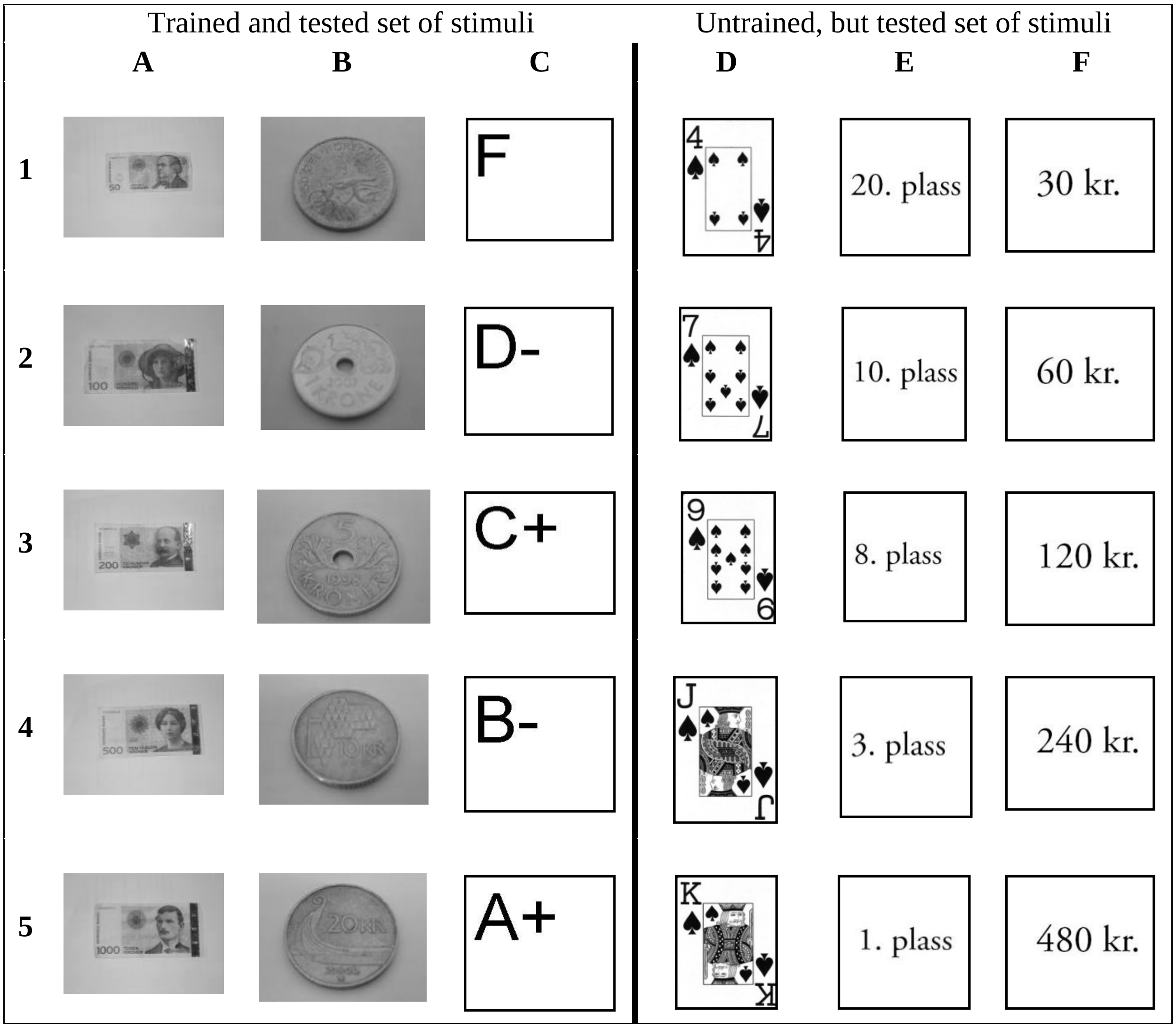

Figure 1. Overview of the stimuli sets which were used in the conditional discrimination training and tests.

and resulted in identical sequences and density of trial outcomes for each participant, as well as identical amount of reinforcers obtained. Each participant ended this task after 50 trials with 100 credits.

Conditional Discrimination Training. Following the slot-machine pretest, conditional discrimination training was conducted to establish the relations of less than (blue) and greater than (yellow). In this condition, the participants were instructed to choose one of three comparisons presented below a single sample stimulus, by mouse clicking one of the comparisons (i.e., only one of the three comparisons would be the correct one in presence of a sample stimulus). There was never two comparisons worth "more than" sample if the contextual cue indicated more than. Similarly, there was never two comparison worth "less than" the sample if the contextual cue indicated less than. Six sets of five stimuli and two contextual cues were used during this procedure. Each of the six sets contained five images or words, and the contextual cue was presented as a blue or yellow rectangle behind the comparisons.

As shown in Figure 1, each of the six sets represented a continuum from least to most. Three of the sets was stimuli related to gambling (playing cards, bills, and coins), while three of the sets not was related to gambling (letter grades on universities, placement in competitions, and written amounts). For example, Set B included pictures of a Norwegian ”50-oring” coin, ”1-krone” coin, ”5-krone” coin, ”10-krone” coin and ”20-krone” coin. The pictures were approximately 5 x 5 cm. The contextual cue was approximately 20 x 8 cm.

At the beginning of the conditional discrimination training condition, the following instructions were presented on the screen (the text in Norwegian was available on the table beside the PC):

> You are going to see five images presented on your screen: one image on top, three on the bottom, and one larger image surrounding the three on the bottom. Your job is to choose one of the three images on the bottom of your screen by clicking on it with the mouse. When you are correct, you will receive one point. Incorrect responses will not result in awarded points. Please try to earn as many points as you can. The more points you earn, the quicker you will finish. There will be parts of the experiment where feedback is not given. The computer is still keeping track of your responses so continue to do your best. Do you have any questions?

The experimenter answered any questions by repeating the relevant part of the instructions in Norwegian and then left the room. During the training phases, a point counter was visible. The counter displayed the cumulative points earned by each correct choice. In addition, a correct answer resulted in the text “Correct” and a 1 s chime. Incorrect choices resulted in the text “wrong” and a 1 s chord. The relations of greater than and less than were trained in three separate phases using three sets of stimuli. Number of trials to criterion in training and test phase was pre-programmed by Dixon and coworkers. There were no limits for number of trials for each participant, and participants were requested to leave if they did not reach mastery criterion.

Less than. The purpose of this phase was to train the relation of less than. When the sample stimulus was presented, comparisons were presented with a blue contextual cue. A click on the comparison less than sample stimulus resulted in the programmed positive consequence. A click on any other comparison resulted in the programmed negative consequence. For example, when the ”5-krone” coin was shown as sample, with the ”1-krone” coin, the ”10-krone” coin and the ”20-krone” coin as comparisons, clicking the ”1-krone” coin would be the correct response in Phase 1. Stimuli from sets A, B, and C were randomly presented. Each block consisted of 30 trials, and 27 correct answers resulted in advance to the next phase. If this criterion was not met, the block of 30 trials was re-presented.

Greater than. The purpose of this phase was to train the relation of greater than. When the sample stimulus was presented, comparisons were presented with a yellow contextual cue. A click on the comparison greater than sample stimulus resulted in the programmed positive consequence. A click on any other comparison resulted in the programmed negative consequence. For example, when the ”10-krone” coin was shown as sample, with the ”1-krone” coin, the ”5-krone” coin and the ”20-krone” coin as comparisons, a click on the ”20-krone” coin would be the correct response in Phase 2. Stimuli from sets A, B, and C were randomly presented. Each block consisted of 30 trials, and 27 correct answers resulted in advance to the next phase. If this criterion was not met, the block of 30 trials was re-presented.

Mixed less than and greater than. During this phase, blue and yellow contextual cues were presented randomly 30 times each in a 60-trial block. A correct answer had to meet

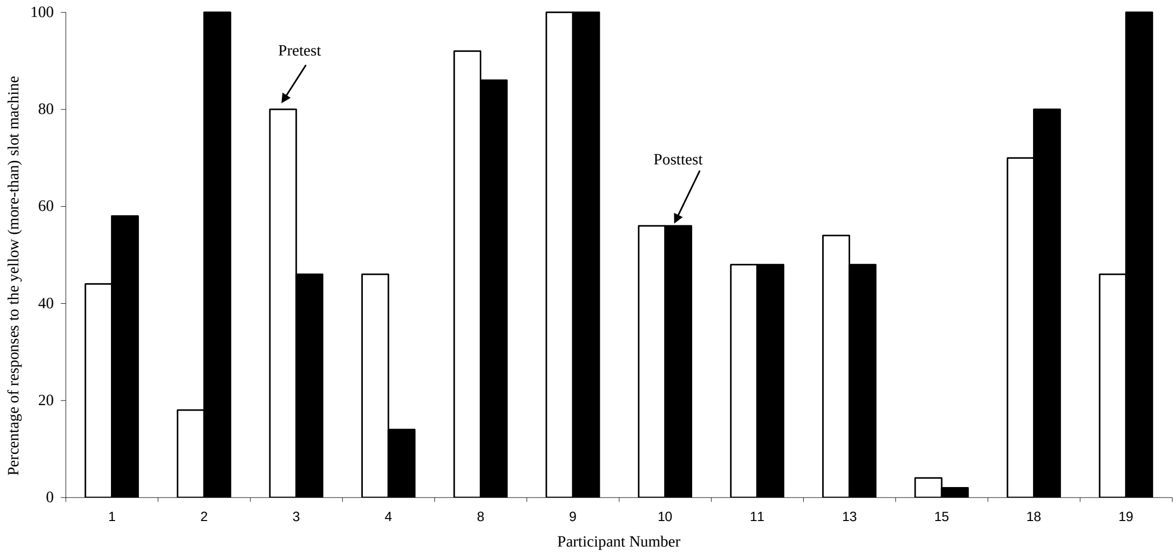

Figure 2. Percent of responses on the yellow slot machine in pre- and posttest in Experiment 1.

the criterion described in Phase 1 and 2. The same stimulus sets as used in Phase 1 and 2 were used. Each block consisted of 60 trials, and 55 correct answers resulted in advance to the next phase. If this criterion was not met, the block of 60 trials was re-presented.

Test. This phase consisted of 120 trials. In addition to stimulus sets A, B, and C, the novel stimulus sets D, E, and F were used to test if the trained relations between contextual cue and comparisons are applied to novel stimuli. The criterion for correct and incorrect choices was the same as in the past phases. Before the first trial in Phase 4, the following text was displayed on the screen: “You will no longer receive feedback following your responses. Continue to do the best you can. The computer is recording your score” (Available on the table was a Norwegian translation). No feedback or points were provided at any time during this test. The criterion for completion of Phase 4 was 103 correct answers in a block of 120 trials. If this criterion was not met, Phase 3 (Mixed training) was re-presented. Completion of Phase 3 then resulted in presentation of a 120-trial block in Phase 4, and so one until participants met criterion.

Slot-Machine Task Posttest

The purpose of this task was to determine whether the participants had changed their preferences and allocated their responses differently than in the pretest. Participants were re-exposed to the exact same slot-machines and conditions as in the pretest.

RESULTS

Twelve participants reached the trials to criterion and finished Experiment 1 (see Figure 2). At pretest, participants chose the yellow slot-machine between 4% and 100% (*M* = 55%, *SD* = 27.9). The blue slot-machine was chosen between 0% and 96% (*M* = 45%, *SD* = 27.9) at pretest. These findings indicate that some of the participants showed a preference for one of the two slot-machines before conditional discrimination training. Twelve participants who completed conditional discrimination training in Phase 1 took between one and seven blocks to meet criteria (*M* = 2), in Phase 2 from one to three blocks (*M* = 2), and in Phase 3 between one to four blocks

Table 1
Data from participants who failed to complete the experiment

Experiment	Participant	Quit During Phase	Number of training blocks in final phase	Number of training trials in final phase	Variation in number of correct responses in final phase		Total number of minutes before requesting to leave
					Lowest - highest	Mastery criteria	
1	5	3	11	644	32-40	55	101
	6	3	21	1240	37-48	55	136
	7	3	12	737	14-29	55	72
	12	2	13	397	10-20	27	69
	14	3	21	1272	30-40	55	121
	16	3	16	918	19-33	55	85
	17	4	3	349	87-97	103	120
	20	3	27	1611	20-34	55	176
2	23	3	6	389	31-48	55	92
	28	4	2	909	39-60	103	133

(*M* = 2). All twelve participants reached the criterion in Phase 4 in one block. Only four of the twelve participants played more on the yellow slot-machine in the posttest; three participants gambled equally on the slot-machines in pre- and posttest, and five participants gambled less on the yellow slot-machine during posttest, as shown in Figure 2. On average, the participants chose to play 55% on the yellow slot-machine in the pretest and 62% on the yellow slot-machine in the posttest. A t-test indicated that the difference between pre- and posttest was not statistically significant (t (11) = 0.49, ($\alpha = 0.05$)).

Table 1 shows data for eight participants who chose to withdraw from the experiment before they had completed discrimination training. Session-length for these 8 participants ranged between 69 to 176 minutes (*M* = 110 minutes), while the participants who completed the conditional discrimination training phase took only 35 minutes on average. In summary, 12 out of 20 participants completed all phases of the Experiment 1, but only 4 showed an increase in preference for the yellow slot machine at posttest.

DISCUSSION

The results from Experiment 1 did not replicate the findings of the Zlomke and Dixon (2006) study. The participants in the current study showed much more variation in their allocation of responses between the slot-machines than participants in Zlomke and Dixon (2006). Our findings from the 12 participants who completed the experiment show an average increase in preference of 7% for the yellow slot-machine, while Zlomke and Dixon (2006) reported a 32% increase. There are several possible explanations for this. First, we used another version of the simulated slot-machines. Our participants choose slot-machines by clicking yellow or blue quadrangle with the written words ”Slot Machine 1” or ”Slot Machine 2”. Participants in Zlomke and Dixon (2006) choose between two concurrently slot-machines, and clicked the one they wanted to continue with for the gambling. The differences in procedures may not be essential since the total number of clicking-responses to access the preferred slot-machine were the same in both experiments. Second, the version we used required at least 240 trials during conditional discrimination training. Zlomke and Dixon’s (2006) version required at least 136 trials. This indicates that the participants in the current study were exposed to more trials in the conditional discrimination training in training yellow color to "more-than" than the participants in Zlomke and Dixon (2006). Nevertheless, the participants in the current study showed a lesser change in preference than in Zlomke and Dixon (2006). Third, we replaced the US training stimuli (pictures of money) with Norwegian training stimuli, we translated written words to Norwegian, and amount of money (US $) was calculated to Norwegian kroner (NOK). We did this to avoid unfamiliarity with the training stimuli from influencing the results. Fourth, verbal reports from at least one participant told us that it was possible for the participants to reach trials to criterion for all phases in conditional discrimination training without paying attention to the contextual cue. This is possible because to avoid that more than one comparison stimulus could be "the right one" at the same time, only one of three comparisons would be "more-than" or "less-than" sample stimulus, as pointed out in Hoon et al. (2007). Two comparisons would always be "the wrong ones". Participants could choose the comparison that was the only one "more-than" or the only one "less-than" sample stimulus and receive feedback, and reach trials to criterion in all phases, without noticing the color of the contextual cue. Eight of twenty participants did not continue with the experiment after struggling to reach trials to criterion in the conditional discrimination training. In contrast, all nine participants in Zlomke and Dixon's (2006) study met the criterion for conditional discrimination training and finished the experiment.

It is possible that instructions could influence different types of attending behavior. Some studies have discussed the influence of general and specific instructions in conditional discrimination procedures (Arntzen, Vaidya, & Halstadtro, in press; Pilgrim, Jackson, & Galizio, 2000; Smyth, Barnes-Holmes, & Barnes-Holmes, 2008) and there is need for further research. Therefore, the purpose of Experiment 2 was to study the effects of extra instructions on the importance of attending to all stimuli on the screen. The instruction was given to the participants who did not reached trial to criterion within a time limit in training conditional discrimination. A short post-experimental interview was conducted to determine if participants noticed the contextual cue during the conditional discrimination training.

EXPERIMENT 2
METHOD

Participants

In the current experiment twenty adults participated, eleven women and nine men. Everyone was more than eighteen years old and had a full time job. All the participants said they had knowledge about slot machines, but no one reported when asked to have any gambling problems. The participants participated voluntarily and were recruited by the two first authors. Before the experimental session started, everyone was told that they could withdraw from the session at any time. After the experiment, all participants received a booklet about applied behavior analysis.

Procedure

The procedure was the same as in Experiment 1 except for two important differences. First, if a participant had not finished the experiment after sixty minutes, the experimenter interrupted the study, repeated the start instruction and emphasized to the participant that they should attend to all the five different images on the screen. The experimenter pointed to the image on top of the screen, the three below and the large image that encompassed the three below to draw participants' attention to the contextual cue of the background color. Second, we conducted a brief interview with every participant who finished the experiment. The following question was asked: "How did you solve the task where

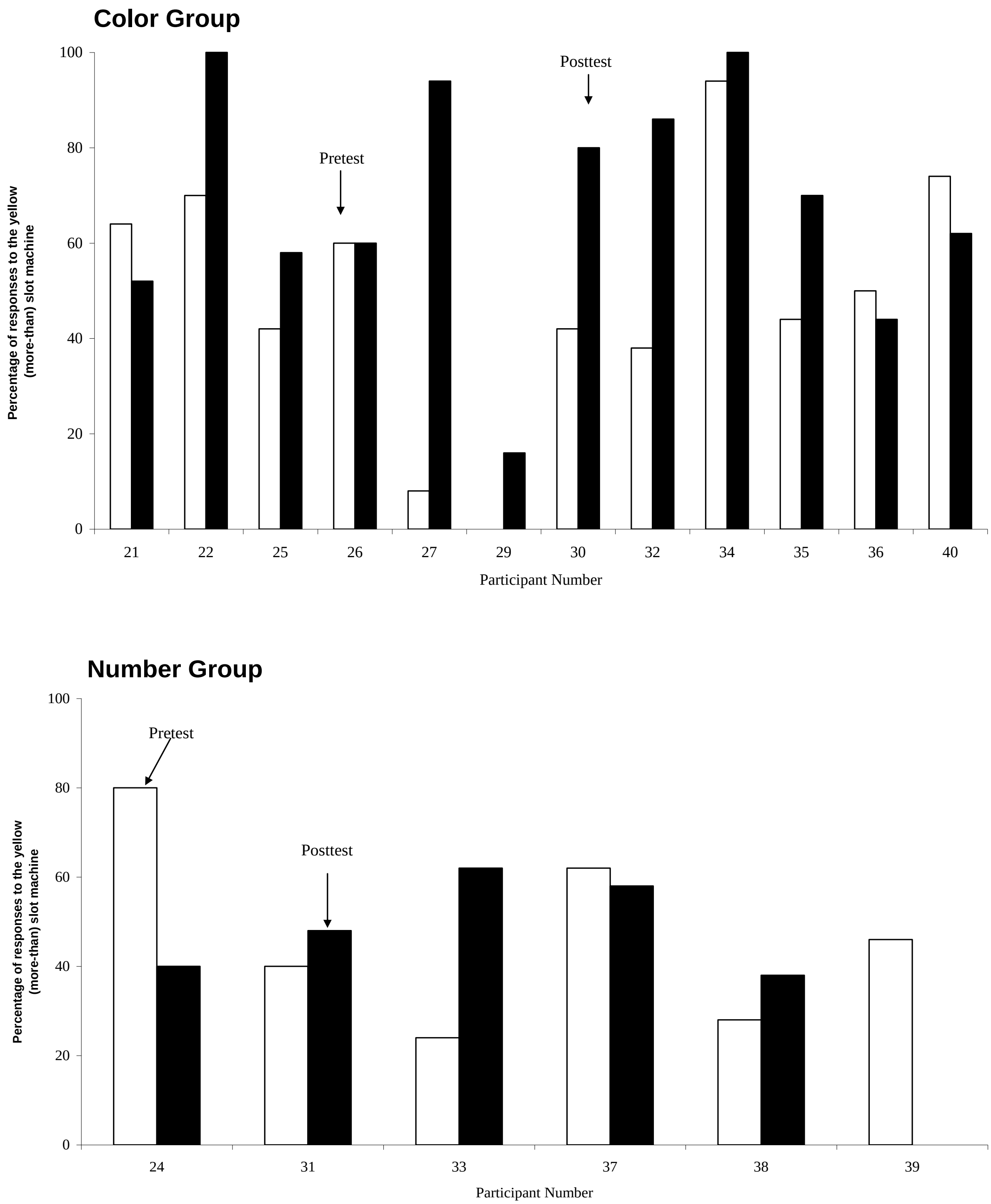

Figure 3. The upper panel shows the percent of responses on the yellow slot machine in pre- and posttest for the Color Group (the participants that reported to have attended to the contextual stimulus in the training phase) in Experiment 2. The lower panel shows the percent of responses on the yellow slot machine in pre- and posttest for the Number Group (the participants that reported not to have attended to the contextual stimulus in the training phase) in Experiment 2.

you were going to choose between three images?" We asked the question to determine if the participants had attended to the color of the contextual stimulus or the number of comparison stimuli.

RESULTS AND DISCUSSION

Eighteen participants finished Experiment 2. In the pretest, the choices for the yellow slot machine were from 0% to 94 % (*M* = 48%, *SD* = 24.3), while the blue slot machine was chosen from 6% to 100% (*M* = 52%, *SD* = 24.3). This finding indicates that some of the participants had a preference for one of the slot machines before the conditional discrimination training was introduced. Thus, the finding is also in accordance with the results in Experiment 1.

The eighteen participants who finished the conditional discrimination training in Phase 1 took between one and nine sessions (*M* = 3 sessions), between one and five sessions (*M* = 2 sessions) in Phase 2, and between one and fifteen sessions (*M* = 3 sessions) in Phase 3. All participants, except for one, finished Phase 4 in one session. Participant #40 finished Phase 4 in two sessions. In Experiment 1, twelve of twenty participants (60%) finished the experiment, while eighteen of twenty participants (90%) finished Experiment 2 (see Table 1). Therefore, it seems reasonable to presume that the detailed instruction was effective. Two of the participants in Experiment 2 did not finish the conditional discrimination training and were not exposed to the post-test. Participant #28 reached the criterion in Phase 3 two times, but did not reach the criterion in Phase 4. Thus, the participant was not re-exposed to Phase 3 and did not finish the experiment.

In the analysis of the results, the participants were divided into two groups dependent on the answers in the post-experimental interview. That is, one group consisted of the participants who reported that they had chosen the comparison stimulus by looking at the color of the contextual stimulus (Color Group), while the other group consisted of the participants who reported to have chosen the one comparison stimulus that was either greater or smaller than the sample stimulus, independent of the color of the contextual stimulus (Number Group). The Color Group consisted of twelve participants, eight of whom gambled more on the yellow slot machine in the posttest than in the pretest, as shown in Figure 3. One of the twelve participants gambled the same on the yellow and the blue slot machine in pretest and posttest. Furthermore, three of the twelve participants gambled less on the yellow slot machine in the posttest. Participants # 21, 22, and 25 received the detailed instruction. Participants in the Color Group gambled a mean of 49% of their responses on the yellow slot machine in the pretest and 69% on the yellow slot machine in the posttest. A t-test indicated a statistically significant difference: t (11) = 0.04 (α = 0.05). This indicates that the procedure was effective in increasing preferences for the yellow slot machine, providing that the color of the contextual stimulus had been attended to.

The Number Group consisted of six participants, two of whom gambled more on the yellow slot machine in the posttest than in the pretest, while four gambled less on the yellow slot machine. It is important to notice that participants # 37, 38, and 39 were given detailed instruction and reported to have solved the task by looking at the comparison stimuli. Since the detailed instruction did not include information about attending to changes in the color of the contextual stimulus, it is possible that the instruction functioned as input to continue the experiment. The Number Group gambled with a mean of 47% of responses allocated to the yellow slot machine in the pretest and 41% in the post-test, as shown in Figure 3. A t-test indicated that the difference was not statistically significant: t (5) = 0.72 (α =0.05).

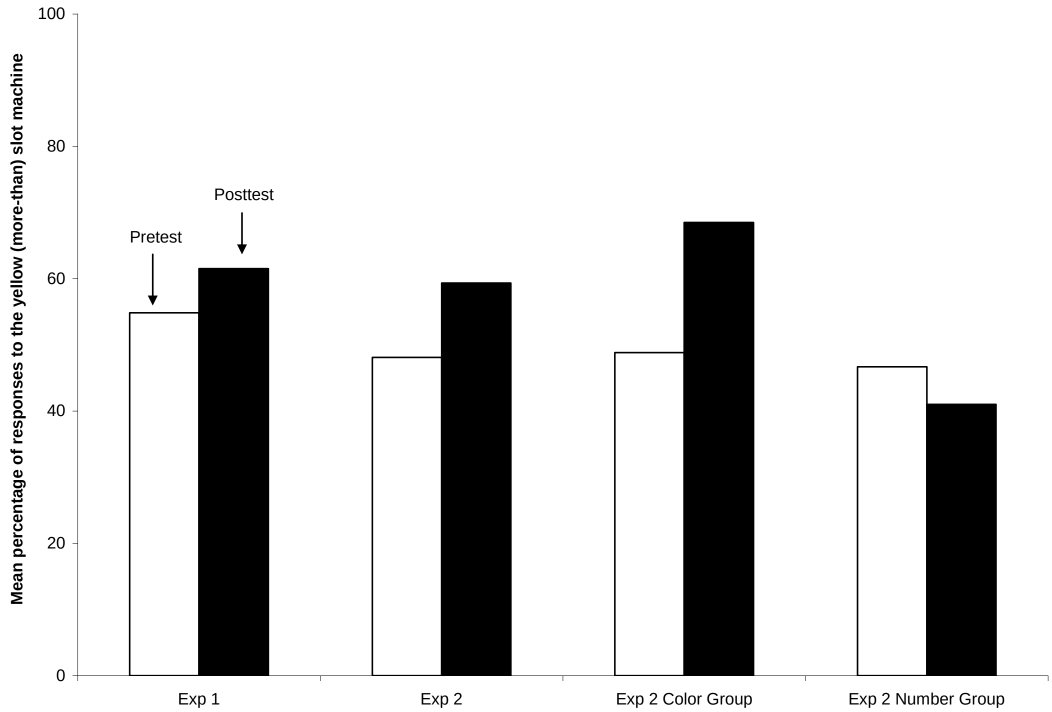

Figure 4. The Figure shows the mean number of responses to the yellow slot machine in pre-and posttest for both Experiment 1 and 2. Furthermore, the results are divided into Color Group and Number Group.

The purpose of Experiment 2 was further to investigate the possible implication that some of the participants did not attend to the colors. We replicated the findings from Experiment 1 as we did find a greater variation than Zlomke and Dixon (2006) in responding to the yellow slot machine in the pretest. The posttest shows that eight of twelve participants (Color Group) who reported to have attended to the contextual stimulus increased their preference to gamble on the yellow slot machine (one participant responded the same in pre and posttest, while three participants gambled less on the yellow slot machine). As a group, these participants had the largest increase in preference from pre- to posttest (see Figure 4) and nearly three times as great an increase in preference change as in Experiment 1.

GENERAL DISCUSSION

We sought to replicate Zlomke and Dixon (2006) and also to expand the knowledge about instructional control in the gambling literature. The results from Experiment 1 in the current study did not replicate all of the findings from Zlomke and Dixon (2006). First, during the pretest we found more variability among participants' preferences for the slot machines. In the study by Zlomke and Dixon (2006), the greatest shift in preference was 20% for the yellow slot machine (M = 49%). Thus, in the current study the shift in preferences ranged from 0% to 100% for the yellow slot machine (M = 51%). Second, the data from the posttest show that eight of the participant in the Zlomke and Dixon (2006) study played more on the yellow slot machine compared to the pretest. The participants in the current study did not show the same consistency in change of preference. Only four of the twelve participants who finished Experiment 1 had an increase in preference for the yellow slot machine, and five participants showed a reduced preference for the yellow slot machine after the conditional discrimination training. Some of the participants reported that they had not attended to the contextual stimulus, even if they finished the training and test phase. We think that this finding could be important since it might be that the participants had not conditioned the yellow color to the contextual stimulus "more than". Furthermore, it could have implications for the interpretation of the results of Experiment 1. One could not account for an increase in preferences on the yellow slot machine for the participants who have not attended to the contextual stimulus (i.e., if the color on the slot machines was not of importance, then the choices in both pre- and posttest will be largely random).

The change in preference for the Color Group is remarkably lower than in the Zlomke and Dixon (2006) study. One implication from the current study seems to be that it is important to find out if the participants are attending to the contextual stimulus or not. The group (Number Group) that had been looking at or attending to comparison stimuli showed a small reduction in change in preference to the yellow slot machine after training.

The results from the current study are in accordance with the results of Hoon et al. (2007), even if in the current study the changes in preferences were greater. Hoon et al. (2007) presented three experiments with six participants in each experiment. Group data from Experiment 1 showed 18% reduction in gambling on the yellow slot machine, while group data from Experiment 2 and Experiment 3 showed a small increase of 4%. In an another study by Hoon et al. (2008), they showed that when we just look at group data an increase in preferences of 20% is observed. They argued that establishment of non-arbitrary contextual control is most efficient with two comparisons and gambling related stimuli. The results from Experiment 2 in the current study, albeit with three comparisons, are in accord with this notion providing that we exclude the participants who reported not to have been attending to the contextual stimulus.

Hoon et al. (2007) reported that 13 of 18 participants finished the experiments. In the current study, all of the participants that finished both experiments showed one self-generated rule that was important in the test phase in which three new stimulus sets were introduced. Therefore, we will argue that the rule about the five stimuli on the screen in training phase was controlling the participants' behavior in the test phase. Furthermore, the self-generated rule was probably also used during the post-test for those who gambled more on the yellow slot machine in the pretest even if it did not produce more reinforcers. Thus, there are some problems with self-report data (e.g., Critchfield & Epting, 1998; Holth & Arntzen, 1998), such as the fact that participants' self-generated rules are asked about in a post-experimental interview and the questioning by itself could influence the self-reports. Therefore, we suggest that future research should include talk aloud procedures (e.g., Cabello & O'Hora, 2002; Rehfeldt & Dixon, 2000). The focus on self-generated rules will be in accordance with researchers who have pointed out that analyses of different verbal behavior are important in understanding gambling behavior (Brandt & Pietras, 2008; Dixon & Delaney, 2006). Thus, it seems important to increase the understanding of self-generated rules in gambling behavior since such rules like "play the yellow slot machines, and you will win more". Such a rule may make individuals gamble more on yellow machines than machines with other colors. Thus, it could be that the gambler thinks he or she can control or have influence on the outcome of gambling (e.g., Ladouceur, Sylvain, Boutin, & Doucet, 2002; Petry, 2005).

There are several limitations to the present findings. First, a potential threat to the validity of the findings is the relatively low requirement of 50 slot machine trials in the pretest, which could be too few responses for the participants to show a stable preference. Also, the participants may have determined the schedules of reinforcement in the pretest and therefore have no reason for gambling more on the yellow slot machine in the posttest. Second, although open-ended questions were used during the post-experimental interview in Experiment 2, participants' responses were readily assigned to one of two categories. This made it clear for the experimenter how to score the answers, but had all verbalizations been audio recorded and later transcribed it would have allowed for reliability testing to be undertaken. Third, we did not use a standardized measure for screening gambling problems. All the participants were given some formal written information about the experiment and they had to answer two questions about gambling. All participants reported knowledge of slot-machines, but no one reported problems with gambling. By this we concluded that the participants may best be described as "non-gamblers" or recreational gamblers. A standardized measure like South Oaks Gambling Screen (SOGS) (Lesieur & Blume, 1987) may be better to screen and categorize participants. Fourth, employing a research design other than the pretest-posttest design, such as a multiple baseline design, is important for future research, as is targeting the least preferred color slot machine from the pretest as the subsequent more-than contextual cue. Finally, it would be helpful to replicate the present procedures with gamblers.

In conclusion, the current study showed that preferences for gambling on one of two slot machines could come under contextual control by two different colors. The results support the studies by Zlomke and Dixon (2006) and Hoon et al. (2007). There is a need for more replications since the results are not quite unambiguous. In any case, the results show that preference for slot machines can be established and transformed to other stimuli. Furthermore, the results showed that self-generated rules can lead to responding in a special pattern even if the reinforcement for such responses is very lean and could be the reason for the choice of some responses and not other even if the contingencies of reinforcement are the same

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USING PERFORMANCE FEEDBACK TO TEACH VIDEO POKER PLAYERS TO GAMBLE BETTER

Mark R. Dixon & James W. Jackson
Southern Illinois University

The present investigation reports two studies that examined the performance of non-pathological recreational video poker gamblers. In the first experiment, seven participants played three types of video poker games in a within participants randomized sequence design. The percentage of errors made across games revealed the game variant "Deuces Wild" yielded more frequent mistakes than "Jacks or Better" or "Bonus Poker." The second experiment consisted of a new sample of 11 participants being exposed to "Deuces Wild" poker to initially assess error percentages. Next, participants were all provided with performance feedback regarding their play, and finally the feedback was removed to assess performance maintenance. Results suggest that all poker players were able to improve performance above baseline level, and changes were maintained when the intervention was removed.

Key words: gambling, video poker, addiction, performance feedback, video game

In recent years behavior analysts have become more active in attempting to understand the behavior of gambling and the unfortunate disorder of pathological gambling (e.g., Dixon, Jacobs, & Sanders, 2006; Weatherly & Dixon, 2007; Zlomke & Dixon, 2006). However, similar to the consumption of alcohol or drugs, not all those who partake in such libations develop a problem. Instead, many individuals find themselves capable of managing consumption at healthy levels resulting in no known detrimental consequences from their behavior. The occasional cigar smoker, beer drinker, or wine taster is hardly considered pathological. A similar distinction has been seen in the context of gambling. While reports suggest that over 80% of adults in the United States have gambled in their lives, only 1-3% of the population develops any pathology from gambling (Petry, 2005). For the remaining percentage, gambling may be considered a recreational activity like sports or a type of entertainment (Ghezzi, Lyons, & Dixon, 2000).

Paying more for the same gambling experience is similar to paying extra for movie tickets, sporting events, or a case of beer. Often gamblers do in fact spend more money than necessary due to playing casino games poorly. Casinos profit from the margin of error by patrons. Optimal play will yield a house advantage of only 1-4%. However, when errors are made by players the odds favoring the casino can rise over 500% (Zamzow Software Solutions, 2006). Performance feedback has been successful at improving skills such as the sports of rugby (Mellalieu, Hanton, & O'Brien, 2006), football (Smith & Ward, 2006), and basketball (Kladopoulos & McComas, 2001). To date, the utility of performance feedback has not been demonstrated in minimizing the many type of errors made by recreational gamblers. Thus, the twofold

Address Correspondence to:
Mark R. Dixon
Behavior Analysis and Therapy Program
Rehabilitation Institute
Southern Illinois University
Carbondale, IL 62901
Email: mdixon@siu.edu

purpose of the present study was first to determine the type of video poker game that would yield the most errors by players, and second to attempt to implement a performance feedback intervention to reduce errors by players in the most error-prone game type.

EXPERIMENT 1
METHOD

Participants, Setting, and Apparatus

Seven undergraduate students participated in the current study for course extra credit and a potential \$20 gift card to use towards a local retailer awarded upon attaining the highest score among all participants. Participants consisted of 4 men and 3 women between the ages of 21 and 32 (M = 23.4, SD = 3.87). Upon completion of informed consent, participants were asked to complete three computer tasks, the first consisting of a basic demographics form with questions regarding gender, age, highest education level completed, and annual income. The second task consisted of an electronic version of the South Oaks Gambling Screen (SOGS; Lesieur & Blume, 1987). Any individual who scored a 5 or more on this instrument (a measure of potential pathological gambling) was dismissed from the study. Participants were then asked to play three 15 minute sessions of video poker using the video poker software WinPoker 6.0 (Zamzow Software Solutions, 2006). Each session consisted of a different 5-card-draw video poker variation (Jacks or Better, Bonus Poker, and Deuces Wild), and were presented in random order. The three specific games were chosen based on prior research on video poker (Weatherly, Austin, & Farwell, 2007).

Procedure

Prior to running each participant, the experimenter determined the order of presentation of the three video poker games through a random drawing. Upon completion of the demographic questionnaire and the SOGS, participants were given basic instruction on how to play video poker using the computer software. Participants were then staked with 300 credits and allowed to play the first video poker variation for 15 min. Upon completion of the first 15-min session participants were given a 2-min break and asked to leave the room. During this time the experimenter recorded data from the software's session information screen.

The software recorded the number times during the given session that the player deviated from optimal play. Any deviation from optimal play represented either holding a card or failing to hold a card which based on the hand dealt and the payoff structure for the given game resulted in a lower than optimal rate of return. Based on the number of hands played these errors are translated by the software into a Percent Correct Play statistic which was used as the dependent measure in the current study.

After recording the Percent Correct Play statistic, the experimenter reset all statistics to zero, reset the number of credits to 300, and switched the game to the next game variation in the sequence. The participant was then allowed to return and asked to complete another 15-min session playing the new game. These steps were repeated for the remaining game variations, and upon completion of the third 15-min session the participant was debriefed and thanked for his or her participation.

EXPERIMENT 1
RESULTS AND DISCUSSION

The results of Experiment 1 yielded mean Percentage Correct Play for Jacks or Better (M=56.12%, SD = 6.83), Bonus Poker (M= 51.25%, SD = 8.13), and Deuces Wild (M= 41.0%, SD = 8.15). A repeated-measures ANOVA was conducted to support the visual inspection of differences across games and yielded significant mean differences ($F(2, 12) = 9.683$, $p = .003$), and no significance on order of game presentation. The observed differences between games supports prior research on poker game error making (Weatherly et al., 2007) that has suggested that players make more mistakes on wild-card games than on non-wild card games. Future research should examine players' relative preference for draw poker games such as Jacks or Better compared to wild card games such as Deuces Wild or other types of wild card games in a concurrent operant paradigm. This type of preparation will allow for analysis of game preference and allow for error types made by players.

EXPERIMENT 2
METHOD

Participants, Setting, and Apparatus

Eleven individuals participated in Experiment 2 for course extra credit and potential $20 gift card. Participants consisted of 1 male and 10 females ranging in age from 22 to 39 (M = 24.8, SD = 4.8). Participants completed an informed consent, demographics questionnaire, and the SOGS as described previously for Experiment 1. No participants scored in the pathological range on the SOGS. Participants were then asked to play a number of 5-min sessions of Deuces Wild video poker on WinPoker 6.0. Deuces Wild was chosen based on results of Experiment 1, which indicated it was the game variant that produced the most errors.

Procedure

Participants were then given basic instructions on playing video poker as described in Experiment 1 and informed that they would be asked to play the game for 5-min sessions, at the end of which the experimenter would ask them to leave the room so that data could be collected. During these breaks between sessions, data were collected as described in Experiment 1.

A non-concurrent multiple-baseline design was used in which the number of baseline sessions varied between 3 and 6 with exact number of sessions contingent upon performance stability for each participant. During baseline, participants were instructed that they could ask questions regarding interacting with the game interface, but that any questions regarding strategy would not be answered. Baseline continued until stable responding of correct play was observed, with stability defined as 3 of 4 consecutive sessions with Percentage Correct Play within a range of 10% observed.

Upon completion of baseline, performance feedback was instated to train participants for correct play. Training consisted of the introduction of a warning pop-up box that would appear on the computer screen informing participants of an error in their play (after desired cards were held and/or discarded) and the overall cost of the current error on their long run financial return. This pop-up warning did not inform participants of what the correct play would be; however, it did give them the option of playing the hand as currently chosen or to go back and change the cards currently held. Participants were instructed to always choose to go back and change the cards held, and that if in 5 attempts at determining the correct play, they were unsuccessful, that they could ask the experimenter for feedback regarding the correct play. When necessary, this personalized feedback consisted of a description of the correct cards to hold and discard based on the payout table for the chosen game. Performance feedback continued until two consecutive sessions were observed with percent correct responding being 20% or greater over the mean of the last 3 baseline sessions' percentage.

If participants displayed more than 2 consecutive data points with no increase over baseline performance, an advanced-training component consisting of prompts on every trial during the next session was instituted. For this advanced training the experimenter sat with the participant and explained the

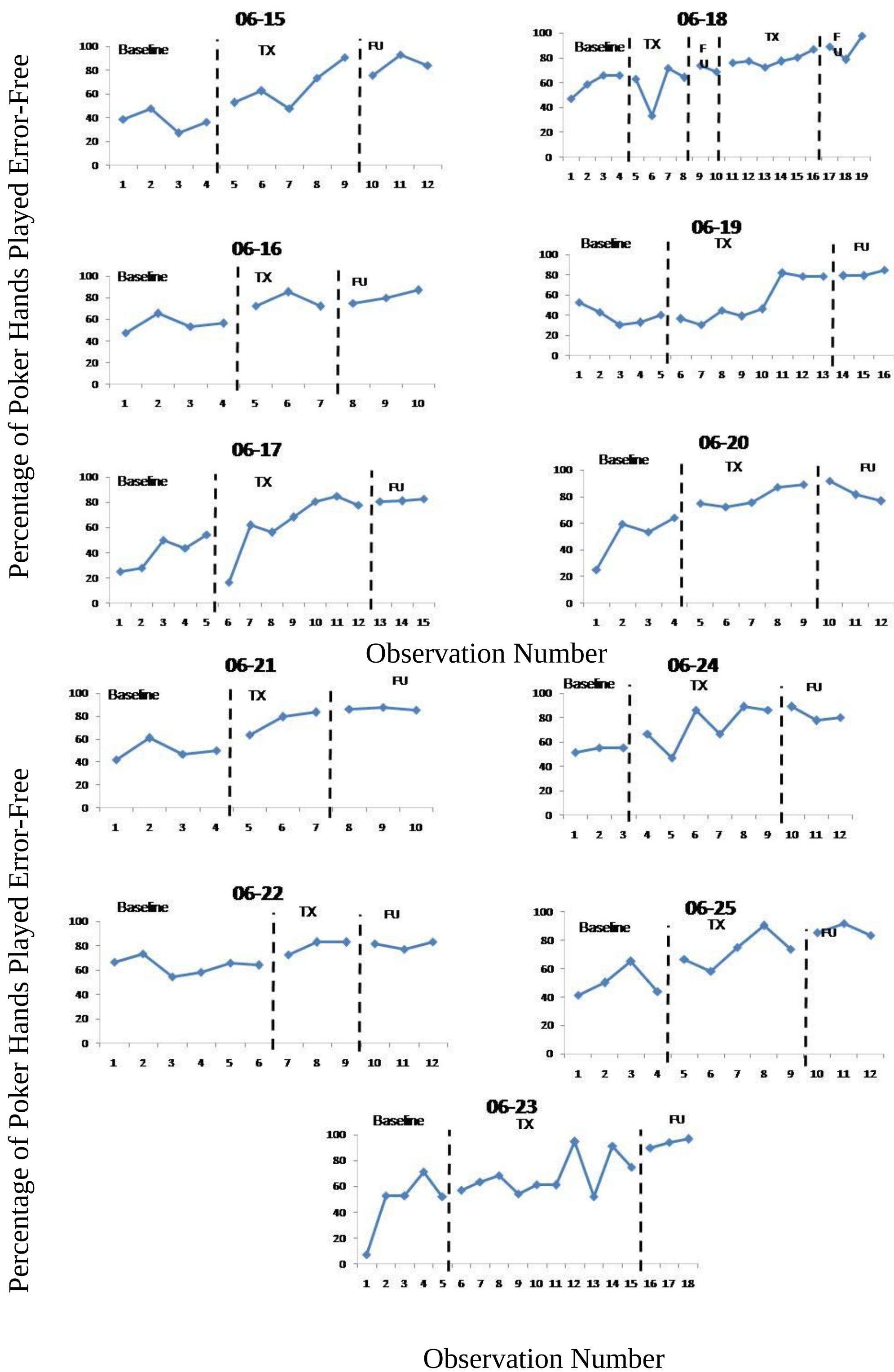

Figure 1. Displays the performance of the eleven participants of Experiment 2. Each participant was initially allowed to play Deuces Wild Poker without any feedback, followed by the performance feedback intervention, and eventually a follow-up condition.

correct play based on the cards dealt and the payout table for the given game for each hand played. These prompted sessions continued until a session with percent correct responding of greater than 20% over the mean of the last three baseline data points was observed. Once this criterion was reached, regular training conditions were reinstated.

Following each participant's attainment of the training criterion increase over baseline, they completed a follow-up phase under the same parameters as baseline. No feedback of any kind was given and participants were instructed that they once again could not ask questions regarding playing strategy. Participants were informed that if their fell back to baseline levels they would have to repeat training. A criteria of no more than two observations with percent correct responding less than 10% over the mean of the last three baseline points was in place during follow up, though no participant failed to maintain responding over baseline levels. Follow up continued for a minimum of three data points.

EXPERIMENT 2 RESULTS AND DISCUSSION

Figure 1 displays the performance of the 11 participants in Experiment 2. Baseline data indicate that many errors were made during every session. In other words, accurate play of optimal poker cards held and discarded was rather low. No participant achieved a Percent Correct Play over 75% during any session, with the lowest observed accuracy being less than 10%. Nonetheless, upon introduction of the performance feedback intervention, error percentages declined dramatically with a concomitant increase in percentage correct play. All 11 participants improved performance over baseline and all 11 maintained these performance gains after the removal of the feedback. No session during follow-up revealed less than 75% percentage correct play in any session for any participant.

GENERAL DISCUSSION

Taken together, the results from the present two experiments suggest that recreational gamblers who play video poker do in fact make a substantial amount of errors. The type of game played can impact the rate of errors, and performance feedback can improve performance. Errors cost the player money, as non-optimal play results in more losing hands at poker than need be if the hand is played more accurately. When a degree of skill is necessary to "win" at a gamble, it is advantageous to develop those skills as best possible. Performance feedback has yielded utility to improve skills in many areas (e.g., Kladopoulos & McComas, 2001; Mellalieu, Hanton, & O'Brien, 2006; Smith & Ward, 2006) outside of gambling, and the present results suggest that such feedback can benefit the recreational gambler.

A potential limitation of the present study is that it cannot conclude error reduction will result in a smaller amount of money being spent at a casino. In fact, teaching someone to play better may only produce a player that plays longer in duration, as the same amount of money will simply go further. Future research should explore length of play, level of risk taken, and resistance to extinction following performance feedback training similar to that of the present study. Finally, experiments such as the present may in fact pose a risk to participants that could eventually develop more severe gambling behavior after exposure to an intervention that taught them to play "better." It may be possible that a participant could develop a self-rule such as "I now know how to beat the house, I will become a millionaire" as suggested by Zlomke and Dixon (2006). Caution should be taken to debrief participants and assure them that the odds will never be in their favor, not even for the most error-free video poker player. Many public campaigns are designed to teach people educated ways to consume alcohol (i.e., in moderation and not while driving). Perhaps similar attention should be paid to persons with no known pathologies for gambling, that through a lack of education pay more than necessary for their recreational pastime.

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