THE EFFECTS OF CERTAIN AND UNCERTAIN REINFORCEMENT PROCEDURES ON THE QUIZ SUBMISSION AND PERFORMANCE OF COLLEGE STUDENTS

by

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Abstract

THE EFFECTS OF CERTAIN AND UNCERTAIN REINFORCEMENT PROCEDURES ON THE QUIZ SUBMISSION AND PERFORMANCE OF COLLEGE STUDENTS

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Shira Melody Berkovits

Advisor: Dr. Alicia M. Alvero

College instructors often provide homework so that their students can review class material; however some students do not take advantage of these review opportunities. This study compared the effects of a certain reward and a lottery reward on the quiz submission rates and accuracy of 112 college students. In Baseline, quizzes were for practice only and had no programmed contingency; in the Certain condition, two extra credit points were available for submission of a perfect quiz; and in the Lottery condition, students who submitted a perfect quiz were entered into a lottery with one winner (actual probability varied) for two points of extra credit. Submission rates averaged 36.50% for Baseline, 62.00% for Certain and 51.67% for Lottery. A one-way repeated measures ANOVA, followed by Fisher's LSD, found the differences in submission rates between all conditions to be significant at the .0001 level. Accuracy rates averaged 82.82% for Baseline, 93.80% for Certain and 93.99% for Lottery. A one-way repeated measures ANOVA, followed by Fisher's LSD found the mean score for the Baseline condition to be significantly lower than the mean scores for the Certain and Lottery conditions ($p < .01$), but did not find a significant difference between the latter two conditions ($p < .05$). This study demonstrates that when all other factors (e.g. magnitude) are equal, certain rewards are more effective than lottery rewards at increasing quiz submissions. It is possible that the lottery was less effective than the certain reward, due to the uncertainty (indirect contingency) inherent in the Lottery condition. These results have implications for business settings that use lottery rewards in an attempt to motivate a large number of employees at low costs. Future research should examine the roles of magnitude, probability and contingency in predicting the relative effectiveness of a lottery reward.

Dedication

This work is dedicated to my grandmother, Naomi Cikk, who is the most resilient woman I know. She is a self-taught perpetual student, with an indomitable spirit and great wisdom. She has always accepted me and loved me for who I am, and for that I am eternally grateful. I like to think that my love of learning and life was modeled after hers. For countless reasons, she is my hero.

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Introduction

Since the publication of Skinner's *Science and Human Behavior* (1953), the use of behavioral consequences has rapidly increased to the point where incentives are now considered standard protocol in many settings, such as sales, business, human-service, and education (Reid & Parsons, 1999). In recent decades, researchers and practitioners have sought innovative applications of behavior management principles in their attempts to increase the efficiency, practicality and success of operant interventions (Cooper, Heron, & Heward, 2007). One area of increasing interest is the lottery procedure (Göritz, 2006). In comparison to guaranteed incentives, lottery incentives are more cost-effective in that they require only a single incentive, or a few incentives, to motivate a large group of people.

With the common purpose of increasing behavior, lottery incentives have been implemented across a variety of settings, with such diverse applications as increasing trash recycling (Geller, Chaffee, & Ingram, 1975), improving the bus-riding behavior of elementary school students (Putnam, Handler, Ramirez-Platt, & Luiselli, 2003) and motivating medication adherence in patients with HIV and AIDS (Haug, Sorenson, Gruber, Lollo, & Roth, 2006). While publications on the use of lotteries can be found frequently in the literature on education, organization, business, and human services, perhaps the most extensive lottery research, within these areas and beyond, has been conducted by surveyors. Today, the use of lotteries to increase survey response rate is steadily increasing (Göritz, 2006), but the extent of the lottery's popularity may be premature given its somewhat limited research base and mixed investigative findings (Baron, De Wals, & Milord, 2001; Porter & Whitcomb, 2003).

The following subsections summarize postal survey studies employing lottery incentives, explore the empirical basis, or lack thereof, for their continued use, contrast the social-cognitive and behavioral approaches to lotteries, and discuss how various parameters of reinforcement might facilitate or impair the effectiveness of a lottery intervention.

Summary of Survey Research using Lotteries

An electronic search was conducted using psycINFO and psycARTICLES for studies that included the terms “lottery” *and* “survey” but excluded the terms “web”, “online”, “gambling”, “gamble”, “draft”, “risk”, and “postcode”. The search returned 348 studies, of which only four met the following criteria: a) the study employed a lottery incentive to increase survey response rate and b) the study was experimental in nature and not quasi-experimental or correlational. 14 additional studies were found by manually searching through the reference sections of these four papers.

Table 1 presents some key details of the methods and findings of the studies summarized below. Seven of the 18 studies presented employed a subject pool from the general population, and four used medical patients. Total sample sizes ranged from 64 to 8,645 potential respondents and averaged 1,7834 (median and mode = 1,000), while lottery sample sizes ranged from 34 to 4,325 potential respondents and averaged 713 (median and mode = 450). Survey lengths ranged from one question to 30 pages. Lottery prizes ranged from $1 to $280 cash (or $2.65 to $435.09, adjusted to 2010 dollars, using the Consumer Price Index) and also included nonmonetary prizes (e.g., resort vacations, pens, and a PDA). Most importantly, only five of the 18 survey studies found lottery incentives to significantly increase response rates over that of a control group. Of the remaining 13 studies, seven found beneficial but insignificant effects of a lottery incentive, two found no effect, and four found detrimental effects.

I have categorized the articles presented in this review into the following groups: a) detrimental or no effect, b) insignificant beneficial effect and c) significant beneficial effect. While it is possible to present groups ‘a’ and ‘b’ in one “insignificant effect” category, I have chosen to specify directionality of the insignificant effects, in the hopes that this distinction will serve to better inform researchers seeking to gain a more nuanced understanding of the survey literature on lottery incentives. I commence this review with the rather inauspicious first use of a lottery incentive in Golden, Anderson, and Sharpe’s 1980 survey study.

Lottery incentives that decreased or had no effect on response rate. In 1980, Golden and colleagues conducted what appears to be the first survey study to examine the effectiveness of a lottery incentive. In a cover letter sent to 3,000 potential respondents, Golden and colleagues described a cash-drawing into which respondents would be entered upon receipt of their survey. Surprisingly, a chi square test revealed that the cash drawing group had a significantly lower response rate (17.7%) than a no-incentive control group (20.2%; $N = 1{,}500$). Since the Golden et al. study, three other studies (Marrett, Kreiger, Dodds, & Hilditch, 1992; Paolillo & Lorenzi, 1984; Warriner, Goyder, Gjertsen, Hohner, & McSpurren, 1996) have also found lottery incentives to have slight detrimental effects on response rate in comparison to no-incentive control groups, but the differences between the lottery and control groups were not statistically significant. Two additional studies, Mortagy, Howell, and Waters (1985) and Roberts, Wilson, Roalfe, and Bridge (2004) found lottery incentives to have no effect on response rate at all. While these six studies suggest that lottery incentives are ineffective motivators for postal survey completion, the studies presented in the following section (though they demonstrate only slight, insignificant effects), indicate the beneficial and promising potential of lottery incentives.

Lottery incentives that insignificantly increased response rate. Few researchers have directly compared the efficacy of lottery incentives, for which receiving the reward is uncertain, with postpaid incentives, in which the reward is certain (Roberts, Roberts, Sibbald & Torgerson, 2000). In an effort to increase return rates to a postal survey about menopause, Roberts and colleagues used unequal randomization (to decrease research costs) to assign 1000 women to one of four groups: a) lottery (N = 375), b) direct payment (N = 125), c) lottery and direct payment (N = 125) and d) no lottery and no direct payment (N = 375). The direct payment was £5 and the lottery was ten times that amount; both were mailed to recipients contingent on the return of a completed survey. Because there were no interaction effects, it was possible to interpret the effects of each incentive separately. Both the lottery and direct payment and the direct payment alone significantly (p =.013) increased the response rate (68.8% and 66.4%, respectively) over the control (53.7%), but the increase in response rate in the lottery group (58.6%) over that of the control group was not statistically significant.

Seven additional studies also found lottery incentives to slightly, but not significantly, facilitate the return of questionnaires. One of these questionnaires asked customers of a major public utility about their energy conservation (Gajraj, Faria, & Dickinson,1990), one was sent to a sample of Texan farmers and ranchers (McDaniel & Jackson, 1984), and the remaining five requested information on health-related issues

(Brown et al., 1997; Moses & Clark, 2004; Spry et al., 1989; Woodward, Douglas, & Miles, 1985; Vogel, Skjostad, & Eriksen, 1992). All of these experiments resulted in insignificant effects, and together with the six studies cited in the previous section, they can be used as further support that lottery incentives are ineffective motivators for survey response. Alternatively, all eight of the experiments listed in this section did result in minimal beneficial effects. Taken in conjunction with the significant beneficial effects of the five studies presented in the following section, the above eight studies support the notion that lotteries can, under certain circumstances, successfully increase response rates.

Lottery incentives that significantly increased response rate. Five experiments, two of them conducted by Hubbard and Little, found lottery incentives to significantly increase response rate over that of a control group (Baron et al., 2001; Blythe, 1986; Hubbard & Little, 1988a and b; and Spry et al., 1989, Exp 2). In their first study, Hubbard and Little mailed a two-page questionnaire related to personal banking and finance to 2,000 randomly selected individuals from the general population. The authors employed a $0.25 prepaid incentive, a $1 prepaid incentive, a $200 lottery incentive, and a $1 postpaid contribution to a charity of the respondent’s choice; all four incentive conditions were compared to a no-incentive control group. No guaranteed incentive postpaid to the respondent (i.e., as opposed to a charity) was employed. The prepaid incentives were the most effective; response rates were 68% for the $1 prepaid group and 56.8% for the $0.25 prepaid group, compared to 40.5% for the control group. The lottery was also an effective motivator; the group’s response rate was 51.8%. Finally, the response rate for the charity group, 33.5%, was less than that of the control group. A chi-squared extension of Scheffe's method of multiple contrasts found the lottery group to be significantly higher than either the charity or control group, not significantly different than the $0.25 prepaid group, and significantly lower than the prepaid $1 group. In terms of cost per actual response, the lottery reward (cost/response = $0.97) was found to be more efficient than the $1 prepaid (cost/response = $1.47) and charity incentives (cost/response = $1.00), but less efficient than the $0.25 prepaid incentive (cost/response = $0.44). In terms of cost per incremental response (i.e., cost per increase over the control), the prepaid incentives were found to be far less costly than the lottery incentive ($0.25 prepaid = $1.53; $1.00 prepaid = $3.63; lottery = $4.44), though the use of a larger sample size would likely have improved the lottery's economic value.

In a follow-up study that appears to be the first and only one of its kind, Hubbard and Little (1988b) compared four levels of a lottery incentive (i.e., $50, $100, $150, and $200) and two levels of a prepaid incentive (i.e., $0.25 and $1) to each other and a control group in their attempts to increase response rate to a postal survey mailed to 3,150 potential respondents. Despite the substantial and varied magnitude of the lottery incentives, only the small prepaid incentives and the $200 lottery significantly increased response rates over the control group ($0.25 = 40.4% response rate, $1 = 56.4% response rate, and $200 lottery = 37.8% response rate). Further, while all of the lottery incentives increased response rates minimally over the control, response rates did not systematically increase with the magnitude of the lottery reward.

While the results of Hubbard and Little's (1988b) second study did not support their original hypothesis (i.e., that a systematic increase in magnitude would result in a systematic increase in response rate), the design of the experiment rightly highlights the need to systematically manipulate magnitude and other parameters of reinforcement affecting lottery incentives. The *Behavioral Theories* section (below) examines a variety of factors that can impact an incentive's effectiveness. First though, the implications of the findings reviewed in this section are discussed and several popular social-cognitive theories for understanding these results are presented.

Implications of the findings on lottery incentives in the survey literature. Taken as a group, the studies presented in this review indicate that lottery incentives are often ineffective at increasing response rates over a control, and that when they are effective, the statistical and practical implications of that improvement are negligible. However, statistical significance aside, Table 1 shows the directionality of the difference between the control and lottery groups to be beneficial in 12 of the 18 studies. Despite the fact that the beneficial effects are slight, their presence in a majority of the studies reviewed is promising.

Continued research on the use of lottery incentives to improve survey response rates will hopefully isolate factors that can augment the success of a lottery procedure (see a more detailed discussion in the sections below). In the meantime, surveyors that use lotteries to motivate potential respondents should be aware of their limited support in the literature. Further, until proven otherwise, postal survey lottery incentives should be considered as experimental procedures and not proven effective practice. To that end, surveyors implementing a lottery incentive should conduct pilot studies, employ control groups, and be vigilant in recording their procedural details and findings while continuously evaluating the success of their interventions.

Social / Cognitive Theories.

Prepaid verses postpaid incentives. Incentives used by surveyors to increase response rate can be divided into two categories: prepaid and postpaid (or promised). A prepaid incentive is delivered to the respondent together with the survey, while a postpaid incentive is given to the respondent contingent on the return of a survey; it is a consequence. It would be reasonable to assume that postpaid incentives generate higher rates of return, since access to the incentives are dependent on the return of a survey, while prepaid incentives are “free” and require no response. In actuality, research studies for the past eight decades have reported that prepaid incentives (even nominal ones) are superior to postpaid incentives in increasing postal survey response rates (Armstrong, 1975; Church, 1993; Golden et al., 1980; Shuttleworth, 1931; Warriner et al., 1996). In their review of the 25 survey-incentive studies that preceded theirs, Golden et al. (1980) concluded that prepaid monetary incentives “unequivocally” resulted in increased response rates over no incentive, while postpaid incentives increased response rates only marginally.

In 1993, Church conducted a meta-analysis of 38 published survey studies employing all types of incentives. These studies yielded a total of 74 observations with data for both incentive and comparison groups. The observations were classified into four categories: monetary and nonmonetary prepaid incentives (N = 43 and 12, respectively), and monetary and nonmonetary postpaid incentives (N = 9 and 10, respectively). Entrance into a lottery was considered a nonmonetary incentive. Monetary incentives, adjusted to 1989 dollars using the Consumer Price index, ranged from $0.36 to $9.29 (mean = $1.38, median = $0.86). Evidence for a significant effect was found for the monetary (*d* = 0.347, medium increase = 19.1%) and non-monetary (*d* = .136, small increase = 7.9%) prepaid incentives, but not for the monetary (*d* = 0.085, small increase = 4.5%) and nonmonetary (*d* = 0.020, small increase = 1.2%) postpaid incentives. Based on his findings, the author concluded that surveyors should avoid postpaid incentives of any sort.

Church's (1993) meta-analysis has been considered authoritative in the debate over prepaid and postpaid incentives, and several other researchers have echoed his recommendation (e.g., Dillman, 2000; Dillman et al., 2007; Heerwegh, 2006). In their review of the literature, Warriner and his colleagues (1996) went so far as to suggest that prepaid incentives are so clearly superior to postpaid incentives, that continued research on the topic is unnecessary.

Social exchange. The norm of reciprocity posits that people will respond to each other in the manner in which they have been treated. According to this theory of social exchange, the recipient of a gift will feel obligated to reciprocate the goodwill of the giver, while one who has been treated harshly will respond in kind (Gouldner, 1960). Several researchers have employed this theory to explain the success of prepaid incentives and the relative ineffectiveness of postpaid incentives in the survey literature. According to social exchange, researchers can activate the norm of reciprocity by treating participants in a positive manner prior to a study's onset (Groves, Cialdini & Couper, 1992). In the case of survey mailings, prepaid incentives serve as gestures of good-will that prompt participants to reciprocate by completing the survey. In contrast, postpaid incentives serve to undermine existing altruistic motives for cooperation and shift the dynamic from a social exchange to one of economic exchange (Heerwegh, 2006).

Economic exchange (similar to behavioral theory) suggests that survey completion is motivated by compensation for time invested (i.e. a postpaid incentive) rather than a sense of obligation triggered by a prepaid incentive (Porter & Withcomb, 2003). However, as described above, research shows that for mail surveys, postpaid incentives have little to no effect on response rate, even when the promised monetary sum is as large as $50 (James & Bolstein, 1992), but that prepaid incentives have significant effects, even when the amount is as little as $1 (Zusman & Duby, 1987). Dillman (2000) suggests that economic exchange theory simply does not hold true for survey incentives and that social exchange alone can account for the overwhelming support for prepaid and against postpaid incentives.

Altruism and cognitive dissonance. Roberts and colleagues (2004) report that response rates may be affected by the nature of the survey or the affiliation of the sender. For example, surveys on medical and health issues may be thought of as more important than surveys soliciting personal opinion, and that surveys from academic, charitable or medical institutions might be given priority over surveys from a retail chain. In a similar vein, Warriner et al., (1996) reported that respondents believed they had a civic duty to provide infrequent public service by responding to surveys that concerned the public's well-being. According to these findings, survey completion can be thought to be triggered by altruistic motives, or an unselfish concern for the welfare of others.

If survey completion is motivated by altruism, and a response contingent monetary incentive appealing to self-interest is employed, the respondent might experience psychological discomfort, or cognitive dissonance, upon responding. According to several researchers, respondents might achieve dissonance reduction, by not completing surveys that they otherwise might have, resulting in lower survey response rates for postpaid incentive groups. Mortagy and colleagues (1985) reported equal response rates for a postpaid lottery group and a control group and suggested that the lottery may have been ineffective because potential respondents were worried that their cooperation would be thought to be motivated by the financial incentive and not by the desire to help medical research. In support of this possibility, Gelb (1975) reported that several respondents requested not to receive a postpaid monetary incentive that they were entitled to.

Some researchers warn that lottery incentives may prove even more distasteful to potential respondents than standard postpaid incentives. Mortagy et al., (1985) received a telephone complaint about the lottery they employed and suggested that other potential respondents that did not call, may have been similarly offended by the principle of using a lottery incentive for a medical survey. Warriner et al., (1996) further elaborated on the use of lottery incentives, by suggesting that

> …the offer of the lottery may appear as a vulgar attempt at inducement, inappropriate in the context of the respondent's perceived duty to provide occasional public service without compensation. (p. 545)

External verses internal locus of control. Golden et al., (1980) sent a three page survey on shopping patterns to 4,500 potential respondents. The final sections of the survey included four psychographic measures: James' Internal-External Locus of Control (Rotter, 1966), Life Satisfaction (Robinson, 1977), Attitude Toward the Past, Present and Future (Rokeach, 1956), and Traditional Family Ideology (Levenson & Huffman, 1955). 3,000 potential respondents were sent a cover letter describing a postpaid lottery and

1,500 received no mention of the lottery. To be entered into the lottery, respondents needed to return a cash-drawing card with a completed survey; completed surveys returned without the card were not entered into the lottery.

A one-way analysis of variance found respondents who returned the cash-drawing card to have a significantly higher external-locus of control and be more future-oriented than those in the lottery condition who returned their survey without the cash-drawing card. Golden et al., (1980) suggested that individuals who are more externally and future oriented may be more likely to take a gamble and therefore more likely to be motivated by a lottery incentive. Golden et al. recommend using an uncertain lottery incentive to minimize incentive cost when surveyors have prior knowledge of psychographic variables (i.e., individuals with an external locus of control or who are future-oriented). Unfortunately the authors did not employ a certain postpaid incentive group, and so it is not possible to compare the psychographic variables of respondents motivated by the lottery incentive with those who would have been motivated by a certain postpaid incentive.

In a similar vein, Evans, Kienast, and Mitchell (1988) suggested that lotteries can be effective reinforcement procedures, specifically because of the uncertainty inherent in the lottery, which can provide a thrill and enjoyment for participants.

Race and socioeconomic status. Gelb (1975) alternately offered a $0.50 prepaid or postpaid incentive together with a survey and request for completion to 200 shoppers in each of two grocery stores: a) one in a middle-class Houston neighborhood with almost exclusively White residents and b) one in a lower-class Houston neighborhood with almost exclusively Black residents. Response rates for the prepaid and postpaid incentives in the middle-class neighborhood were 54% and 45%, respectively, and response rates for the prepaid and postpaid incentives in the lower-class neighborhood were 15% and 25%, respectively. Because the shoppers surveyed were from a convenient sample and not a random one, a nonparametric chi-square test was used ($p \leq .01$). The results indicate that the prepaid incentive was a more effective motivator for the middle-class group, but that the post-paid incentive was a more effective motivator for the lower-class group. Gelb recommended that researchers consider the values of potential respondents before deciding which incentive to use. She concluded by suggesting that the researcher

> …not expect middle-class "dissonance" at "taking money for nothing" to extend to the lower class in equal measure. (p. 109)

Behavioral Theories

According to operant psychology, reinforcement occurs when a stimulus (or its removal) immediately follows a response, and as a result, the future likelihood of such a response increases (Cooper et al., 2007). This definition of reinforcement will be used to evaluate lottery incentives in the sections below. Specifically, three parameters of reinforcement (i.e., probability, temporal contiguity, and magnitude) will be presented and the ability of each to facilitate or impair the effectiveness of a lottery intervention will be discussed. Four additional factors impacting reinforcement will also be discussed in relation to postpaid incentives, especially lotteries (i.e., uncertainty of reward distribution, response effort, reinforcer assessment, and motivating operations), as well as an alternative explanation to that of reinforcement theory (i.e., rule-governed behavior).

Reinforcement increases future responses. As noted above, reinforcement does not strengthen the response that it follows, but rather increases the probability that a similar response will occur in the future. Skinner (1953) wrote:

> It is not correct to say that operant reinforcement strengthens the response which precedes it. The response has already occurred and cannot be changed. What is changed is the future probability of responses in the same class. It is the operant as a class of behavior, rather than the response as a particular instance, which is conditioned. (p. 87)

In each of the cross-sectional, one-time incentive studies in this review, prepaid incentives were distributed together with the surveys. In contrast to the distribution method of prepaid incentives, postpaid incentives were promised to participants and distributed after the response occurred. According to operant reinforcement, we would expect the postpaid incentives to increase the response rate to similar *future* mailings from these same surveyors, but we would not expect the promise of a reward to increase the initial response.

In a unique study, Collins, Ellickson, Hays, and McCaffrey (2000) examined the effects of incentive timing and magnitude (see section titled *Magnitude*) in a longitudinal postal survey study. The sample consisted of 4,280 individuals who had participated in seven previous mailing waves since they were recruited 10 years earlier in junior high school. Participants were assigned to three experimental conditions: 40% to a prepaid $20 condition ($N$ = 1,689), 40% to a postpaid $20 condition ($N$ = 1,734), and 20% to a postpaid $25 condition ($N$ = 857). The authors labeled the $20 postpaid condition as the “control group”. Participants in all conditions were told that they would be entered into a lottery upon return receipt of their survey.

Response rates were 62%, 59%, and 66% for the prepaid $20 group, postpaid $20 group, and postpaid $25 group respectively. Chi square tests found the differences in final response rates across conditions to be significant ($p = .002$). Follow-up tests found response rates for the prepaid and postpaid $20 groups to be equivalent to each other but significantly lower than that of the $25 postpaid group. These results indicated that incentive magnitude rather than timing accounted for the difference. Collins et al. (2000) contrasted their findings to those presented in Church’s (1993) review. Church reported an average prepayment advantage of 15% compared to postpayment incentives of equal magnitude, while Collins and colleagues reported a difference of only 3% between the two conditions. The implications of this experiment could have been made stronger by the inclusion of a $25 prepaid comparison group. Further, no conclusions about the impact of this lottery incentive can be formed, as the experimenters did not employ no-lottery control conditions.

It is possible that the relatively limited advantage of the prepaid incentive in Collins et al., (2000) is attributable to the study’s longitudinal nature. To explain their findings, the authors employ the social-exchange theory (described above). They suggested that because of their previous studies, the kind of trusting relationship ordinarily generated by prepayment was already established prior to the experiment’s onset. Collins and colleagues hypothesized that as a result of this existent bond between researchers and participants, prepayment had only a small advantage over postpayment.

A behavioral approach might also implicate longitude to explain the anomalous findings of Collins et al. (2000), but by focusing on the effect of reinforcement, rather than the norm of reciprocity. Participants in this study, unlike the others described in this paper, had prior exposure to postpaid incentives from the surveyors in the seventh mailing wave. Assuming that the postpaid incentive functioned as a reinforcer (a complex assumption which must also account for other variables, like temporal contiguity - described below), it would be expected to increase response rates for future waves (i.e., the eighth wave), which it did. (The authors did not provide response rates for the seventh wave which included a $15 postpaid incentive to all participants). The study conducted by Collins et al. (2000) is supported by operant theory and provides a plausible explanation for the failure of other postpaid incentives to increase response rates.

Uncertainty of reward distribution. In addition to providing an opportunity for the reoccurrence of a previously rewarded response, Collins' et al. (2000) mitigated a related complication inherent in one-time cross-sectional studies with postpaid incentives: uncertainty. Surveyors are almost always unknown to potential respondents, yet they request from them an investment of effort and time. To increase the likelihood of receiving a return survey from potential respondents, researchers offer incentives. Social-cognitive theorists promote the use of prepaid incentives which they claim foster a relationship of trust between the surveyor and respondent and consequently augment response rates by triggering the norm of reciprocity.

Whether the norm of reciprocity is activated or not, prepaid incentives have been received by the time a request for survey completion is made, and as such are guaranteed. In contrast, postpaid incentives, though promised by researchers, might not be viewed as guaranteed by the participant. Because the participants have not had previous experience with the researcher or the promised incentive, they may be uncertain if engaging in the requisite behavior will result in the promised postpaid reward. Due to the potential for uncertainty with a postpaid reward, it is unsurprising that researchers have found these incentives to be of limited value. As Collins et al. wrote, “An incentive of any size has less value if one is uncertain if it will be received” (p. 350). A longitudinal study like Collins et al. provided participants with repeated exposure to both the researchers and their incentives, thereby minimizing the effects of uncertainty on the postpaid incentive. The uncertainty inherent in a first time reward, may partially explain why reinforcement only increases the likelihood of future responses.

Postpaid lottery incentives may incur additional uncertainty complications, in that a potential respondent must be certain that a) the lottery exists and b) the lottery will be run fairly (Porter & Whitcomb, 2003). It may be particularly difficult for participants promised entry into a lottery to be certain that their response will result in an entry, as lotteries may elicit skepticism in a public that has been inundated with sweepstake offers. In 1980, Golden and colleagues reported a detrimental effect of a postpaid lottery incentive on response rate. The authors speculated that the decrease might have been due to a “raffle effect”, whereby the inclusion of a lottery incentive lead respondents to conclude that the survey was “just another piece of junk mail”.

Probability. The main difference between a lottery procedure and other incentive procedures is the certainty of obtaining a reward following the emission of a target behavior. Lottery incentives are indirect acting contingencies. If an individual engages in the target behavior, the reward is not guaranteed; in fact, it is often more likely that an individual will not be rewarded for his/her behavior. Porter and Whitcomb (2003) explained that the uncertainty of winning a lottery diffuses its potential benefits as an incentive, thereby limiting its effectiveness relative to prepaid incentives:

> With a lottery the expected benefit is not the monetary amount of the incentive but the amount of the incentive multiplied by the probability that the respondent will be selected a winner in the lottery. (p. 392)

Probability of reinforcement plays a crucial role in determining the likelihood of response occurrence. Specifically, response rate and reinforcer probability share a direct positive relationship, in that a high probability of reinforcement will establish a high likelihood for the reinforced behavior (Pierce & Cheney, 2004).

Unlike the other studies reviewed in this paper which conducted private drawings, Gajraj et al. (1990) and Marret et al. (1992) offered entry into a public lottery; both found their lotteries to be insignificant motivators (response rates were 45% and 72.6% for the lottery group and 34% and 74.4% for the control group, respectively). It is widely known that public lotteries attract many customers, so the probability of winning is quite low. In contrast, the chance of winning in a private lottery is much greater. Accordingly, authors who use entry into a public lottery may limit the incentive's effectiveness relative to those who offer entry into a private lottery. Perhaps, it is even appropriate to regard a public lottery ticket as a guaranteed nonmonetary postpaid reward (i.e., the ticket) and to compute its value by determining the cost of the ticket (e.g., $1), rather than multiplying the amount of the lottery with the probability of winning (as Porter and Whitcomb [2003] recommend).

While the probability of winning a private lottery is likely to be much higher than that for a public lottery, probability in studies that employ private lotteries varies greatly too. Interestingly, despite the importance of probability to the value of a lottery, survey researchers have neglected to investigate it as a variable that could impact response rate. Of the 18 postal studies employing lottery incentives that were reviewed in this paper, only three (McDaniel & Jackson, 1984; Paolillo & Lorenzi, 1984; Vogel et al., 1992) indicate that they informed participants of their probability of winning the lottery. The remaining 15 studies either did not inform participants of their chance of winning, or did inform participants, but did not inform readers of this key procedural detail. Further, probability does not seem to be a factor that is controlled for or manipulated in any of the postal survey studies presented in this review or elsewhere (Porter & Whitcomb, 2003).

Temporal contiguity. For a response to be reinforced, a consequence must be presented (or removed) immediately following the target behavior. If there is a delay between the target behavior and the consequence, other behaviors will occur in the interval, and the one that is temporally closest to the consequence will be reinforced (Cooper et al., 2007). Carton (1996) extends the importance of temporal contiguity beyond reinforcement, to the deliverance of rewards:

> Temporal contiguity, which refers to the interval between the occurrence of a target behavior and the delivery of a reward, is an important factor in determining the likelihood that an event will increase the rate of a behavior. Specifically, research has demonstrated that rewards are more likely to increase the frequency of a target behavior the more closely they follow the behavior's occurrence. (p. 242)

Selecting a lottery winner from a pool of survey respondents is usually delayed, as researchers need to wait for a predetermined number of surveys to arrive or a set time to pass before they can run a lottery. Accordingly, lottery incentives, even web-based ones, are rarely delivered immediately, except “accidentally” in the case of an individual submitting a response seconds before a winner is announced, in which case that individual is not likely to have been included in the draw. Furthermore, not only are lotteries incentives not immediate, but researchers often wait weeks or months before drawing a winner (e.g., Porter & Whitcomb, 2003), and may not even inform participants exactly when the drawing is scheduled to take place. The majority of studies reviewed in this paper do not mention if they informed participants in advance of an outcome-notification time, when they notified lottery participants of the outcome, or even if they notified participants of the outcome at all (an exception is McDaniel & Jackson, 1984). Previous experience with surveys that do not notify participants of lottery outcomes (i.e., participants never find out if a respondent won the lottery, who the winner was, or even if the lottery was ever run) may be partly to blame for the skepticism with which participants view lottery incentives (see *Uncertainty of reward distribution* above).

In a web-based study by Tuten, Galesic, and Bosnjak (2004), participants were randomly assigned to one of four conditions: no incentive, an offer to receive the first results of the survey, entrance into a lottery for 1,000 Kuna (approximately $170, the then mean Croatian weekly salary) with delayed notification of the outcome (1 month), or entrance into a lottery for 1,000 Kuna with immediate notification of the outcome upon completion of the questionnaire. The authors did not specify how the lottery was conducted for the immediate group (i.e., who was included in the draw). Response rates were significantly ($P < 0.01$) higher for the immediate group (76.6%) than the delayed group (70.5%), indicating that informing participants that they will be notified of the lottery's outcome immediately upon survey completion is a practical means for enhancing the effectiveness of a lottery incentive.

Rule-governed behavior. As noted in the previous section, reinforcement must be delivered immediately following the occurrence of a target behavior. The belief that delayed consequences can reinforce behavior is erroneous. Michael (2004, as cited by Cooper et al., 2007) presents an alternative explanation:

> When human behavior is apparently affected by long-delayed consequences, the change is accomplished by virtue of the human's complex social and verbal history, and should not be thought of as an instance of the simple strengthening of behavior by reinforcement. (p. 36)

Malott (1988) and others (e.g., Malott & Garcia, 1991; Rachlin, 1993; Skinner, 1969) use the notion of rule governed behavior to explain indirect-acting contingencies. A rule is a verbal description of an underlying antecedent-behavior-consequence contingency. A statement of a rule can alter the value of a delayed consequence by making its influence immediate. Rules enable behavior (i.e., responding to a survey) to come under the indirect control of temporally remote or improbable, but potentially significant consequences (i.e. winning a lottery). Agnew and Redmon (1992) cautioned against overusing the concept of rule-governed behavior; the brief description presented here, is intended simply as a theoretical framework from which to understand the hypothetical possibility of success of a survey's lottery incentive assuming proper construction with consideration of the various parameters of reinforcement presented in this *Behavioral Theories* section.

Response effort. The above review focused on the impact of a consequence (i.e., a lottery) to increase response rate to postal surveys. While the interested reader can look elsewhere for a review of antecedent manipulations that affect survey response rate (see Edwards et al., 2002), one antecedent, response effort, appears to vary across the studies presented in this review, and therefore warrants mentioning here. Response effort refers to the amount of energy that must be exerted by an individual in order to engage in a target behavior. Behavior analysts often arrange antecedent conditions so that less effort is required to emit a desirable response and more effort is required to emit an undesirable response. If two behaviors result in reinforcers of equal value, then the behavior that requires less response effort will be more likely to occur (Miltenberger, 2004).

Two studies described earlier, Spry et al. (1989) and Vogel et al. (1992) manipulated response effort by modifying survey length (Spry et al., short form two pages, long form eight pages; Vogel et al., Experiment 1: short form one page, long form three pages, Experiment 2: short form one question, long form one page) and found no significant effect of survey length alone on response rates. Despite this, it is still possible that response effort may impact survey response rates. The studies presented in this review mailed surveys to potential respondents that ranged in length from one question to 30 pages, a far larger discrepancy than that of the short and long forms employed by Spry et al. and Vogel et al. For example, Hubbard and Little (1988b) offered entrance into a $200 lottery to one group of participants for completing a *two-page* postal survey and found a significant beneficial effect of the lottery incentive. In contrast, Warriner et al.

(1996) offered entrance into a lottery for $200 (Canadian) to one group of participants for completing a *30-page* survey and found the lottery to result in an insignificant detrimental effect.

Drawing conclusions about response effort based on a comparison of Hubbard and Little (1988b) and Warriner et al. (1996) would be misleading, as a) the comparison does not account for inflation or the difference in value between the American and Canadian dollar (after adjusting for deflation and the mean Canadian exchange rate for the 1988 year [0.81], Warriner et al.'s lottery would have only been worth 130.33 American dollars in 1988) and b) comparing other studies reviewed in this paper would suggest that survey length does not play a significant role in determining the effectiveness of a lottery incentive. For example, in their second experiment Vogel et al. (1992) offered entrance into a $280 lottery for completion of a one-question survey, and found no significant beneficial effect of shortening the survey form. The $280 Vogel and colleagues spent on their lottery exceeded the $200 Hubbard and Little spent (i.e., increased magnitude), even accounting for inflation (Hubbard and Little's $200 lottery would have been valued at $237.19 in 1992), and the one-question survey Vogel et al. used was shorter than the two-page survey Hubbard and Little used (i.e., decreased response effort). Despite these advantages, Vogel et al. did not find the lottery to significantly increase response rate, while Hubbard and Little did. The varied survey lengths used by the studies in this review does not support the conclusion that the mixed effectiveness of these lottery incentives was influenced by response effort, but it does highlight a complication in drawing conclusions across a group of studies in which this antecedent condition was for the most part an uncontrolled variable.

Reinforcer assessment. Parents, teachers and employers frequently employ incentives in an attempt to motivate individuals to engage in specific behaviors. Too often though, those designing incentive programs do not first ensure that the reward functions as a reinforcer (Cooper et al., 2007). For example, Alexander, Corbett, and Smigel (1976) and Berkovits, Sturmey, and Alvero (in press) described successful uses of a monetary incentive to improve adolescent attendance, but also reported that the rewards which were effective for most of the participants may have been ineffective for a minority of participants who had access to money from alternate sources and for whom the monetary incentives may have simply been ineffective reinforcers.

Behavior analysts directly test whether a particular reward is reinforcing by a) making it available to an individual contingent on a target behavior and then measuring the future effects of that reward on responding and b) comparing it to a non-contingent reinforcement condition. While reinforcer assessments remain the only means by which to determine whether a particular reward can function as a reinforcer (Skinner, 1953), stimulus preference assessments can be used to identify probable reinforcers in instances where a reinforcer assessment is either impossible or impractical (Cooper et al., 2007).

> Stimulus preference assessment refers to a variety of procedures used to determine a) the stimuli that the person prefers, b) the relative preference values of those stimuli, and c) the conditions under which those preference values change when task demands, deprivation states, or schedules of reinforcement are modified. (p. 275-276)

None of the studies presented in this review report the use of procedures to ascertain if a given reward would function as a reinforcer. While it is true that the use of a lottery incentive cannot be understood through simple reinforcement as described above -- there are complications due to temporal contiguity and the one-time cross sectional nature of the experiments -- it is still considered good practice to use incentives that, assuming other preconditions are met, could effectively function as reinforcers. While money is a powerful generalized conditioned reinforcer (Miltenberger, 2004), presumptive use may partially account for the statistically ineffective results of the lottery incentives reported in this review. Further, while a given reward may function as a reinforcer in one instance, the same reward may not function as a reinforcer in another instance or for another individual. For example, the chance of winning $50 might be an effective motivator for one survey, but an ineffective motivator for another survey that is triple the length, or questions the respondent about an obscure or boring topic.

Magnitude, response effort, and other antecedent variables must all be accounted for when selecting an incentive. Ideally, small pilot experiments should have been conducted for all the studies presented in this review, so that proper conclusions regarding reinforcer assessment could be made. In the event that a reinforcer assessment would have been unfeasible, some sort of stimulus preference assessment might have been performed.

Magnitude. Warriner et al. (1996) theorized that postpaid incentives, unlike prepaid ones, are viewed as compensation for time and effort, and that minimal compensation (i.e., a $2 postpaid reward) would therefore be unlikely to serve as adequate motivation for survey completion. The section on *Social-cognitive Theories* (above) noted that Gelb (1975) and Mortagy et al., (1985) reported returned/rejected remuneration and/or a negative reaction on behalf of some participants to offers of compensation for survey completion. It would be interesting to note whether these same participants would continue to object if the monetary sum being offered were increased substantially, to $200. While the offer of a $200 postpaid reward to all participants could hypothetically increase response rates by 100%, such an undertaking would obviously be financially untenable. In contrast, the use of one $200 postpaid reward – a lottery – to motivate all participants, could very well prove cost-efficient. Porter and Whitcomb (2003) supported Warriner et al.'s magnitude assertion by suggesting that the value of a lottery incentive can only be determined by multiplying the amount of the lottery by the probability of winning; consequently, only lotteries of high magnitude will be effective.

Hubbard and Little (1988a) hypothesized that the negative findings of Golden et al. (1980), McEnally (1984), and Paolillo and Lorenzi (1984) were due to the insufficient magnitude of their lottery rewards. In an attempt to correct this limitation and build on the previous studies, Hubbard and Little employed a $200 lottery incentive, and found that it significantly increased response rates over that of a control group, thereby supporting the idea that an increase in the magnitude of a lottery can lead to an increase in the lottery's effectiveness. The researchers wondered whether it would be possible to identify a specific lottery amount that would be just large enough to motivate survey return. They wrote:

> Further research on the size of both the sample and the cash prize, conceived within a Weber's Law type of framework by altering the sample size or the cash prize amount until a (just) "noticeable difference" in response rates is detected, could prove to be extremely valuable and deserves further attention. (p. 228)

In a follow-up study, Hubbard and Little (1988b) sought evidence to support the absolute threshold hypothesis and the differential threshold hypothesis. The absolute threshold hypothesis states that there is a minimal threshold for a successful lottery reward, while the differential threshold hypothesis states that as the value of a reward is systematically increased corresponding increases should be observed in response rates. The authors compared four levels of a lottery incentive (i.e., $50, $100, $150, and $200) to a control and two prepaid incentives and found support for the absolute threshold hypothesis (i.e., a $200 lottery) but not the differential threshold hypothesis (i.e., response rates did not increase systematically with the magnitude of the lottery reward). The authors suggested that support for the differential threshold hypothesis might only be evident at increments above $200.

Like Hubbard and Little (1988a), Mortagy et al. (1985) speculated that a lottery of a larger magnitude might have proven more successful at increasing response rates, but that such a change would have made their lottery incentive more costly than simply sending a second reminder. It is possible that financial affordability for lotteries of large magnitudes may depend on the use of comparably large sample sizes. With an increased sample size, researchers might increase the incremental efficiency of the lottery, such that it would approach or even supersede that of prepaid incentives (Hubbard & Little, 1988a).

Motivating operations. The three-term contingency (i.e., antecedent-behavior-consequence) is often used by behavior analysts to experimentally analyze behavior. Following an influential publication by Michael (1982), it became clear that the three-term contingency was incomplete; behavior could more accurately be described by a four-term contingency that included motivating operations (Michael actually used the term ‘establishing operation’ in his initial article, but Laraway, Snycerski, Michael, and Poling [2003] later presented ‘motivating operation’ as the more appropriate term). A motivating operation is an omnibus term that subsumes both establishing and abolishing operations. Motivating operations have three momentary effects: a) they cause a change in the effectiveness of an operant consequence, this is called the value altering effect (i.e., establishing or abolishing), b) they cause a change in the frequency of behaviors relating to that consequence, this is called the behavior altering effect (i.e., abative or evocative), and c) they modify the effectiveness of a discriminatory stimulus for that consequence (i.e., they make discrimination training possible and alter the behavioral control exerted by previously established discriminatory stimuli). (For a more detail discussion of motivating operations, including the distinction between unconditioned and the three categories of conditioned motivating operations, see Laraway et al.).

The *Social-cognitive Theories* section (above) discussed Gelb’s (1975) finding that prepaid incentives were more effective motivators for middle-class participants, while postpaid incentives were more effective motivators for lower-class participants. Gelb advised that researchers not expect cognitive dissonance associated with postpaid incentives to extend to the lower class as it does to the middle class, but does not explain why the concept cannot be generalized. A behavioral approach might be able to account for the discrepancy in survey completion rates.

This *Behavioral Theories* section discusses the importance of response effort and magnitude in determining the motivating effects of a postpaid reward. Specifically, it describes the limitations of using rewards of low magnitude to motivate behaviors that require a substantial (or even minimal) amount of response effort. Gelb (1975) offered $0.50 postpaid incentives for completing a five-page survey. Even after adjusting for inflation ($1.97, using the 2010 Consumer Price Index) Gelb's incentives, and most of the non-lottery postpaid incentives offered in survey studies, might just have been too modest to function as effective motivators. However, if an individual lives in a perpetual state of monetary deprivation, the requirements for an effective monetary motivator might be lower. In the case of Gelb's study, lower socioeconomic status might have functioned as an establishing operation for the postpaid incentive; this state of monetary deprivation did not apply to Gelb's middle-class participants and therefore may account for the discrepant results obtained for the two groups. An analysis that includes motivating operations, such as one of monetary deprivation described here, could also be applied to lottery incentives, but would require a randomized control trial incorporating the socioeconomic status of potential respondents; to my knowledge postal survey studies employing lottery incentives have not accounted for this variable.

Summary of Survey Literature As A Basis For Current Study

Results of the 18 studies presented in this review are inconsistent, but overall do not support the use of lotteries to augment postal survey response rates in a statistically or practically significant manner. Despite the lack of support, lotteries continue to be used as survey incentives, especially on the web and in academic institutions. Porter and Whitcomb (2003) distributed a web survey to academic institutions inquiring about their survey practices. They found that one-third of respondents (N = 374) conducted at least one survey a year employing a lottery incentive, and that approximately 50% of private institutions used lotteries at least once a year. The authors questioned whether such an allocation of time and resources is defensible, given the limited empirical support of lottery incentives.

The survey literature presents the most extensive and cohesive exploration of experimental procedures using lottery incentives, and accordingly was summarized above. Despite the mixed findings of the survey literature, both business (Cook, & Dixon, 2006; Gravina, Wilder, White, & Fabian, 2005) and classroom (Layer, Hanley, Heal, & Tiger, 2008; Martens et al., 2002) settings continue to use conditions in which reinforcer obtainment is uncertain (i.e., lotteries). In addition, as mentioned above, studies that employ lotteries usually compare the consequence to no consequence (i.e., a control group or prepaid incentive), and rarely compare the uncertain lottery to direct postpaid incentives in which reward receipt is certain (Roberts et al., 2000).

It is possible that a direct comparison of certain postpaid rewards and uncertain lottery rewards in a within-subjects design would alter the relative value of the lottery reward. Further, such a preliminary study should hold all factors other than certainty constant, in order to first demonstrate the differential effects of lottery and certain rewards. To date no studies have begun with this basic experiment and instead simultaneously manipulate multiple parameters of reinforcement. Only once the differential effects of a lottery and certain reward are established, can a carefully planned manipulation of magnitude, probability, or contingency be initiated in an attempt to systematically augment the effectiveness of a lottery reward, as described in the *Behavioral Theories* section above. The current research addressed these two limitations by comparing the effects of a certain reward and a lottery reward of equal magnitude in a within-subjects alternating treatments design.

Method

A pilot study of similar methodology to the current study was run to ensure that the extra credit used in this experiment could function as a reinforcer.

Participants and Setting

Participants were 112 undergraduate students enrolled in an introductory psychology course at a public university in the Northeastern United States. Students were a minimum of 18 years of age. Gender distribution is unknown, as this information was unavailable.

Materials

Each class lecture included the material in one half of a chapter of the textbook assigned for the course. A corresponding five-question, multiple choice, practice quiz was created for each class lecture. Quizzes were created by randomly selecting five questions for each half-chapter from a multiple choice test bank that accompanied the instructor's edition of the course textbook (King, 2008). Each question was tested on five undergraduate research assistants (all in their junior or senior years). Quiz questions indicated the page number in the textbook where the answer could be found, and all research assistants were given textbooks to use while completing a quiz. An item analysis was conducted on all questions, to determine the p-values. Questions with p-values less than 1.0 (i.e., at least one of the five research assistants had gotten the question wrong) were not used, and new questions were randomly selected and tested. This process was repeated until all quizzes had five multiple choice questions that had been randomly selected and tested on five research assistants, with resultant p-values of 1.0.

At the start of the semester, practice quizzes were posted online using the accompanying course website software, called *BlackBoard*©. Each quiz question indicated the page number in the textbook where the answer could be found, and students were encouraged to use their textbooks to complete the quizzes. Students answered the quiz questions electronically by clicking on the answer. Upon completion of a practice quiz, students transmitted their answer choices by clicking on the "submit" button. Electronic submission of a practice quiz resulted in the immediate transmission of feedback previously programmed by the experimenter to the student which included a) a score (i.e. percentage of questions answered correctly), b) a list of the correct answers, and c) an explanation of why each answer choice was correct or incorrect. In addition to providing feedback, *BlackBoard*© automatically recorded the student's ID number and score into an electronic grade book. The feedback and grade book data were observable at all times by both the students and the instructor and were transmitted for quiz submissions in all conditions.

Practice quizzes became available following the end of a class meeting, and stayed available until the end of the following class meeting (e.g. the practice quiz for lecture one became unavailable at the same time that the practice quiz for lecture two became available – immediately following lecture one). This availability setting prevented students from completing multiple quizzes in one sitting (i.e. advance or backlog) and ensured that the collected data reflect student behavior at multiple, predetermined intervals throughout the semester.

Independent Variable and Experimental Design

The experiment consisted of three conditions. Extra credit points were available for completing practice quizzes in two of three experimental conditions. An alternating treatments design was used to systematically vary the criterion (i.e., the independent variable) necessary to receive these points. The participants were exposed to the following conditions (described below): Baseline, Certain, and Lottery. Each condition was in effect for one class meeting, and was presented six times throughout the study. Complete counterbalancing was used to assign conditions to blocks of three. The sequence of the first block was: Baseline, Certain, and Lottery. This sequence was chosen so that students could become acquainted with the experimental conditions gradually, by experiencing the most straightforward conditions first (i.e., Baseline did not require an understanding of the extra credit incentive, and the Certain condition did not require an understanding of the more complicated extra credit lottery). Conditions in blocks two through six were presented in a random order.

Experimental procedures were communicated via a syllabus that was distributed on the first day of class and posted on *BlackBoard*©, announcements at the beginning of each class, bi-weekly postings on *BlackBoard*©, and email messages. A description of the procedures used to manipulate the independent variable follows below.

Baseline. During the Baseline condition no extra credit points were available for quiz submissions.

Certain. In the Certain condition, students who submitted completely accurate quizzes received two points of extra credit. It was not possible to program *BlackBoard*© to automatically add extra credit points to the grade book, though all quiz scores were automatically recorded. Consequently, the experimenter manually added two points for all students who submitted perfect quizzes. Students were permitted to submit quizzes as long as a quiz was available. To distribute the extra credit systematically, the experimenter did not record extra credit points until the condition ended and that quiz became unavailable (i.e., all quiz submissions had ceased), but made certain to add the points before the end of following condition.

Lottery. In the Lottery condition, students who submitted completely accurate quizzes were entered into a lottery. Students were not told how many other students submitted quizzes (i.e., maximum possible probability) nor were they told how many students were entered into the lottery (i.e., actual probability). ID numbers for students who submitted completely accurate quizzes were compiled into a list and numbered. The experimenter used the website *www.randomizer.org* to generate a random number. The number generated by the website corresponded with a student; that student was the winner of the lottery. The experimenter manually inputted two points of extra credit into the *BlackBoard*© grade book for the winner of the lottery by the end of the following experimental condition (for details see *Certain* above). At that time, the following sentence was also posted on *BlackBoard*© and emailed to all students: "Congratulations to _________ (insert student's name) for winning the lottery for two points of extra credit and for obtaining a perfect score on Quiz ___ (insert quiz number). Great job!"

Extra Credit Points and Final Grades

The final course grade was the average of four test scores; extra credit points were added to one of the four tests. Students could earn up to 12 extra credit points, excluding additional points distributed to the winners of the lotteries, which amounted to as much as one grade shift on their final grade. For example, if a student scored 72, 67, 77, and 84 on tests 1-4 respectively, the average of these four tests would be a 75 and the final course grade would be a C. If this same student earned 12 extra credit points to be added to one of the tests (e.g. 72 + 12 = 84) the new average would be 78 and the final course grade would be a C+.

Dependent Variables and Data Collection

The dependent variables were the number of quiz submissions per class meeting and quiz scores. *BlackBoard*© automatically recorded individual student ID numbers, submissions and scores. In addition to individual data, *BlackBoard*© recorded the rate of submissions and the score for the entire class for each class meeting. The proportion of submissions for an individual class meeting was a tally of all the quizzes submitted electronically since the last class meeting, divided by 112 and multiplied by 100. The score for an individual class meeting was expressed as a percentage and calculated by dividing the total number of items answered correctly on all submitted quizzes by the total number of items that could possibly have been answered correctly (i.e., *N* submitted * 5), multiplied by 100.

The above data were available for viewing by the instructor and research assistants only. Students could only view their own quiz submission and performance history. Research assistants recorded the receipt of quizzes and obtained quiz scores as displayed by *BlackBoard*©, onto a datasheet, and constructed a corresponding graph for the group data and a chart for individual data. Use of the student ID number for all submissions and recordings ensured that no one besides the instructor knew the submission and performance histories of individual students.

Item Analysis

An item analysis was conducted on all quiz questions, to determine the p-values. Questions with p-values less than 0.50 (i.e., more than half of the students who submitted the quiz got the question wrong) were not used. Four questions, out of 90, were discarded due to low p-values.

The decisions to conduct this item analysis and the one used for quiz creation (described above) were made purposefully, so that scores of 100%, and subsequently the extra credit, would be obtainable to all students investing time to complete and submit the quizzes. It was determined a priori that it would be less problematic to have a ceiling effect, than to use difficult questions, which might discourage students from attempting to complete the quizzes.

Reliability

Two research assistants independently recorded the individual and group data as displayed by *BlackBoard©* onto separate datasheets, while a third research assistant compared these datasheets and determined the extent to which they concurred. Agreement between the datasheets was defined as identical lists of student ID numbers for quiz submission and matching scores for each student ID number for quiz performance. Inter-observer agreement (IOA) was calculated for both submission and performance by determining the number of same responses (i.e. agreements) divided by the total number of responses, multiplied by 100%. IOA for the recording of quiz submissions and performance was collected for all experimental sessions and averaged 99% (range 99%-100%) and 98% (range 98%-100%) respectively.

Treatment Integrity

In the Certain condition, extra credit was added to the *BlackBoard©* grade book by the end of the following experimental condition. In the Lottery condition, the lottery was run, a winner was selected and announced to the class, and extra credit was distributed to the winner by the end of the following experimental condition. Treatment integrity for both the Certain and Lottery conditions was calculated by dividing the number of times extra credit should have been delivered before the end of the following experimental condition by the number of times it actually was delivered in that time frame, and multiplying by 100%. Treatment integrity for both conditions was 100%.

Data Analysis

Submission rates and quiz scores per class meeting were graphed for the group and charted for each individual participant. Data analysis included statistical and visual inspection of the graph and chart. In order for a particular condition to be considered effective at the group level, quiz submissions and/or performance scores during that condition needed to be significantly higher than during Baseline or other experimental conditions. Significance was determined using a one-way repeated measures analysis of variance (ANOVA) at α .001. A post-hoc analysis (Fisher's LSD) was used to determine specific differences. In addition, a factorial ANOVA was also run at α .001 to determine if there was a significant trend in the rate of quiz submissions over time.

Accuracy scores for the Certain and Lottery conditions were not expected to be significantly different, as both conditions shared the same performance criterion. When it is anticipated that no significant difference will be found between two conditions, choosing the larger alpha level is the more conservative approach. Therefore the .05

alpha level was used to determine accuracy differences between the Certain and Lottery conditions.

Questionnaire

Following the final test, the course instructor distributed a questionnaire to all students, containing eight 5-point Likert-scale feedback statements and two additional statements. The statements referred to the students' motivation for completing quizzes and the degree to which they collaborated on quiz answers with other students. These statements and the students' answers can be seen in Table 2.

Results

Submission

The rate of quiz submissions varied as a function of the experimental condition and was ordered from highest to lowest as follows: Certain (M = 62.00%, SD = 9.30, range = 49-76), Lottery (M = 51.67%, SD = 11.44, range = 39-66) and Baseline (M = 36.50%, SD = 11.31, range = 25-50). A one-way ANOVA detected a significant difference in submission rates per condition, F (2, 230) = 57.22, $p < .0001$. Fisher's LSD found the mean number of submissions between all conditions to be significantly different at $p < .01$. As can be seen in Figure 1, submission rates trended downward across sessions. A two-way univariate factorial ANOVA found a significant main effect for conditions F (2, 222) = 17.14, $p < .001$ and sessions F (5, 555) = 22.52, $p < .001$, but did not find a significant interaction F (10, 1110) = 0.57, $p = .8404$ (i.e., the linear trend among sessions did not change significantly across conditions).

Table 3 indicates the proportion of participants (i.e., 26.79%) whose submission rates matched the group data (i.e., submission rates were highest for Certain and lowest for Lottery) and lists the order of conditions, from highest to lowest submission rates, for the participants whose data did not match the group. Table 4 shows individual submission and performance scores for each condition.

Insert Figure 1 about here

Accuracy

A one-way ANOVA of the accuracy scores of individuals who had submitted at least one quiz per condition was conducted and a significant difference was found, $F\ (2, 166) = 38.88$, $p < .0001$. Fisher's LSD found the mean score for the Baseline condition (M = 82.82%, SD = 4.88, range = 81-91) to be significantly lower than the mean scores for the Certain (M = 93.80%, SD = 1.87, range = 91-96) and Lottery (M = 93.99%, SD = 2.59, range = 91-98) conditions ($p < .01$), but did not find a significant difference between the latter two conditions (these conditions were also not significantly different at $p < .05$).

Individual Data

Figures 2 and 3 show submissions and scores for the six students who won the lottery. The data indicate little change in submission or accuracy behavior for these six students following their wins. Winners 1 and 5 submitted an equal number of quizzes during the Certain and Lottery conditions and the least amount of quizzes during the Baseline condition. Like the group, Winners 1 and 5 had equal accuracy rates for the Certain and Lottery conditions. Data for Winners 2 and 4 reflect group data in that they submitted the most quizzes during the Certain conditions and the least quizzes during the Baseline conditions. However, unlike the group data, accuracy rates were highest during the Lottery condition for Winner 2 and during the Certain condition for Winner 4. Winners 3 and 6 submitted six quizzes in all three experimental conditions, with Winner 3 showing little difference in accuracy between conditions, and Winner 6 showing the greatest accuracy during the Certain condition.

Insert Figures 2 and 3 about here

Correlation

A positive one-tailed correlation ($r = .44$, $p < .0001$) was found between the submission of practice quizzes and final grades in the course (i.e., the more practice quizzes a student submitted, the higher his/her final course grade was likely to be), while a smaller positive one-tailed correlation ($r = .20$, $p = .03$) was found for accuracy on submitted quizzes and final grades in the course (i.e., the better a student scored on the quizzes, the more likely s/he was to receive a high final grade in the course).

Questionnaire

The group responses to the questionnaire statements are summarized in Table 2. Overall, students reported that the extra credit motivated them to submit quizzes and try to answer the quiz questions correctly in both the Certain and Lottery conditions. Students further indicated that the quizzes helped them to understand the reading and prepare for examinations. Students were split as to whether they thought they would have submitted quizzes without the extra credit incentive. Most indicated that they did not always know when extra credit was available or what was required to obtain it. Nineteen students reported cheating on the quizzes by selecting “Yes” to the following statement: “I collaborated on the quizzes with another student (completed the quiz together) at least one time throughout the semester.”

Discussion

Submission

Differential motivating effects. The lower rate of quiz submissions in Baseline as compared to the Certain and Lottery conditions, and the greater effectiveness of the Certain condition as compared to the Lottery, were functions of the differential availability of the extra credit. While the Lottery condition did serve to increase submission rates compared to Baseline, it was less effective than the Certain condition. In the Lottery condition, part of the criteria necessary to obtain the reward was outside of the individual's control and students could not be certain that submission of a completely accurate quiz would result in extra credit. In fact, students could be more certain that they would *not* obtain the extra credit, as only one person was chosen as the winner of each lottery. It is likely that this uncertainty regarding extra credit obtainment (i.e. there was no direct contingency between behavior and extra credit) was a key factor in the decision of whether or not to submit a quiz and may account for the condition's limited motivational ability.

Porter and Whitcomb (2003) explained that the uncertainty of winning a lottery diffuses its potential benefits as an incentive, thereby limiting its effectiveness relative to guaranteed incentives:

> With a lottery the expected benefit is not the monetary amount of the incentive but the amount of the incentive multiplied by the probability that the respondent will be selected a winner in the lottery (p. 392)

Pierce and Cheney (2004) supported this notion by stating that probability of reinforcement plays a crucial role in determining the likelihood of response occurrence.

Specifically, they explained that response rate and reinforcer probability share a direct positive relationship, in that a high probability of reinforcement will establish a high likelihood for the reinforced behavior. While the current study used a reward and not reinforcement (as the delivery of extra credit did not immediately follow the behavior of submitting a quiz), parameters of reinforcement can have similar impact on reward effectiveness (Carton, 1996).

Downward trend. The trend analysis confirmed that students submitted significantly fewer quizzes at the end of the semester than they did at the beginning. The trend does not differ across conditions, as all three conditions have similar downward trends while maintaining their differential effects on the rate of quiz submissions. Due to the rapid shift between conditions, the alternating treatments design is more powerful than other experimental designs (e.g. the ABACA reversal design). It is because of this power that the effects of the independent variable remain unthreatened even with a significant downward trend. Had a reversal design been used, the downward trend may have appeared in only one or two of the conditions, thereby complicating and potentially confounding any conclusions regarding the experimental manipulation. In contrast, the alternating treatments design switched between conditions so frequently, that the presence of the linear trend can be seen across all conditions. Further, even as the trend progressed, the difference in submission rates was maintained and highlighted by the experimental design, and therefore it is obvious even with simple visual analysis that the trend does not threaten experimental control.

While the declining linear trend does not jeopardize the internal validity of this study, it does raise questions about the long-term success of the interventions and the cause of the trend. One possible explanation for the trend is that both the Lottery and Certain conditions increased the rate of quiz submissions over Baseline, but only temporarily. According to this interpretation, if the experiment were to have been extended, the incentives would have gradually lost effectiveness and the downward trend would have continued until submission rates in all conditions reached zero. Alternatively, it is possible that as the semester progressed, the students' overall workload increased and preparation for final exams was required for multiple classes at the same time. Assignments and exams accounted for a larger percentage of the students' final grades than did the extra credit; consequently, the value of the extra credit may have been diminished in comparison. According to this theory of competing contingencies, the incentives still held value as the semester progressed, but they were "overshadowed" by other tasks with greater rewards. It follows then that the incentives would have proportionately regained their affect on submission rates once the "busy-time" of final exams had passed. To determine whether the differential effectiveness of the conditions could have been maintained long term, the experiment simply needed to be extended. Unfortunately, in the college setting semesters are of a predetermined length, and so it was impossible to extend this experiment. However, since the incentives continued to significantly increase submission rates over Baseline, even at the end of the semester (last data point: Baseline = 25%, Lottery = 39%, Certain = 49%), these incentives remain statically and socially significant.

Individual data. Table 3 indicates that 20 students showed no control by the independent variable; submission rates were equal in all conditions. However, it is important to note that while these 20 students shared an imperviousness to the experimental condition, they did so in different ways. 10 students submitted all six available quizzes in each condition, two submitted one quiz per condition, and eight submitted zero quizzes per condition.

There are several possible explanations for the perfect submission rates demonstrated by 10 of the students. First, it is possible that the feedback delivered immediately following quiz submissions in each condition functioned as a reinforcer for these students, while the extra credit available in the Certain and Lottery conditions only functioned as a reward (i.e., due to its delayed delivery). Similarly, the opportunity to practice quiz questions may have in itself motivated these 10 students. Both feedback and practice were available to all students, but appear to have been sufficient motivators for these 10 students, making the extra credit unnecessary to evoke submission behavior. The differential responding seen for these 10 students as compared to the rest of the class, is most likely attributable to their reinforcement history, in which establishing operations or previous exposure may have served to increase the value of practice and feedback as reinforcers. Alternatively, the perfect submission rates of these 10 students may be explained by the notion of rule governed behavior and the students' past histories with rules (see *Behavioral Theories* above). In this instance, the syllabus, class announcements, emails, and bi-weekly postings on *BlackBoard*© that indicated the availability of a new quiz, may have prompted students to self-generate rules such as "I am going to do everything the instructor suggests so that I can excel" or "If I do every assignment I will surely get an A+". These rules would then have served as indirect-acting contingencies for the delayed consequence available in all conditions (i.e., a good grade), by making its influence on quiz submissions immediate. Students with a history of following rules and obtaining distant appetitive consequences would be more likely than others to exhibit rule-governed behavior.

The behavior of the students that did not submit even one quiz throughout the entire study may have been a function of a different kind of rule governed behavior. In this instance, the statement of the rule may have served as a proxy for the reward and paradoxically suppressed the target behavior (e.g. the student may have thought "Practice quizzes never help me do better on tests" or "I don't need to practice to do well" or even "Don't tell me what to do."). Students with a history of following rules and not obtaining appetitive consequences or obtaining aversive consequences, would be more likely than others to exhibit this kind of inverse behavioral reaction to the statement of a rule. Alternatively, for these students two points of extra credit may have been an ineffective reinforcer to begin with, and even more so an ineffective reward (e.g., the student was scoring in the 30s on exams, and the addition of two or even 12 extra credit points would have been irrelevant to obtaining a passing grade). Finally, it is possible that these students had no relevant reinforcement history with this kind of reward.

Probability. While students surely were aware of the statistical unlikelihood of winning any of the six lotteries, they were unaware of how many other students, of the 112 total, submitted completely accurate quizzes, and so could not calculate their exact probability of winning. The actual probabilities of winning extra credit for the six lotteries, in order of presentation, were .0172, .0185, .0164, .0256, .0303, and .0323 (M = .0234, SD = .0070, Range = .0164 - .0323), compared to 0.0 in Baseline and 1.0 in Certain. In general, when researchers use a lottery to motivate performance, participants do not know the actual probability of winning, as this number is unknown even to the researcher until the behavior occurs; though participants may be informed of the minimum probability (e.g., 1 out of 112). While it would raise ethical questions to manipulate the minimum probability of winning the lottery in a classroom setting (e.g., 80% of those that submit completely accurate quizzes would receive extra credit, and 20% would receive nothing) future research should examine this possibility in other settings.

Accuracy

The performance criterion (i.e., a perfect score) necessary to obtain the extra credit in the Certain and Lottery conditions can account for the similar increase in correct answers as compared to Baseline, which did not share the performance criterion. The relatively high accuracy rates obtained throughout the experiment (range = 81% –98%) demonstrate a ceiling effect. As described above, only questions that had perfect p-values were selected for inclusion in the practice quizzes, and all questions indicated the page in the textbook where the answers could be found. Furthermore, once student transmissions for a given quiz were complete (i.e., the quiz became unavailable) an item analysis was conducted for each of its questions. Any question that had a p-value less than .50, was dropped, and not included in the students' scores. As mentioned above *(see Item Analysis)* these decisions were made a priori, with the understanding that a ceiling effect might result, but that its existence would be less problematic than imposing substantial barriers (i.e., difficult questions that only the top ranked students could answer correctly) to the extra credit reward. Unfortunately though, the relatively low difficulty of the quiz questions does limit the external validity of the study, as it is unknown whether the incentives used in this study would have yielded similar results if more difficult quiz questions were used. Further, it is probable that the relative ease of the quizzes is also responsible for the low correlation obtained between accuracy and final grades.

Questionnaire

Results of the questionnaire indicate that the students appreciated the practice quizzes and opportunities for extra credit. Given the clear experimental control of the group's submission behavior, it is surprising to find that most students disagreed or strongly disagreed with the statement "I always knew when extra credit points were available and I knew what I had to do to obtain them." Despite the schedule distributed at the start of the semester and posted on *BlackBoard*©, biweekly postings on *BlackBoard*©, biweekly emails, and class announcements regarding the order of experimental conditions, students reported being uncertain of the experimental schedule; however, the data suggest otherwise. It is possible that the above questionnaire statement, and not the availability of the extra credit, was unclear to the students. Students that answered "Disagree" or "Strongly disagree" might have thought the statement was referring to the Lottery condition, during which they did not know who would win the lottery and receive the extra credit points. While it is not possible to determine why many students disagreed with the statement, future use should include revisions to make its meaning more explicit. In addition, researchers should consider using step-by-step *PowerPoint*© presentations to instruct the students on how to use *BlackBoard*© to determine which experimental condition is in effect, as well as quiz titles that include a salient indication of the experimental condition.

The term "collaborated" was intentionally used in the questionnaire instead of the term "cheating" (see statements 9 and 10 in Table 2). Typically, "collaboration" is used in a neutral or constructive context, while "cheating" is most often used disparagingly or in a negative context. It was important that the statements encouraged honesty, and it was determined that the word "collaboration" would be less threatening to students. Interestingly four of these "collaborations" occurred during Baseline when no extra credit was available, 15 occurred during the Certain condition when all students were guaranteed two points of extra credit for submitting a completely accurate quiz, and zero collaborations occurred during the Lottery condition, when only one student could win extra credit points (i.e., the students were in competition with one another).

Strengths

Seamless integration of self-help and intervention tools into an already existent daily schedule is an important factor in determining their acceptance and use (Ricciardi & Luiselli, 2007). In 2003, the U.S. Census Bureau reported that 88% of college students use the internet either at home or at school and that email and internet postings are popular methods of communication among college students. Accordingly, data collection methods that require internet use, an activity in which many students are already engaged, are likely to be a better fit in a college classroom than data collection methods that require more response effort and a greater time commitment (e.g. filling out a scantron, having to first print a quiz in order to complete it, or having to remember to submit a hard copy of quiz answers to an instructor prior to the commencement of a lecture). Accordingly, the current research seems particularly appropriate and easy to integrate into a college classroom.

Limitations

Due to the nature of the classroom setting, there exists a small difference between the Certain and Lottery conditions that is not related to the independent variable. In addition to the extra credit, lottery winners also received public recognition of their submission, score, and win. Public recognition was a necessary component of the lottery intervention as it informed students that the lottery was indeed run and a winner selected (i.e., treatment integrity was intact); however, in the Certain condition, students that obtained the extra credit were not publicly recognized. From an experimental perspective, all variables besides the independent should have been held constant (i.e., the names of students that received extra credit should have been announced in both conditions). However, from an educational perspective, it is inappropriate to publicly list the names of students who earned extra credit in the Certain condition, as students not on that list could only have been excluded for two reasons, both of which would have reflected negatively on their quality/quantity of academic performance: they did not submit a quiz or they submitted a quiz and did not receive a perfect score. This problem does not exist in the Lottery condition, as students may have not received extra credit for one of these two reasons, or for a third reason reflecting chance and not academic ability: they submitted a completely accurate quiz but simply did not win the lottery. As such, it was determined that it would be better to introduce the potential for a minor confound, than to compromise students' privacy.

Relevance to Business Settings

The results of this study may be relevant to business settings in the current economic climate. In an attempt to cut costs, many businesses are using entries into a lottery for a bonus, rather than a guaranteed bonus, to motivate their employees to meet performance management criteria. Some companies use lotteries for tangible (e.g., a big screen television) and intangible items (e.g., extra vacation days) to motivate tens, or hundreds of employees, where previously a guaranteed reward (e.g. a $500 bonus) was available to all employees meeting or exceeding specific requirements.

Future Research

The current study was the first to directly compare lottery and certain incentives of equal magnitude. The results of this study support the notion that, all other things being equal, a lottery reward is less attractive to participants than a certain reward. Future research should consider applying Hubbard and Little's (1988b) threshold analysis, in which they systematically increased the magnitude of the lottery reward for postal survey completion, to lottery incentives in the classroom. Such a study should increase the magnitude of the lottery reward relative to the certain reward in an attempt to increase the lottery's effectiveness. If the increased magnitude of the lottery reward results in a proportional increase in its effectiveness as a reinforcer, it follows that continued research in this direction should allow for the determination of a theoretical threshold beyond which the detrimental effects of uncertainty can be negated by increasing the magnitude (and reinforcing value) of a lottery reward. As mentioned above, a similar threshold analysis could be conducted in a non-educational setting, by manipulating the probability of winning the lottery (e.g., probability = 0.01, 0.25, 0.50, 0.75, 0.99) and comparing the various values to Baseline (probability = 0.0) and Certain (probability = 1.0) conditions.

The idea of systematically manipulating various parameters of reinforcement to predict the relative value of a reward has been mathematically demonstrated by the hyperbolic decay function. The hyperbolic decay function states that if a reinforcer is delivered immediately following a behavior, the value of that reinforcer (V) is directly related to its magnitude (M), and that the longer the reinforcer is delayed (D), the smaller its value becomes (Domjan, 2003). In the following equation, K represents the steepness of the individual's discounting curve.

$$V = M / (1 + KD)$$

Mazur (1987) replaces D in the hyperbolic decay function with variable X, which can also represent delay, or it can represent probability, effort, or initial cost. In Mazur's version of the formula, A represents the amount of the reinforcer, which is equivalent to the M used in the original formula to represent magnitude.

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

Mazur's version of the formula does not combine factors for delivery of the reinforcer (e.g., delay, probability, etc.), but it does acknowledge the potential applicability of all parameters of reinforcement. A series of lottery studies systematically analyzing magnitude, probability, and temporal contiguity should lend further support to the above formula.

Table 1

First Author	Year	Recipients	*N* Total[1]	*N* Lottery	Length	Lottery Incentive	RR[2] Control	RR Lottery	Effect[3]
Golden	1980	General	4,500	3000	3 pg	1=$25, 5=$5, 25=$1	20.2	17.7	✓↓
Paolillo	1984	Bus Exec	400	100	Unknown	1 win: $20, $30, $50	36	33	X ↓
Warriner	1996	General	3,044	98	30 pg	5 win: $200 Can	68	67	X ↓
Marrett	1992	Patients	954	477	12 pg	Ontario lottery ticket	74.4	72.6	X ↓
Mortagy	1985	General	2,712	1762	7 pg	1 win: £20, £30, £50	68.1	68.1	X =
Roberts	2004	Patients	8,645	4,325	Unknown	£100	62.1	62.1	X =
McDaniel	1984	Farmers	3,001	750	36-38 Q	$100	23.5	25.8	X ↑
Gajraj	1990	Utility cust	700	100	12 Q	Postpaid: share 5 tix	34	45	X ↑
Spry	1989	General	600	Unknown	8 pg	Assorted: e.g., TV,	32.4	32.7	X ↑
-----	---	General	400	200	1 pg; 8 pg	$100, Las Vegas trip	Unknown	Unknown	✓↑
Woodward	1985	General	200	100	21 Q	Free $100 dinner	60	73	X ↑
Roberts	2000	Women	1000	374	Unknown	£50	53.7	58.6	X ↑
Vogel	1992	Alcoholics	68	34	1 pg; 3 pg	$70	59.00	62.00	X ↑
-----	---	Alcoholics	64	34	1 pg; 1 Q	$280	39.00	52.50	X ↑
Brown	1997	Patients	1307	654	15 pg	£25 gift-voucher	65.8	70.5	X ↑
Moses	2004	Physicians	1,410	716	10 Q	PDA	62	64	X ↑
Blythe	1986	Soc. Work	518	259	7 pg	9 win: $20; 1 win: trip	39	52.12	✓↑
Hubbard	1988a	General	2,000	400	2 pg	$200	40.5	51.8	✓↑
Hubbard	1988b	Bank cust	3,150	450	2 pg	$50	26.9	29.6	X ↑
-----	---	---------	---	450	---	$100	---	28.2	X ↑
-----	---	---------	---	450	---	$150	---	28.7	X ↑
-----	---	---------	---	450	---	$200	---	37.8	✓↑
Baron	2001	Physicians	1,000	500	5 pg	Resort weekend	34.8	41.2	✓↑

[1] N Total includes all initial mailings.
[2] RR (response rate) reflects percentages. In the case of 2 mailings, these were obtained after the second wave.
[3] ✓ = a significant effect, X = an insignificant effect, ↓ = a decrease in RR, ↑ = an increase in RR, and = represents equal RR in both control and lottery

Table 2 [4]

Questionnaire Statements and Results

Statement	SD	D	NA/ND	A	SA
1) The practice quizzes helped me understand the textbook reading.	9%	4%	11%	45%	32%
2) The practice quizzes helped me prepare for exams.	6%	7%	23%	38%	27%
3) Receiving EC for a completely accurate quiz motivated me to complete & submit the quiz.	8%	4%	7%	23%	59%
4) Receiving EC for a completely accurate quiz motivated me to try & answer the questions correctly.	9%	2%	5%	17%	67%
5) Being given the opportunity to participate in a lottery for EC for a completely accurate quiz, motivated me to complete and submit the practice quiz.	11%	17%	21%	27%	24%
6) Being given the opportunity to participate in a lottery for EC for a completely accurate quiz, motivated me to try and answer the quiz questions correctly.	6%	12%	20%	34%	28%
7) Even without the EC points, I would have completed the quizzes; practice was sufficient motivation.	13%	21%	30%	22%	14%
8) I always knew when EC points were available & I knew what I had to do to obtain them.	65%	25%	2%	1%	7%

	Yes	No
9) I collaborated on the quizzes with another student at least once throughout the semester.	18	83

	B	C	L
10) If you answered yes to statement 9, please indicate the time of your collaboration.	4	15	0

[4] KEY
EC = Extra Credit
SD = Strongly Disagree
D = Disagree
NA/ND = Neither Agree nor Disagree
A = Agree SA = Strongly Agree

10) B (Baseline) = At a point where no EC was available for quiz submission.
C (Certain) = At a point when EC was available for submission of a completely accurate quiz (100%).
L (Lottery) = At a point when there was a lottery for EC.

Table 3

Conditions Ordered by Highest Submission Rates for Individual Participants

Order of Conditions	Baseline	Certain	Lottery	N	Percent
Certain, Lottery, Baseline	3	1	2	30	26.79
All Conditions Equal	1	1	1	20	17.86
Never Submitted	0	0	0	8	7.14
Submitted 1 in each	1	1	1	2	1.79
Always Submitted	6	6	6	10	8.92
Lottery = Certain, Baseline	2	1	1	19	16.96
Certain, Baseline = Lottery	2	1	2	12	10.71
Lottery, Certain, Baseline	3	2	1	7	6.25
Certain, Baseline, Lottery	2	1	3	6	5.36
Baseline = Lottery, Certain	1	2	1	6	5.36
Certain = Baseline, Lottery	1	1	2	4	3.57
Lottery, Certain = Baseline	2	2	1	4	3.57
Baseline, Lottery, Certain	1	3	2	2	1.79
Baseline, Certain, Lottery	1	2	3	1	0.89
Baseline, Lottery = Certain	1	2	2	1	0.89
			Total	112	100

Table 4

Individual Total Quiz Submissions and Scores for Baseline, Certain and Lottery Conditions

	Baseline		Certain		Lottery	
Participant	Submitted	Score	Submitted	Score	Submitted	Score
1	5.00	5.00	6.00	6.00	6.00	5.60
2	1.00	1.00	1.00	1.00	1.00	1.00
3	1.00	1.00	2.00	2.00	0.00	0.00
4	1.00	0.80	3.00	3.00	2.00	1.75
5	1.00	0.80	2.00	2.00	0.00	0.00
6	1.00	0.40	1.00	0.80	0.00	0.00
7	0.00		2.00	2.00	0.00	0.00
8	4.00	3.80	3.00	2.80	4.00	3.80
9	5.00	4.20	4.00	4.00	5.00	4.80
10	2.00	1.60	4.00	4.00	3.00	2.80
11	2.00	1.00	3.00	2.80	1.00	1.00
12	4.00	3.25	6.00	5.80	5.00	4.40
13	3.00	2.80	4.00	4.00	3.00	3.00
14	6.00	5.60	5.00	4.40	6.00	5.40
15	1.00	0.20	3.00	2.65	0.00	0.00
16	0.00		0.00		0.00	0.00
17	2.00	1.40	5.00	4.75	3.00	2.80
18	2.00	2.00	4.00	4.00	3.00	3.00
19	0.00		2.00	2.00	1.00	0.80
20	1.00	0.80	3.00	2.80	1.00	1.00
21	2.00	1.80	1.00	0.80	2.00	2.00
22	6.00	5.40	6.00	6.00	5.00	5.00
23	1.00	0.80	0.00		1.00	1.00
24	0.00		2.00	2.00	0.00	0.00
25	1.00	1.00	1.00	1.00	1.00	1.00
26	6.00	6.00	6.00	6.00	5.00	5.00
27	0.00		1.00	1.00	2.00	2.00
28	2.00	1.80	6.00	5.80	4.00	3.40
29	0.00		4.00	3.80	2.00	1.75
30	2.00	1.60	3.00	3.00	3.00	2.80
31	1.00	0.80	3.00	2.80	0.00	0.00
32	1.00	0.60	3.00	3.00	1.00	1.00
33	0.00		0.00		0.00	

Table 4 Continued

	Baseline		Certain		Lottery	
Participant	Submitted	Score	Submitted	Score	Submitted	Score
34	2.00	1.80	6.00	5.80	4.00	3.00
35	2.00	1.20	3.00	2.60	1.00	1.00
36	4.00	4.00	5.00	5.00	5.00	4.80
37	4.00	2.80	6.00	5.80	5.00	4.60
38	6.00	5.15	6.00	6.00	5.00	4.80
39	0.00		1.00	1.00	0.00	0.00
40	6.00	5.15	6.00	5.80	6.00	6.00
41	1.00	1.00	3.00	2.80	1.00	1.00
42	2.00	1.80	4.00	4.00	2.00	2.00
43	1.00	0.80	4.00	3.80	2.00	2.00
44	2.00	1.80	5.00	5.00	4.00	3.35
45	3.00	0.80	6.00	5.65	3.00	3.00
46	1.00	0.80	3.00	2.80	5.00	5.00
47	4.00	2.50	5.00	4.60	6.00	5.60
48	0.00		1.00	1.00	1.00	1.00
49	1.00	0.40	0.00		0.00	0.00
50	1.00	0.80	2.00	1.75	1.00	1.00
51	2.00	1.00	5.00	5.00	4.00	4.00
52	2.00	1.40	5.00	5.00	5.00	5.00
53	6.00	5.60	6.00	6.00	6.00	6.00
54	3.00	2.60	5.00	5.00	5.00	5.00
55	4.00	3.80	4.00	3.80	2.00	1.80
56	5.00	4.80	6.00	5.80	4.00	4.00
57	0.00		0.00	0.00	0.00	0.00
58	6.00	5.20	6.00	6.00	6.00	6.00
59	0.00		1.00	1.00	1.00	1.00
60	4.00	3.80	6.00	5.80	5.00	4.80
61	0.00		0.00		0.00	0.00
62	0.00		4.00	3.80	1.00	0.80
63	1.00	0.60	4.00	3.80	4.00	3.80
64	1.00	0.00	5.00	4.60	4.00	4.00
65	3.00	2.20	3.00	3.00	4.00	3.80
66	1.00	0.60	6.00	5.65	6.00	6.00
67	2.00	1.80	5.00	5.00	4.00	3.60
68	3.00	2.80	0.00	0.00	2.00	2.00

Table 4 Continued

	Baseline		Certain		Lottery	
Participant	Submitted	Scores	Submitted	Scores	Submitted	Scores
69	4.00	4.80	5.00	4.60	6.00	6.00
70	0.00		0.00		1.00	0.75
71	1.00	0.80	2.00	1.80	2.00	2.00
72	3.00	1.60	5.00	4.80	3.00	3.00
73	2.00	1.80	2.00	2.00	1.00	1.00
74	6.00	5.20	6.00	5.80	6.00	6.00
75	0.00		4.00	4.00	1.00	1.00
76	2.00	1.80	5.00	4.80	5.00	5.00
77	1.00	1.00	3.00	3.00	2.00	2.00
78	2.00	1.40	1.00	0.80	2.00	2.00
79	0.00		0.00		0.00	0.00
80	2.00	3.00	5.00	5.00	4.00	4.00
81	1.00	0.40	4.00	3.80	5.00	4.00
82	3.00	2.35	4.00	3.60	5.00	5.00
83	0.00		2.00	2.00	2.00	2.00
84	3.00	2.60	4.00	3.60	6.00	5.80
85	1.00	0.80	5.00	4.80	4.00	4.00
86	2.00	1.80	5.00	5.00	3.00	3.00
87	6.00	4.60	6.00	6.00	6.00	6.00
88	4.00	3.35	6.00	5.80	6.00	5.80
89	0.00		0.00		0.00	0.00
90	1.00	1.00	4.00	3.80	2.00	2.00
91	0.00		5.00	5.00	4.00	4.00
92	0.00		4.00	3.80	1.00	1.00
93	2.00	1.40	5.00	4.40	3.00	3.00
94	5.00	4.60	6.00	5.80	6.00	6.00
95	3.00	2.80	4.00	3.80	2.00	2.00
96	6.00	6.00	6.00	6.00	6.00	6.00
97	3.00	2.20	6.00	5.60	4.00	3.75
98	0.00		2.00	2.00	1.00	1.00
99	3.00	2.10	3.00	2.80	3.00	3.00
100	0.00		6.00	6.00	5.00	5.00
101	0.00		6.00	6.00	5.00	5.00
102	5.00	5.00	6.00	6.00	6.00	5.80
103	2.00	1.60	6.00	5.00	6.00	6.00

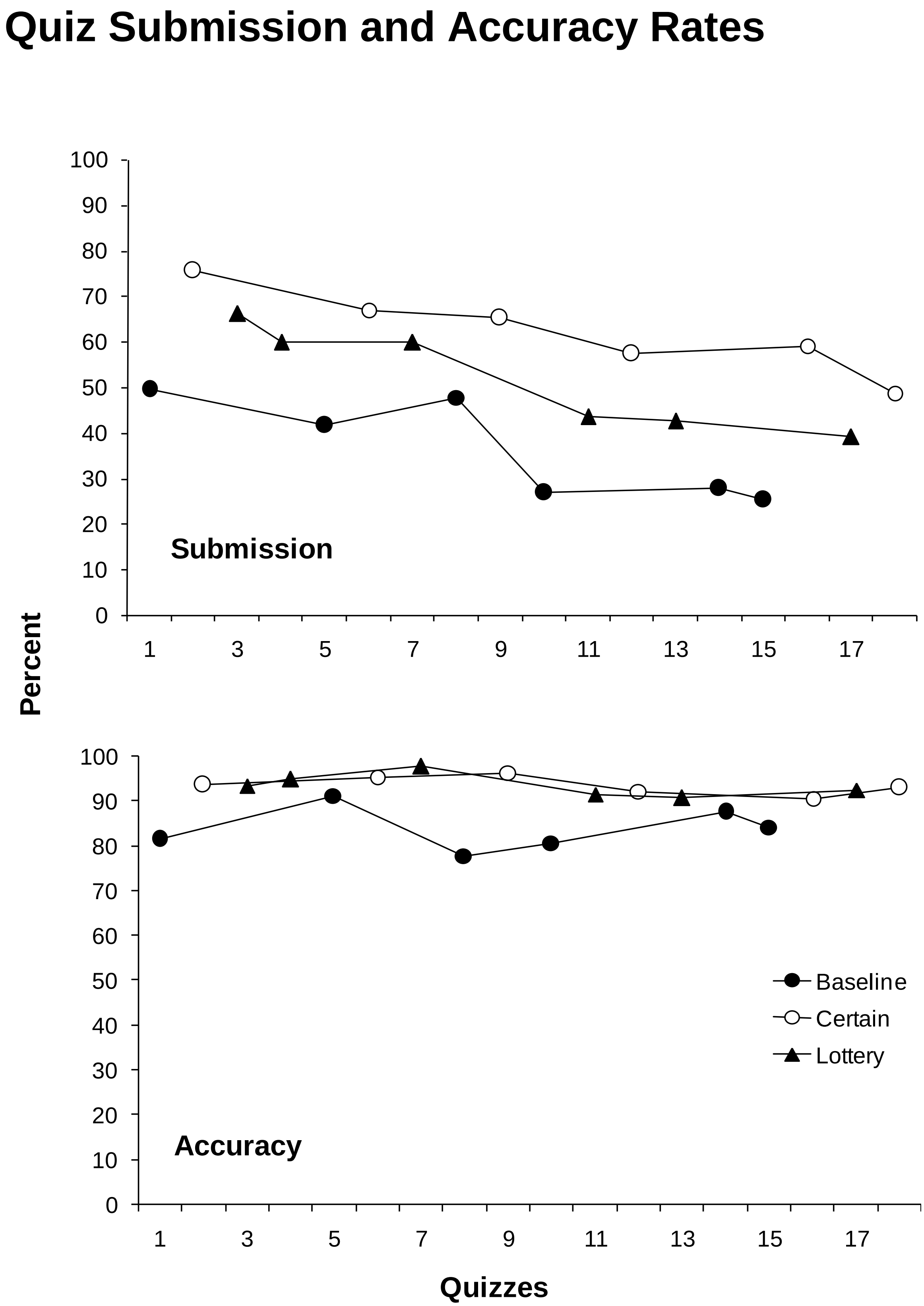
Table 4 Continued

	Baseline		Certain		Lottery	
Participant	Submitted	Scores	Submitted	Scores	Submitted	Scores
104	2.00	1.80	4.00	3.80	5.00	5.00
105	5.00	4.80	2.00	2.00	4.00	3.80
106	1.00	0.60	1.00	1.00	3.00	3.00
107	2.00	1.25	5.00	4.80	5.00	5.00
108	1.00	1.00	6.00	5.80	6.00	5.80
109	6.00	5.80	6.00	5.60	6.00	6.00
110	1.00	0.80	6.00	5.80	3.00	3.00
111	6.00	6.00	6.00	5.80	6.00	6.00
112	1.00	1.00	1.00	1.00	1.00	1.00
Total	245	208.7	416	401.45	347	335.55
Percent	[5]36.50%	[6]82.82%	62.00%	93.80%	51.67%	93.99%

[5] Group submission percents were derived by dividing the total number of submissions per condition by the total number of possible submissions (i.e., 672, because N = 112 and students were able to submit a total of six quizzes per condition) and multiplying by 100.

[6] Group accuracy percents were derived by dividing the total of all individual scores for that condition by the total number of submissions for that condition and multiplying by 100. Scores on a given quiz ranged from 0 – 1 (i.e., with .5 being a score of 50% and 1 being a score of 100%). Individual scores were calculated by summing the scores on each participant's submitted quizzes for a given condition, making six the highest possible individual score (but equal in accuracy to a student who only submitted two quizzes and had a score of two for a given condition).

Figure 1. Submission and accuracy rates in Baseline, Certain and Lottery conditions.



<u>Figure 2</u>. Accuracy rates for quizzes submitted by lottery winners 1-3. Sessions without data points indicate a session in which the participant did not submit a quiz. The y axis includes both numbers and letters. Numbers indicate the quiz number, and letters indicate the condition: B = Baseline, C = Certain, L = Lottery.

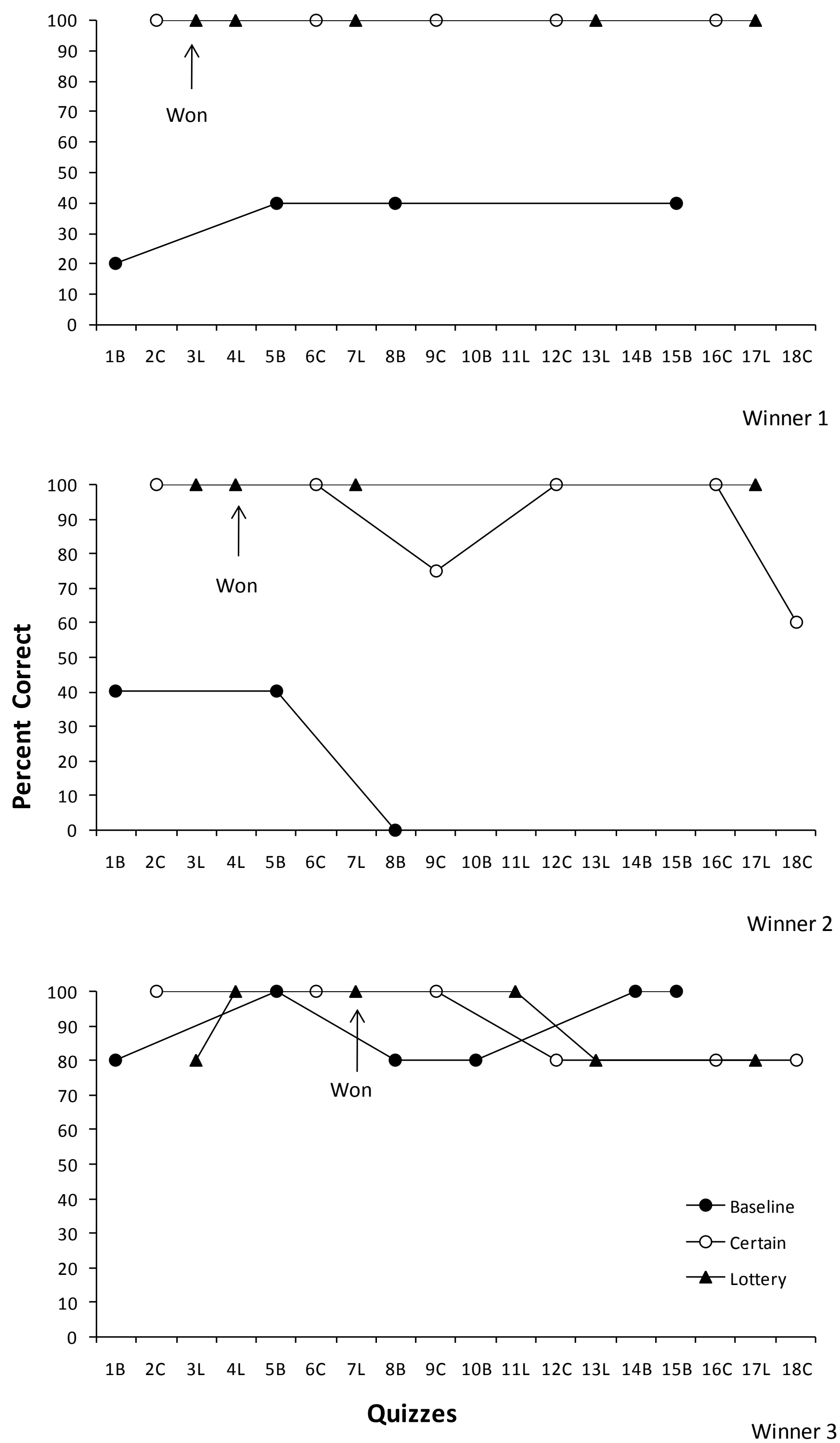
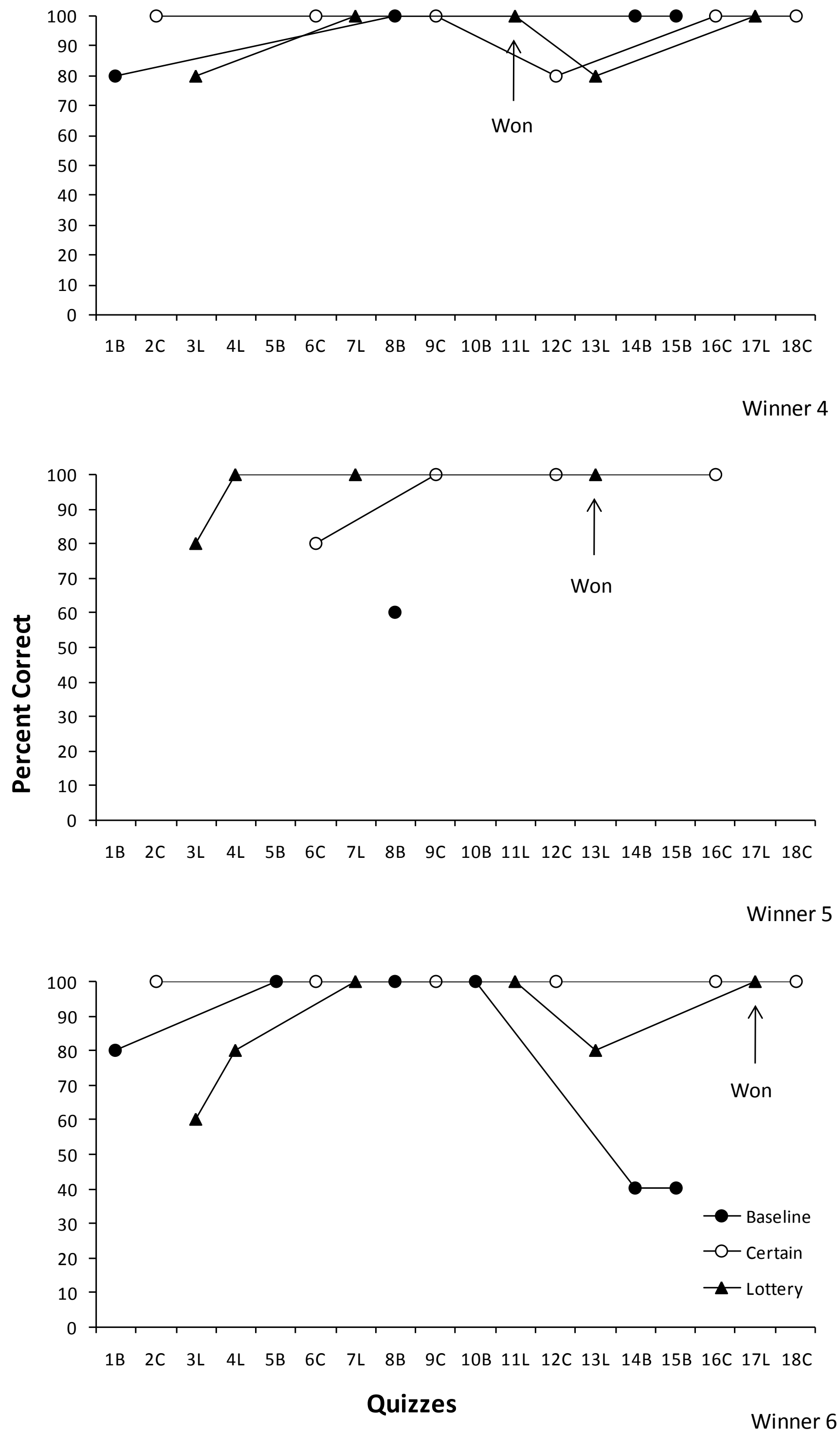


Figure 3. Accuracy rates for quizzes submitted by lottery winners 4-6. Sessions without data points indicate a session in which the participant did not submit a quiz. The y axis includes both numbers and letters. Numbers indicate the quiz number, and letters indicate the condition: B = Baseline, C = Certain, L = Lottery.



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