Journal of the
Experimental Analysis of Behavior

TEMPORAL FRAMING AND THE HIDDEN-ZERO EFFECT: RATE-DEPENDENT OUTCOMES ON DELAY DISCOUNTING

Gideon P. Naudé, Brent A. Kaplan, Derek D. Reed, Amy J. Henley, and Florence D. DiGennaro Reed

UNIVERSITY OF KANSAS

Recent research suggests that presenting time intervals as units (e.g., days) or as specific dates, can modulate the degree to which humans discount delayed outcomes. Another framing effect involves explicitly stating that choosing a smaller–sooner reward is mutually exclusive to receiving a larger–later reward, thus presenting choices as an extended sequence. In Experiment 1, participants ($N = 201$) recruited from Amazon Mechanical Turk completed the Monetary Choice Questionnaire in a 2 (delay framing) by 2 (zero framing) design. Regression suggested a main effect of delay, but not zero, framing after accounting for other demographic variables and manipulations. We observed a rate-dependent effect for the date-framing group, such that those with initially steep discounting exhibited greater sensitivity to the manipulation than those with initially shallow discounting. Subsequent analyses suggest these effects cannot be explained by regression to the mean. Experiment 2 addressed the possibility that the null effect of zero framing was due to within-subject exposure to the hidden- and explicit-zero conditions. A new Amazon Mechanical Turk sample completed the Monetary Choice Questionnaire in either hidden- or explicit-zero formats. Analyses revealed a main effect of reward magnitude, but not zero framing, suggesting potential limitations to the generality of the hidden-zero effect.

Key words: delay discounting, framing, temporal attention, computer mouse click, humans

Temporal discounting is a behavioral process wherein subjective reward value is discounted as a function of the delay to its receipt. The behavioral manifestation of delay discounting involves an organism's relative allocation of responding in favor of smaller immediate rewards ("smaller–sooner rewards"; SSR) over larger-delayed alternatives ("larger–later rewards"; LLR; Mazur, 1987). In this higher-order relation between reward magnitude and delay, consistent preference for SSR is considered analogous to a form of temporal myopia, with consistent preference for LLR considered analogous to a form of more disciplined responding (Ainslie, 1974; Critchfield & Reed, 2009; Madden & Bickel, 2010; however, see Holt, Green, & Myerson, 2003). As a temporally extended pattern of decision making, researchers have debated whether delay discounting is best classified as either a trait or a state variable. Strain differences in rats' delay discounting, as well as epigenetic work and evidence of enduring patterns of discounting over time provide support for trait considerations (e.g., Anokhin, Golosheykin, Grant, & Heath, 2011; Anokhin, Grant, Mulligan, & Heath, 2015; Mitchell, 2011; Wilhelm & Mitchell, 2009; see review by Gray & MacKillop, 2015; also see Odum, 2011a). Nevertheless, a proliferation in efforts to identify ways to modulate delay discounting provides strong evidence that this phenomenon can be state influenced (Gray & MacKillop, 2015; Koffarnus, Jarmolowicz, Mueller, & Bickel, 2013) by factors including choice context (e.g., gambling and nongambling environments; Dixon, Jacobs, & Sanders, 2006), sleep deprivation (Reynolds & Shiffbauer, 2004), and time constraints (Ebert, 2001); thus, delay discounting appears to be an interaction of *both* trait and state variables (see Odum, 2011a, b).

Successful attempts to modulate temporal discounting (for a thorough review, see

Gideon P. Naudé, Department of Applied Behavioral Science, University of Kansas. Brent A. Kaplan, Department of Applied Behavioral Science, University of Kansas. Derek D. Reed, Department of Applied Behavioral Science, University of Kansas. Amy J. Henley, Department of Applied Behavioral Science, University of Kansas. Florence D. DiGennaro Reed, Department of Applied Behavioral Science, University of Kansas. The authors thank Bryan Yanagita for assisting with survey development, Todd McKerchar for helpful feedback on data interpretations, as well as Brady DeHart and Greg Madden for their suggestions regarding analysis. Brent A. Kaplan is now at Virginia Tech Carilion Research Institute. Amy J. Henley is now at Western New England University.

Address correspondence to Derek D. Reed, 4048 Dole Human Development Center, 1000 Sunnyside Avenue, Lawrence, KS 66045. E-mail: dreed@ku.edu

doi: 10.1002/jeab.328

Koffarnus et al., 2013) have included therapeutic interventions such as working memory training (e.g., Bickel, Yi, Landes, Hill, and Baxter, 2011; cf., Rass et al., 2015), money management training (Black and Rosen, 2011), contingency management (e.g., Landes, Christiansen, & Bickel, 2012; Yi et al., 2008), episodic future thinking (e.g., Daniel, Said, Stanton, & Epstein, 2015; Kaplan, Reed, & Jarmolowicz, 2016b; Stein et al., 2016) as well as transitory exposure to Acceptance and Commitment Therapy (Black and Rosen, 2011; Hendrickson & Rasmussen, 2013; Morrison, Madden, Odum, Friedel, & Twohig, 2014) and mindfulness training (see review by Ashe, Newman, & Wilson, 2015). Although the experimental groups in these studies demonstrated less temporal discounting than their respective controls, the adoptability of these interventions may be tempered by the time and resources required in their implementation (DeHart & Odum, 2015).

An alternative approach requiring minimal resources for changing temporal discounting involves manipulating the manner in which choices are framed (see review by Koffarnus et al., 2013). For example, Read, Frederick, Orsel, and Rahman (2005) examined the degree to which students and community-recruited participants devalued monetary rewards when the delay to receipt of the rewards was framed either as calendar units (e.g., 21 weeks or 18 months) or as specific dates (e.g., March 7, 2008). The researchers found participants more frequently chose the LLR when delays were framed as specific dates than when delays were framed in calendar units. This *date-delay effect* was consistent across both hypothetical and real rewards, as well as across response modes that included fill-in-the-blank, fixed sequence, and computerized titration procedures, suggesting the effect to be rather robust in terms of experimental preparation.

Using a more established discounting instrument, Klapproth (2012) compared the date-delay effect using modifications to the 27-item Monetary Choice Questionnaire (Kirby, Petry, & Bickel, 1999; e.g., SSR "today" vs. a delay framed as calendar units ["7 days from now" or a specific date "September 14, 2016"]) between individuals receiving inpatient treatment for substance use disorders ($n = 30$) and a control group with no history of drug use ($n = 30$) composed largely of undergraduate students. The individuals with substance use disorders showed higher LLR choices in the *date*-framed condition compared to the calendar units condition, whereas the control group displayed minimal differences between conditions. These results appear to suggest some boundary conditions for the magnitude of the date-delay effect; specifically, the date-delay manipulation may disproportionately affect individuals exhibiting steeper delay discounting (e.g., those with substance use disorders; cf. Mitchell & Wilson, 2012). Alternatively, individuals already exhibiting shallow discounting may experience a ceiling effect as a function of having less room to detect subtle changes evoked by the date-delay manipulation.

In a further examination of the date-delay effect, DeHart and Odum (2015) assessed temporal discounting via a task modeled after the Du, Green, and Myerson (2002) adjusting amount procedure using calendar units (e.g., "1 week") and either specific dates (e.g., "September 21, 2016") or a number of calendar *days* (e.g., "7 days") as the units of delay in 76 undergraduate participants. Trials in each framing condition began with a choice between a hypothetical $50 now and $100 after (delay). The value of the SSR either decreased or increased by $25 in the subsequent trial (half the difference of the SSR and LLR on the initial trial) depending on whether the participant chose the SSR or LLR, respectively. Each subsequent adjustment was half of the previous adjustment, narrowing in on an indifference point after the 10th choice, and then repeating across the remaining five delays (e.g., 2 weeks, 1 month, 6 months, 5 years, and 25 years). Consistent with previous studies, undergraduate participants made more LLR choices when the task framed delays as calendar units than as specific dates (Read et al., 2005; Klapproth, 2012). However, the condition in which the delays were framed as a number of *days* resulted in steeper discounting than calendar units. These findings suggest that framing delays as days results in fewer LLR choices than when framing delays as either calendar units or specific dates. Lempert, Johnson, and Phelps (2016) produced results at odds with previous demonstrations of the date-delay effect, reporting that a sample consisting primarily of university students failed to demonstrate a relative preference for the LLR in the specific date conditions. The authors addressed several possible explanations for the discrepant finding, offering that the lack of effect may have been the result of procedural differences (e.g., sequential, rather than simultaneous presentation of the SSR and LLR, within individual comparisons) and/or the length of the delays used in the intertemporal choice task. Indeed, participants experienced several delays notably shorter than those used in published demonstrations of the date-delay effect (e.g., DeHart & Odum, 2015; Klapproth, 2012; LeBoeuf, 2006; Read et al., 2005). Lempert et al. note that the use of shorter delays and the absence of the date-delay effect aligns with results reported by LeBoeuf (2006), who found that participants differentially responded to the specific date framing only when the delays exceeded 2 months. Lempert et al. provided supporting evidence of this temporal boundary by comparing the two framing conditions using either the longest delays (100 and 180 days) or the shortest delays (7 and 30 days), finding statistically significant effects of date-delay framing for only the longer delays ($t_{50} = 2.26$; $p = 0.03$; Cohen's $d = 0.64$). Ultimately, the findings in Lempert et al. support the date-delay framing phenomenon while identifying possible boundaries to its demonstration.

Another potential framing phenomenon known as the *hidden-zero effect* has been offered as a possible explanation for why some individuals tend to place reduced value on distal outcomes. Magen, Dweck and Gross (2008) provided a sample of online-recruited participants with a 15-item binary-choice monetary temporal discounting questionnaire using either hypothetical ($n = 112$) or real ($n = 57$) rewards, presented either in standard form (e.g., "Which would you prefer: $5 today or $6.20 in 26 days") or appended with an *explicit-zero statement* (e.g., "Which would you prefer: $5 today and $0 in 26 days or $0 today and $6.20 in 26 days"). In this task, LLR delays ranged from 7 to 140 days, with LLR magnitudes ranging from $5.40 to $8.50. The explicit-zero statement stipulated that the consequence of choosing the SSR meant forgoing the LLR ($0 at its respective delay), and, conversely, that selecting the LLR meant receiving $0 now (no SSR). The authors found that participants demonstrated less discounting in the explicit-zero condition across both real and hypothetical rewards. In response to these findings, Magen et al. noted that monetary discounting tasks typically present each choice as a discrete decision (i.e., implicit communication regarding the alternative choice); however, by revealing the hidden-zero, the choice explicitly becomes an extended temporal tradeoff/sequence, resulting in choices favoring future outcomes.

Radu, Yi, Bickel, Gross, and McClure (2011) assessed hidden- and explicit-zero framing of past and future rewards and evaluated two hypotheses as possible explanations for the phenomenon. The *improving sequence hypothesis* (Magen et al., 2008; see also Loewenstein & Prelec, 1993; Loewenstein & Sicherman, 1991; and Ross & Simonson, 1991) purports that choices framed as receiving nothing today but something later renders the chooser more likely to select the LLR due to the positively changing circumstances. On the other hand, when the choice is framed as receiving something now, yet nothing after the respective delay, the outcome is a declining sequence. The alternative *temporal attention hypothesis* offers that introducing distal $0.00 outcomes as a consequence of choosing the SSR may shift attentional mechanisms away from the present, thereby increasing the subjective value of the LLR. The authors concluded that the temporal attention hypothesis better explains the effect of zero framing by accounting for a greater number of LLR choices in both past and future discounting. The improving sequence hypothesis, when applied to past discounting, predicts more SSR choices (e.g., "$6.90 one hour ago and $0 102 days ago, or $0 one hour ago and $8.70 102 days ago," where the former is an improving sequence, yet the SSR of the two choices). Toward this end, shifting temporal attention and reducing bias toward the present may account for greater preference for the LLR reported in the current literature on zero framing.

Examining the combined effects of framing delays and outcomes may add to the body of evidence supporting the malleability of discounting. The present study examined the date-delay effect (comparing days and specific dates) in conjunction with the hidden-zero effect using a validated monetary discounting assessment (i.e., the 27-item Monetary Choice

Questionnaire [MCQ]; Kirby, Petry, & Bickel 1999) in a crowdsourced United States sample. Despite the MCQ being one of the most psychometrically evaluated (e.g., Duckworth & Seligman, 2005; Kirby, 2009; Kirby & Finch, 2010; Kirby & Petry, 2004; Kirby et al., 1999) and widely used discounting instruments (see reviews by Kaplan, Amlung et al., 2016, MacKillop et al., 2011), we are aware of no published research investigating the hidden-zero effect using this procedure. Thus, a secondary aim was to evaluate the degree to which these framing effects—in isolation or in combination—are reproduced using the MCQ (cf. Klapproth, 2012).

Experiment 1

Methods

Participants and setting. Two hundred and one participants were recruited from the online crowdsourcing platform Amazon Mechanical Turk (MTurk; www.mturk.com) where requesters post brief tasks (referred to as Human Intelligence Tasks or HITs) that individuals can complete for pay. In the current study workers could only view the HIT if their payment account is based in the United States and if their previous work on MTurk had been approved by requesters in at least 95% of at least 100 HITs. Upon completion of our HIT, participants were provided a unique code which authorized a payment of $0.25 to their Amazon account.

One hundred and ninety participants (103 females; age: M = 33.36 years, SEM = 0.82) were retained in the study (see Results for exclusionary criteria). Seventy-one percent of participants (n = 134) reported their combined household income as being under $60,000 and 46% ($n$ = 88) reported holding at least bachelor's degree, with an additional 40% (n = 76) reporting some college experience. When asked the question "Have you smoked cigarettes?", 18.37% (n = 36) of the total sample identified as current smokers, 27 % (n = 53) as previous smokers, and 53.15 % (n = 101) as never having smoked. Group assignment was stratified using a randomization feature in Qualtrics® (www.qualtrics.com) that evenly distributed smokers into each condition by reported cigarette use (*more than 1 pack per day; 1 pack per day; half a pack per day; less than half a pack per day; more than 1 cigarette per week*). Note, however, that our current smokers reported a mean Fagerström Test of Nicotine Dependence (Heatherton, Kozlowski, Frecker, & Fagerström, 1991) score of 3.68 (SEM = 0.31), within the low to moderate dependence range, with a mean of 2.23 cigarettes smoked per day (SEM = 0.23). Thus, we do not interpret such participants as constituting a clinical sample.

We employed chi-squared analyses to examine proportions of men to women, as well as self-reported smoking status, across the groups; in both cases, we identified no statistically significant differences for either gender or smoking status, $\chi^2(3, N = 190) = 1.812$, $p = 0.612$, and $\chi^2(6, N = 190) = 0.868$, $p = 0.990$, respectively. Kruskal-Wallis tests suggested no statistically significant differences across groups in age, $\chi^2(3) = 2.38$, $p = 0.497$, or scores on the Consideration of Future Consequences Scale (see below), $\chi^2(3) = 4.027$, $p = 0.259$.

Materials

All materials were created and administered using Qualtrics® and approved by the Human Subjects Committee of the institutional review board under KU IRB# STUDY00001950.

Initial discounting assessment. Each participant completed the standard 27-item MCQ (Kirby, Petry, & Bickel, 1999), which consists of 27 dichotomous choices between a small, immediately available amount of money (SSR) and a larger amount delivered some number of days in the future (LLR).[1] The 27-item MCQ can be grouped into three magnitude categories: small ($25, $30, or $35), medium ($50, $55, or $60) and large ($75, $80, or $85). Within each magnitude grouping are nine logarithmically spaced k values (Kirby, 2009; Myerson, Baumann, & Green, 2016) that comprise the discount rate parameter for which the SSR and LLR are equivalent according to Mazur's (1987) single parameter hyperbolic discounting equation.

[1]For this and all other MCQ conditions described in this paper, participants received the following instructional text at the start of each MCQ task: "For each of the next 27 choices, please mark which hypothetical reward you would prefer: the smaller reward today, or the larger reward in specified number of days. While you will not actually receive the rewards, pretend you will actually be receiving the amount you indicate and answer honestly."

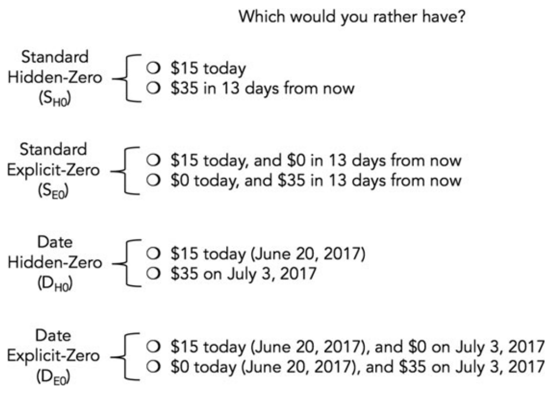

Fig. 1. Depictions of one trial from the MCQ in each framing condition (labeled on far left). See text for details.

MCQ manipulations. Participants received one of four versions of the MCQ (see Fig. 1 for examples). Participants assigned to the *standard hidden-zero* (S_{H0}; $n = 52$) condition received the original MCQ a second time and served as our control group. The *standard explicit-zero* (S_{E0}; $n = 48$) condition varied from the original MCQ in that it was affixed with a statement indicating that as a consequence of choosing the smaller–sooner reward (SSR), one would receive $0 a specific number of calendar day units in the future (e.g., "$15 today, and $0 in 13 days from now"). Conversely, the larger–later reward (LLR) option was accompanied by a statement indicating that the participant would receive $0 today (e.g., "$0 today, and $35 in 13 days from now"). Participants in the *date hidden-zero* (D_{H0}; $n = 45$) condition received statements equivalent to those in the S_{H0} condition, but with a specific date depicted in lieu of and equivalent to the calendar day units. Finally, participants in the *date explicit-zero* (D_{E0}; $n = 45$) condition received a statement indicating that choice of the SSR would result in $0 on a specific *date* in the future and that choosing the LLR would result in $0 today (e.g., "$15 today (June 20, 2017), and $0 on July 3, 2017 [OR] $0 today (June 20, 2017), and $35 on July 3, 2017"). All dates were equivalent to the number of days used in the S_{H0} and S_{E0} conditions. The Date/Time Piped Text function of the Qualtrics® research software displayed the specific date of the HIT to participants and also produced the accurate future date corresponding to the delay.

Consideration of Future Consequences. The HIT concluded with the Consideration of Future Consequences Scale (CFC; Strathman, Gleicher, Boninger, & Edwards, 1994) which assesses the degree to which an individual is oriented toward present versus future outcomes. The scale has demonstrated both good internal consistency and test–retest reliability (Murphy et al., 2012; Strathman et al., 1994). Participants read 12 statements pertaining to immediate and delayed consequences and indicated how characteristic these statements were of themselves on a 5-point Likert scale. As an example, participants would read the statement, "I think that sacrificing now is usually unnecessary since future outcomes can be dealt with at a later time," and then provide a rating between 1 ("extremely uncharacteristic") and 5 ("extremely characteristic").

Results

Discount rate and consistency. We obtained overall *k* values and consistency scores for each participant using an automated scorer for the 27-item MCQ (Kaplan, Lemley, Reed, & Jarmolowicz, 2014; see Kaplan, Amlung et al., 2016 for specific instructions). Consistency scores reflect the proportion of choices that align with an assigned *k* value as defined by the MCQ (Kirby et al., 1999). A consistency score is calculated by summing the number of SSR choices prior to an assigned *k* value and the number of LLR choices occurring at and after that *k* value, then dividing by the total number of choices possible (Kaplan, Amlung et al., 2016). Participants with overall consistency below 75% were flagged and excluded listwise from the analyses; we excluded 1, 3, 2, and 5 participants from S_{H0}, S_{E0}, D_{H0}, and D_{E0}, respectively. The overall values of *k* were natural-log (ln) transformed to better approximate a Gaussian distribution (Kirby et al., 1999; initial k_{skew} = 2.097; initial $\ln k_{skew}$ = -0.431). Skewness values for the four variants of the MCQ before and after (in parentheses) ln transformation were as follows: $S_{H0\text{-control}}$ = 1.689 (-0.493); S_{E0} = 2.008 (-0.38); D_{H0} = 2.451 (-0.127); and D_{E0} = 3.2 (-0.114).

Table 1 summarizes the MCQ ln*k* values and consistency scores for all groups/conditions. Notably, ln*k* values were reliably highest in the small magnitudes and lowest in the large magnitudes across all groups and conditions (see Fig. 2), consistent with the robust magnitude effect in the delay discounting literature

Table 1

Mean discounting parameters and consistency scores across magnitudes, groups, and conditions

		Initial (standard MCQ)				Post (framed MCQ)			
Group	Size	ln*k*	*SEM*	Consistency (%)	*SEM* (%)	ln*k*	*SEM*	Consistency (%)	*SEM* (%)
Standard $_{H0}$	Small	-3.63	0.24	99.16	0.41	-3.78	0.25	99.57	0.30
	Medium	-4.22	0.26	98.53	0.52	-4.35	0.27	99.57	0.30
	Large	-4.69	0.26	98.53	0.52	-4.66	0.28	99.36	0.36
Standard $_{E0}$	Small	-4.11	0.27	99.55	0.32	-3.99	0.26	98.84	0.76
	Medium	-4.46	0.28	99.32	0.38	-4.57	0.29	99.54	0.32
	Large	-5.04	0.29	99.77	0.23	-4.98	0.29	99.31	0.39
Date $_{H0}$	Small	-3.74	0.25	100.00	0.00	-4.10	0.25	99.01	0.48
	Medium	-4.28	0.27	98.52	0.57	-4.48	0.27	98.77	0.53
	Large	-4.67	0.27	98.02	0.64	-5.01	0.28	99.01	0.59
Date $_{E0}$	Small	-3.95	0.22	98.15	0.90	-4.61	0.26	98.55	0.56
	Medium	-4.52	0.26	96.76	0.74	-5.08	0.27	96.86	0.82
	Large	-4.88	0.25	96.99	0.86	-5.44	0.28	98.55	0.66

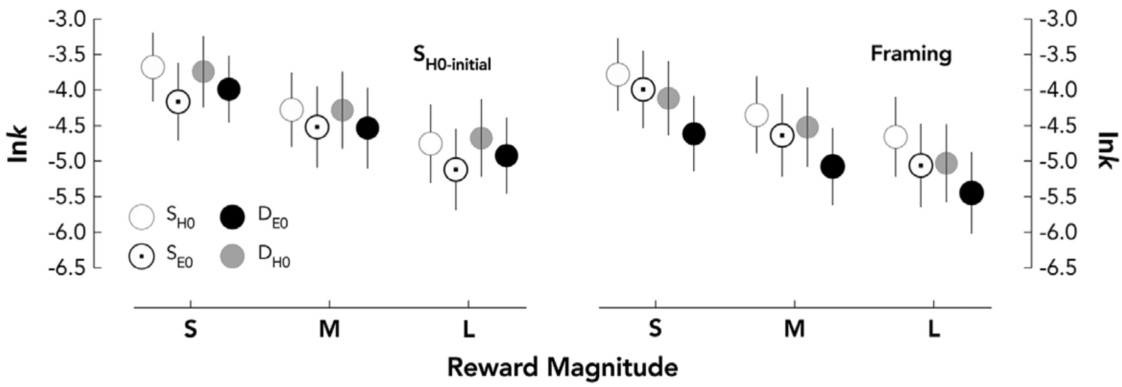

Fig. 2. Left panel depicts magnitude effects (mean ln*k* with 95% CIs) in the standard (hidden-zero: H0) MCQ across all participants during initial measurement. The right panel depicts magnitude effects (mean lnk with 95% CIs) for each of the four groups in the second experimental presentation of the MCQ. S_{H0} represents the standard hidden zero, S_{E0} represents the standard explicit zero, D_{E0} represents the date explicit zero, and D_{H0} represents the date hidden zero groups, respectively.

(Green, Myerson, Oliveira, & Chang, 2013). All data were analyzed using SPSS® version 23, with the exception of the linear regression analyses, which were performed in R 3.3.2 (R Core Team, 2016) using the rockchalk (Johnson, 2016) and lmtest (Zeileis & Hothorn, 2002) packages. A two-way repeated measures analysis of variance (ANOVA) of the aggregate initial MCQ dataset yielded a statistically significant effect of reward magnitude on ln*k*, $F(2, 372) = 144.663$, $p < .0001$, $\eta_p^2 = 0.437$, but not of group assignment, $F(3, 186) = 2.018$, $p = 0.113$. This result suggests that initial rates of discounting did not differ between groups prior to the framing manipulations. Mean consistency scores were greater than 98% across all groups/conditions (see Table 1). A two-way repeated measures ANOVA failed to yield a statistically significant interaction effect of (post-) framing group ×reward magnitude, $F(2, 372) = 1.231$, $p = 0.29$. Subsequent analyses assessed the effects of demographic variables and initial ln*k* as predictors of postmanipulation ln*k* values for each of the groups, focusing on the overall measure of ln*k* instead of organizing ln*k* by individual magnitudes.

Regression. We first fit a full, unrestricted model[2] to predict post-lnk from the following

[2]We conducted model diagnostics and the Breusch-Pagan test (Breusch & Pagan, 1979) to examine the presence of heteroscedasticity. Neither method revealed significant heteroscedasticity so we chose not to use robust (sandwich) estimation of standard errors.

predictor variables: gender, smoking status, age, CFC, experimental group, and initial ln$k$. Despite no apparent differences with respect to gender, smoking status, age, or CFC distributions across groups (see *Participants & Setting* section above), we chose to retain these variables in the regression (Model 1; see Table 2) in an effort to statistically evaluate whether adjusted group data (using these variables) potentially influence post-lnk. Results of this analysis revealed no statistically significant effects of age, gender, smoking status, or CFC on post-lnk. To confirm that none of these variables were significantly associated with post-lnk, we manually dropped each predictor variable one at a time and compared the nested models using an F-test with attention to fluctuations in the regression coefficients. For brevity, we present a subset of these models in Table 2. Model 2 retained the predictors' initial ln$k$, manipulation group, and the interaction term: initial ln$k$ × manipulation group (AIC = 423.95). The final models included manipulation group and initial ln$k$ (Model 3; AIC = 421.96) and temporal framing group (i.e., standard calendar units vs. specific date) and initial ln$k$ (Model 4; AIC = 422.13). In Model 3, with all else in the model held constant, the effects of D_{H0} and D_{E0} group assignment resulted in a decrease of 0.41 and 0.69 units, respectively, compared to the control (i.e., S_{H0}) group. Additionally, each unit increase in initial ln$k$ was associated with nearly a unit increase in post-lnk, as would be expected given the relative dependency of these measures. Results of an F-test suggested no statistically significant differences between the two date conditions, $F(2, 185) = 2.05$, $p = 0.13$. In Model 4, we collapsed the four manipulation groups into two temporal framing groups: the combined standard calendar units delay (S_{H0}/S_{E0}) and combined specific dates (D_{H0}/D_{E0}). These results corroborated those obtained in Model 3. Analyses of the full

Table 2
Multiple regression nested model comparison

	Model 1		Model 2		Model 3		Model 4	
	β	*SE*	*β*	*SE*	*β*	*SE*	*β*	*SE*
(Intercept)	-0.703	(0.639)	-0.038	(0.255)	-0.277	(0.159)	-0.221	(0.149)
Age	-0.002	(0.005)						
Gender	0.043	(0.111)						
Consideration of Future Consequences	0.022	(0.014)						
Initial ln$k$	1.024***	(0.062)	0.992***	(0.056)	0.935***	(0.029)	0.936***	(0.029)
Ex-smoker	-0.658	(0.344)						
Current smoker	-0.080	(0.360)						
Manipulation group-S_{E0}	0.159	(0.384)	0.044	(0.377)	0.105	(0.145)		
Manipulation group-D_{H0}	-0.675	(0.397)	-0.762*	(0.377)	-0.406**	(0.147)		
Manipulation group-D_{E0}	-1.417***	(0.400)	-1.346***	(0.395)	-0.694***	(0.147)		
Initial ln$k$ × Ex-smoker	-0.166*	(0.074)						
Initial ln$k$ × Current smoker	-0.020	(0.086)						
Initial ln$k$ × Manipulation group: S_{E0}	0.004	(0.080)	-0.018	(0.079)				
Initial ln$k$ × Manipulation group: D_{H0}	-0.068	(0.088)	-0.085	(0.083)				
Initial ln$k$ × Manipulation group: D_{E0}	-0.166	(0.085)	-0.150	(0.084)				
Temporal framing: Date							-0.600***	(0.105)
N	190		190		190		190	
RMSE	0.716		0.720		0.721		0.725	
R^2	0.860		0.853		0.850		0.847	
Adjusted R^2	0.849		0.847		0.847		0.845	
AIC	428.653		423.95		421.96		422.133	

Note: For categorical variables, gender was coded as 0 = Female, smoking status was coded as 0 = Never smoker, manipulation group was coded as 0 = S_{H0}, and temporal framing group was coded as 0 = Delay framing. * $p \leq 0.05$ ** $p \leq 0.01$ *** $p \leq 0.001$.

sample (prior to consistency score exclusions) produced highly similar results (see Appendix).

Rate dependence and regression to the mean. Effects of rate dependence and regression to the mean (RtM) were assessed in accordance with the methods used by Bickel, Landes, Kurth-Nelson, and Redish (2014) and to address the call for such findings by Snider, Quisenberry, and Bickel (2016). To determine for which of the collapsed manipulation groups initial ln*k* predicted the difference between initial ln*k* and post-lnk—hereafter referred to as Δ ln*k* —we regressed Δ ln*k* onto initial ln*k* centered around its mean (see Fig. 3), and in doing so, were able to use the regression intercept as a measure of predicted change in Δ ln*k* independent of initial ln*k* (Bickel et al., 2014). We detected statistically significant rate dependent effects within the date-framed group ($p < 0.05$), but not within the standard group ($p = 0.834$). In testing for RtM, post-lnk as predicted by initial ln*k*, is expressed as $(1- \rho)\ \mu + \rho \ln$ (initial k), where μ is the mean of the initial ln*k* values and ρ is the rank correlation between initial- and post-ln*k*. RtM occurs when mean Δ ln*k* is equal to 0. Subsequent RtM analyses using a 95% confidence interval (CI) for slope estimates in the *date* condition did not contain zero (-0.72 to -0.38), allowing us to reject the possibility that the significant rate dependent effects are merely the result of RtM.

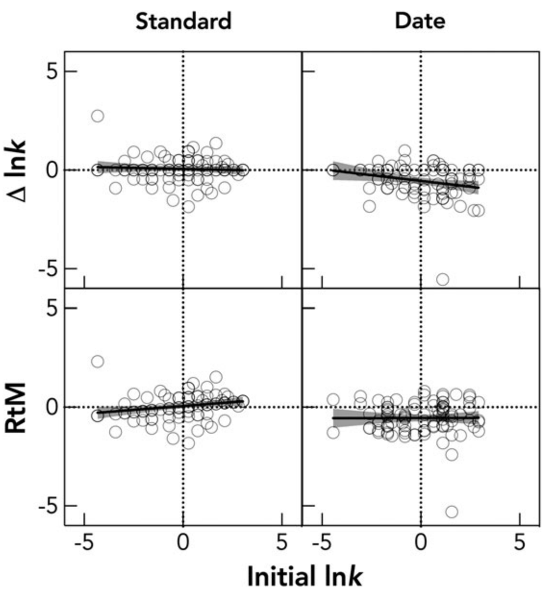

Fig. 3. Top panel: Change in discounting (ln*k*) plotted as a function of initial discounting to depict potential rate dependent effects. Bottom panel: Regression to the mean plotted as a function of initial discounting. Shaded gray bands depict 95% CI. See text for details.

Experiment 2

The goal of Experiment 2 was to address the possibility that the absence of an effect of explicit-zero framing in the S_{E0} condition was due to participants providing responses comparable to their initial discounting assessment (S_{H0}). That the tasks were identical except for the explicit-zero statement raises the question as to whether participants assigned to the S_{E0} condition in Experiment 1 were merely providing similar responses to those they provided in the initial S_{H0} condition. Therefore, we conducted an independent samples analysis of the S_{H0} and S_{E0} versions of the MCQ with another crowdsourced (MTurk) sample.

Methods

Participants and setting. Two hundred and five participants were recruited from MTurk, with the HIT only available to those meeting the same criteria as in Experiment 1. We used a Unique Turker script (see https://uniqueturker.myleott.com) to prevent participants from Experiment 1 from seeing and thereby participating in the Experiment 2 HIT. Participants received compensation in the amount of $0.25 upon completion of the HIT.

Manipulation. Participants received either the S_{H0} or S_{E0} versions of the MCQ. Due to nonsignificant effects of demographic variables in Experiment 1, Experiment 2 focused exclusively on group status. As in Experiment 1, however, group assignment was stratified such that smokers were evenly distributed into each condition by reported smoking status (current-, ex-, and never-smoker).

Results

Data from 196 participants were retained in the analyses (107 females; age: $M = 36.58$, $SEM = .88$). Nine participants provided responses that produced consistency scores below 75% ($S_{H0} = 4$; $S_{E0} = 5$) and are not included in the analyses presented here. Overall consistency for the S_{H0} group after exclusions was 97.08% (*SEM* = 0.40%) and for the S_{E0} group was 96.37% (*SEM* = 0.40%).

Sixty-four percent of participants ($n = 125$) reported a combined household income of under $60,000 and 45.4% ($n = 89$) reported holding at least bachelor's degree, with another 41.33% ($n = 81$) reporting having had some college experience. Current smokers comprised 23.47% ($n = 46$) of the total sample, with ex- and never-smokers accounting for 34.18% ($n = 67$) and 42.34% ($n = 83$), respectively. Chi-squared analyses suggested the two groups were similar with respect to reported gender, $\chi^2(1, N = 196) = 0.764$, $p = .382$, and smoking status, $\chi^2(2, N = 196) = 0.126$, $p = .939$. A Kruskal-Wallis test indicated no statistically significant differences in age, $\chi^2(1) = 0.360$, $p = .549$.

As in Experiment 1, we obtained overall ln*k* values for each participant using the Kaplan et al. (2014; also see Kaplan, Amlung et al., 2016) automated scorer for the 27-item MCQ. The overall ln*k* value (collapsed across all reward magnitudes) was -4.86 for the S_{H0} group ($n = 99$, *SEM* = 0.20) and -4.97 for the S_{E0} group ($n = 97$, *SEM* = 0.21). Differences in overall ln*k* were not significant between the two groups $t(194) = 0.39$, $p = .70$. Results of a two-way repeated measures ANOVA yielded a main effect of reward magnitude, $F(2, 372) = 118.424$, $p < .0001$, $\eta_p^2 = 0.389$, such that participants discounted small, medium, and large rewards differently across both S_{H0} and S_{E0} conditions. There was no statistically significant effect of zero framing, $F(1, 186) = 0.661$, $p = .577$.

Discussion

Delay discounting is a form of intertemporal choice providing researchers and clinicians alike a unique behavioral marker with implications for theoretical and practical applications (Bickel et al., 2014; Landes et al., 2012). From a theoretical standpoint, understanding the malleability of delay discounting helps advance our understanding of decision making processes and hypotheses (Bickel, MacKillop, Madden, Odum, & Yi, 2015; Green et al., 2013; Kirby, 2009). As a trans-disease marker of impaired decision making, delay discounting is a prime dependent variable for procedures aimed at correcting myopic decision making (also see review and discussion in Koffarnus et al., 2013) associated with a range of risky health behaviors (Daugherty & Brase, 2010; also see reviews by Amlung, Vedelago, Acker, Balodis, & MacKillop, 2017; Stojeck & MacKillop, 2017).

The present study examined the degree to which a validated discounting instrument detects changes in discounting rate in the presence of date-delay and/or hidden-zero manipulations. We report four main findings. First, results of regression models indicate only initial discounting rate and date-delay framing emerging as significant predictors when all other variables are statistically controlled (e.g., CFC, smoking status, demographics), suggesting a potential rate-dependent effect. Second, RtM analyses suggest that the significant rate-dependent effect in the date-framed group is valid and not merely a RtM artifact. Third, results of between-group analyses in Experiment 2 provide additional confidence that the lack of hidden-zero effects in Experiment 1 was not an artifact of repeated administration in the standard calendar unit framed delays in the MCQ. Fourth, as a secondary aim of this study, we demonstrate that the MCQ task, as administered across an MTurk workforce, provides expected magnitude effects and low numbers of inconsistent discounting data, providing additional confidence that the task was appropriate for detecting any potential framing effects.

The date-delay framing effects in the present study reproduce those found in other populations and discounting tasks (i.e., DeHart & Odum, 2015; Klapproth, 2012; LeBoeuf, 2006; Read et al., 2005). While Klapproth (2012) previously used the MCQ to examine the date-delay effect on discounting, the effect was observed in substance users from a clinical sample, but not in the control sample primarily composed of undergraduates. Several demographic differences between the groups obfuscate interpretation of their findings, however. For example, the substance-user group was primarily male and most of them were unemployed (p. 421; compared to a primarily female control group of undergraduates with no information on employment). Thus, the present study provides the first comprehensive test of the date-delay effect using the MCQ, accounting for demographic variables. Taken collectively, it appears that date-delay framing renders a robust effect on delay discounting rate.

A question remains as to whether framing manipulations such as the date/delay effect can engender permanent behavior change. Although the permanency of these effects has not been evaluated, one way that state-change manipulations may, by proxy, induce relative permanency is when choices made in a single context result in long term outcomes. As an example, individuals eligible to receive monetary compensation for periods of abstinence from drugs or other commodities of abuse (i.e., contingency management) may exhibit a greater likelihood of entering into a contract when required periods of abstinence are presented as specific dates rather than calendar units (e.g., days, weeks, or months). Proposing regular support meetings and negative drug screenings *until June 20,* rather than regular support meetings and negative drug screenings *for 90 days* may increase rates of compliance. State-change framing manipulations may thereby offer a promising line of application-aimed research requiring minimal resources.

Contrary to other published reports (Koffarnus & Bickel, 2014; Magen et al., 2008; Radu et al., 2011), the present study failed to detect any hidden-zero effects after controlling for variables like age, gender, CFC, or smoking status. However, we are aware of no other published accounts of using the MCQ and/or statistically controlling for potential modulating (i.e., mediating or moderating) variables, which might suggest the hidden-zero effect is task specific or potentially masked by different covariate factors. The initial hidden-zero effect described by Magen et al. (2008) used a 15-trial MCQ-like series of dichotomous choice pairs of SSR and LLR options and sampled an on-line crowdsourced population, rendering it most similar to the present study. Despite Magen and colleagues' attempt to use online recruitment to produce a general population sample, 98 of their 112 participants reported being female. The present study used MTurk and returned an approximately balanced mix of gender. Nonetheless, regression analyses in the present study examining main effects of gender did not suggest gender as a significant predictor of change in discounting rate. Moreover, the Radu et al. (2011) study using the Magen et al. task specifically examined gender effects and found none, but did report a significant hidden-zero effect similar to Magen et al. and at odds with the present study. Finally, Koffarnus and Bickel (2014) employed a five-trial adjusting-delay procedure and reported a significant hidden-zero effect using a $1,000 reward magnitude in 111 undergraduate participants. Beyond differences in task and statistical analysis approaches, a potential difference with the present study is the use of the 27-item MCQ, which is considerably longer than the 15-item Magen et al. approach or the five-trial Koffarnus and Bickel task, the latter of which returns one of 32 different discounting rates. In addition, the 27-item MCQ consists of a series of fixed questions regardless of participants' responses, whereas the five-trial discounting task presents specific questions that are *dependent* on the participant's pattern of responding. A comparison of effect sizes generated in previous demonstrations of the hidden zero effect also suggests task type as a potential moderator; the 15-item assessment used in both Magen et al. and Radu et al. yielded Cohen's d effect sizes of $d = 0.84$ and $d = 0.89$, respectively (reported in a review by Koffarnus et al., 2013), whereas the five-trial adjusting-delay procedure used by Koffarnus and Bickel produced an effect size of $d_z = 0.34$ (d_z based on the paired t statistic for repeated measures data, rather than the nonpaired Cohen's d; Koffarnus, personal communication). As such, it is possible that the absence of a statistically significant effect of zero framing in the MCQ may be have been due to differences in construction of the task (e.g., the range of possible k values, the relation between the magnitude of the rewards and the length of the delays, or time to completion). Thus, differences in the task and methods of deriving discounting rates may influence whether the hidden-zero effect can be captured.

The date-delay effect detected in the present study may be the product of either perceptual or neuroeconomic influences, either of which are sensitive to covert verbal behavior or overt operant learning histories. DeHart and Odum (2015) presented a number of potential theories regarding such influences (see discussion on p. 18 of DeHart & Odum)—interested readers are encouraged to consult their discussion. Speaking to explanatory variables in the date-delay framing effects is beyond the scope of this study. Future research should seek to parse magnitude sensitivity and neuroeconomic influences from specific date-delay framing to better identify the variables accounting for such differences (note, however, that the neuropsychological bases of broad framing effects remains relatively unknown; see De Martino, Kumaran, Seymour, & Dolan, 2006). Nonetheless, our findings contribute to the small, but growing, body of literature on the date-delay effect (see reviews in DeHart & Odum, 2015; Koffarnus et al., 2013). A notable advantage to this effect—and all framing effects, in general—is the relative resource efficiency in changing discounting rates. Framing effects are well understood to be robust and reliable and can substantially alter decision making (e.g., Tversky & Kahneman, 1981). Thus, identifying simple stimulus control procedures such as the date-delay manipulation to momentarily nudge decision making toward a more beneficial outcome may yield positive effects in one-time decision scenarios committing to a LLR (e.g., signing up for retirement plans, choosing a college savings plan, selecting mortgage/loan options).

Despite the relative merits of and contributions rendered by this study, a number of additional limitations temper our conclusions and should guide interpretation of these data. First, despite our crowdsourcing approach and sample sizes, we cannot rule out insufficient power as an explanation for the null hidden-zero effects. Second, our study used the MCQ as the discounting task, rather than Magen et al.' (2008) procedure that was also employed by Radu et al. (2011). Our inability to reproduce the Magen et al. and Radu et al. effects may thereby be due to task differences, notwithstanding apparent similarities between the Magen et al. procedure and the MCQ. That we used 75% consistency scores to exclude data on the MCQ constitutes another difference from Magen et al. and Radu et al. In the Magen et al. study, only one participant was excluded as an outlier with a mean greater than 2 standard deviations from the group mean, while Radu et al. excluded eight participants who choose either all LLR or SSR responses—both Magen et al. and Radu et al. report that such exclusions did not result in different conclusions. We believe the use of the MCQ consistency score is a more appropriate approach given our use of the MCQ and the ability to identify, by definition, inconsistent responding on this task. Nevertheless, the extent to which the use of consistency scores improves upon alternative methods for detecting outliers or nonsystematic responding is an area for future research. Third, our study did not assess substance use beyond cigarette smoking. As described above in regards to the Klapproth (2012) investigation and the recent work by Mellis, Woodford, Stein, and Bickel (2017), it may be possible that substance users are more sensitive to date-delay framing and/or magnitude effects. The current smokers included in our study reported relatively few cigarettes smoked per day and relatively low dependence, and are thereby do not constitute clinical samples. We did not assess participants' use of other drugs, so we cannot covary substance use to help explain our findings. Future research should explicitly recruit larger numbers of substance users and matched controls to determine whether substance use helps moderate sensitivity to framing effects using the approaches employed in this study.

In sum, our findings support the notion that delay discounting is sensitive to framing effects, but raises doubts about the robustness of the hidden-zero effect across differing tasks. The date-delay framing effect appears to be robust beyond participants' demographic variables, corroborating findings summarized by Koffarnus et al. (2013) and those recently described by DeHart and Odum (2015). Additional analyses suggest the date-delay effect is rate dependent, such that those with greater discounting have more to gain via such a manipulation, and that such effects are not a regression-to-the-mean artifact. Additional research into the perceptual and/or neuroeconomic underpinnings of these effects—or lack thereof—is necessary to provide a more basic account of how such effects operate.

References

Ainslie, G. W. (1974). Impulse control in pigeons. *Journal of the Experimental Analysis of Behavior, 21*(3), 485-489. https://doi.org/10.1901/jeab.1974.21-485

Amlung, M., Vedelago, L., Acker, J., Balodis, I., & MacKillop, J. (2017). Steep delay discounting and addictive behavior: A meta-analysis of continuous associations. *Addiction, 112*(1), 51-62. https://doi.org/10.1111/add.13535

Anokhin, A. P., Golosheykin, S., Grant, J. D., & Heath, A. C. (2011). Heritability of delay discounting in adolescence: A longitudinal twin study. *Behavior Genetics, 41*(2), 175-183. https://doi.org/10.1007/s10519-010-9384-7

Anokhin, A. P., Grant, J. D., Mulligan, R. C., & Heath, A. C. (2015). The genetics of impulsivity: Evidence for the heritability of delay discounting. *Biological Psychiatry, 77*(10), 887-894. https://doi.org/10.1016/j.biopsych.2014.10.022

Ashe, M. L., Newman, M. G., & Wilson, S. J. (2015). Delay discounting and the use of mindful attention versus distraction in the treatment of drug addiction: A conceptual review. *Journal of the Experimental Analysis of Behavior, 103*(1), 234-248. https://doi.org/10.1002/jeab.122

Bickel, W. K., Landes, R. D., Kurth-Nelson, Z., & Redish, A. D. (2014). A quantitative signature of self-control repair: Rate-dependent effects of successful addiction treatment. *Clinical Psychological Science, 2*(6), 685-695. https://doi.org/10.1177/2167702614528162

Bickel, W. K., MacKillop, J., Madden, G. J., Odum, A. L., & Yi, R. (2015). Experimental manipulations of delay discounting & related processes: An introduction to the special issue. *Journal of the Experimental Analysis of Behavior, 103*(1), 1-9. https://doi.org/10.1002/jeab.133

Bickel, W. K., Yi, R., Landes, R. D., Hill, P. F., & Baxter, C. (2011). Remember the future: Working memory training decreases delay discounting among stimulant addicts. *Biological Psychiatry, 69*(3), 260-265. https://doi.org/10.1016/j.biopsych.2010.08.017

Black, A. C., & Rosen, M. I. (2011). A money management-based substance use treatment increases valuation of future rewards. *Addictive Behaviors, 36*(1-2), 125-128. https://doi.org/10.1016/j.addbeh.2010.08.014

Breusch, T., & Pagan, A. (1979). A simple test for heteroscedasticity and random coefficient variation. *Econometrica, 47*(5), 1287-1294. https://doi.org/10.2307/1911963

Critchfield, T. S., & Reed, D. D. (2009). What are we doing when we translate from quantitative models? *The Behavior Analyst, 32*(2), 339-362. https://doi.org/10.1007/BF03392197

Daniel, T. O., Said, M., Stanton, C. M., & Epstein, L. H. (2015). Episodic future thinking reduces delay discounting and energy intake in children. *Eating Behaviors, 18*, 20-24. https://doi.org/10.1016/j.eatbeh.2015.03.006

Daugherty, J. R., & Brase, G. L. (2010). Taking time to be healthy: Predicting health behaviors with delay discounting and time perspective. *Personality and Individual Differences, 48*(2), 202-207. https://doi.org/10.1016/j.paid.2009.10.007

DeHart, W. B., & Odum, A. L. (2015). The effects of the framing of time on delay discounting. *Journal of the Experimental Analysis of Behavior, 103*(1), 10-21. https://doi.org/10.1002/jeab.125

De Martino, B., Kumaran, O., Seymour, B., & Dolan, R. J. (2006). Frames, biases, and rational decision-making in the human brain. *Science, 313*(5787), 684-687. https://doi.org/10.1126/science.1128356

Dixon, M. R., Jacobs, E. A., & Sanders, S. (2006). Contextual control of delay discounting by pathological gamblers. *Journal of Applied Behavior Analysis, 39*(4), 413-422. https://doi.org/10.1901/jaba.2006.173-05

Du, W., Green, L., & Myerson, J. (2002). Cross-cultural comparisons of discounting delayed and probabilistic rewards. *The Psychological Record, 52*(4), 479-492.

Duckworth, A. L., & Seligman, M. E. P. (2005). Self-discipline outdoes IQ in predicting academic performance of adolescents. *Psychological Science, 16*(12), 939-944. https://doi.org/10.1111/j.1467-9280.2005.01641.x

Ebert, J. E. (2001). The role of cognitive resources in the valuation of near and far future events. *Acta Psychologica, 108*(2), 155-171. https://doi.org/10.1016/S0001-6918(01)00033-6

Gray, J. C., & MacKillop, J. (2015). Impulsive delayed reward discounting as a genetically-influenced target for drug abuse prevention: A critical evaluation. *Frontiers in Psychology, 6*, 13. https://doi.org/10.3389/fpsyg.2015.01104

Green, L., Myerson, J., Oliveira, L., & Chang, S. E. (2013). Delay discounting of monetary rewards over a wide range of amounts. *Journal of the Experimental Analysis of Behavior, 100(3)*, 269-281. https://doi.org/10.1002/jeab.45

Heatherton, T. F., Kozlowski, L. T., Frecker, R. C., & Fagerström, K. O. (1991). The Fagerström test for nicotine dependence: A revision of the Fagerström Tolerance Questionnaire. *British Journal of Addiction, 86*(9), 1119-1127. https://doi.org/10.1111/j.1360-0443.1991.tb01879.x

Hendrickson, K. L., & Rasmussen, E. B. (2013). Effects of mindful eating training on delay and probability discounting for food and money in obese and healthy-weight individuals. *Behaviour Research and Therapy, 51*(7), 399-409. https://doi.org/10.1016/j.brat.2013.04.002

Holt, D. D., Green, L., & Myerson, J. (2003). Is discounting impulsive? Evidence from temporal and probability discounting in gambling and non-gambling college students. *Behavioural Processes, 64*(3), 355-367. https://doi.org/10.1016/S0376-6357(03)00141-4

Johnson, P.E. (2016). rockchalk: Regression Estimation and Presentation. R package version 1.8.101. Retrieved from https://CRAN.R-project.org/package=rockchalk

Kaplan, B. A., Amlung, M., Reed, D. D., Jarmolowicz, D. P., McKerchar, T. L., & Lemley, S. M. (2016a). Automating scoring of delay discounting for the 21- and 27-item monetary choice questionnaires. *The Behavior Analyst, 39*(2), 293-304. https://doi.org/10.1007/s40614-016-0070-9

Kaplan, B. A., Lemley, S. M., Reed, D. D., & Jarmolowicz, D. P. (2014). *21-and 27-Item Monetary Choice Questionnaire Automated Scorers*. Retrieved from http://kuscholarworks.ku.edu

Kaplan, B. A., Reed, D. D., & Jarmolowicz, D. P. (2016b). Effects of episodic future thinking on discounting: Personalized age-progressed pictures improve risky long-term health decisions. *Journal of Applied Behavior Analysis, 49*(1), 148-169. https://doi.org/10.1002/jaba.277

Kirby, K. N. (2009). One-year temporal stability of delay-discount rates. *Psychonomic Bulletin & Review, 16*(3), 457-462. https://doi.org/10.3758/PBR.16.3.457

Kirby, K. N., & Finch, J. C. (2010). The hierarchical structure of self-reported impulsivity. *Personality and Individual Differences, 48*(6), 704-713. https://doi.org/10.1016/j.paid.2010.01.019

Kirby, K. N., & Petry, N. M. (2004). Heroin and cocaine abusers have higher discount rates for delayed rewards than alcoholics or non-drug-using controls. *Addiction, 99*(4), 461-471. https://doi.org/10.1111/j.1360-0443.2003.00669.x

Kirby, K. N., Petry, N. M., & Bickel, W. K. (1999). Heroin addicts have higher discount rates for delayed rewards than non-drug-using controls. *Journal of Experimental Psychology: General, 128*(1), 78-87. https://doi.org/10.1037/0096-3445.128.1.78

Klapproth, F. (2012). The date-delay framing effect in temporal discounting depends on substance abuse. *Behavioural Processes, 90*(3), 420-423. https://doi.org/10.3758/PBR.16.3.457

Koffarnus, M. N., & Bickel, W. K. (2014). A 5-trial adjusting delay discounting task: Accurate discount rates in less than one minute. *Experimental and Clinical Psychopharmacology, 22*(3), 222-228. https://doi.org/10.1037/a0035973

Koffarnus, M. N., Jarmolowicz, D. P., Mueller, E. T., & Bickel, W. K. (2013). Changing delay discounting in the light of the competing neurobehavioral decision systems theory: A review. *Journal of the Experimental Analysis of Behavior, 99*(1), 32-57. https://doi.org/10.1002/jeab.2

Landes, R. D., Christensen, D. R., & Bickel, W. K. (2012). Delay discounting decreases in those completing treatment for opioid dependence. *Experimental and Clinical Psychopharmacology, 20*(4), 302-309. https://doi.org/10.1037/a0027391

LeBoeuf, R. A. (2006). Discount rates for time versus dates: The sensitivity of discounting to time-interval description. *Journal of Marketing Research, 43*(1), 59-72. https://doi.org/10.1509/jmkr.43.1.59

Lempert, K. M., Johnson, E., & Phelps, E. A. (2016). Emotional arousal predicts intertemporal choice. *Emotion, 16*(5), 647-656. https://doi.org/10.1037/emo0000168

Loewenstein, G. F., & Prelec, D. (1993). Preferences for sequences of outcomes. *Psychological Review, 100*(1), 91-108. https://doi.org/10.1037/0033-295X.100.1.91

Loewenstein, G., & Sicherman, N. (1991). Do workers prefer increasing wage profiles? *Journal of Labor Economics, 9*(1), 67-84. https://doi.org/10.1086/298259

MacKillop, J., Amlung, M. T., Few, L. R., Ray, L. A., Sweet, L. H., & Munafò, M. R. (2011). Delayed reward discounting and addictive behavior: A meta-analysis. *Psychopharmacology, 216*(3), 305-321. https://doi.org/10.1007/s00213-011-2229-0

Madden, G. J., & Bickel, W. K. (2010). *Impulsivity: The Behavioral and Neurological Science of Discounting.* Washington, DC: American Psychological Association.

Magen, E., Dweck, C. S., & Gross, J. J. (2008). The hidden-zero effect representing a single choice as an extended sequence reduces impulsive choice. *Psychological Science, 19*(7), 648-649. https://doi.org/10.1111/j.1467-9280.2008.02137.x

Mazur, J. E. (1987). An adjusting procedure for studying delayed reinforcement. In M. L. Commons, J. E. Mazur, J. A. Nevin & H. Rachlin (Eds.), *The effect of delay and of intervening events on reinforcement value* (Vol. *5* pp. 55-73). Hillsdale, NJ: Erlbaum.

Mellis, A. M., Woodford, A. E., Stein, J. S., & Bickel, W. K. (2017). A second type of magnitude effect: Reinforcer magnitude differentiates delay discounting between substance users and controls. *Journal of the Experimental Analysis of Behavior, 107*(1), 151-160. https://doi.org/10.1002/jeab.235

Mitchell, S. H. (2011). The genetic basis of delay discounting and its genetic relationship to alcohol dependence. *Behavioural processes, 87*(1), 10-17. https://doi.org/10.1016/j.beproc.2011.02.008

Mitchell, S. H., & Wilson, V. B. (2012). Differences in delay discounting between smokers and nonsmokers remain when both rewards are delayed. *Psychopharmacology, 219*(2), 549-562. https://doi.org/10.1007/s00213-011-2521-z

Morrison, K. L., Madden, G. J., Odum, A. L., Friedel, J. E., & Twohig, M. P. (2014). Altering impulsive decision making with an acceptance-based procedure. *Behavior Therapy, 45*(5), 630-639. https://doi.org/10.1016/j.beth.2014.01.001

Murphy, J. G., Dennhardt, A. A., Skidmore, J. R., Borsari, B., Barnett, N. P., Colby, S. M., & Martens, M. P. (2012). A randomized controlled trial of a behavioral economic supplement to brief motivational interventions for college drinking. *Journal of Consulting and Clinical Psychology, 80*(5), 876-886. https://doi.org/10.1037/a0028763

Myerson, J., Baumann, A. A., & Green, L. (2016). Individual differences in delay discounting: Differences are quantitative with gains, but qualitative with losses. *Journal of Behavioral Decision Making, 30*(2), 359-372. https://doi.org/10.1002/bdm.1947

Odum, A. L. (2011a). Delay discounting: I'm a K, you're a K. *Journal of the Experimental Analysis of Behavior, 96*(3), 427-439. https://doi.org/10.1901/jeab.2011.96-423

Odum, A. L. (2011b). Delay discounting: Trait variable? *Behavioural Processes, 87*(1), 1-9. https://doi.org/10.1016/j.beproc.2011.02.007

Radu, P. T., Yi, R., Bickel, W. K., Gross, J. J., & McClure, S. M. (2011). A mechanism for reducing delay discounting by altering temporal attention. *Journal of the Experimental Analysis of Behavior, 96*(3), 363-385. https://doi.org/10.1901/jeab.2011.96-363

Rass, O., Schacht, R. L., Buckheit, K., Johnson, M. W., Strain, E. C., & Mintzer, M. Z. (2015). A randomized controlled trial of the effects of working memory training in methadone maintenance patients. *Drug and Alcohol Dependence, 156*, 38-46. https://doi.org/10.1016/j.drugalcdep.2015.08.012

R Core Team (2016). *R: A language and environment for statistical computing.* R Foundation for Statistical Computing, Vienna, Austria. URL https://www.R-project.org/

Read, D., Frederick, S., Orsel, B., & Rahman, J. (2005). Four score and seven years from now: The date/delay effect in temporal discounting. *Management Science, 51*(9), 1326-1335. https://doi.org/10.1287/mnsc.1050.0412

Reynolds, B., & Schiffbauer, R. (2004). Measuring state changes in human delay discounting: An experiential discounting task. *Behavioural Processes, 67*(3), 343-356. https://doi.org/10.1016/j.beproc.2004.06.003

Ross, W. T., & Simonson, I. (1991). Evaluations of pairs of experiences: A preference for happy endings. *Journal of Behavioral Decision Making, 4*(4), 273-282. https://doi.org/10.1002/bdm.3960040405

Snider, S. E., Quisenberry, A. J., & Bickel, W. K. (2016). Order in the absence of an effect: Identifying rate-dependent relationships. *Behavioural Processes, 127*, 18-24. https://doi.org/10.1016/j.beproc.2016.03.012

Stein, J. S., Wilson, A. G., Koffarnus, M. N., Daniel, T. O., Epstein, L. H., & Bickel, W. K. (2016). Unstuck in time: Episodic future thinking reduces delay discounting and cigarette smoking. *Psychopharmacology, 233*(21-22), 3771-3778. https://doi.org/10.1007/s00213-016-4410-y

Stojek, M. M., & MacKillop, J. (2017). Relative reinforcing value of food and delayed reward discounting in obesity and disordered eating: A systematic review. *Clinical Psychology Review, 55*, 1-11. https://doi.org/10.1016/j.cpr.2017.4.007

Strathman, A., Gleicher, F., Boninger, D. S., & Edwards, C. S. (1994). The consideration of future consequences: Weighing immediate and distant outcomes of behavior. *Journal of Personality and Social Psychology, 66*(4), 742-752. https://doi.org/10.1037/0022-3514.66.4.742

Tversky, A., & Kahneman, D. (1981). The framing of decisions and the psychology of choice. *Science, 211*(4481), 453-458. https://doi.org/10.1126/science.7455683

Wilhelm, C. J., & Mitchell, S. H. (2009). Strain differences in delay discounting using inbred rats. *Genes, Brain & Behavior, 8*(4), 426-434. https://doi.org/10.1111/j.1601-183X.2009.00484.x

Yi, R., Johnson, M. W., Giordano, L. A., Landes, R. D., Badger, G. J., & Bickel, W. K. (2008). The effects of reduced cigarette smoking on discounting future rewards: An initial evaluation. *The Psychological Record, 58*(2), 163-174.

Zeileis, A., & Hothorn, T. (2002). Diagnostic checking in regression relationships. *R News 2*(3), 7-10. Retrieved from https://CRAN.R-project.org/doc/Rnews/

Received: June 20, 2017
Final Acceptance: March 29, 2018

Appendix

Multiple regression nested model comparison without participant exclusions

	Model 1		Model 2		Model 3		Model 4	
	β	*SE*	*β*	*SE*	*β*	*SE*	*β*	*SE*
(Intercept)	-0.643	(0.707)	-0.036	(0.284)	-0.348	(0.174)	-0.255	(0.162)
Age	-0.005	(0.006)						
Gender	0.011	(0.123)						
Consideration of Future Consequences	0.021	(0.015)						
Initial ln$k$	1.003***	(0.070)	0.992***	(0.063)	0.917***	(0.032)	0.914***	(0.032)
Ex-smoker	-0.443	(0.385)						
Current smoker	-0.018	(0.395)						
Manipulation group-S_{E0}	-0.237	(0.421)	-0.325	(0.410)	0.216	(0.162)		
Manipulation group-D_{H0}	-0.608	(0.435)	-0.666	(0.417)	-0.355*	(0.165)		
Manipulation group-D_{E0}	-1.142**	(0.400)	-1.077	(0.427)	-0.663***	(0.162)		
Initial ln$k$ × Ex-smoker	-0.108	(0.084)						
Initial ln$k$ × Current smoker	0.006	(0.095)						
Initial ln$k$ × Manipulation group: S_{E0}	-0.108	(0.088)	-0.125	(0.086)				
Initial ln$k$ × Manipulation group: D_{H0}	-0.065	(0.096)	-0.075	(0.091)				
Initial ln$k$ × Manipulation group: D_{E0}	-0.111	(0.095)	-0.099	(0.093)				
Temporal framing: Date							-0.620***	(0.117)
N	201		201		201		201	
RMSE	0.829		0.825		0.824		0.830	
R^2	0.821		0.816		0.814		0.809	
Adjusted R^2	0.808		0.809		0.810		0.807	
AIC	511.58		503.07		499.42		500.65	

*$p \leq 0.05$ **$p \leq 0.01$ ***$p \leq 0.001$.