Commentary

Psychological Science
2018, Vol. 29(6) 1010–1019

DOI: 10.1177/0956797617754219
www.psychologicalscience.org/PS

Does Power Reduce Temporal Discounting? Commentary on Joshi and Fast (2013)

Min Zhang and Pamela K. Smith
Rady School of Management, University of California, San Diego

Received 7/13/16; Revision accepted 12/21/17

Devaluing future outcomes, known as temporal discounting (Frederick, Loewenstein, & O'Donoghue, 2002), hinders one's ability to act in line with long-term over short-term interests. It is associated with maladaptive behaviors such as smoking (Kirby, Petry, & Bickel, 1999), drug use (Harrison, Lau, & Rutström, 2010), and not saving for retirement (Gubler & Pierce, 2014). Because temporal discounting may affect a variety of behaviors, ranging from exercising to energy consumption (Urminsky & Zauberman, 2015), understanding the factors that influence temporal discounting is critical for researchers in psychology, economics, business, and public policy.

Joshi and Fast (2013) provided evidence that increased social power (i.e., control over valued resources; Magee & Galinsky, 2008) reduces temporal discounting. This finding has important applied, theoretical, and empirical implications. First, it implies the potential to mitigate temporal discounting by giving individuals power. Psychological and health-science researchers have thus recently advocated for empowerment as an intervention to improve balancing of long-term and short-term interests in decision making (Gubler & Pierce, 2014; Patton et al., 2016; Urminsky & Zauberman, 2015).

Second, this finding informs the theoretical debate on how power affects self-control. Temporal discounting may be thought of as a self-control conflict between having a smaller reward sooner versus having a larger reward later (Frederick et al., 2002; Mischel, Shoda, & Rodriguez, 1989). The approach-inhibition theory of power (Keltner, Gruenfeld, & Anderson, 2003) posits that high power activates the behavioral approach system, which increases impulsivity and sensitivity to rewards. Therefore, high-power individuals, compared with low-power individuals, should be more likely to prefer earlier to delayed rewards, and thus show more temporal discounting. In contrast, the social distance theory of power (Magee & Smith, 2013) predicts the reverse, that high-power individuals should show less temporal discounting than low-power individuals. According to the social distance theory, because of their greater independence, high-power individuals feel more psychologically distant from low-power individuals than vice versa. Increased psychological distance has been shown to decrease temporal discounting (e.g., Pronin, Olivola, & Kennedy, 2008), in part by leading individuals to construe situations more abstractly (Trope & Liberman, 2010), which highlights the value of the delayed reward (e.g., Fujita, Trope, Liberman, & Levin-Sagi, 2006).

Third, this finding could further current understanding of the self-reinforcing nature of power (Magee & Galinsky, 2008). If having power decreases temporal discounting, such heightened self-control on the part of the powerful may help maintain existing power hierarchies. In a United Nations Development Programme report on poverty reduction, Sheehy-Skeffington and Haushofer (2014) extrapolated from Joshi and Fast's (2013) finding to suggest that poverty harms one's chance of long-term success by reducing one's sense of power and thus one's self-control.

Given the theoretical and real-world significance of this reported effect, it is important to examine its reproducibility. Other published experiments on this topic used similar procedures but produced inconsistent results. Duan, Wu, and Sun (2017) found that power reduced temporal discounting for Chinese participants (Study 1), but this effect was specific to participants of Han ethnicity, and was not found among participants

Corresponding Author:
Min Zhang, Rady School of Management, UC San Diego, 9500 Gilman Dr., La Jolla, CA 92093-0553
E-mail: min.zhang@rady.ucsd.edu

of Tibetan ethnicity (Study 3). Both Tost, Wade-Benzoni, and Johnson (2015, Experiment 2) and Heller and Ullrich (2017) produced null results. These inconsistent findings raise questions about the robustness of the effect of power on temporal discounting. However, these replication studies had critical problems that limit their conclusiveness. The studies of Tost et al. ($N = 69$) and Duan et al. ($N = 78$ and 80, respectively) were underpowered—power to detect the original effect: 64%, 70%, and 71%, respectively; power to detect a medium-sized ($d = 0.5$) effect: 53%, 59%, and 60%, respectively.[1] Heller and Ullrich's (2017) study suffered from differential attrition between conditions; significantly more high-power participants (61%) than low-power (50%) and control (32%) participants dropped out of the study. Selective attrition introduces experimental confounds and violates the assumption of random assignment (Zhou & Fishbach, 2016).

Well-powered, rigorous replication studies are needed to test the validity of the original findings (Simons, 2014). To this end, we conducted preregistered close replications of two different experiments in Joshi and Fast (2013).[2] Both of our studies had sample sizes more than 2.5 times the original (see Table 1), as Simonsohn (2015) recommended for informative replications. The problem of selective attrition was avoided by using an undergraduate-student participant pool. Though students have a right to end their participation in a study at any time, they rarely do so.

In Study 1, we attempted to replicate Joshi and Fast's (2013) Study 1, manipulating real power by assigning participants low or high amounts of control over team members' outcomes and then measuring temporal discounting with monetary outcomes. In Study 2, we attempted to replicate Joshi and Fast's Study 3, manipulating power with a well-established recall paradigm (Galinsky, Gruenfeld, & Magee, 2003) and then measuring temporal discounting with environmental outcomes. By replicating different paradigms from the original report, we have provided a strong test of the claim that power reduces temporal discounting. In both studies, we also tested Joshi and Fast's proposed mediator of the effect of power on temporal discounting, connection with the future self.

In the following sections, we present overviews of both of our studies as well as the critical analyses. Each study was run as the first in a series of studies that lasted for about an hour. Participants completed the studies in individual cubicles in a common room. Further details on the procedures and analyses can be found in the Supplemental Material available online.

Study 1

In Study 1, power was manipulated by assigning participants to the role of a low-power worker or a high-power manager in a virtual team task. Temporal discounting was measured with a titration procedure followed by a free-response matching question. In the titration procedure, participants made nine choices between receiving a $120 prize that day and receiving $113, $120, $137, $154, $171, $189, $206, $223, or $240 in a year.

Table 1. Comparison of Effects in Joshi and Fast's (2013) Studies 1 and 3 and the Current Studies

				Discount rate[c]			Difference between conditions[d]			
Study	N[a]	Exclusion rate (%)	Sample[b]	Low-power condition	Control condition	High-power condition	t	p	Effect size	Power[e] (%)
Joshi and Fast (2013, Study 1)	67	8.2	MTurk	0.73 (0.42)	—	0.43 (0.30)	$t(65) = -2.32$	.023	$d = -0.57$, 95% CI = [–1.07, –0.07]	62.76
Current Study 1	342	18.6	University	0.43 (0.32)	—	0.43 (0.29)	$t(340) = -0.08$	.940	$d = -0.01$, 95% CI = [–0.22, 0.20]	99.95
Joshi and Fast (2013, Study 3)	78	7.6	University	0.57_a (0.27)	0.52_a (0.24)	0.40_b (0.27)	$t(76) = -2.32$	.023	$\eta^2 = .06$, 95% CI = [.00, .19]	57.91
Current Study 2	399	13.8	University	0.57_a (0.26)	0.49_b (0.28)	0.55_{ab} (0.27)	$t(397) = 0.76$	.449	$\eta^2 < .001$, 95% CI = [.00, .02]	99.96

Note: CI = confidence interval.

[a]The samples sizes reported are the sample sizes used in the analyses, after exclusions. The percentage of participants excluded was statistically larger in our Study 1 than in Joshi and Fast's Study 1, $\chi^2(1, N = 493) = 4.01$, $p = .045$. This is likely because of the additional attention check we included (8% of participants were excluded for failing it). The percentage of participants excluded did not differ significantly between our Study 2 and Joshi and Fast's Study 3, $\chi^2(1, N = 548) = 1.52$, $p = .217$. [b]Participants in the Mechanical Turk (MTurk) sample were recruited from the Amazon Mechanical Turk platform and completed the study online. Participants in the university samples were university students who completed the study in a laboratory. [c]For each condition, the mean discount rate is shown, with the standard deviation inside parentheses. Within a row, values with different subscripts differ significantly ($p < .05$), as determined by independent-samples t tests. [d]For Joshi and Fast's Study 3 and our Study 2, the t tests contrasted the high-power condition with an average of the low-power and control conditions. [e]Power was calculated as the probability of detecting an effect the same size as or larger than that found in Joshi and Fast's corresponding study.

The free-response matching question asked participants to fill in the amount of the prize to be received in a year that would make them indifferent between receiving $120 that day and receiving the promised amount in a year. Then, as a manipulation check, we asked participants to report the extent to which they had power over other group members in the virtual team task (unnumbered 7-point scale labeled with *strongly disagree, disagree, somewhat disagree, neither agree nor disagree, somewhat agree, agree*, and *strongly agree*). For this portion of the study, we used the same manipulation and measures as Joshi and Fast (2013) reported for their Study 1.

After the original procedure, we added an attention check in which participants indicated the role to which they had been assigned. We also measured connection with the future self with a scale of seven overlapping circles representing the overlap between the current self and the self in 10 years (P. D. Joshi, personal communication, September 29, 2016), as well as multiple potential moderators. In particular, nonnaiveté of participants has been shown to reduce effect sizes (Chandler, Mueller, & Paolacci, 2014), so we included two questions probing participants' previous experience with the power manipulation and the discounting measure. The perceived legitimacy of a person's low- or high-power position (i.e., how fair or justified it is) has been shown to moderate the effect of this position on approach tendencies (Lammers, Galinsky, Gordijn, & Otten, 2008, see also Smith, Jost, & Vijay, 2008) and on social distance (Lammers, Galinsky, Gordijn, & Otten, 2012). As discussed in the introduction, behavioral approach and social distance are the mechanisms through which power affects temporal discounting according to the approach-inhibition (Keltner et al., 2003) and social distance (Magee & Smith, 2013) theories, respectively. Thus, we included perceived legitimacy as a potential moderator, asking participants to rate how legitimate the role assignment was. We also measured participants' socioeconomic status. Finally, after participants completed the other studies in the hour-long session, we measured two additional potential moderators: participants' trait general sense of power (Anderson, John, & Keltner, 2012) and goals related to money.

We followed the same exclusion criteria as did Joshi and Fast (2013), and we also excluded participants who incorrectly identified their assigned roles. Discount rate was calculated as in the original study, using the hyperbolic discounting formula $k = (A/V - 1)/D$, where A was the indifference point, or the future amount that made participants indifferent between the present and future rewards; V was the present reward (i.e., $120); and D was the delay (i.e., 1 year). Thus, k indicates how much a participant valued present rewards relative to future rewards. We followed the same procedures as in Joshi and Fast's Study 1 to determine the indifference point (P. D. Joshi, personal communication, March 24, 2017). Specifically, we used the point at which participants switched from preferring the present option to preferring the future option in the titration measure, unless participants chose $120 that day over all future options. In that case, we used their answer in the free-response matching question. To correct for the positive skew of the discount-rate distribution, we also excluded participants with discount rates more than 3 times the interquartile range (Baguley, 2012).

The manipulation check confirmed that the power manipulation was effective: High-power participants ($M = 4.71$, $SD = 1.37$) reported having more power over their team members than did low-power participants ($M = 3.61$, $SD = 1.41$), $t(340) = 7.33$, $p < .001$, $d = 0.79$, 95% confidence interval (CI) = [0.57, 1.01]. Table 1 reports the primary statistics for Study 1. After the exclusions, Study 1 still had over 99% power to detect the original effect. In contrast to Joshi and Fast's (2013) results in their Study 1, we found no significant difference in discount rate between the two power conditions. According to an equivalence test (Lakens, 2017), these data provide evidence for the null hypothesis of no effect (relative to the hypothesis that the value of d is larger than 0.38),[3] $t(336.85) = -3.42$, $p < .001$. We also ran complementary nonparametric tests on the untrimmed discount rate, as well as discount rates based solely on titration responses or matching responses (alternative temporal discounting calculations used in some research, e.g., Hardisty, Thompson, Krantz, & Weber, 2013), to test the robustness of our findings. These analyses also showed no effect of power. Additionally, power had no effect on connection with the future self.

Out of the five potential moderators we tested, only perceived legitimacy significantly moderated the effect of power on temporal discounting. When the role assignments were perceived as low in legitimacy, we found a pattern consistent with the results of Joshi and Fast (2013): Participants in the high-power role discounted less than participants in the low-power role. However, when the assignments were perceived as high in legitimacy, the reverse was true: Participants in the high-power role discounted more than participants in the low-power role. Because legitimacy was not experimentally manipulated, and was measured in only one of our studies, we consider this result suggestive but not conclusive. This moderation effect is in line with the approach-inhibition theory of power and specifically with past research on legitimacy as a moderator of the relationship between power and behavioral

approach tendencies. Lammers et al. (2008) found that when power was experienced as legitimate, high-power individuals displayed more approach behavior than low-power individuals did. However, when power was experienced as illegitimate, high-power individuals displayed the same degree of approach as, or even less approach than, low-power individuals, and such reduced approach tendencies have been associated with less impulsive behavior and greater self-control (Avila, 2001; Keltner et al., 2003; Schmeichel, Harmon-Jones, & Harmon-Jones, 2010).

Study 2

Participants in this study were randomly assigned to write about a situation in which they either lacked power (low-power condition) or had power (high-power condition), or about their last trip to the grocery store (control condition). We measured temporal discounting by asking participants to make eight choices between immediately improved air quality for 21 days and improved air quality for 35, 33, 31, 29, 27, 25, 23, or 21 days in 1 year. As in Joshi and Fast's (2013) Study 3, this measure and the measure of connection with the future self were presented in counterbalanced order. After the original procedure, we added a common manipulation check for the power manipulation (e.g., Smith, Jostmann, Galinsky, & van Dijk, 2008, Study 3; Tost, Gino, & Larrick, 2012, Experiment 3), asking participants to use a 7-point scale to report how much power they had in the incident they recalled (1 = *very little*, 7 = *a lot*). The original study did not include a manipulation check. Experience with the tasks was again measured after the original procedure as a potential moderator, but it did not have a significant effect. Participants' indifference point was determined from the titration measure in the same way as in Study 1. For participants who always chose 21 days of immediate air improvement over all future options, we followed the method used in Joshi and Fast's Study 3 and extended the titration scale one step further, assigning them an indifference point of 37 days (P. D. Joshi, personal communication, March 24, 2017). We used the same discount-rate calculation (the hyperbolic discounting formula, as in Study 1) and the same exclusion criteria as in the original study.

The power manipulation was effective: High-power participants ($M = 5.35$, $SD = 1.17$) reported having more power in the incident they recalled than did low-power participants ($M = 2.75$, $SD = 1.26$), $t(256) = 17.09$, $p < .001$, $d = 2.13$, 95% CI = [−1.89, 2.38]. Control participants ($M = 5.32$, $SD = 1.29$) also reported having more power than low-power participants, $t(277) = -16.84$, $p < .001$, $d = 2.02$, 95% CI = [1.78, 2.25]. The difference between high-power and control participants was not significant, $t(259) = 0.20$, $p = .840$, $d = 0.02$, 95% CI = [−0.22, 0.27].

Table 1 reports the primary statistics for Study 2. A planned contrast comparing the discount rate of high-power participants with the average discount rate of low-power and control participants (as in Joshi & Fast, 2013, Study 3) showed no significant effect of condition on temporal discounting. Independent-samples *t* tests comparing the three conditions found that control participants discounted less than low-power participants, but all other comparisons were nonsignificant. According to an equivalence test (Lakens, 2017) focusing on the difference between the low- and high-power conditions, these data provide evidence for the null hypothesis of no effect (relative to the hypothesis that d is larger than 0.43),[4] $t(249.06) = -2.96$, $p = .002$.

As in Joshi and Fast's (2013) study, a planned contrast showed that high-power participants' reported connection with the future self ($M = 3.85$, $SD = 1.45$) was higher than low-power ($M = 3.43$, $SD = 1.45$) and control ($M = 3.58$, $SD = 1.38$) participants' averaged scores, $t(397) = 2.20$, $p = .030$, $\eta^2 = .01$, 95% CI = [0.00, 0.03]. However, connection with the future self did not mediate the effect of power on temporal discounting, estimated indirect effect = 0.00, 95% bootstrapped CI = [0.00, 0.00], $p = .960$. Thus, even though in Study 2 we found evidence that power affected connection with the future self, which was Joshi and Fast's proposed mediator, we found no evidence that power affected temporal discounting directly, or that power had an indirect effect on temporal discounting via connection with the future self. Tost et al. (2015, Experiment 2) and Heller and Ullrich (2017) also did not find evidence that connection with the future self acted as a mediator.

Meta-Analysis

We conducted a meta-analysis of experiments examining the effect of low versus high power on temporal discounting. This meta-analysis included our two close replications reported here, three additional replication studies we report in the Supplemental Material (Studies 3, 4, and 5), the four previously published replications (Duan et al., 2017, Studies 1 and 3; Heller & Ullrich, 2017; Tost et al., 2015, Experiment 2), and the target studies of these replications (Joshi & Fast, 2013, Studies 1 and 3). Correlational studies (e.g., Duan et al., 2017, Study 2; Joshi & Fast, 2013, Study 4) were not included because our goal was to assess the causal evidence that power affects temporal discounting. Details of the meta-analysis, including the full selection criteria, are in the Supplemental Material. Figure 1 shows the effect size for each experiment and the overall meta-effect in a

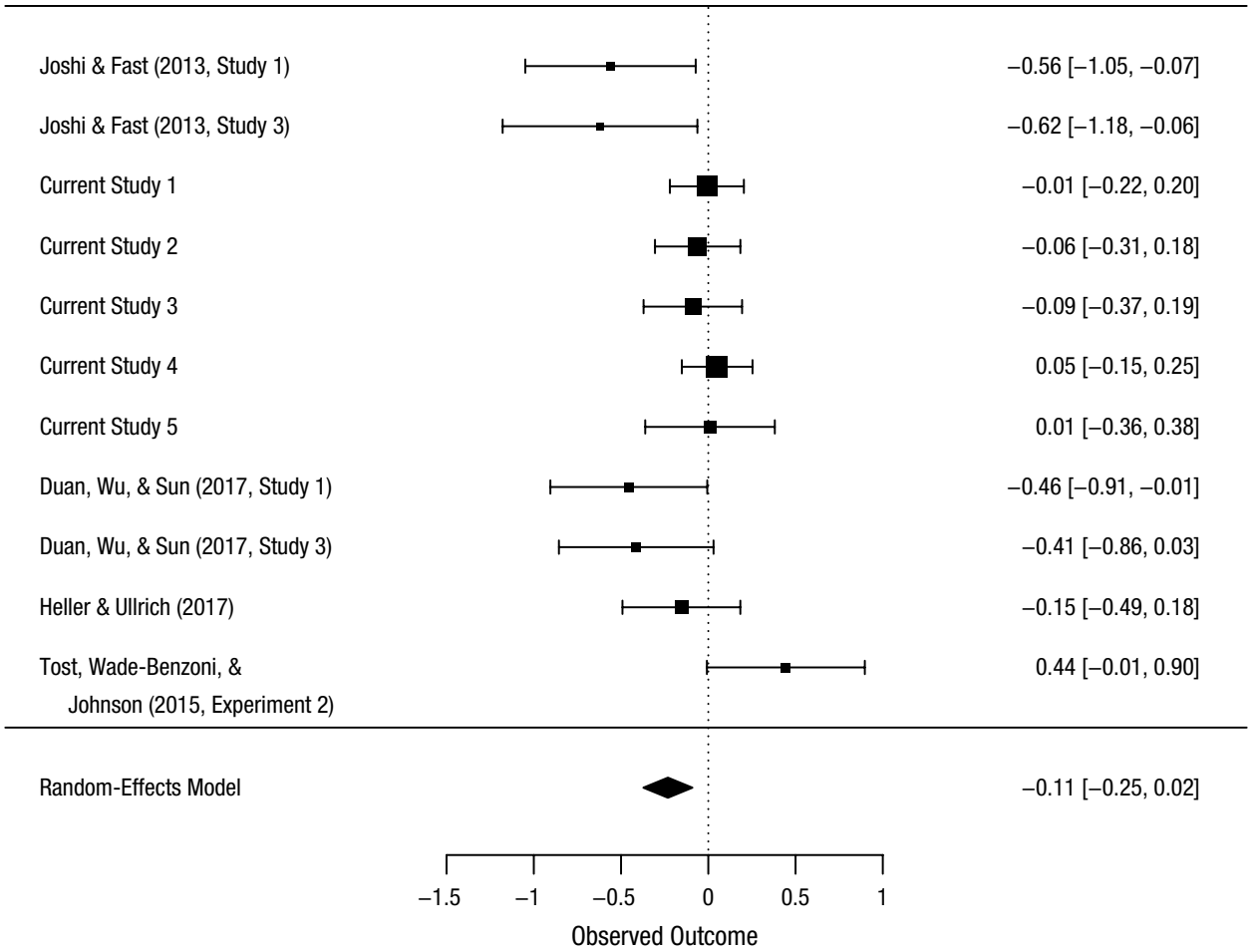

Fig. 1. Forest plot of the random-effects meta-analysis of the standardized difference in discount rate between the low-power and high-power conditions. The squares show the observed effect sizes (Hedges's *g*s), the error bars indicate the 95% confidence intervals (CIs) around the effect sizes, and the size of each square indicates the weight of the corresponding study in the meta-analysis. To the right of the forest plot, the figure shows the numerical values for the effect sizes and 95% CIs. The bottom row in the figure presents the overall meta-effect of power.

forest plot. The overall meta-effect of power, calculated as the standardized effect size (Hedges's *g*) of the difference in discount rate between the low-power and high-power conditions, was −0.11, 95% CI = [−0.25, 0.02]. Looking separately at the two different outcomes tested, discounting rates for money and air quality, we found that the meta-effect was −0.11, 95% CI = [−0.29, 0.08] within the monetary discounting experiments and −0.15, 95% CI = [−0.40, 0.10] within the air-quality discounting experiments. Thus, the evidence overall, as well as within each experimental design, is not consistent with an effect of power on temporal discounting.

The meta-analysis also showed a small to moderate amount of heterogeneity across experiments, $I^2 = 45.89\%$, $Q(10) = 19.82$, $p = .03$; the I^2 statistic indicates that heterogeneity accounted for 45.89% of the total variability in the data (Hamilton, 2017). The meta-effect was not moderated by whether temporal discounting was measured in the context of money or air quality, $Q(1) = 0.08$, $p = .78$, nor did we identify any other moderators.

General Discussion

Joshi and Fast (2013) presented initial evidence that power reduces temporal discounting. With much larger samples, however, we found no effect of power on temporal discounting in two preregistered close-replication studies. Using various methods of calculating temporal discounting and various analysis strategies, including those used by the original authors, we never replicated Joshi and Fast's finding and never found a significant difference in temporal discounting between low- and high-power conditions. Furthermore, a meta-analysis of known replication studies and the target studies

showed a nonsignificant effect of power on temporal discounting.

Why did we fail to find an effect of power on temporal discounting? One possibility is that our studies differed in small but critical ways from those of Joshi and Fast (2013), and that these methodological differences led to our different results. Indeed, our Studies 1 and 2 both involved multistudy, group-testing setups: Participants completed our studies as part of a series of unrelated studies while seated in individual cubicles in a common room. In contrast, participants in Study 3 of Joshi and Fast completed only that specific study during their session and apparently took part one person at a time. We tried to mitigate the potential influence of the multistudy setting by having our study always be the first one participants completed in their session. A priori, we also have no strong reason to believe that the power manipulations we used would not be successful in a setup such as we employed for Studies 1 and 2. Both the role-based manipulation in Study 1 and the recall manipulation in Study 2 have been successfully employed in previous research with multistudy and group-testing setups (e.g., Dubois, Rucker, & Galinsky, 2016, Experiment 3; Galinsky et al., 2003, Study 3; Garbinsky, Klesse, & Aaker, 2014, Experiment 1; Rucker, Dubois, & Galinsky, 2011, Experiment 1; Rus, van Knippenberg, & Wisse, 2010, Studies 1 and 2). Additionally, many studies using these manipulations (including Joshi and Fast's Study 1) have been conducted online via Amazon Mechanical Turk; in these cases, it is unknown whether other people were around while the participants completed the study, and whether these participants were completing multiple online studies in a row.

However, it is possible that the effects of power manipulations are strongest in a single-study, individual-testing setup. This issue is especially important for researchers to consider as more social psychological research, including research on power, is conducted with online samples. Researchers have limited ability to control the environment surrounding such participants.

A second possibility is that we failed to manipulate power successfully. It is important to distinguish between failures to manipulate the construct of interest and failures to find an effect of the construct of interest, though even the former can be informative for researchers (e.g., Cheung et al., 2016; Finkel, 2016). In Study 1, we confirmed the effectiveness of the role-based power manipulation by asking participants to report how much power they had in their role, the same manipulation check used by Joshi and Fast (2013, Study 1). Such manipulation checks are commonly used with role manipulations of power (e.g., Hildreth & Anderson, 2016, Studies 1a, 1b, 3, and 4; Mooijman, van Dijk, Ellemers, & van Dijk, 2015, Studies 1b and 4c).

In Study 2, though the original target study did not include a manipulation check, we included a common manipulation check for the recall power prime, asking participants how much power they had in the episode they recalled (e.g., Smith, Jostmann, et al., 2008, Study 3; Tost et al., 2012, Experiment 3). Low-power participants reported having less power in their recalled episode than did high-power and control participants, but high-power participants did not report having more power than control participants. Because Joshi and Fast (2013) did not use a manipulation check in their Study 3, we do not know if there was any difference in effectiveness between our manipulation and theirs. Notably, our results for the manipulation check do not mean we would have been unable to find effects of power on temporal discounting. Past researchers have found effects of power, including effects of the priming manipulation we used, on their critical dependent measures even when the control condition did not differ significantly from the low- or high-power condition on a manipulation check (e.g., Schmid, Kleiman, & Amodio, 2015, Study 1). Furthermore, our manipulation-check results do suggest that the recall prime successfully produced a power difference between low-power and high-power participants, which is the critical comparison. As reported in the Supplemental Material (p. 22), even a targeted analysis comparing only the low- and high-power participants in Study 2 yielded no effect of power on temporal discounting.

In Studies 1, 3, and 5 (see the Supplemental Material for the latter two studies), we also measured participants' general sense of power (Anderson et al., 2012). In Study 1, this measure came after several intervening studies to avoid any influence of the power manipulation on responses to it. In Studies 3 and 5, participants completed this measure soon after the power manipulation. Replicating results reported by Tost et al. (2015, Experiment 2) and Heller and Ullrich (2017), we found that participants' general sense of power did not differ between power conditions in any of these studies. Heller and Ullrich interpreted their null finding as a sign of an ineffective power manipulation. However, the version of the General Sense of Power scale employed in all these studies was designed to assess trait-level personal sense of power and asks how much control and influence respondents have in their relationships with other people in general (Anderson et al., 2012). Although the power manipulations employed in these studies may affect participants' momentary feelings of power, these manipulations are unlikely to affect participants' perception of their power and influence in all their social relationships. Indeed, other researchers have found significant effects of power manipulations on manipulation checks and key dependent

measures when these manipulations did not affect participants' trait general sense of power (e.g., Anderson & Galinsky, 2006, Study 2; Tost et al., 2015, Experiment 2). In short, the General Sense of Power scale as administered in our Studies 1, 3, and 5 is not a manipulation check.

We do share other researchers' concern that the standard manipulation checks used in the research literature on power may be subject to demand effects (e.g., Sturm & Antonakis, 2015). Because many commonly used power manipulations make it clear that the experiment has to do with power, participants may respond to manipulation checks by indicating how they think they are supposed to feel, rather than how powerful they actually feel. Though this topic is beyond the scope of the present replication attempts, as we were focused on conducting close replications of past work, future power research needs to grapple with this issue.

A third possible explanation for our failure to replicate Joshi and Fast's (2013) results is that our participants responded to the temporal discounting measure in unusual or extreme ways, or otherwise responded carelessly, which would have interfered with the ability of our power manipulations to have an effect. We believe this is not the case. As Table 1 shows, the discount rates for our conditions fall within the range of those of Joshi and Fast, and our standard deviations are similar. Comparing our data with data from studies involving temporal discounting measures similar to ours (Hardisty et al., 2013; Hardisty & Weber, 2009; Heller & Ullrich, 2017; Tost et al., 2015, Experiment 2), we also confirmed that other aspects of our data (e.g., the percentage of participants who always preferred the immediate option) were not unusual (see footnote 2 in the Supplemental Material for details). Researchers often report skewed discount-rate distributions, as we did for Study 1, and such skewness and outliers are dealt with in various ways, including dropping outliers, transforming data, and using nonparametric tests (e.g., Hardisty et al., 2013; Hardisty & Weber, 2009; Lempert, Glimcher, & Phelps, 2015; Tost et al., 2015). To ensure the robustness of our findings, we preregistered analyses using multiple common ways of dealing with skewness and outliers, and these analyses are reported in the Supplemental Material.

In addition, because discount rates are measured and calculated in a variety of ways in the temporal discounting literature, using responses to either titration questions or matching questions by themselves (e.g., Hardisty et al., 2013), or a combination of both (e.g., Hardisty & Weber, 2009), we preregistered analyses using multiple methods of calculating discount rates, and these analyses are reported in the Supplemental Material. Regardless of the method used, we found no evidence for an effect of our power manipulations on temporal discounting. We encourage researchers who investigate power and temporal discounting to be mindful of all these methodological differences in eliciting and calculating temporal discounting to increase the robustness of future investigations.

Finally, though we did not replicate Joshi and Fast's (2013) finding that elevated power reduces temporal discounting via increasing connection with the future self, we want to highlight that our power manipulations did affect some key dependent measures. In Study 1, we found that power and perceived legitimacy of the role manipulation had an interactive effect on temporal discounting. The effect of power was similar to the effect Joshi and Fast observed when our participants felt that their role assignment was not very legitimate, but the effect reversed when our participants felt the assignment was fairly legitimate. No other experiments on power and temporal discounting, conducted either by us or by other researchers, have measured or manipulated legitimacy, so this finding is suggestive but tentative. In Study 2, high-power participants reported greater connection with their future selves than did low-power and control participants. However, replicating the findings of Tost et al. (2015, Experiment 2) and Heller and Ulrich (2017), we found no evidence for connection with the future self as a mediator of the effects of power on temporal discounting.

Although we found little evidence that power reduces temporal discounting in our replication studies and in the meta-analysis, other research has shown an effect of power on some behaviors conceptually related to temporal discounting, such as saving (Garbinsky et al., 2014) and delaying consumption (May & Monga, 2014). Our finding of a null effect of power on temporal discounting does not necessarily cast doubt on these other findings. These behaviors, though related to temporal discounting, are also conceptually distinct from it (e.g., they are not always correlated with temporal discounting; Urminsky & Zauberman, 2015) and are affected by multiple other mechanisms. For instance, Garbinsky et al. (2014) found that high-power, compared with low-power, individuals were more willing to save because they were motivated to maintain their power by accumulating wealth; this result suggests that power should reduce temporal discounting only for rewards seen as means for maintaining power. Differences in constructs and causal attributions distinguish these other effects from the effect of power on temporal discounting, highlighting the importance of conducting close replications as well as conceptual ones (Cesario, 2014; Simons, 2014).

Identifying whether and how power affects temporal discounting is important for theory testing because two

prominent theories of power, the approach-inhibition theory (Keltner et al., 2003) and the social distance theory (Magee & Smith, 2013), make divergent predictions on this issue. Such an effect would also have important real-world implications for understanding and improving intertemporal decision making. The implications of Joshi and Fast's (2013) work have already created excitement in the policy area (e.g., Patton et al., 2016; Sheehy-Skeffington & Haushofer, 2014). However, as shown in our meta-analysis, the cumulative data are not consistent with an effect of power on temporal discounting. We also found suggestive evidence for a moderation effect of perceived legitimacy in Study 1: Relatively illegitimate power tended to decrease temporal discounting, but relatively legitimate power tended to increase it. Future research should investigate this moderation effect by manipulating legitimacy so that causality can be assessed. In sum, more high-powered research testing the conditions under which power influences temporal discounting is needed before incorporating this effect into theory or practice.

Action Editor

D. Stephen Lindsay served as action editor for this article.

Author Contributions

M. Zhang designed the experiments and collected and analyzed the data under the supervision of P. K. Smith. M. Zhang drafted the manuscript, and P. K. Smith provided critical revisions. Both authors approved the final version of the manuscript for submission.

ORCID iD

Min Zhang https://orcid.org/0000-0001-9212-2651

Declaration of Conflicting Interests

The author(s) declared that there were no conflicts of interest with respect to the authorship or the publication of this article.

Supplemental Material

Additional supporting information can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797617754219

Open Practices

All data, experimental materials, and analysis scripts for Studies 1 and 2 have been made publicly available via the Open Science framework and can be accessed at osf.io/gsv84 and osf.io/24mej, respectively. The design and analysis plans for the studies were preregistered at the Open Science framework and can be accessed at the same URLs. The complete Open Practices Disclosure for this article can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797617754219. This article has received badges for Open Data, Open Materials, and Preregistration. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

Notes

1. When discussing Tost et al.'s Experiment 2, we report statistics only for the personal discounting condition, which replicated Joshi and Fast's (2013) Study 1. Tost et al. also had an intergenerational discounting condition, which is irrelevant for the current purpose.
2. We also conducted three other studies that were procedurally different from the original studies; two of these studies were attempts to replicate Joshi and Fast's (2013) Study 1, and one was an attempt to replicate their Study 3. These studies did not find any effect of power on temporal discounting. Their methods and results are reported in the Supplemental Material available online.
3. With an N of 67, Joshi and Fast had 33% power to detect an effect of this size. This is the smallest effect size we aimed to detect with our replication, so we used it as the equivalence bound for Study 1.
4. Using the same rule as in Study 1, we identified the equivalence bound for Study 2 as $d = 0.43$.

References

Anderson, C., & Galinsky, A. D. (2006). Power, optimism, and risk-taking. *European Journal of Social Psychology*, *36*, 511–536. doi:10.1002/ejsp.324

Anderson, C., John, O. P., & Keltner, D. (2012). The personal sense of power. *Journal of Personality*, *80*, 313–344. doi:10.1111/j.1467-6494.2011.00734.x

Avila, C. (2001). Distinguishing BIS-mediated and BAS-mediated disinhibition mechanisms: A comparison of disinhibition models of Gray (1981, 1987) and of Patterson and Newman (1993). *Journal of Personality and Social Psychology*, *80*, 311–324. doi:10.1037/0022-3514.80.2.311

Baguley, T. (2012). *Serious stats: A guide to advanced statistics for the behavioral sciences*. New York, NY: Palgrave Macmillan.

Cesario, J. (2014). Priming, replication, and the hardest science. *Perspectives on Psychological Science*, *9*, 40–48. doi:10.1177/1745691613513470

Chandler, J., Mueller, P., & Paolacci, G. (2014). Nonnaïveté among Amazon Mechanical Turk workers: Consequences and solutions for behavioral researchers. *Behavior Research Methods*, *46*, 112–130. doi:10.3758/s13428-013-0365-7

Cheung, I., Campbell, L., LeBel, E. P., Ackerman, R. A., Aykutoğlu, B., Bahník, Š., . . . Yong, J. C. (2016). Registered Replication Report: Study 1 from Finkel, Rusbult, Kumashiro, & Hannon (2002). *Perspectives on Psychological Science*, *11*, 750–764. doi:10.1177/1745691616664694

Duan, J., Wu, S. J., & Sun, L. (2017). Do the powerful discount the future less? The effects of power on temporal discounting. *Frontiers in Psychology*, *8*, Article 1007. doi:10.3389/fpsyg.2017.01007

Dubois, D., Rucker, D. D., & Galinsky, A. D. (2016). Dynamics of communicator and audience power: The persuasiveness of competence versus warmth. *Journal of Consumer Research*, *43*, 68–85. doi:10.1093/jcr/ucw006

Finkel, E. J. (2016). Reflections on the commitment-forgiveness Registered Replication Report. *Perspectives on Psychological Science*, *11*, 765–767. doi:10.1177/1745691616664695

Frederick, S., Loewenstein, G., & O'Donoghue, T. (2002). Time discounting and time preference: A critical review. *Journal of Economic Literature*, *40*, 351–401. doi:10.1257/002205102320161311

Fujita, K., Trope, Y., Liberman, N., & Levin-Sagi, M. (2006). Construal levels and self-control. *Journal of Personality and Social Psychology*, *90*, 351–367. doi:10.1037/0022-3514.90.3.351

Galinsky, A. D., Gruenfeld, D. H., & Magee, J. C. (2003). From power to action. *Journal of Personality and Social Psychology*, *85*, 453–466. doi:10.1037/0022-3514.85.3.453

Garbinsky, E. N., Klesse, A.-K., & Aaker, J. (2014). Money in the bank: Feeling powerful increases saving. *Journal of Consumer Research*, *41*, 610–623. doi:10.1086/676965

Gubler, T., & Pierce, L. (2014). Healthy, wealthy, and wise: Retirement planning predicts employee health improvements. *Psychological Science*, *25*, 1822–1830. doi:10.1177/0956797614540467

Hamilton, W. K. (2017). MAVIS: Meta Analysis via Shiny (R Package, Version 1.1.3) [Computer software]. Retrieved from http://kylehamilton.net/shiny/MAVIS/

Hardisty, D. J., Thompson, K. F., Krantz, D. H., & Weber, E. U. (2013). How to measure time preferences: An experimental comparison of three methods. *Judgment and Decision Making*, *8*, 236–249. doi:10.1007/s10826-012-9600-6

Hardisty, D. J., & Weber, E. U. (2009). Discounting future green: Money versus the environment. *Journal of Experimental Psychology: General*, *138*, 329–340. doi:10.1037/a0016433

Harrison, G. W., Lau, M. I., & Rutström, E. E. (2010). Individual discount rates and smoking: Evidence from a field experiment in Denmark. *Journal of Health Economics*, *29*, 708–717. doi:10.1016/j.jhealeco.2010.06.006

Heller, S., & Ullrich, J. (2017). Does power increase self-control? Episodic priming may not provide the answer. *Collabra: Psychology*, *3*(1), Article 3. doi:10.1525/collabra.48

Hildreth, J. A. D., & Anderson, C. (2016). Failure at the top: How power undermines collaborative performance. *Journal of Personality and Social Psychology*, *110*, 261–286. doi:10.1037/pspi0000045

Joshi, P. D., & Fast, N. J. (2013). Power and reduced temporal discounting. *Psychological Science*, *24*, 432–438. doi:10.1177/0956797612457950

Keltner, D., Gruenfeld, D. H., & Anderson, C. (2003). Power, approach, and inhibition. *Psychological Review*, *110*, 265–284. doi:10.1037/0033-295X.110.2.265

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*, 78–87. doi:10.1037/0096-3445.128.1.78

Lakens, D. (2017). Equivalence tests. *Social Psychological & Personality Science*, *8*, 355–362. doi:10.1177/1948550617697177

Lammers, J., Galinsky, A. D., Gordijn, E. H., & Otten, S. (2008). Illegitimacy moderates the effects of power on approach. *Psychological Science*, *19*, 558–564. doi:10.1111/j.1467-9280.2008.02123.x

Lammers, J., Galinsky, A. D., Gordijn, E. H., & Otten, S. (2012). Power increases social distance. *Social Psychological & Personality Science*, *3*, 282–290. doi:10.1177/1948550611418679

Lempert, K. M., Glimcher, P. W., & Phelps, E. A. (2015). Emotional arousal and discount rate in intertemporal choice are reference dependent. *Journal of Experimental Psychology: General*, *144*, 366–373. doi:10.1037/xge0000047

Magee, J. C., & Galinsky, A. D. (2008). Social hierarchy: The self-reinforcing nature of power and status. *The Academy of Management Annals*, *2*, 351–398. doi:10.1080/19416520802211628

Magee, J. C., & Smith, P. K. (2013). The social distance theory of power. *Personality and Social Psychology Review*, *17*, 158–186. doi:10.1177/1088868312472732

May, F., & Monga, A. (2014). When time has a will of its own, the powerless don't have the will to wait: Anthropomorphism of time can decrease patience. *Journal of Consumer Research*, *40*, 924–942. doi:10.1086/673384

Mischel, W., Shoda, Y., & Rodriguez, M. I. (1989). Delay of gratification in children. *Science*, *244*, 933–938. doi:10.1126/science.2658056

Mooijman, M., van Dijk, W. W., Ellemers, N., & van Dijk, E. (2015). Why leaders punish: A power perspective. *Journal of Personality and Social Psychology*, *109*, 75–89. doi:10.1037/pspi0000021

Patton, G. C., Sawyer, S. M., Santelli, J. S., Ross, D. A., Afifi, R., Allen, N. B., . . . Viner, R. M. (2016). Our future: A *Lancet* commission on adolescent health and wellbeing. *The Lancet*, *387*, 2423–2478. doi:10.1016/S0140-6736(16)00579-1

Pronin, E., Olivola, C. Y., & Kennedy, K. A. (2008). Doing unto future selves as you would do unto others: Psychological distance and decision making. *Personality and Social Psychology Bulletin*, *34*, 224–236. doi:10.1177/0146167207310023

Rucker, D. D., Dubois, D., & Galinsky, A. D. (2011). Generous paupers and stingy princes: Power drives consumer spending on self versus others. *Journal of Consumer Research*, *37*, 1015–1029. doi:10.1086/657162

Rus, D., van Knippenberg, D., & Wisse, B. (2010). Leader power and leader self-serving behavior: The role of effective leadership beliefs and performance information. *Journal of Experimental Social Psychology*, *46*, 929–933. doi:10.1016/j.jesp.2010.06.007

Schmeichel, B. J., Harmon-Jones, C., & Harmon-Jones, E. (2010). Exercising self-control increases approach motivation. *Journal of Personality and Social Psychology*, *99*, 162–173. doi:10.1037/a0019797

Schmid, P. C., Kleiman, T., & Amodio, D. M. (2015). Power effects on cognitive control: Turning conflict into action. *Journal of Experimental Psychology: General*, *144*, 655–663. doi:10.1037/xge0000068

Sheehy-Skeffington, J., & Haushofer, J. (2014). Decision-making barriers and opportunities. In *Barriers and opportunities at the base of the pyramid: The role of the private*

sector in inclusive development (pp. 111–125). Istanbul, Turkey: UNDP Istanbul International Center for Private Sector in Development. Retrieved from http://www.undp.org/content/undp/en/home/librarypage/poverty-reduction/private_sector/barriers-and-the-opportunities-at-the-base-of-the-pyramid-the-/

Simons, D. J. (2014). The value of direct replication. *Perspectives on Psychological Science*, *9*, 76–80. doi:10.1177/1745691613514755

Simonsohn, U. (2015). Small telescopes: Detectability and the evaluation of replication results. *Psychological Science*, *26*, 559–569. doi:10.1177/0956797614567341

Smith, P. K., Jost, J. T., & Vijay, R. (2008). Legitimacy crisis? Behavioral approach and inhibition when power differences are left unexplained. *Social Justice Research*, *21*, 358–376. doi:10.1007/s11211-008-0077-9

Smith, P. K., Jostmann, N. B., Galinsky, A. D., & van Dijk, W. W. (2008). Lacking power impairs executive functions. *Psychological Science*, *19*, 441–447. doi:10.1111/j.1467-9280.2008.02107.x

Sturm, R. E., & Antonakis, J. (2015). Interpersonal power: A review, critique, and research agenda. *Journal of Management*, *41*, 136–163. doi:10.1177/0149206314555769

Tost, L. P., Gino, F., & Larrick, R. P. (2012). Power, competitiveness, and advice taking: Why the powerful don't listen. *Organizational Behavior and Human Decision Processes*, *117*, 53–65. doi:10.1016/j.obhdp.2011.10.001

Tost, L. P., Wade-Benzoni, K. A., & Johnson, H. H. (2015). Noblesse oblige emerges (with time): Power enhances intergenerational beneficence. *Organizational Behavior and Human Decision Processes*, *128*, 61–73. doi:10.1016/j.obhdp.2015.03.003

Trope, Y., & Liberman, N. (2010). Construal-level theory of psychological distance. *Psychological Review*, *117*, 440–463. doi:10.1037/a0018963

Urminsky, O., & Zauberman, G. (2015). The psychology of intertemporal preferences. In G. Keren & G. Wu (Eds.), *The Wiley Blackwell handbook of judgment and decision making* (pp. 141–181). Chichester, England: John Wiley & Sons. doi:10.1002/9781118468333.ch5

Zhou, H., & Fishbach, A. (2016). The pitfall of experimenting on the Web: How unattended selective attrition leads to surprising (yet false) research conclusions. *Journal of Personality and Social Psychology*, *111*, 493–504. doi:10.1037/pspa0000056