

RUNNING HEAD: Postponing Temptations

**The Taming of Desire:**

**Unspecific Postponement Reduces Desire for and Consumption of Postponed Temptations**

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## Abstract

The present investigation began with the conjecture that people may do better by saying “some other time” instead of “no, not ever” in response to temptations. Drawing from learning theories, we hypothesized that people interpret unspecific postponement (“I can have it some other time”) as a signal that they do not strongly value the postponed temptation. In this way, unspecific postponement may reduce desire for and consumption of postponed temptations, both in the present moment and over time. Four experiments tested those hypotheses. A multi-phase study using the free-choice paradigm supported the learning account for the effects of postponement: unspecific postponement reduced immediate desire for a self-selected temptation which in turn statistically accounted for diminished consumption during the week following the manipulation – but only when postponement was induced, not when it was imposed (Experiment 1). Supporting the hypothesis that unspecific but not specific postponement connotes weak valuation, only unspecific postponement reduced attention to (Experiment 2) and consumption of (Experiment 3) the postponed temptation. Additionally, unspecific postponement delayed consumption primarily among those who were highly motivated to forgo consumption of the temptation (Experiment 3). A final multi-phase experiment compared the effectiveness of unspecific postponement to the classic self-control mechanism of restraint, finding that unspecific postponement (vs. restraint) reduced consumption of the temptation in the heat of the moment and across one week post-manipulation (Experiment 4). The current research provides novel insight into self-control facilitation, the modification of desire, and the differential effects of unspecific and specific intentions for reducing unwanted behavior.

Keywords: desire, self-control, self-perception, consumption, postponement

“The only way to get rid of a temptation is to yield to it. Resist it, and your soul grows sick with longing for the things it has forbidden to itself.” – Oscar Wilde

To desire is human. What seems equally human, however, is to attempt to tame one’s desires with less than ideal success. To illustrate, an experience-sampling study estimated that people feel some manner of desire during half of their waking hours, that they try to resist half of their experienced desires, but that they give into two-thirds of the desires they wish to restrain (Hofmann, Baumeister, Förster, & Vohs, 2012; Hofmann, Vohs, & Baumeister, 2012). Given the pervasiveness of desire and the fallibility of restraint, a critical goal for research is to identify ways to temper desire in order to promote self-control success.

The current investigation examined a novel response to transient temptations that was hypothesized to promote self-control through the weakening of desire: postponing consumption. Drawing from classic theories which suggest that behaviors and intentions can serve as guides to inner states (Bem, 1972; Festinger, 1957; Festinger & Carlsmith, 1959), we hypothesized that people interpret unspecific postponement (“I can have it some other time”) as a signal that they do not strongly value that which they have postponed. From a learning perspective, then, unspecific postponement may weaken desire for and consumption of transient temptations in the heat of the moment and over time. Theoretically, this implies that desire and consumption are at least partially determined by the inferences that people make about their reactions to temptations.

To test the learning account for the effects of unspecific postponement, boundary conditions implied by the logic underlying our hypothesis were tested. Specifically, we examined whether postponement was induced or imposed, whether postponement was unspecific or specific, and individual differences in the motivation to forgo the temptation. Thus, in addition to delineating the consequences of postponement for desire and consumption at multiple points in time, the present investigation sought to illuminate factors that determine when and why

postponement reduces desire and facilitates self-control. A subsidiary goal of the investigation was to test the hypothesis that postponement may be more effective at curbing consumption than the classic self-control mechanism of restraint. In other words, people may do better by saying “some other time” instead of “no, not ever”.

### **Inferences about Inner States**

People come to know their attitudes and preferences at least in part by observing and interpreting their actions (e.g., Bem, 1972; Festinger, 1957; Festinger & Carlsmith, 1959; Khan & Dhar, 2006; Koo & Fishbach, 2010; Wilson, Dunn, Kraft, Lisle, 1989). For example, the now classic forbidden-toy studies showed that children who resisted playing with an appealing but prohibited toy subsequently devalued that toy (Aronson & Carlsmith, 1963; Carlsmith et al., 1969; Lepper, Zanna, & Abelson, 1970; Ostfeld & Katz, 1969; Turner & Wright, 1965).

Notably, derogation of the forbidden toy was observed only among children who were given a mild threat for disobeying the rule. Children who were given a severe threat did not change their attitudes toward the toy. That finding illustrates a cornerstone of learning theories: when behaviors are under external rule, they do not shape inner attitudes. In contrast, when behaviors are perceived as freely chosen, or when the absence of free choice is not made salient, behaviors can serve as clues to one’s inner states (Arkes & Ayton, 1999; Arkes & Blumer, 1985; Bem, 1972; Finkelstein & Fishbach, 2010; Thaler, 1985).

Research showing that external behaviors can shape inner attitudes is plentiful. Is that same process tenable for the modification of desire? After all, many appetitive desires, such as those for food, drugs, and sex are considered especially problematic because of their automatic and physiological nature. Initial evidence is encouraging, suggesting that top-down processes can help shape the emergence, experience, and down-regulation of desire (Fishbach, Friedman, &

Kruglanski, 2003; Fujita & Han, 2009; Houben, Havermans, & Wiers, 2010; Metcalfe & Mischel, 1999; Van Dillen, Papiés, & Hofmann, 2013; Wiers, Rinck, Kordts, Houben, & Strack, 2010).

Most relevant to the present investigation is work showing that desire can be constructed by people's inferences about the conditions surrounding their non-consumption of a temporarily unavailable good (e.g., a favorite breakfast food while traveling; Dai & Fishbach, 2014). When alternatives for the unavailable good were not salient (e.g., one thought only of one's preferred bagel for breakfast), desire was positively related to time since consumption, ostensibly because people inferred that unfulfilled desires become stronger over time. In contrast, when substitutes for the temporarily unavailable good were made salient or were actually consumed (e.g., granola in lieu of the typically preferred bagel), desire was inversely related to length of abstinence, arguably because people inferred they had developed new tastes. The work by Dai and Fishbach (2014) is instructive because it hints that people may learn about their desires just as they learn about their attitudes – by examining and explaining their behavior.

### **Learning From Postponement**

In light of evidence suggesting that desire can be constructed through inferences about one's behavior, it is possible that a similar process may occur when people observe themselves postponing consumption. In the current work, postponement was conceptualized as the intention or act of putting off consumption without external reward for incurring the delay (cf. delay of gratification; Metcalfe & Mischel, 1999). Although a learning perspective suggests that postponement should modify desire primarily when postponement is chosen, not imposed or incentivized, the inferences that people will make about postponement are not clear.

At first blush, one may expect that postponement will be interpreted as a signal of strong

desire for the temptation. After all, postponement implies that one will have the temptation, albeit at some other point in time. Consistent with that intuition, people savor upcoming positive experiences (e.g., Loewenstein, 1987) and interpret waiting for a product as a cue that they strongly care about the product (Dai & Fishbach, 2013; Koo & Fishbach, 2010). Although specific delays may boost desire, theory and research on behavior and goal specificity suggest that postponement may lead to inferences of low desire when the postponement intention is imprecise and vague.

It is generally accepted that specific plans and goals are beneficial for goal pursuit (cf. Mishra, Mishra, & Shiv, 2011) at least in part because they initiate a cascade of helpful downstream consequences. For example, specific goals direct attention to the focal goal (Locke & Bryan, 1969; Locke & Latham, 1990), shield attention from irrelevant tasks (Achtziger, Gollwitzer, & Sheeran, 2008; Shah, Friedman, & Kruglanski, 2002), and stimulate the development of contingency plans that help people overcome the inevitable roadblocks that block the path to success (Earley, Connolly, & Ekegren, 1989; Smith, Locke, & Barry, 1990). Although specific plans are helpful for completing outcomes that require willpower (e.g., forcing oneself to go to the gym instead of laying on the couch), we anticipated that the “perils” of unspecific intentions could sometimes be leveraged against unwanted impulses to reduce relatively more automatic and unwanted outcomes (e.g., forgoing a temptation).

When people encounter vague information, they typically stop and try to make sense of that information (Mishra et al. 2011). In this way, postponement intentions containing unspecific information, such as a vague time, place, or event, may shift people from acting on impulse to unpacking the intention (e.g., “what do I mean by some other time?”). In contrast, specific goals tend to narrow attention to the focal goal (e.g., Locke & Latham, 1990) and remain active in the

mind until fulfilled (Förster, Liberman, & Higgins, 2005; Klinger, 1975; cf. Masicampo & Baumeister, 2011). In this way, specific postponement intentions (e.g., “I can have it in 1 week”) may not be very helpful for desire reduction because they may focus people’s attention on the temptation.

In addition to their diverging effects on attention, unspecific and specific intentions foster different perceptions about one’s commitment to the intended outcome, where commitment is conceptualized as the motivation to achieve the outcome (Hollenbeck & Klein, 1987; Locke, Shaw, Saari, & Latham, 1981; Naylor & Ilgen, 1984; Naylor, Pritchard, & Ilgen, 1980; Salancik, 1977; Wright & Kacmar, 1994). When behaviors or goals are explicit and specific, people feel a strong commitment to the specified outcome. In contrast, when behaviors or goals are vague and unspecific, people feel a weak commitment to the outcome. In this way, vague postponement intentions (e.g., “some other time”) but not specific postponement intentions (e.g., “1 week”) may induce people to feel weakly motivated toward the temptation, leading to inferences of lackluster desire.

In summary, two lines of research suggest that unspecific (but not specific) postponement may weaken desire for a postponed temptation. Vague information encourages people to decipher the meaning of the imprecise information, and unspecific goals and intentions weaken motivation toward the outcome. Taken together, these lines of research suggest that unspecific (but not specific) postponement may lead people to infer that they do not strongly value the postponed temptation.

### **Motivation to Forgo the Temptation**

Research on motivated perception and wishful thinking suggests that ambiguous information tends to be decoded in line with one’s goals (e.g., Balci et al., 2006;

Caruso, Mead, & Balcetis, 2009; Sanford 1936, 1937). To the extent that people vary in their motivation to forgo a temptation, and to the extent that people's goals shape their interpretation of ambiguous stimuli, it may be that those who are highly motivated to reduce their intake of a temptation are particularly likely to interpret vague postponement as a signal of low valuation for the temptation. In contrast, those who are only weakly motivated to reduce their intake of a temptation may not draw such strong inferences about their desires from unspecific postponement. Thus, we expected that unspecific postponement would have the strongest effect among those highly motivated to reduce their intake of the postponed temptation.

### **Consumption**

Consumption is typically determined by both the strength of desire and the strength of willpower (Hoch & Loewenstein, 1991; Hofmann, Friese, & Strack, 2009). If unspecific postponement reduces desire for the postponed temptation, then it may reduce consumption in the heat of the moment.

In addition to reducing immediate consumption, the learning account for the effects of postponement suggests that unspecific postponement may reduce future consumption. When people encounter stimuli in their environment, desire is determined in part by the degree to which the stimuli have been pleasurable in the past (Kavanagh, Andrade, & May, 2005; Niedenthal, Barsalou, Winkielman, Krauth-Gruber, & Ric, 2005). To the extent that postponement has weakened previous perceptions of desire, and perhaps even reduced actual levels of consumption, postponement may sustain relatively lower levels of desire and consumption. Indirect support for the positive distal consequences of postponement comes from research showing that children who abstained from playing with the forbidden toy during a "no play" period continued to avoid the previously forbidden toy for up to six weeks (Freedman,



1965; Pepitone, McCauley, & Hammond, 1967).

### **The Present Research**

Four experiments tested the theory that unspecific postponement reduces desire for and consumption of a postponed temptation through inferences of low valuation for the temptation. Experiment 1 used the free-choice paradigm to test the learning account for the effects of unspecific postponement: unspecific postponement was hypothesized to reduce consumption of the temptation through diminished desire but only when postponement was induced, not when it was externally forced. Experiments 2-3 tested additional boundary conditions. Unspecific (but not specific) postponement was predicted to reduce desire for (Experiment 2) and consumption of (Experiment 3) the postponed temptation. Additionally, the effect of unspecific postponement was expected to be the strongest among individuals highly motivated to give up the temptation (Experiment 3). Experiment 4 was a multi-phase experiment that compared the effectiveness of unspecific postponement to the classic self-control mechanism of restraint. We predicted that unspecific postponement (vs. restraint) would be more effective for reducing consumption of a temptation and that that effect would be mediated by weakened desire for the temptation.

Sample sizes for Experiments 1-3 were determined in advance and were based on sample sizes of published studies that have used similar experimental procedures. The sample size of Experiment 4 was restricted to the number of participants who were recruited for a wave of data collection that was conducted in one week. Because of the logistics of the laboratories in which Experiments 1-3 were conducted, the lengths of studies were pre-specified (in days) to reach the target sample size. Running the experiments for a pre-determined amount of days resulted in final samples that were slightly lower or higher in number than the predetermined target. Data were analyzed upon termination of data collection.

### **Experiment 1: Induced versus Imposed**

Experiment 1 tested the learning account for the hypothesized effects of postponement by manipulating whether postponement could be perceived as freely chosen or externally imposed. Given that inner states follow from behavior so long as the absence of free choice is not made salient (Arkes & Ayton, 1999; Arkes & Blumer, 1985; Finkelstein & Fishbach, 2010), we anticipated that unspecific postponement would reduce consumption of the target temptation through the weakening of desire but only when it was induced, not when it was imposed.

To test those hypotheses, a multi-stage experiment was conducted. Participants completed an initial online survey during which they were induced (vs. forced) to formulate an unspecific-postponement intention (vs. no plan) for a self-selected target temptation (i.e., an unhealthy food that they strongly desired but wished to forgo). To check the assumption that induced (but not imposed) unspecific postponement was interpreted as signal of low valuation for the temptation, desire for the temptation was measured immediately after the manipulation.

To assess distal desire and consumption, participants completed a nightly diary for seven consecutive days. We predicted that unspecific postponement would reduce desire for and consumption of the target temptation across the week following the manipulation but only when it was induced, not when it was imposed. Drawing from a learning perspective, we anticipated that the beneficial effects of unspecific postponement would be limited in scope to the target temptation and would not spill over to other temptations.

#### **Participants and Procedure**

**Intake.** Based on research using similar methods and procedures, we aimed to recruit 160 participants to complete this experiment. Because we anticipated attrition from the experiment, we aimed to recruit an initial sample of 200 participants. By the end of the recruitment period

(i.e., the Spring Quarter at the university), 198 participants from a university online paid subject pool (80 male;  $M_{\text{age}} = 22.67$ ,  $SD_{\text{age}} = 3.22$ , age range 18-43) enrolled in this 2 (free vs. forced) x 2 (unspecific postponement vs. no-plan control) experiment in exchange for monetary compensation. Participants first selected a target temptation (described as the unhealthy food they desired most but wished to avoid) from a list of common snack foods: ice cream, chocolate, salty snacks, cookies, candy, cake, and “other” (the “other” category allowed participants to indicate their own temptation). The chosen snack food served as the target temptation for the remainder of the experiment.

The postponement manipulation was adapted from previous research (Sheeran & Webb, 2015). All participants first received a rationale for the manipulation: “the best thing to do to prevent yourself from eating something more than you want to is to get the urge out of your head. Don't let the voices in your head encourage you to eat [target temptation]!” Subsequently participants in the postponement condition typed the intention: “If I have the urge to eat [target temptation], I will tell myself that I can eat [target temptation] some other time!” The experimental software automatically inserted the target temptation in the rationale and the intention. Participants in the control condition received the rationale, which included their target temptation, but they did not formulate a plan for how to respond to the target temptation.

The free-choice paradigm (e.g., Linder & Jones, 1969) was used to manipulate whether the assigned strategy could be perceived as freely chosen or externally imposed. In the induced (free-choice) conditions, participants were not required to follow the given strategy; they were simply requested to read the rationale, and postponement participants were further asked to formulate the postponement intention. In contrast, participants in the imposed (forced-choice) conditions were told that they were yoked to another participant's response and therefore they

had to follow the strategy chosen by the other participant. Specifically, they were told “Participant 214 chose the following response to their temptation so you must too”. To validate the assumption that participants in the induced (but not imposed) condition would make inferences about their desire from the manipulation, desire to consume the target temptation at that very moment was measured using a sliding scale of 0 (*not at all*) to 100 (*very much so*). After this, participants were redirected to another website to register for the nightly-diary phase of the experiment.

**Nightly diary.** One hundred and fifty-three participants successfully registered their smartphone for the nightly-diary phase of the study (77% of the intake sample; 57 male;  $M_{\text{age}} = 22.46$ ,  $SD_{\text{age}} = 3.27$ , age range 18-43). Attrition from the experiment did not differ as a function of the postponement manipulation ( $p = .655$ ), the free-choice manipulation ( $p = .222$ ), nor an interaction between the two ( $p = .978$ ). A raffle for an iPad Mini was used to incentivize participants to complete all seven nightly diaries.

Beginning the day after the manipulation, and continuing for seven consecutive evenings, participants received a text message on their smartphone at 9pm (local time) with a hyperlink to a survey. Participants reported how much they a) noticed and b) desired each of the snack foods from the intake survey (0 = *not at all*; 100 = *very much so*). The measures of attention and desire were highly correlated for their target temptation ( $r(131) = .79$ ,  $p < .0001$ ) and non-target temptations ( $r(131) = .83$ ,  $p < .0001$ ) so those measures were combined to form indices of average daily desire for the target ( $M = 31.94$ ,  $SD = 19.61$ ) and non-target temptation ( $M = 20.88$ ,  $SD = 11.42$ ). Then, participants indicated (yes or no) whether they had consumed from any of the snack-food categories. If they did, they reported the quantity (in portion sizes) for each snack-food category. Hence, the nightly diary enabled assessments of desire,

consumption quantity, and the length of time that elapsed between the manipulation and subsequent consumption.

To ensure that one-week encapsulated the timeframe implied by “some other time” for the participants in the study, the meaning of “some other time” was assessed in the final nightly survey. All participants rated the extent to which “I can have [target temptation] some other time!” was represented by the following categories: within the day, tomorrow, in a few days, in a week, within a few weeks, and never (each rated on a 101-point Likert scale; 0 = *not at all*; 100 = *very much so*).

Post-test analyses suggest that “some other time” was best represented by “in a few days”. For ratings of representativeness, the category mean for “a few days” ( $M = 49.97$ ,  $SD = 29.22$ ) was significantly higher than all other category means (vs. “day”:  $M = 32.99$ ,  $SD = 32.01$ ,  $p < .0001$ ; vs. “tomorrow”:  $M = 40.05$ ,  $SD = 28.08$ ,  $p = .001$ ; vs. “week”:  $M = 38.10$ ,  $SD = 30.35$ ,  $p < .0001$ ; vs. “weeks”:  $M = 26.33$ ,  $SD = 27.75$ ,  $p < .0001$ ; and vs. “never”:  $M = 10.57$ ,  $SD = 19.46$ ,  $p < .0001$ ). None of the other category means differed significantly from one another. These results suggest that on average “some other time” represents “in a few days” and that the one-week assessment period captured the average timeframe implied by the unspecified-postponement manipulation.

To strike a balance between statistical power and representativeness of consumption, it was predetermined that participants who completed 5 or more nightly surveys would be retained for analyses. Hence, the indices of desire and consumption were computed by summing each of the daily measures and dividing those sums by the number of nightly diaries completed. One hundred and thirty-one participants met the criterion (86% of participants who registered in the mobile phase of the survey; 49 male;  $M_{\text{age}} = 22.55$ ,  $SD_{\text{age}} = 3.43$ , age range 18-43). Completion

rate did not differ as a function of the postponement manipulation ( $p = .549$ ), the free-choice manipulation ( $p = .616$ ), nor an interaction between the two manipulations ( $p = .238$ ).

## Results

**Post-manipulation desire.** The first key prediction was that postponement would immediately reduce desire for the target temptation in the induced (but not imposed) condition. A 2 (induced vs. imposed) x 2 (postponement vs. control) analysis of variance (ANOVA) predicting post-manipulation desire revealed the predicted interaction between postponement and choice,  $F(1, 194) = 9.279, p = .003, \eta^2 = .046^1$ . There were no significant main effects ( $F_s < 1.207, p_s > .272$ ).

Simple-effects analyses supported predictions. Examining the effect of postponement in the induced conditions only, the results indicate that postponement participants ( $M = 37.69, SD = 31.47$ ) reported lower desire for the temptation relative to control participants ( $M = 50.96, SD = 27.94$ ),  $F(1, 194) = 5.06, p = .026$ . The opposite pattern emerged in the imposed conditions: postponement participants reported higher desire for the temptation ( $M = 55.28, SD = 29.23$ ) than did control participants ( $M = 42.69, SD = 30.27$ ),  $F(1, 194) = 4.23, p = .041$ . Hence, induced postponement immediately lowered desire for the target temptation, but imposed postponement may have heightened desire through reactance.

Examining the results a different way also supported predictions. Participants in the induced-postponement condition reported lower desire as compared to participants in the imposed-postponement condition,  $F(1, 194) = 8.61, p = .004$ . In contrast, there was no difference between the no-plan control conditions ( $F(1, 194) = 1.88, p = .172$ ).

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<sup>1</sup> For transparency, the analyses of post-manipulation desire included data from all participants who completed the intake survey. Descriptively, results did not change when including data from only those who completed the entire study (i.e., the intake and the nightly surveys).

**Average daily desire.** Analyses predicting average daily desire for the target temptation across the week post-intake provided additional support for the hypothesis that induced but not imposed postponement reduces desire for the target temptation. A 2 (induced vs. imposed) x 2 (postponement vs. control) predicting average daily desire for the target temptation yielded the predicted interaction  $F(1, 128) = 4.320, p = .040, \eta^2 = .033$ . No main effects emerged ( $F$ s < 1.490,  $p$ s > .224).

Simple-effects analyses supported predictions. In the induced conditions, postponement (vs. induced control) reduced average daily desire for the target temptation across the week following the manipulation,  $F(1, 128) = 5.38, p = .022$  ( $M_{\text{induced postponement}} = 25.15, SD = 20.31$ ;  $M_{\text{induced control}} = 36.34, SD = 19.64$ ). In contrast, imposed postponement (vs. imposed control) did not reduce average daily desire ( $F < 1$ ;  $M_{\text{imposed postponement}} = 35.00, SD = 20.09$ ;  $M_{\text{imposed control}} = 32.09, SD = 17.41$ ).

Looked at a different way, participants in the induced-postponement condition desired the target temptation less during the week following the manipulation as compared to participants in the imposed-postponement condition,  $F(1, 128) = 4.42, p = .038$ . There was no difference between control conditions as a function of the choice manipulation ( $F < 1$ ).

**Consumption.** Unspecific postponement (vs. control) was hypothesized to reduce consumption of the target temptation in the induced but not imposed condition. This was tested two ways. We first tested the conjecture that participants in the induced-postponement condition would take longer to consume the target temptation after the manipulation as compared to those in the control condition and those in the imposed-postponement condition. Then we tested the hypothesis that induced postponement would reduce quantity consumed of the target temptation across the week following the manipulation.

*Consumption delay.* Analyzing consumption delay – the number of days that transpired between the manipulation and subsequent consumption of the target temptation – yielded results that were supportive of hypotheses. Predicting consumption delay, a 2 (induced vs. imposed) x 2 (postponement vs. control) ANOVA revealed an interaction between the manipulations,  $F(1, 126) = 4.689, p = .032, \eta^2 = .036$  (see Figure 1). No main effects emerged ( $F_s < .902, p_s > .343$ ).

Simple-effects analyses supported predictions. In the induced conditions, postponement participants waited longer to eat their target temptation ( $M_{\text{free postponement}} = 4.67, SD = 2.84$ ) as compared to participants in the control condition ( $M = 3.12, SD = 2.82$ ),  $F(1, 126) = 4.74, p = .031$ . In contrast, imposed postponement (vs. control) did not delay consumption of their target temptation ( $F < 1; M_{\text{imposed postponement}} = 3.20, SD = 2.70; M_{\text{imposed control}} = 3.81, SD = 2.91$ ). Looked at a different way, participants induced to postpone waited longer to consume the target temptation as compared to participants forced to postpone,  $F(1, 126) = 4.23, p = .035$ . There was no difference between control conditions ( $F < 1$ ).

*Consumption quantity.* We predicted that induced (but not imposed) postponement would diminish the amount that participants consumed of the target temptation across the week following the manipulation. A 2 (free vs. forced) x 2 (postponement vs. control) ANOVA predicting quantity consumed revealed the hypothesized interaction,  $F(1, 126) = 4.422, p = .037, \eta^2 = .034$  (see Figure 1). No main effects emerged ( $F_s < 1.152, p_s > .284$ ).

Simple-effects analyses supported predictions. In the induced conditions, postponement (vs. control) reduced average daily consumption of the target temptation,  $F(1, 126) = 5.02, p = .027$  ( $M_{\text{free postponement}} = .41, SD = .41; M_{\text{free control}} = .75, SD = .69$ ). In contrast, forced postponement (vs. control) did not reduce quantity consumed on a daily basis ( $F < 1; M_{\text{forced postponement}} = .71, SD = .66; M_{\text{forced control}} = .60, SD = .65$ ).



Looked at a different way, participants induced to postpone consumed less of the target temptation during the week post-manipulation relative to participants forced to postpone consumption,  $F(1, 126) = 4.23, p = .041$ . There was no difference between control conditions as a function of the free-choice manipulation ( $F < 1$ ).

**Mediation.** We hypothesized that postponement reduced consumption of the target temptation because participants inferred from the unspecific-postponement intention that they did not strongly desire the target temptation. If that hypothesis is correct, then diminished consumption of the target temptation during the week following the manipulation should be explained by diminished desire for the target temptation but only in the induced-postponement condition.

The bootstrapping method (Preacher & Hayes, 2008) was used to test for moderated mediation (Preacher, Rucker, & Hayes, 2007). The results, based on 5,000 bootstrap samples, indicated that when the putative mediator of post-manipulation desire was included in the model, it significantly predicted average daily consumption of the target temptation,  $\beta = .006, t = 3.389, p < .001$  and the interaction between postponement and free choice was no longer significant.  $\beta = -.297, t = 1.402, p = .163$ . Additionally, the 95% confidence intervals surrounding the index of moderated mediation (Hayes, 2015) did not include 0 (LLCI =  $-.340$ ; ULCI =  $-.019$ ; see Figure 2).

As a robustness check, a similar set of analyses tested for the mediating role of average daily desire for the target temptation. A similar set of analyses as reported above suggested that average daily desire statistically accounted for the effect of unspecific postponement on average daily consumption in the free-choice condition only. Specifically, the results, based on 5,000 bootstrap samples, showed that when the putative mediator of average daily desire was included

in the model, it significantly predicted consumption of the target temptation,  $\beta = .015$ ,  $t = 6.38$ ,  $p < .001$  and the interaction between unspecific postponement and free choice was no longer significant.  $\beta = -.213$ ,  $t = 1.116$ ,  $p = .267$ . Additionally, the 95% confidence intervals surrounding the index of moderated mediation (Hayes, 2015) did not include 0 (LLCI =  $-.502$ ; ULCI =  $-.044$ ). Taken together, the two moderated-mediation analyses provided convergent support for the theory that postponement reduced consumption of the target temptation because it led participants to infer that they did not strongly desire that which they had freely postponed.

**Supplementary analyses.** Consistent with a learning account for postponement, the interactive effects of the postponement manipulation and the choice manipulation on desire and consumption were limited in scope to the target temptation and did not carry over to non-target temptations ( $F_s < 1.16$ ). These results are supportive of a learning account because they suggest that unspecific postponement changed inferences about the postponed temptation specifically but not unhealthy food generally. These results also indicate that induced-postponement participants did not compensate for reduced consumption of the postponed temptation with increased consumption of other unhealthy foods.

## **Discussion**

The results of Experiment 1 provided initial support for the hypothesis that postponement reduces desire for and consumption of postponed temptations because it leads people to infer that they do not strongly value the postponed temptation. Unspecific postponement (vs. control) reduced desire for the self-selected temptation, both immediately after the manipulation and across the week following the manipulation, but only among participants who could attribute the postponement intention to themselves. In contrast, and consistent with predictions, participants forced to formulate the postponement intention (vs. control), ostensibly because they were yoked

to another participant's choice, did not show a similar drop in desire. In addition to its effects on desire, induced postponement reduced consumption of the target temptation, both in terms of delaying consumption and reducing quantity consumed during the week following the intake survey. Furthermore, reduced consumption of the target temptation in the induced-postponement condition was statistically explained by reduced desire for the target temptation.

### **Experiment 2: Postponement Specificity**

Experiment 2 sought additional support for the learning account for the effect of postponement on desire by varying specificity of the postponement intention. Whereas unspecific plans foster perceptions of weak commitment to planned outcomes, specific plans promote feelings of relatively stronger commitment to planned outcomes (e.g., Salancik, 1977). We therefore hypothesized that unspecific but not specific postponement would weaken desire for a postponed temptation.

Experiment 2 was designed to achieve two additional goals. The first was to use a more rigorous measure of desire than the desire measures used in Experiment 1. In particular, we sought to measure implicit desire in the presence of the temptation. We therefore conducted a laboratory experiment in which we assessed participants' attention to a bowl of tempting cookies on their desk while they completed a reading-comprehension test on the computer. If people infer from unspecific postponement (but not specific postponement) that they do not strongly value the temptation, then their attention should be released from the target temptation and they should be able to perform well on the reading comprehension task, as evidenced by performance levels similar to those of (baseline) participants who were not exposed to cookies. The second goal was to validate the assumption that induced postponement is attributed more to the self than to external factors.

## Participants and Procedure

Based on the sample sizes of experiments using similar procedures, we aimed to recruit 90 participants for the study. By the end of the recruitment period, 108 undergraduate students completed this 3 (unspecific vs. specific vs. baseline) x 1 experiment in exchange for a small monetary payment (69 female;  $M_{\text{age}} = 22.39$ ,  $SD_{\text{age}} = 2.32$ ; age range: 18 – 30). Participants randomly assigned to the unspecific or specific postponement conditions were seated in cubicles that contained a bowl of twelve caramel cookies (i.e., Stroopwafels, a common cookie in Rotterdam where this study was conducted). Participants randomly assigned to the baseline condition sat in a cubicle devoid of cookies; extreme care was taken to ensure that baseline participants were not physically exposed to the cookies during the experiment.

At the beginning of the experiment, participants in the postponement conditions received the rationale for the postponement intention used in Experiment 1. Then, participants in the unspecific condition (*specific condition*) wrote: “If I have the urge to eat cookies, I will tell myself that I can eat cookies some other time (*in exactly one week*).” Baseline participants did not receive the rationale nor did they write an intention, they simply began with the attention task (described next).

To assess desire, we examined the allocation of attention to the target temptation and the reading comprehension task (Hofmann & van Dillen, 2012). These measures were taken from previous work (Masicampo & Baumeister, 2011; Smallwood, Beach, Schooler, & Handy, 2008). Participants read the first 2,000 words of *The Case of the Velvet Claws* by Erle Stanley Gardner. They were told the text was from a popular novel and they would later answer questions about the plot of the novel. All participants were asked to focus their attention on the task. In the postponement conditions, the cookies remained on the desk for the duration of the task.

Immediately after reading the passage, participants answered questions about their attention during the task. Using 7-point scales, participants reported how well they were able to focus on the story (1 = *very difficult*; 7 = *very easy*), how distracted they were during the reading task (1 = *not at all*; 7 = *very distracted*; reverse scored), and how much they thought about Stroopwafels during the reading task (1 = *not at all*; 7 = *a lot*; reverse scored). (Even though baseline participants were not physically exposed to Stroopwafels, we reasoned that inclusion of the item would not be disconcerting to participants given that Stroopwafels are culturally iconic in the Netherlands. Indeed, no participants reported concern about that item.) The above three items were combined to form an index of attentional focus ( $M = 4.68$ ,  $SD = 1.33$ ;  $\alpha = .63$ ).

Attentional focus was also measured by assessing performance on six reading comprehension questions. If participants could easily keep their attention from the tempting cookies while they read the story, then they should perform well on the test. The test came after the self-report measures of focus to ensure that perceptions of performance on the test would not influence participants' responses to the attentional measures. Correct answers were summed to form an index of reading comprehension ( $M = 4.48$ ,  $SD = 1.73$ ; range 0 – 6;  $\alpha = .75$ ).

Then participants in the two postponement conditions rated how much they attributed the postponement intention to themselves (1 = *not at all*; 5 = *very much*) and external factors such as the experiment or someone else (1 = *not at all*; 5 = *very much*). Finally, all participants completed demographics (gender and age) and a funnel suspicion probe. Participants were thanked, paid, and debriefed.

## **Results**

To check whether participants accurately completed the reading task, we timed the reading task. We predetermined that participants who advanced through the (2,000 word)

passage in less than 1 min would be excluded from data analysis because it is likely they skipped through the passage. Ten participants were excluded (4 in the baseline and 3 in each of the postponement conditions) resulting in a final sample of 98 participants (63 female).

**Attribution of postponement.** We first tested the assumption that participants attributed the postponement intention to themselves, which they did. Participants attributed postponement more to the self ( $M = 3.96$ ,  $SD = 1.25$ ) than to external factors ( $M = 2.55$ ,  $SD = 1.20$ ),  $t(96) = 7.486$ ,  $p < .0001$ . Attributions to internal (vs. external) factors did not differ across the specific and unspecific conditions: attribution to the self ( $t < 1$ ); attribution to external factors ( $M_{\text{specific}} = 2.76$ ,  $SD = 1.38$ ;  $M_{\text{unspecific}} = 2.32$ ,  $SD = 1.11$ :  $t(67) = 1.436$ ,  $p = .156$ ). These results affirm that the induced postponement intentions used in Experiments 1-3 can shape inner states because participants attributed the intentions more to the self than external factors.

**Attentional focus.** Unspecific postponement (but not specific postponement) was hypothesized to release attention from the temptation during the reading task as measured by the attentional index, which captured attention to the reading task rather than the temptation in front of them. A one-way ANOVA predicting attentional focus during the reading task revealed a significant effect of the manipulation,  $F(2, 95) = 6.693$ ,  $p = .002$ ,  $\eta^2 = .124$ .

Planned comparisons supported hypotheses. Unspecific-postponement participants ( $M = 4.85$ ,  $SD = 1.33$ ) focused on the reading task to a similar degree as baseline participants even though the former but not the latter completed the task in front of a bowl of cookies ( $M = 5.25$ ,  $SD = 1.04$ ),  $t(95) = 1.223$ ,  $p = .224$ . Additionally, unspecific-postponement participants reported greater ease of attention to the reading passage as compared to specific-postponement participants ( $M = 4.15$ ,  $SD = 1.47$ ),  $t(95) = 2.329$ ,  $p = .022$ ,  $d = .50$ . As expected, specific-postponement participants found it more difficult to focus on the reading comprehension task

relative to baseline participants,  $t(95) = 3.595, p = .001, d = .86$ . These results suggest that unspecific postponement (but not specific postponement) released participants' attention from the temptation, thereby allowing them to focus their attention on the reading task.

**Reading comprehension.** Performance on the reading comprehension task was a behavioral measure of ability to attend to the reading passage rather than on the temptation. Predicting performance on the reading task, a one-way ANOVA revealed a significant effect of experimental condition,  $F(2, 95) = 5.507, p = .005, \eta^2 = .104$ . Planned contrasts yielded results supportive of hypotheses. Unspecific postponement participants ( $M = 4.64, SD = 1.72$ ) did not perform any worse than baseline participants ( $M = 5.17, SD = 1.12$ ),  $t(95) = 1.236, p = .219$ , even though the former (but not the latter) completed the task in front of cookies. In contrast, and in line with predictions, participants in the specific-postponement condition ( $M = 3.85, SD = 2.04$ ) performed worse on the test relative to participants in the baseline condition,  $t(96) = 3.005, p = .003, d = .78$  and participants in the unspecific-postponement condition,  $t(95) = 2.006, p = .048, d = .40$ .

**Mediation.** We checked whether attentional focus during the reading task accounted for performance on the reading comprehension test using process analyses. As mentioned, specific-postponement participants reported being less able to attend to the passage and they performed worse on the test relative to baseline and unspecific-postponement participants. Therefore, specific postponement served as the comparison condition.

Process analyses confirmed that allocation of attention during the task statistically accounted for performance on the reading-comprehension test. The results, based on 5,000 bootstrap samples, revealed that the effect of specific postponement on reading comprehension dropped to non-significance when attentional focus was included in the model (vs. baseline:  $t(95)$

= 1.727,  $p = .088$ ; vs. specific postponement:  $t(95) = 1.036, p = .303$ ). Ninety-five percent bias-corrected bootstrap confidence intervals for the relative indirect effects excluded zero (based on 5,000 bootstrap samples), for the comparison to baseline (.282 to 1.163) and unspecific postponement (.080 to .854). Given that the confidence intervals did not include zero, these results suggest that unspecific postponement enabled participants to perform well on the reading task (as compared to specific-postponement participants) because it enabled them to focus on the story instead of the temptation.

## **Discussion**

The results of Experiment 2 were supportive of the hypothesis that unspecific postponement (but not specific postponement) reduces desire for postponed temptations. Participants induced to postpone cookies to “some other time” kept their attention away from the cookies and on the story, as evidenced by levels of attentional focus and reading comprehension similar to those of baseline participants who read the story without the presence of the tempting cookies. In other words, unspecific postponement enabled participants to perform as though the cookies were not even there.

Whereas unspecific postponement freed participants’ minds from the temptation, the evidence from this experiment suggests that specific postponement did not. Those who were induced to formulate the intention to have the temptation “in exactly 1 week” struggled to keep their attention on the reading task instead of the cookies. Compared to participants in the baseline and unspecific-postponement conditions, participants in the specific-postponement condition were less able to focus their attention on the reading-comprehension task. As such, they performed worse on the test compared to the participants in the other conditions. This finding corroborates previous work indicating that specific plans and goals narrow attention to the focal



goal (e.g., Locke & Bryan, 1969) and are interpreted as signaling relatively high commitment to the intended outcome (e.g., Salancik, 1977). The results also cast doubt on the alternative possibility that unspecific postponement reduces desire primarily through increased temporal distance given that unspecific but not specific postponement reduced desire even through the timeframe of the latter was slightly longer than the timeframe of the former.

Although the results of Experiment 2 provided support for the hypothesis that unspecific (but not specific) postponement reduces desire for the postponed temptation, consumption of the postponed temptation was not assessed. We turned to that next.

### **Experiment 3: Consumption Delay**

Experiment 3 tested the hypothesis that unspecific (but not specific) postponement reduces consumption of the postponed temptation. As a further test of the learning account for the effects of postponement, individual differences in motivation to forgo the temptation were examined. Because ambiguous information tends to be interpreted in line with one's goals (Balcetis & Dunning, 2006; Caruso et al., 2009; Sanford 1936, 1937), we reasoned that an unspecific postponement intention should be decoded as a signal of low commitment to the temptation particularly among those strongly motivated to give up the temptation. Trait self-control was also measured to show specificity for the moderating effect of motivation to forgo the temptation (i.e., high self-control could be positively or negatively associated with motivation to forgo the temptation).

Participants first completed an intake survey, which included self-selection of a target temptation, measurement of individual differences in desire to forgo consumption of the target temptation, and the postponement manipulation. The very next time that participants consumed the target temptation, they notified us by completing a short survey on their smartphone. In this

way, the outcome of interest was the amount of time that elapsed between the manipulation and subsequent consumption. We predicted that unspecific postponement (but not specific postponement) would produce a relatively longer delay in consumption as compared to the no-plan control condition. Additionally, we anticipated that that effect would be the strongest among participants who were highly motivated to forgo the target temptation.

### **Participants and Procedure**

Based on experiments using similar procedures, we aimed to recruit 240 participants in this 3 (unspecific vs. specific vs. no-plan control) x 2 (high vs. low motivation to forgo the temptation). Target sample size was higher than in Experiment 2 because we expected attrition from the experiment. By the end of the recruitment period (i.e., the Spring Quarter), 235 undergraduate students (120 female; age was not recorded in this experiment) enrolled in this experiment in exchange for partial course credit.

**Intake.** Participants first selected the snack food they strongly desired but wished to avoid from the list of common snack-food categories used in Experiment 1. The selected snack food served as the target temptation for the remainder of the experiment.

Then participants rated how motivated they were to give up the temptation (1 = *not at all*; 5 = *very much so*;  $M = 3.02$ ,  $SD = 1.10$ )<sup>2</sup>. Self-control was measured using the 13-item trait self-control scale (Tangney, Baumeister, & Boone, 2004) which assesses people's ability to exert self-control (e.g., "Pleasure and fun sometimes keep me from getting work done"; 1 = *not at all* to 5 = *very much*;  $M = 3.22$ ,  $SD = .51$ ;  $\alpha = .75$ ). Individual differences in motivation to forgo consumption and trait self-control were weakly negatively correlated ( $r(234) = -.118$ ,  $p = .056$ ).

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<sup>2</sup> General eating restraint was also measured using a 5-item dieting subscale (Herman & Polivy, 1975). That measure assesses general motivation to restrain consumption and not specific motivation to forgo a specific temptation. Hence, the latter was the individual difference measure of interest and was therefore included in the main text. Analyses using general eating restraint yielded a similar albeit weaker pattern of results.

The manipulation of postponement was similar to that used in Experiments 1-2. All participants first received the rationale for the manipulation. Participants in the postponement conditions were then induced to re-write an intention using their target temptation. In the unspecific (*specific*) postponement conditions participants wrote: “If I have the urge to eat [target temptation], then I will tell myself I can eat [target temptation] some other time (*in the next few days*)”. The target temptation was piped into the text by the experimental software. As in Experiment 1, participants randomly assigned to the no-plan control condition received the rationale, including the target temptation, but they did not formulate an intention.

After the manipulation, participants were asked to complete a follow-up survey the next time they consumed the snack food they had selected during the experiment. One participant declined. The remainder gave their smartphone numbers to the research assistant who sent them a text message containing the link to the follow-up survey. The research assistant carefully explained that they should click on the link in the text message the very next time they ate the snack food they had selected during the experiment. To reduce demand characteristics, all participants were assured anonymity through the creation of a unique ID that would link their responses across sessions

**Follow-up survey.** The purpose of the follow-up survey was to measure the amount of time that elapsed between the manipulation and subsequent consumption of the target temptation. Because both the laboratory survey and the follow-up survey were stamped with date and time (DD/MM/YYYY HH:MM:SS), we obtained a relatively precise measure of delay (to the unit of seconds). During the follow-up survey, participants were also prompted to select the snack food they had consumed, enabling us to ensure that participants reported consumption of the target temptation identified in the intake; all participants did. To ensure that longer delays did

not result in higher quantities of consumption, participants reported how many portions they consumed at the time of consumption. Consumption delay and quantity upon time of consumption were not correlated ( $r(156) = .107, p = .198$ ) meaning that sooner or later consumption did not result in less or more consumption.

We anticipated that some participants might not be able to complete the follow-up survey immediately at the time of consumption. To ensure that delayed responding would not affect results, participants reported how long it had been since they had eaten the snack food in question. The amount of time that elapsed between eating and responding did not vary as a function of experimental condition ( $F < 1$ ). A robustness check indicated that the main analyses were similar regardless of whether analyses predicted the raw or adjusted time measure; reported analyses therefore used the raw measure.

To determine a cutoff point for the consumption-delay measure, we conducted a pilot test with 104 Amazon Mechanical Turk workers. They indicated that “some other time” ranged from hours to 14 days ( $M = 40.35$  hours,  $SD = 47.46$  hours). Hence, the predetermined cutoff was the maximum value, 14 days. When two weeks had passed since the manipulation, participants received a request to complete the survey if they had not already done so. Participants who completed the questionnaire after the two-week cutoff but no later than three weeks were included in analyses. All participants who completed the survey after two weeks, regardless of the exact day they completed the survey, were assigned the same value for consumption: the predetermined ceiling of 14 days.

One hundred and seventy participants completed the follow-up consumption measure (response rate = 72.64%). Attrition rates did not differ as a function of the manipulation, individual differences in motivation to forgo the temptation, nor the interaction between the

experimental manipulation and the individual difference measure ( $ps > .333$ ). Data from 13 participants could not be included in the analyses because the participants did not report an ID that could be matched with an intake ID. Before beginning analyses, data were thoroughly checked for repeat participants. For participants who submitted multiple responses, data from the first consumption response were retained. The final sample was composed of 157 participants (91 females).

## Results

**Consumption delay.** The primary hypothesis was that unspecific postponement (vs. no-plan control) would delay consumption of the target temptation. Additionally, the effect of unspecific postponement on consumption delay was predicted to be the strongest among those who were highly motivated to forgo the target temptation. To test these hypotheses, time until consumption was regressed on the individual difference measure (centered), condition (dummy coded to compare each postponement condition to the no-plan control), and all two-way interactions. The model revealed a main effect of unspecific postponement (vs. no-plan control;  $\beta = .207$ ),  $t(150) = 2.296$ ,  $p = .023$ , partial  $r = .184$ , such that unspecific postponement delayed consumption of the temptation. As predicted, specific postponement (vs. no-plan control) did not delay consumption ( $t < 1$ ). The predicted interaction between unspecific postponement (vs. control) and motivation to forgo temptation was marginally significant ( $\beta = .188$ ),  $t(150) = 1.808$ ,  $p = .073$ , partial  $r = .146$  (see Figure 3). No other effects emerged ( $ts < 1.129$ ,  $ps > .260$ ).

Although the interaction between unspecific postponement (vs. control) and motivation to forgo consumption was marginally significant, we proceeded with hypothesis testing by conducting the planned simple-slope tests (Iacobucci, 2001; Kirk, 1995; Keppel & Wickens, 2004; Winer, Brown, & Michels, 1991). Consistent with predictions, unspecific postponement

(vs. control) increased consumption delay among those who were highly motivated to curb their consumption of the temptation ( $\beta = .368$ ),  $t(150) = 2.914$ ,  $p = .004$ , partial  $r = .231$ ; there was no significant effect of unspecific postponement (vs. control) among those low in motivation to restrain their consumption ( $t < 1$ ).

Additional analyses indicated that the above results did not descriptively change when including trait self-control in analyses. Those analyses also indicated that unspecific postponement did not interact with trait self-control to influence consumption delay. Taken together, obtained results cast doubt on the possibility that individual differences in trait self-control are responsible for the effect of unspecific postponement on delayed consumption among those strongly motivated to forgo consumption.

**Supplementary analyses.** Additional analyses examined whether participants ate more to compensate for waiting longer to consume. There was no evidence of that possibility. Although an interaction between unspecific postponement (vs. control) and motivation to forgo the temptation [ $(\beta = .228)$ ,  $t(142) = 2.169$ ,  $p = .032$ , partial  $r = .179$ ] emerged when predicting quantity consumed, unspecific postponement (vs. no-plan control) did not increase amount consumed upon consumption among those highly motivated to forgo consumption, even though it had delayed their consumption ( $t < 1$ ). Unspecific postponement (vs. control) decreased quantity consumed among those relatively low in motivation to forgo consumption ( $\beta = -.278$ ),  $t(142) = 2.128$ ,  $p = .035$ , partial  $r = -.176$ ), possibly because those individuals tend to consume more than others at baseline. No other significant effects emerged in the model ( $ts < 1.376$ ,  $ps > .170$ ).

## Discussion

Two pieces of evidence from this quasi-naturalistic experiment supported the learning account for the effects of postponement. First, participants who formulated an unspecific-postponement intention (vs. no plan control) but not a specific-postponement intention waited longer to consume the target temptation. Second, unspecific postponement (vs. control) delayed consumption the most among those who were highly motivated to reduce their intake of the temptation (from approximately 2 days until 7 days). Although motivation to reduce consumption and trait self-control were weakly negatively correlated, supplementary analyses do not suggest that individual differences in trait self-control can account for the interactive effect of postponement and individual differences in motivation to forgo the temptation. Taken together, obtained results lend support to the hypothesis that unspecific postponement delays consumption because people interpret unspecific postponement as a signal of low valuation for the temptation.

#### **Experiment 4: Postponement versus Restraint**

Experiment 4 was designed to compare the effectiveness of postponement against the two most common outcomes of a self-control conflict: indulgence and restraint. Previous research suggests that restraint connotes a focus on external barriers to consumption, making people feel that they do not have the power to cope with the temptation (Patrick & Hagtvedt, 2012). Additionally, initial acts of restraint tend to result in subsequent increases in consumption (e.g., Jansen, Mulken, & Jansen, 2007; Mann & Ward, 2001; Vohs & Heatherton, 2000). We therefore predicted that postponing a temptation (i.e., saying “some other time”) would be more effective for taming desire and consumption than the classic self-control mechanism of restraint (i.e., saying “no, not ever”) and indulgence. A multi-phase experiment was conducted to test these hypotheses.

During the intake, participants were exposed to an unhealthy snack food while they watched a film preview. Participants were induced to follow one of three eating strategies (restraint, indulgence, or postponement) under the illusion of free choice while the snack food remained in front of them for the duration of the film preview. Consumption was measured as a manipulation check. Then, after a series of unrelated experiments and a false debriefing, the temptation was re-introduced to participants. We expected that unspecific-postponement participants would consume less of the possible temptation as compared to restraint participants.

We administered follow-up surveys to assess desire and future consumption. Desire was measured 24 hours later and consumption of the target temptation was measured 1 week later. We expected that unspecific postponement would reduce consumption across the week following the manipulation through weakened desire.

### **Participants and Procedure**

This experiment was run during a wave of data collection that lasted one week. At the end of recruitment, 96 participants from a paid subject pool (60 male;  $M_{\text{age}} = 21.46$ ,  $SD_{\text{age}} = 4.60$ ; age range = 18-60) participated in this 3 (unspecific postponement vs. restraint vs. indulgence) x 1 experiment in exchange for monetary compensation. All participants were included in the analyses.

**Intake: manipulation and short-term consumption.** Participants arrived individually at the laboratory and were seated in a cubicle where a bowl of M&Ms had been placed. To disguise the purpose of the study, participants were informed that the study investigated how consumption decisions affect memory for film previews. To bolster this cover story, participants were asked to imagine walking to the movie theater, entering and purchasing their ticket, and walking by the concession stand.



The manipulation came next. Participants in the indulgence condition were asked to tell themselves “I can eat M&Ms”; participants in the restraint condition, “No, I will not have M&Ms”; participants in the postponement condition, “I can have M&Ms some other time”. Participants were requested to enact that response for the duration of the film preview. To ensure that participants experienced the illusion of choosing the strategy, instructions from free-choice studies were used (e.g., Linder & Jones, 1969). Participants were told, “For the duration of the film preview, we would appreciate it if you would continue following the behavior you just visualized. However, you are not required to do so. It is your choice.”

Participants then watched a 3-minute film preview of the documentary *Origami*. A benign documentary preview was chosen because changes in mood can affect consumption (Garg, Wansink, & Inman, 2007). After the film preview, the experimenter collected and weighed the bowl of M&Ms. Amount eaten during the film preview served as the manipulation check.

To maintain the cover story of the experiment, participants completed a set of questionnaires about the film preview. A mood scale (Brief Mood Introspection Scale; Mayer & Gaschke, 1988) was embedded in these questionnaires to ascertain whether the manipulation altered mood.

Participants were given a second opportunity to consume the M&Ms at the conclusion of the experimental session. This served as a short-term measure of consumption. To minimize demand effects, several other unrelated experiments came between the mood measure (described above) and the second consumption opportunity. Additionally, consumption took place after the experiment was ostensibly over (i.e., participants were paid and given a false debriefing). Participants were told there were some leftover M&Ms and, because they were the last

participant of the day, they could eat as many of the M&Ms as they wanted. To hold constant the amount of time available for the second consumption opportunity, participants were asked to complete a filler survey that assessed the laboratory conditions (e.g., Was it warm enough? Did all the computer equipment work? Was the experimenter on time?). After participants exited the laboratory room, the bowl of M&Ms was weighed to determine post-preview consumption.

To gauge suspicion, a funnel debriefing was administered before participants left the laboratory. Participants were asked to report any suspicions they had during the experimental session and the purpose of the tasks in the experiment. These measures enabled us to gauge whether demand effects could have influenced the results. Participants also recorded their email addresses for the follow-up surveys. To disguise the purpose of the follow-up measures, participants were told that their memory for the film preview would be assessed at two points in time (24 hours and 1 week later).

**Desire.** Twenty-four hours after the manipulation, participants received an email with a link to the follow-up survey. To disguise the purpose of the questionnaire (i.e., measure their desire for the temptation), participants were told their memory for the film preview was being assessed. After the filler questions, participants were asked to complete a separate survey for a different researcher, which was activated by clicking on a hyperlink. The second survey assessed desire for a variety of healthy and unhealthy snacks (i.e., how much they wanted to eat the food at that very moment); each was rated using a 7-point scale (1 = *not at all*; 7 = *very much so*). The primary variable of interest was participants' desire for the target-temptation category (chocolate candy). Participants were included in analyses if they responded within 24 hours of receiving the link. Eighty-four participants (30 female) completed the measure of desire (87.5% response rate; attrition did not vary across conditions).

**Weeklong consumption.** One week after the manipulation, participants received another email with a link to a survey. As in Phase 2, participants were told the survey assessed their memory for the film preview. After completing filler questions, they were asked to complete a short survey for the university about students' snack-food consumption. Participants accessed the consumption survey by clicking on a hyperlink.

Because we wanted to administer a surprise measure of consumption, participants had not been asked to record their consumption every day. We therefore anticipated that it would be difficult for participants to recall specific quantities of what they consumed during the past week. Thus, participants were asked to report the number of times they had consumed a variety of healthy and unhealthy snacks during the past 7 days (i.e. "How much did you consume X in the last 7 days?"). Participants were urged to be as honest and accurate as possible. The number of times that participants consumed the target temptation ('chocolate candy') during the week after the manipulation was the key dependent measure. Participants were included in analyses if they responded within 24 hours of receiving the link to the survey. Seventy participants (23 female) of the total sample completed the measures in time (72.9% response rate; attrition did not vary across conditions).

## Results

**Manipulation check.** We first checked that participants followed the induced strategy by measuring the amount of chocolate they ate during the film preview. An ANOVA indicated that amount eaten during the film preview varied as a function of experimental condition,  $F(2, 93) = 9.060, p < .0001, \eta^2 = .16$ . Planned comparisons revealed that participants followed the induced strategy. Participants in the restraint ( $M = 10.45\text{g}, SD = 13.25$ ) and postponement conditions ( $M = 5.41\text{g}, SD = 7.52$ ) ate less than participants in the indulgence condition ( $M = 21.71\text{g}, SD =$

21.79;  $t(93) = 2.935, p = .004, d = .62$ ;  $t(93) = 4.109, p < .0001, d = 1.00$ , respectively). Restraint and postponement participants did not differ ( $t(93) = 1.262, p = .210$ ). Hence, participants followed the induced strategy.

**Short-term consumption.** The hypothesis that postponement reduces consumption of the postponed temptation in the heat of the moment (i.e., when the temptation was re-introduced) was tested next. A one-way ANOVA revealed that consumption at the end of Phase 1 (i.e., after the fake debriefing) varied as a function of the manipulation,  $F(2, 93) = 3.280, p = .042, \eta^2 = .07$ . Consistent with predictions, postponement participants ( $M = 2.62\text{g}, SD = 4.39$ ) ate less chocolate than restraint participants ( $M = 9.03\text{g}, SD = 10.93$ ;  $t(93) = 2.561, p = .012, d = .77$ ). Hence saying “some other time” was more effective than saying “no, not ever”. Consistent with satiation research (McSweeney & Swindell, 1999; Rolls, Rowe, & Rolls, 1982), participants in the indulgence condition reduced their intake during the second consumption opportunity in the laboratory, eating similar amounts as those participants in the postponement condition ( $M = 6.15\text{g}, SD = 11.85$ ;  $t(93) = 1.419, p = .159$ ). In other words, for curtailing consumption, postponement was just as effective as actual consumption.

Analyses examining total amount eaten across the intake (i.e., summing consumption during the film preview and after the false debriefing) were supportive of predictions (see Figure 4). The manipulation had a significant effect on total amount eaten during the intake session,  $F(2, 93) = 7.624, p = .001, \eta^2 = .14$ . Postponement participants ( $M = 8.03\text{g}, SD = 10.40$ ) ate less chocolate than restraint ( $M = 19.48\text{g}, SD = 17.69$ ;  $t(93) = 2.238, p = .028, d = .79$ ) and indulgence participants ( $M = 27.85\text{g}, SD = 27.26$ ;  $t(93) = 3.901, p < .001, d = .96$ ). Indulgence and restraint participants did not differ ( $t(93) = 1.704, p = .092, d = .36$ ). Taken together, results

from the intake session provided initial support for the hypothesis that unspecific postponement may be more effective at reducing consumption as compared to restraint.

**Desire.** We hypothesized that the manipulation would affect participants' desire to eat chocolate candy (measured 24 hours after the conclusion of the experiment), which it did,  $F(2, 81) = 6.760, p = .002, \eta^2 = .12$ . Planned contrasts supported predictions and the patterns reported above. Postponement participants ( $M = 1.68, SD = .89$ ) reported lower desire to eat chocolate candy as compared to restraint ( $M = 3.03, SD = 1.70; p = .001, d = .97$ ) and indulgence participants ( $M = 3.10, SD = 1.66; p = .001, d = 1.04$ ). Restraint and indulgence participants, who ate a similar amount in the intake session, did not differ ( $t < 1$ ).

**Weeklong consumption.** Consumption during the week following the manipulation was examined next. An ANOVA revealed a significant effect of the manipulation on weeklong consumption of the target temptation,  $F(2, 67) = 4.851, p = .011, \eta^2 = .13$ . As predicted, participants in the postponement condition reported eating less of the target temptation during the week following the experiment ( $M = 1.15, SD = 1.14$ ) as compared to participants in the restraint ( $M = 4.46, SD = 5.12; t(67) = 3.101; p = .003, d = .89$ ) and indulgence conditions ( $M = 3.29, SD = 2.84; t(67) = 1.970, p = .053, d = .99$ ), although the latter difference was only marginally significant. Restraint and indulgence participants did not differ from one another ( $t(67) = 1.151, p = .254$ ). These results suggest that, during the seven days following the experiment, postponement diminished consumption of the postponed pleasure as compared to the classic self-control mechanism of restraint.

**Mediation.** Unspecific postponement was expected to reduce weeklong consumption of the postponed temptation through a weakening of desire. The mediating role of desire was examined by adding the putative mediator (desire) into a regression model predicting weeklong

consumption (postponement was coded as the comparison condition). As predicted, when desire was added into the model, desire remained a significant predictor of weeklong consumption ( $\beta = .34$ ),  $t(64) = 2.770$ ,  $p = .007$  (partial  $r = .31$ ) while postponement was no longer a significant predictor of consumption (vs. indulgence:  $\beta = .14$   $t(64) = .991$ ,  $p = .326$ ; partial  $r = .12$ ; vs. restraint:  $\beta = .28$ ,  $t(64) = 1.889$ ,  $p = .063$ ; partial  $r = .232$ ). Ninety-five percent bias-corrected bootstrap confidence intervals for the relative indirect effects excluded zero (based on 5,000 bootstrap samples), both for the comparison of postponement to indulgence (95% CI = 0.17 to 3.33) and the comparison of postponement to restraint (95% CI = 0.19 to 3.51). These analyses confirm that weakened desire for the postponed pleasure was statistically responsible for diminished consumption during the week following the experiment.

**Supplementary analyses.** Consistent with Experiments 1 and 3, postponement did not reduce desire or weeklong consumption of non-target unhealthy food (desire:  $F < 1.24$ ; consumption:  $F(2, 62) = 2.166$ ,  $p = .123$ ;  $M_{\text{indulgence}} = 5.63$ ;  $M_{\text{restraint}} = 8.10$ ;  $M_{\text{postponement}} = 3.52$ ). These results suggest that postponement did not cause people to compensate for reduced intake of the postponed pleasure with other unhealthy food, although the pattern of means suggest that restraint may have increased participants consumption of non-target unhealthy foods. Additionally, the manipulation did not affect desire for and consumption of healthy food ( $F_s < 1$ ) suggesting that postponement may not have changed self-perceptions of healthiness.

Although extreme care was taken to minimize suspicion and demand characteristics, we examined whether results could be attributed to suspicion or demand. In total, eight participants reported substantial suspicions: six (6.3%) were suspicious about the true purpose of the experiment (i.e., the true purpose of the M&Ms) and two participants (2.1%) were suspicious about the eating suggestion. Exclusion of those participants did not change reported results.

The experimental manipulation did not alter mood valence or arousal ( $F_s < 1.02$ ). Thus, obtained results cannot be attributed to changes in mood.

## **Discussion**

Results from this multi-phase experiment supported the hypothesis that unspecific postponement may be a more effective response to temptations as compared to the classic self-control mechanism of restraint. Participants who were induced to postpone consumption of chocolate candy subsequently consumed less of that candy as compared to participants who had been induced to restrain consumption. Additionally, unspecific postponement reduced consumption beyond the length of time implied by the intention of “some other time”. Across one week following the manipulation, unspecific postponement participants consumed less of the target temptation as compared to participants who had been induced to restrain or indulge. Consistent with the learning theory for the effects of unspecific postponement, reduced desire for the temptation after the postponement manipulation was statistically responsible for diminished weeklong consumption.

## **General Discussion**

Modern human life is replete with desires. Because of the prevalence of desire and its power to overwhelm willpower, an important endeavor for self-control researchers is to better understand how desire can be modified in order to promote self-control, particularly in the heat of the moment. Although putting things off until some other time is typically considered to be a counterproductive self-control response, the present research suggests that the pitfalls of vague plans can be leveraged against desires to promote self-control success.

Results from four diverse but complementary experiments provided support for the learning account for the effects of unspecific postponement. The most direct support came from

Experiment 1 which showed that unspecific postponement reduced desire for and consumption of a self-selected temptation but only when postponement was induced and could be attributed to the self, not when it was imposed and attributed to external factors (Experiment 1). Additional support came from the identification of theoretically and practically relevant boundary conditions. It was predicted and found that unspecific but not specific postponement diminished desire and consumption (Experiments 2-3), arguably because unspecific behaviors signal weak commitment to and hence motivation for the outcome (e.g., Salancik, 1977). Additionally, the effects of unspecific postponement on consumption were strongest among those most motivated to forgo the temptation (Experiment 3), conceivably because ambiguous information -- such as a vague postponement intention -- tends to be decoded in line with one's overarching goals.

Supporting the hypothesis that postponement reduces consumption through desire reduction, two multi-phase experiments demonstrated that unspecific postponement (vs. various control conditions) reduced consumption of the postponed temptation across one week and that that effect was statistically explained by weakened appetitive desire (Experiments 1 and 4). The current studies also provide initial evidence that unspecific postponement may sometimes be more effective for facilitating self-control as compared to the classic self-control strategy of restraint (Experiment 4).

The effects of unspecific postponement on desire and consumption were robust across different instantiations of unspecific postponement (Experiments 1-3 vs. 4). What mattered most from a theoretical and empirical perspective was that participants could attribute postponement to internal rather than external factors (Experiment 1). Results were also robust across different types of temptations (salty or sweet; chocolate, desserts, cookies and chips), whether the



temptation was self-selected or situationally introduced (Experiments 2 and 3), and the context in which the variables were investigated (laboratory and everyday life).

### **Implications for Self-Control**

The current research contributes to the understanding of self-control mechanics and self-control facilitation (see also Inzlicht, Legault, Teper, 2014; Masicampo, Martin, & Anderson, 2014). More specifically, to growing streams of research that examine the construction (e.g., Dai & Fishbach, 2013, 2014) and modification of desire to facilitate self-control (Fujita, 2011; Fujita & Carnevale, 2012; Fujita & Han, 2009; Hofmann & Van Dillen, 2012; Papies, Pronk, Keesman, & Barsalou, 2014). Indeed, the work reported in this manuscript is an initial answer to the call for self-control research to give a better understanding of how problematic desires can be managed in the heat of the moment (e.g., Hofmann & Van Dillen, 2012).

Theoretically, the current research suggests that people's reactions to temptations can shape their future desire and consumption. Specifically, the findings intimate that people may infer personal characteristics (i.e., "I am the type of person who does not like chocolate"), which, from a learning perspective, can continue to influence future behavior. The multi-stage approach taken in the current work enabled a dynamic insight into how the modification of current desire can shape future consumption. Future work should continue to identify factors that shift people's inferences about their desire and identify the boundary conditions for those effects. For example, it would be important for researchers to delineate when interventions cause people to infer personal versus situational characteristics about desire, given that the former but not the latter should have relatively more lasting changes. Additionally, future research could investigate processes through which inferences in one domain (e.g., eating) could potentially have spillover effects to other domains (e.g., exercise).

The current work provides a departure from the notion that specific plans and goals are superior to their nebulous counterparts (e.g., Gollwitzer & Sheeran, 2006; Webb & Sheeran, 2006). In particular, the results of the present investigation suggest that when people want to refrain from engaging in an unwanted, desire-driven outcome, unspecific plans can weaken the unwanted desire more effectively than specific plans. Thus, researchers and professionals interested in shaping and modifying desire should consider what outcome they are trying to encourage or discourage and match the plan accordingly.

A question with notable implications for the external validity of this work is whether people will choose to postpone of their own accord. Although we do not think that postponement is a naturally scripted response to temptations, an unpublished study from our laboratory suggests that people will employ it when it comes on offer. When high school students were given the option to postpone, restrain, or indulge eating a bag of potato chips, 41% of adolescents chose postponement whereas 0% of chose restraint. Hence, pleasure postponed may be hedonically more optimal than pleasure denied (see Patrick, Chun, & MacInnis, 2009 and Patrick & Hagtvedt, 2012, for hedonically rewarding efficacious self-control strategies).

### **Limitations and Future Directions**

Several limitations provide opportunities for future research. First, although the current work focused on postponing unhealthy food, the theory for the effects of postponement suggest that it could be effective for other temptations as well. Ideally the current work will provide researchers studying other self-control failures such as impulsive spending, eating disorders, and smoking with novel insight into those problematic behaviors. For example, smoking research suggests that the desire to smoke drops dramatically after people decide to forgo smoking, but that re-emergent desires are the primary reason for lighting up again (Shiffman et al., 1997).

Future research could examine whether postponement could help combat re-emergent smoking desires, thereby increasing people's chance of quitting. As for eating disorders, we learned from a colleague that postponement is an informal tool used by clinicians to help treat patients with eating disorders. Yet empirical support for the strategy, an understanding of the psychological mechanisms responsible for the effect, and practical boundary conditions for the strategy's effectiveness were all still wanting.

Second, the current work focused exclusively on manipulating whether the future time of consumption was specific (e.g., exactly 1 week) or unspecific (e.g., some other time). Although the conceptual model in the current work suggests that varying the specificity of the intention matters more than the content of specificity, future research could examine whether manipulating specificity of other cues (e.g., location, occasion, consumption companions) does indeed yield similar effects to those reported in this manuscript. In addition, research could examine the effects of naturally occurring postponement (e.g., "We can meet up some other time"), to gauge whether unspecific and specific postponement that is naturally used in a variety of settings produce similar outcomes to those established in the current work.

Third, the current research did not investigate whether postponement is an effortful strategy. Several pieces of evidence cast doubt on that possibility but future research should test the question directly. One piece of evidence is that postponement did not increase consumption after an initial period of deprivation (Experiment 4). Recall that, although participants in the restraint condition did show an uptick in consumption after initial restraint, no such increase was found among participants in the unspecific postponement condition. Another perhaps more direct piece of evidence is that unspecific postponement released participants' attention to the cookies, enabling them to perform equally well on a reading comprehension task (Experiment 2). Indeed,

participants in the unspecific postponement condition performed as though the cookies were not even there. Third, trait self-control did not moderate the effects of unspecific postponement on consumption (Experiment 3). Taken together, initial evidence casts doubt on the possibility that postponement is a taxing strategy to implement and maintain.

### **Concluding Remarks**

Successful self-regulation is perhaps the most important human skill for achieving a large number of meaningful outcomes in life. Until recently, research has predominantly focused on self-control facilitation through the lens of willpower suppressing transient desire. The current research adds a novel strategy to a growing body of research focusing on self-control facilitation through desire reduction. When perceived as freely chosen, the simple intention or act of unspecific postponement was found to have powerful consequences for self-control through desire reduction, both in the immediate moment and over time. In this way, putting things off until some later time may be better for humans than originally thought.

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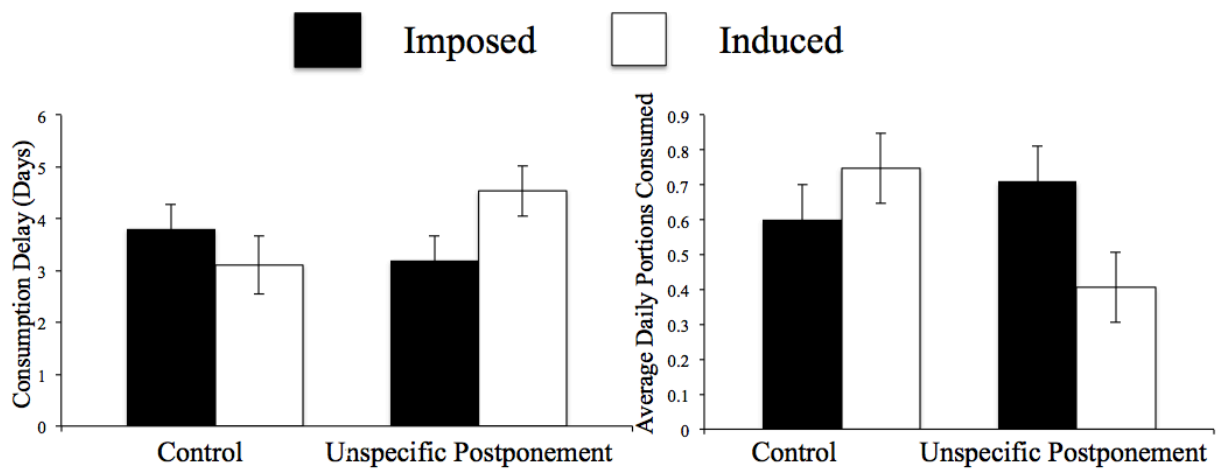
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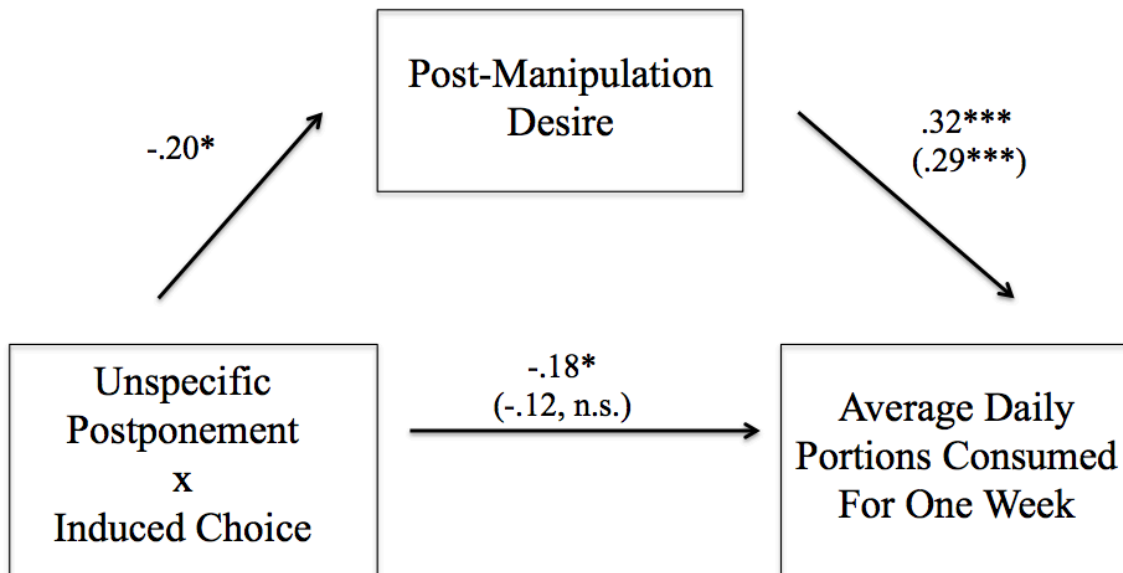
**Figure 1. Experiment 1.** Unspecific postponement (vs. control) delayed consumption of the target temptation (left panel; higher values = better self-control) and reduced average daily consumption of the target temptation one week post-manipulation (right panel; lower values = better self-control) but only when it was induced, not when it was imposed. Error bars denote  $\pm 1$  SEM.



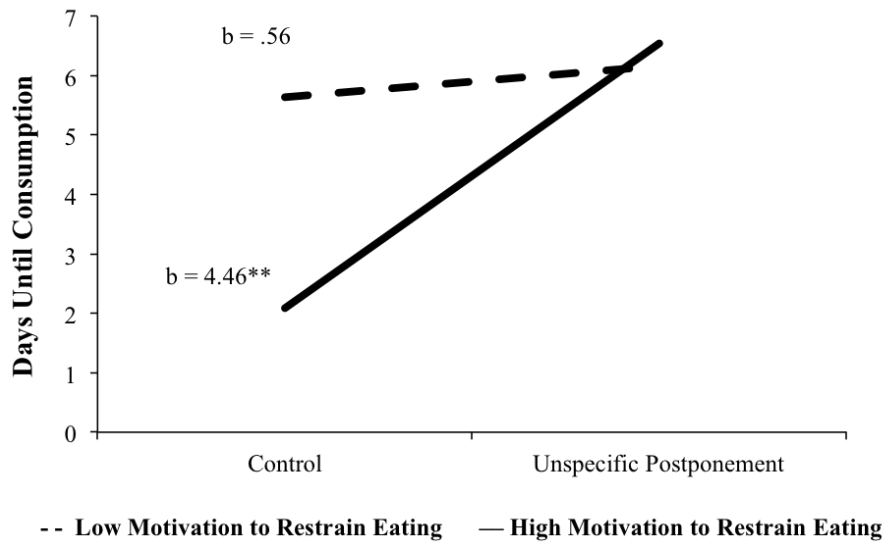


**Figure 2. Experiment 1.** The interaction between unspecific postponement (vs. no-plan control) and the induced- (vs. imposed-) choice manipulation on average daily portions consumed of the target temptation during the week following the manipulation was mediated by post-manipulation desire for the target temptation. Numbers refer to standardized regression weights.

\*  $p < .05$  \*\*\*  $p < .001$  n.s. = nonsignificant.



**Figure 3. Experiment 3.** Unspecific postponement delayed consumption of the self-selected consumption among participants who were high (but not low) in motivation to restrain themselves from the self-selected temptation. Vertical axis numbers reflect the number of days elapsed between the manipulation and subsequent consumption. Unstandardized regression coefficients reflect comparisons of the unspecific-postponement condition to the control (no-plan) condition. \*\*  $p < .01$ .



**Figure 4. Experiment 4 (Intake).** Grams of M&Ms eaten as a function of the experimental manipulation during the film preview (Preview), after receiving a fictitious debriefing (Post-Preview), and total amount eaten across the intake (Total). Error bars denote  $\pm 1$  SEM.

