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Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA  
RIVERSIDE

Moving Past Option and Future Payment Closer to Now Affects Decision and Behavior:  
Newness Effect and Prepayment Effect

A Dissertation submitted in partial satisfaction  
of the requirements for the degree of

Doctor of Philosophy

in

Management

by

Yun Jie

March 2017

Dissertation Committee:

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Dr. Thomas Kramer

Dr. Rami Zwick

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2017

The Dissertation of Yun Jie is approved:

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Committee Chairperson

University of California, Riverside

## ABSTRACT OF THE DISSERTATION

Moving Past Option and Future Payment Closer to Now Affects Decision and Behavior:  
Newness Effect and Prepayment Effect

by

Yun Jie

Doctor of Philosophy, Graduate Program in Management  
University of California, Riverside, March 2017  
Dr. Ye Li, Chairperson

This dissertation documents two time-related effects: newness effect and prepayment effect. The newness effect occurs when we move a past option closer to now along the timeline, while the prepayment effect occurs when we move a future payment closer to now along the timeline. The two effects are more specifically elaborated as follows. (1) Newness effect: In philosophy, the logical fallacy *argumentum ad novitatem* refers to preferring an option just because it is newer; yet, people consistently fall prey to this fallacy. Across ten studies ( $N = 3435$ ), the large majority of people prefer newer options to identically good but older options. This newness bias persists even though our study designs preclude rational reasons to choose the newer option including it being better, trendier, fresher, more socially relevant, or more informative. People even prefer to bet on newer die rolls or coin tosses and pay more for newer raffle tickets, contexts where

newer transparently cannot be better. We propose an association-based heuristic process and provide convergent evidence for this mechanism: Those who choose newer options spend less time deciding and neither time pressure nor cognitive load affects the bias. On the other hand, newness bias attenuates after deliberation and in domains where the opposite association exists, such as classic artwork. As indicated by the effect's name, both the easiness to access to the newness cue and the extent of newness perception can affect people's newness bias. We also showed that newness *per se* can be translated to a monetary premium in a real book auction. We conclude by ruling out some alternative explanations and offering implications.

(2) Prepayment effect: Extensive literature studied how to make task participants work harder and perform better. The question of what constitutes effective means to turn non-participants into participants is largely ignored. The current research has extended prepayment's effect on people's effort and performance to people's task participation. In two quasi field experiments, we have successfully motivated more people to participate by decoupling task and payment temporally and moving payment ahead of the required task. The higher participation rate is mainly caused by a.) temporal discounting to lure people to accept the prepayment b.) loss aversion once people accept the prepayment.

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# ESSAY 1 NEWNESS EFFECT: CONSUMERS' BIAS TOWARD NEWER OPTIONS

## *INTRODUCTION*

From 1992 to 2010, new product introductions of consumer packaged goods tripled from 15,718 to 47,772 (DataMonitor, 2016). Portending a continuation of this trend in new product growth, the number of published patent documents grew by 58% from 2005 to 2015 (AcclaimIP, 2015). As a result, consumers are constantly inundated with new products to choose from. Although newer products are often better in some way, is it possible that consumers also like newer options *just because* they are newer? That is, do consumers prefer newness *per se*?

The traditional view in marketing assumes that people prefer newer options because they are objectively superior in some way but some recent findings challenge this assumption. For example, Sela and LeBoeuf (2016) found that people fail to compare a potential upgraded option with the product they already own, a phenomenon they call “comparison neglect.” In the same vein, John, Garcia-Rada, and Norton (2016) showed evidence for a “revision bias” whereby people prefer revised experiences and products regardless of whether the revised version is actually any better. Both findings suggest that

people do not always prefer newer options because of objective product differences. Since both the upgraded and revised options are also chronologically newer, if people prefer newness *per se*, it may be one reason driving both comparison neglect and revision bias.

In this paper, we study how chronological newness affects people's decisions. Just as people prefer variety *per se* (Ratner, Kahn, & Kahneman, 1999), do they also prefer newness (or oldness) *per se*, above and beyond any objective quality considerations (where quality is defined broadly as a product's ability to fulfill consumer needs and expectations)? By definition, chronological cues (e.g., production date or release date or any chronological label attached to a product) are irrelevant if they are all that differentiate two otherwise identical options. For example, there is little reason to prefer one car over another identical model that rolled off the assembly line moments earlier or today's word of the day to yesterday's. Yet, our studies consistently found that people *do* often prefer such merely newer options.

Across ten studies, participants preferred newer options to older options of identical expected quality, despite the studies being designed so that participants could not make rational inferences about newer options being better, trendier, fresher, more socially relevant, or more informative. People even prefer to place consequential bets on newer die rolls and coin tosses—where newness transparently cannot matter—and are willing to pay to pay for this mere newness.

Our work therefore contributes to the existing marketing and psychology literatures on novelty seeking, consumer innovativeness, and new product development by showing that consumers generally prefer newer options for newness's sake. Indeed, people's preference for newness *per se* may be one neglected factor contributing to existing findings such as novelty seeking, comparison neglect, and revision bias.

### ***PREFERENCE FOR NEWNESS***

Both marketing and psychology researchers have been interested in whether and why people prefer newer (or older) options, whether those options are products in a store or abstract stimuli in a lab. Here, we briefly review these literatures and identify a gap therein that the present research fills.

#### **Novelty in Marketing**

Novelty has played a prominent role in marketing research, ranging from research on new product development and launch (Cooper, 1979; Goldenberg, Lehmann, & Mazursky, 2001; Henard & Szymanski, 2001; McAlister & Pessemier, 1982) to consumer innovativeness (Hirschman, 1980; Midgley & Dowling, 1978; Roehrich, 2004) and novelty seeking (Bianchi, 1998; Hirschman, 1980). For example, research on the relationship between new products' novelty and their market success has found mixed findings, including linear, U-shaped, and inversed U-shaped relationships (van Trijp & van Kleef, 2008).

On the other hand, research on consumer innovativeness takes an individual differences approach. Marketing scholars used a variety of terms to describe consumers' preference for new products such as novelty seeking, curiosity drive, and exploratory drive. (Faison, 1977). Steenkamp, Hofstede, and Wedel (1999, p. 56) define consumer innovativeness as "the predisposition to buy new and different products and brands rather than remain with previous choices and consumption patterns," a personality trait regarding how early a consumer adopts new products. By definition it is highly related to new product trial (Steenkamp & Gielens, 2003) and adoption (Mittelstaedt, Grossbart, Curtis, & Devere, 1976; Rogers, 1976).

The extant marketing research generally focuses on novelty as the extent to which a product is unique, original, or radical (Henard & Szymanski, 2001; Steenkamp & Gielens, 2003). Indeed, newer products usually do differ from older products in substantive ways that may lead consumers to prefer them. Yet, products can also be objectively newer on some chronological dimension, such as a newer production date or release date, without substantive quality differences. Is it possible that part of the reason that consumers prefer newer options is that they prefer newness for newness's sake, separate from any actual benefits provided by newer options?

### **Novelty Seeking in Psychology**

The preference for newer options in the absence of underlying value differences has been of interest to psychologists since as early as Berlyne (1966). In this parallel psychology literature on novelty seeking, novelty is defined by unfamiliarity or the lack of exposure to stimuli (Förster, Liberman, & Shapira, 2009). In a typical psychological

study of novelty seeking, participants exposed to a variety of stimuli (e.g., images, patterns, or products) that vary in terms of their degree of prior exposure will tend to choose the newer, less familiar stimuli. This preference for new and unfamiliar stimuli is a fundamental drive in humans and many other foraging species (Mesulam, 1998), one that is identifiable at a neurological level (Krebs, Schott, Schütze, & Düzel, 2009; Wittmann, Daw, Seymour, & Dolan, 2008).

Although this type of basic novelty seeking seems fairly general, it does not make a prediction when people face a choice between two equally familiar options, a common occurrence in everyday life. For example, it does not make a prediction when consumers choose between the same product produced on different dates, or between two novel products both of which they have not seen before. When people have equal or no exposure to the choice options, could they still prefer a newer option based on external chronological cues?

In this paper, we propose to fill a gap in the research on novelty in marketing and psychology by understanding how newness *per se* affects consumer judgments and choices. Although people could choose newer options for a variety of reasons, our current research isolates people's preference for newer options even when there are no rational reasons to do so and in the absence of familiarity differences. By doing so, we identify a basic pattern of human preferences. We expand on this hypothesis and its underlying process below.



## **Preferring Newness Per Se**

In the present research, we propose that preferring chronologically newer options is driven by a heuristic process based on an overgeneralized strong default association people form between “newer” and “better.” This association exists because of its ecological validity and is learned from a lifetime of exposure to ever-improving consumer goods (Gigerenzer & Gaissmaier, 2011). Newer products tend to incorporate the latest technology and designs, are manufactured with the latest methods, and improve upon shortcomings in previous versions. In some domains, newer products are also fresher (e.g., food), trendier (e.g., music), and more informative and relevant (e.g., news). After many repeated exposures to newer products of ever improving quality, people start to more generally associate “newer” with “better,” which in turn becomes a heuristic people automatically use in daily lives.

Specifically, we hypothesize that chronological newness cues (e.g., production date or release date) can bias people towards choosing newer options over older ones. The newer option does not need to be newer in any meaningful way or less familiar than the older option.

**H1:** People will prefer newer options to older options, all else equal, based on the default association of “newer” with “better.”

Since we expect this type of preference for newness to manifest as a heuristic process, we can also generate hypotheses about the underlying choice process. For example, because heuristics are effort-saving strategies that simplify and expedite information processing (Gigerenzer & Gaissmaier, 2011; Shah & Oppenheimer, 2008),

people who choose based on this heuristic process will generally make their decisions faster than those who do not rely on it.

**H2a:** People who choose newer options will generally make faster decisions than those who choose the older options.

From a dual-process perspective (Kahneman, 2011), attention depletion techniques such as cognitive load and time pressure will hamper people's System 2 while leaving System 1 largely unaffected (Brandstätter, Lengfelder, & Gollwitzer, 2001; De Neys, 2006; Eidelman & Crandall, 2014; Ferreira, Garcia-Marques, Sherman, & Sherman, 2006; Shiv & Fedorikhin, 1999). On the other hand, if people use a mix of System 1 and System 2 processes, depleting attention could shift those who use System 2 to System 1 (Evans, 2008). The result is that heuristic biases could be accentuated (Evans & Curtis-Holmes, 2005; Roberts & Newton, 2001). More specifically, if there is a non-zero proportion of people using a deliberation strategy in our studies, we would observe that cognitive load or time pressure would make the newness bias larger. If instead most people adopted a heuristic process or just choose randomly, as we hypothesize, we would observe that cognitive load and time pressure manipulation will leave the bias size largely unchanged. Based on our heuristic process hypothesis, we put forth the following:

**H2b:** The preference for newer options will be unaffected by cognitive load or time pressure.

On the other hand, judgment based on heuristic, System 1 processes can often be modified or overridden by deliberate System 2 operation (De Neys, 2006; Kahneman,

2003). Reflection and effort (Devine, 1989; Gilbert & Osborne, 1989) can overcome or correct the initial heuristic response by uncovering information inconsistent with the output of heuristic process. For example, deliberation may lead to the realization that there is no difference in expected benefit between the two options. As such, we expect that deliberation will attenuate the preference toward newer options.

**H2c:** Preference toward newer options will attenuate after deliberation.

Because this heuristic process relies on an overgeneralized association between newer and better, we should be able to disrupt the default association by activating an opposing association. In fact, there are already naturally occurring domains in which this association is active, such as domains in which older options are more valued such as artwork, wine, whiskey, and academics.

**H3:** In domains where older is associated with better, people's preference for newer options will be attenuated because the default association is disrupted.

Finally, heuristic activation depends on appropriate triggering of cues (Ferreira et al., 2006). Because the effect is a heuristic process, if the chronological information is not straightforward and requires deliberation to perceive, an automatic heuristic process may not be triggered. Furthermore, since perceived newness drives the effect, the extent of perceived newness (e.g., one year newer vs. one day newer) can affect the strength of people's preference toward the newer option.

**H4:** Easiness to perceive newness and the extent of newness perceived determines the existence as well as the magnitude of the newness effect.

### ***OVERVIEW OF RESEARCH***

In ten studies, we show that, when choosing between newer and older options with identical quality and no difference in prior familiarity, people prefer newer options across a range of domains (studies 1-8). The bias for newer options persists even for incentivized choices between newer and older die rolls (study 2) and coin tosses (study 3), where newness clearly cannot affect outcomes. We provide convergent process evidence that the newness bias is driven by a heuristic process: People choosing newer options spend less time deciding than people choosing older options (studies 1), and newness bias is not affected by cognitive load (study 4) or time pressure but is attenuated by deliberation (studies 5 and 6). In domains where people associate older with better (e.g., classic artwork), the preference for newness disappears (study 6). If people need to deliberate in order to access the newness information, the newness effect disappears (study 7). We further confirm that the extent of perceived newness drives the magnitude of the effect, and identify feelings of freshness and relevance as two mediators (study 8). Finally, we show that people are willing to pay more for newness in both a hypothetical scenario (study 9) and an incentive compatible scenario (study 10).

## ***STUDY 1: PREFERENCE FOR GEORGE CARLIN JOKES***

Many consumer domains feature products that do not appreciably improve in quality over time, such as jokes, at least not over short periods such as days or weeks. We therefore chose the joke domain to provide a first test of our main hypothesis. To exclude other inferences about newer options being trendier or having been heard by fewer people, our design used randomly selected jokes from a comedian who is no longer telling new jokes.

### **Method**

In study 1, 265 undergraduates at a large public university read “George Carlin was an American stand-up comedian and is ranked No.1 on The Funniest Stand Up Comedians of All Time on Ranker.com.” Importantly, they also learned that he died on June 22, 2008. Then, they read, “We have saved a randomly chosen joke in a password-protected computer every day for two weeks.”

After reading the choice instructions, participants saw a separate screen where they learned their choice was between today’s joke and yesterday’s joke. By separating the instruction-reading stage and decision-making stage, we could more accurately measure the time participants spent making their decisions. The order of the choice options was randomized in this study and in all remaining studies. Next, participants self-reported why they made the choice they made and read their chosen joke. Finally, participants reported whether they had previously heard this joke and their familiarity with George Carlin’s humor and completed a 5-item faith in intuition scale (Epstein,

Pacini, Denes-Raj, & Heier, 1996; Godek & Murray, 2008). We expected participants high on faith in intuition to rely more on heuristics (Epstein et al., 1996; Levin, Gaeth, Schreiber, & Lauriola, 2002).

## **Results**

Overall, 80% of participants chose to read today's joke over yesterday's, a proportion much higher than chance ( $\chi^2(1) = 94.20, p < 0.001$ ), suggesting that people prefer the newer option. Importantly, participants who chose the newer option spent less time deciding ( $M = 4.23$  seconds) than participants who chose the older option ( $M = 6.03, t(263) = 4.83, p < 0.001$ ), consistent with choosing newer options using a heuristic process. Results remain unchanged when we log-transformed the time data.

As a point of comparison, we also examined the effect of option order. Of the participants who saw the older option (yesterday's joke) on top, 75.2% chose the newer option, whereas 84.6% chose the newer option in the opposite order. Although participants showed a marginal preference toward the option that appeared on top ( $\chi^2(1) = 3.07, p < 0.10$ ), this bias was dwarfed by the bias for the newer option. This dominated order effect was consistent across all studies.

*Faith in intuition.* Next, we examined participants' faith in intuition ( $\alpha = 0.78$ ) to see whether it predicted greater heuristic usage and therefore greater newness bias. Indeed, participants with higher faith in intuition spent less time choosing ( $r = -.14, p < .05$ ), which in turn led to a higher proportion choosing the newer option ( $r = -.29, p < .05$ ). We conducted a mediation analysis of the effect of faith in intuition on choice via

decision time using 10,000 bootstrapped samples (Preacher & Hayes, 2008) and found a significant mediation (indirect effect = 0.024, biased corrected 95% CI = 0.006 to 0.062). That is, participants' faith in intuition affected their choices by reducing their decision times

### **Discussion**

A large majority of participants chose to read the newer joke despite there being no rational reason to believe that it is any more entertaining or relevant. Both days' jokes were randomly selected from the same static pool of jokes, so they had equal expected quality, and the newer joke is not newer in a meaningful way. Moreover, these were old jokes from a famous but dead comedian, so all jokes had already been heard by countless people. Therefore, the bias toward the newer joke was unlikely to have been driven by a rationally justified inference of it being funnier, trendier, or more socially relevant. Instead, the response time and faith in intuition results suggested that people's bias toward newer options was driven by a fast, heuristic process.

Note that all participants in this and all subsequent studies knew that the "today" option has already being picked or occurred when they were making choices, so indeterminacy (Vosgerau, Wertenbroch, & Carmon, 2006) cannot explain the newness effect.

## ***STUDY 2: DIE ROLLS***

In study 1, we designed a scenario designed to eliminate any quality differences between older and newer options, but a large majority of participants nonetheless expressed a preference for the newer option. Next, we provide an even stronger test using a domain in which newness transparently cannot affect the options.

### **Method**

In study 2, we used a domain in which chronological time is transparently unrelated to expectations about the choice options: betting on the results of die rolls. We adopted a  $2 \times 2$  between-subjects design, in which the first factor is a randomly selected betting rule (bigger or smaller roll wins) and the second factor is the order of betting and prediction questions.

Two hundred and four Amazon Mechanical Turk (MTurk) participants were paid \$0.08 to play a real bet in which winners received an additional bonus payment of \$0.08. They read, “Our lab has launched a dice game. Every morning the same staff member will roll a die and record its result.” After reading the instructions, participants determined whether the bigger or smaller die would win based on the results self-reported random coin flip. We randomized the winning rule to make it clear to participants that we were not fixing the die roll results. Next, participants bet on the result of a die rolled today versus exactly 1 week ago. They also made predictions of which result is more likely to be smaller or bigger depending on the rule determined earlier. The order of betting and predicting results was randomized so that half of the participants placed their



bet first and the other half predicted the result first. Finally, all participants learned that the die landed on five today and landed on two 1 week ago.

## **Results**

*Choices.* Overall, 69.6% of participants chose to bet on the newer die roll ( $\chi^2(1) = 30.59, p < 0.001$ ). Among the 94 participants (46.1%) playing for “larger die wins,” 75.5% chose to bet on today’s die ( $\chi^2(1) = 23.5, p < 0.001$ ). Among the 110 participants (53.9%) playing for “smaller die wins,” 65.0% chose to bet on today’s die ( $\chi^2(1) = 8.73, p < 0.01$ ). The difference between 75.5% and 65.0% was not significant ( $\chi^2(1) = 2.40, NS$ ).

*Predictions.* Recall that participants also predicted which die roll would be larger or would be smaller, either before or after they chose which die roll to bet on. Predictions did not differ between smaller and larger roll win conditions ( $\chi^2(1) = 3.43, NS$ ) and were consistent with choices. The majority of participants playing for larger die wins predicted that today’s die is more likely to be larger (76.6%,  $\chi^2(1) = 25.54, p < 0.001$ ). Similarly, the majority of participants playing for smaller die wins predicted that today’s die is more likely to be smaller (63.6%,  $\chi^2(1) = 7.65, p < 0.01$ ).

The order of prediction and choice questions did not significantly affect predictions (“larger wins” participants: 79.2% (choice/prediction) vs. 73.2% (prediction/choice),  $\chi^2(1) = 0.20, NS$ ; “smaller wins” participants: 57.4% (choice/prediction) vs. 69.6% (prediction/ choice),  $\chi^2(1) = 1.78, NS$ ). Looking only at participants that made predictions before choosing, the majority still predicted that the

newer die roll would result in the better result (“larger wins” participants 73.2%,  $\chi^2(1) = 7.90$ ,  $p < 0.01$ ; “smaller wins” participants 69.6%,  $\chi^2(1) = 7.88$ ,  $p < 0.01$ ).

Finally, a logistic regression of participants’ choices on betting rule (smaller vs. larger roll wins), the order of the choice and prediction questions, and the order of choice options (today on top or bottom) showed that none of these variables explained people’s choices. In sum, this evidence suggests a direct association between newer and better<sup>1</sup>.

## Discussion

Despite the fact that the expected value and degree of uncertainty for the die roll from this morning and for the roll from one week ago are statistically identical, and regardless of whether smaller die or larger die wins, participants showed a preference for a newer die roll and predicted that the newer die roll is more likely to win. Since participants randomly chose their own betting rules, it is very unlikely for them to think the older roll was more likely to be rigged due to a longer existence<sup>2</sup>. Furthermore, 53.9% of the 204 participants (difference from 50%:  $\chi^2(1) = 1.1$ , *NS*) actually won the bet and were paid a bonus, so there was no evidence of cheating among participants.

---

<sup>1</sup> In a different MTurk study (N = 300) we randomized participants into three groups (free choice, forced new option, forced old option) and used similar materials in Study 1. Regardless of whether the joke they read was forced or self-selected, and regardless of whether it was labelled a newer joke or an older joke, participants rated the same joke equally funny after they read it (post-consumption). The two studies, taken together, showed that people’s prediction and post-consumption rating were inconsistent.

<sup>2</sup> There is still some chance that participants would think that the die rolls are rigged after they made the betting rule but before we revealed the result. However, there is no reason to suspect that the experimenters were more likely to rig the newer die to win than vice versa. In addition, none of the reported reasons indicated the possibility of cheating as their reason of choice.

### ***STUDY 3: COIN TOSSES***

Study 3 used a similar design as study 2 but using coin tosses. We removed the need to compare the results of the two coin tosses. If the coin tossed on the chosen day landed heads, participants won a bonus payment of \$0.25 in addition to their \$0.25 participation fee.

#### **Methods**

In a between-subjects design, 186 MTurk participants chose between two tosses of a fair coin. Half were assigned to choose between coin tosses this morning versus yesterday morning, and the other half choose between coin tosses yesterday morning versus the morning of the day before yesterday (i.e., 2 days ago). This latter condition allowed us to test whether the newness bias is limited to options related to today. As per the instructions, the experimenter tossed a coin for three consecutive days (landing heads, heads, and tails) and rewarded the winners accordingly.

#### **Results and Discussion**

Overall, 72% of participants chose to bet on the newer coin toss ( $\chi^2(1) = 35.27, p < 0.001$ ). This was also true in each condition: 73.1% of participants ( $\chi^2(1) = 18.97, p < 0.001$ ) chose the coin toss from this morning over the coin toss from yesterday morning, and 71.0% of participants ( $\chi^2(1) = 15.53, p < 0.001$ ) chose the coin toss from yesterday morning versus over the coin toss from the morning of the day before yesterday. There was no difference between the two proportions ( $\chi^2(1) = 0.027, NS$ ). This study shows that newness bias is not limited to preferring today's option, thus ruling out explanations

based on the match between choices that occur today and choosing something that happened today.

#### ***STUDY 4: COGNITIVE LOAD***

The first three studies provided strong empirical support for the existence of bias toward chronologically newer options as well as results consistent with a heuristic process explanation. We more directly test this heuristic process in the next three studies by manipulating people's ability to use deliberate System 2 processes, such as by placing them under cognitive load. According to the dual-process approach, when people are under high (vs. low) cognitive load, the depletion of the attentional resources is expected to decrease System 2 processing but to leave System 1 estimates largely invariant (Ferreira et al., 2006).

#### **Method**

Study 4 replicated the die roll procedure in study 2 while manipulating cognitive load. The study was the first of a series of laboratory studies in which 329 undergraduate business students completed 45 minutes of studies in exchange for course credit. We used a standard cognitive load manipulation (Gilbert, Giesler, & Morris, 1995; Shiv & Fedorikhin, 1999) by requiring students to first memorize a letter string and then immediately recall the string after making their choice. In the high cognitive load condition, participants were instructed to memorize a nine-character string, "GXNTDPLRW," whereas participants in the low cognitive load condition only needed

to memorize a three-character string, “GXN.” (Kessler & Meier, 2014; Van't Veer, Stel, & Van Beest, 2014).

After seeing the letter string, participants read the same cover story used in study 2. They were also told that all winners would be entered into a lottery in which we would randomly selected three winners to win \$5. In the next screen, they made their choice between betting on “the die result from this morning” or “the die result from one week ago.” In the third screen, participants were prompted to recall the sequence and type it in a blank box. The three randomly chosen winning participants were rewarded two weeks later in class.

### **Results and Discussion**

We again found that participants preferred the newer option overall (79%,  $\chi^2(1) = 109.73, p < 0.001$ ). More importantly, we found no difference in preference between participants choosing under high load (79%,  $\chi^2(1) = 53.39, p < 0.001$ ) and those choosing under low load (79%,  $\chi^2(1) = 55.19, p < 0.001$ ). The fact that the preference for newer options is not affected by cognitive load suggests that is likely to be the result of a relatively effortless heuristic process (Shah & Oppenheimer, 2008). Note that these results remained unchanged if we only analyzed the participants who correctly reported the strings.

## ***STUDY 5: TIME PRESSURE AND DELIBERATION***

Our next study further tests the heuristic hypothesis using two different manipulations that affect cognitive resources in opposite ways: deliberation and time pressure. If reliance on heuristic processes can be moderated by rational thinking, then making people deliberate should attenuate the preference for newer options. On the other hand, pressuring people to make decisions faster should have no effect, consistent with the cognitive load results.

### **Method**

Study 5 used the same cover story as in study 1, with 301 MTurk participants choosing one of two random selected George Carlin jokes to read. To increase the plausibility of the scenario, participants read that the randomly selected joke was sent to a private mailing list to pay tribute to George Carlin (as opposed to saved in a password-protected computer as in study 1). Participants were randomly assigned to one of three conditions: time pressure, deliberation, and control.

In the time pressure condition, participants were told that they should make their decision as rapidly as possible (Dror, Basola, & Busemeyer, 1999; McConnell & Leibold, 2001) and that they would have only 5 seconds (Dhar & Nowlis, 1999) to make their decision on the next screen before the Qualtrics survey automatically advanced. On the choice screen, they were asked to choose between “today’s joke” and “yesterday’s joke.” A 5-second countdown timer was also shown on the same screen (Dhar & Nowlis, 1999;

Pieters & Warlop, 1999) and the screen automatically advanced when the timer reached zero.

In the deliberation condition, participants were told that they had an unlimited amount of time to choose (Dhar & Nowlis, 1999) and were encouraged to think carefully before making their decision on the next screen. On the choice screen, we further encouraged participants to think longer (Dijksterhuis, 2004) by showing the submit button only after 15 seconds had elapsed. In the control condition, participants were just instructed to make a choice between the two options on the next screen and the choice screen did not contain a timer.

### **Results and Discussion**

In both the time pressure (80.6%,  $\chi^2(1) = 35.52, p < 0.001$ ) and control conditions (79.2%,  $\chi^2(1) = 33.31, p < 0.001$ ), we replicated the results in study 1 that the majority of participants chose the newer joke. There was no difference between the two proportions ( $\chi^2(1) = 0.005, NS$ ). However, in the deliberation condition, only 64.7% of the participants ( $\chi^2(1) = 8.25, p < 0.01$ ) chose to read today's joke. While 64.7% is still higher than chance, it is significantly lower than the proportion choosing the newer joke in the time pressure ( $\chi^2(1) = 5.57, p < 0.05$ ) and control conditions ( $\chi^2(1) = 4.59, p < 0.05$ ).

In this study, we have successfully attenuated people's preference for newer options by asking them to deliberate before making choices. However, even with deliberation, people still showed some newness bias. In our next study, we tried to

eliminate the newness bias by introducing a different domain that activated an opposing association between older and better.

### ***STUDY 6: MOVIES VS. CLASSIC ART DOMAINS***

Studies 4 and 5 showed evidence that the bias toward chronologically newer options is driven by a heuristic process. We designed study 6 to test that the heuristic process is due to the association between newer and better, by showing that choices in domains where the opposite association is salient will show attenuated newness bias or even the opposite bias. In domains where older products are regarded as better (e.g., red wines, many liquors, antiques, and classic artwork), this opposite association could interfere with the default association between newer and better. Furthermore, the differences in newness bias between domains should be narrowed or eliminated with deliberation but unaffected by time pressure.

#### **Method**

A total of 561 MTurk participants chose between two posters in a 2 (domain: movie vs. art)  $\times$  3 (decision time: time pressure vs. deliberation vs. control) between-subjects design. Participants read, "Our lab is giving away some 24" x 36" (2ft x 3ft) posters of recent blockbuster movies [*famous pieces of classic art*] left over from a different study. Each day of the past week, we randomly designated one poster to give away. We will give all five posters away today. One random participant will be selected to have their chosen poster sent to him or her for real." The decision time manipulations were the same as used in study 5.



Participants, all of whom completed this study on a Saturday, then chose between Wednesday's poster and Thursday's poster in the next screen. No further information was provided about either poster.

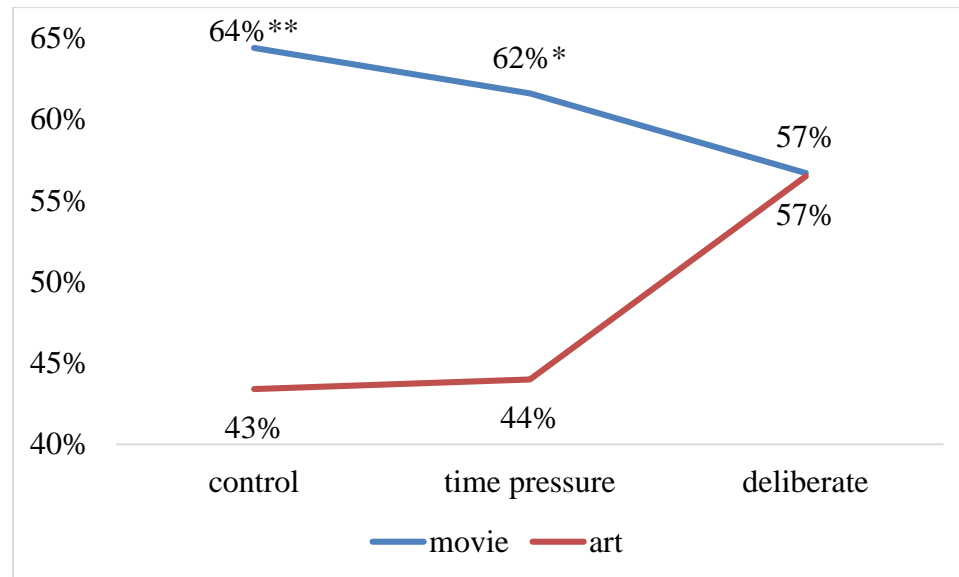
## Results

Figure 1 shows the percentage of participants who chose the newer (Thursday's) poster. For participants choosing between movie posters, those in the time pressure (61.6%,  $\chi^2(1) = 4.89$ ,  $p < 0.05$ ) and control conditions (64.4%,  $\chi^2(1) = 6.94$ ,  $p < 0.01$ ) showed a significant preference for the newer option, whereas the preference was attenuated in the deliberation condition (56.7%,  $\chi^2(1) = 1.34$ , *NS*). In comparison, participants choosing between classic art posters did not show a preference for newer options in any condition (Time Pressure: 44%,  $\chi^2(1) = 1.10$ , *NS*; Deliberation: 56.5%,  $\chi^2(1) = 1.32$ , *NS*; Control: 43.4%,  $\chi^2(1) = 1.45$ , *NS*).

A logistic regression of choices on domain, group, and the domain  $\times$  group interaction yielded a significant effect of domain ( $\beta = 0.86$ ,  $z = 2.87$ ,  $p < 0.01$ ) and a domain  $\times$  deliberation interaction ( $\beta = -0.85$ ,  $z = -2.02$ ,  $p < 0.05$ ). Further analysis showed that the domain difference in choice proportions was significant in both the time pressure (61.6% vs. 44%,  $\chi^2(1) = 4.89$ ,  $p < 0.05$ ) and control conditions (64.4% vs. 43.4%,  $\chi^2(1) = 7.54$ ,  $p < 0.01$ ) but was eliminated when participants were asked to deliberate (56.7% vs. 56.5%,  $\chi^2(1) = 0$ , *NS*).

Figure 1 Percentage of participants choosing the newer (Thursday's) poster

\*\*  $p < .01$ , \*  $p < .05$



## Discussion

Studies 5 and 6 provided further support for our proposed heuristic hypothesis. Although explicit instructions to deliberate attenuated the preference for newer options, increasing reliance on intuitive thinking by putting people under time pressure did not increase the preference for newer. Considering that Thursday was already two days ago from the day the participants made their choice, the fact that we still found a preference for newer options further increases our confidence that this effect is not limited to today or yesterday.

More importantly, we have identified an important boundary condition. Newness bias can be attenuated or perhaps even eliminated for domains where older products are valued, such as classic art, perhaps because people generally associate older art with

greater value (Eidelman, Pattershall, & Crandall, 2010). This difference between domains provides support for our assertion that the newness bias is driven by learned associations. Activating an opposing association between older and better disrupts the default association, although not enough to reverse the bias in this case. Notably, the domain difference found in the control and time pressure conditions was completely absent when participants were asked to deliberate before making their choice. That is, when participants were explicitly told to not rely on heuristic processes to make their choice, having different associations activated no longer affected choices.

According to our newness hypothesis, people's choice patterns are driven by newness itself. Therefore, if we make the newness cue less salient, the bias for newer options should attenuate. Past research showed that for identical options, different framing could lead to different choice patterns (Tversky & Kahneman, 1981). In our next study, we tried to use different framings of the same choice options to change people's choice behavior.

### ***STUDY 7: DAY VERSUS ORDER FRAMING***

In our previous studies, the older option is always drawn first and the newer option drawn second. Leveraging this fact, we adopted an order versus date framing manipulation to gain further insight into the newness effect.

## Method

Three hundred and twenty-eight MTurk participants learned that they would choose between two “daily crossword puzzles” from a crossword website and that “Each puzzle was **randomly** drawn from a pool of crosswords published between 2010 to 2015.” Importantly, all participants read that “The first puzzle was drawn the day before yesterday; the second puzzle was drawn yesterday.”

Half of the participants were prompted to select between “the day before yesterday’s puzzle” and “yesterday’s puzzle”; the rest were prompted to select between “the first puzzle” and “the second puzzle.” Although the framings are different, the information conveyed is equivalent. That is, “the day before yesterday’s puzzle” *is* “the first puzzle”, while “yesterday’s puzzle” *is* “the second puzzle.” For the students in the order condition, they were prompted to answer a manipulation check question to indicate which day it was (yesterday or the day before yesterday) for their chosen crossword.

## Results and Discussion

Consistent with previous studies, out of the 175 participants who chose between “the day before yesterday’s puzzle” and “yesterday’s puzzle,” the majority chose the newer option (78.3%,  $\chi^2(1) = 54.88, p < 0.001$ ). More importantly, as predicted, for the 153 participants who chose between the “the first puzzle” and “the second puzzle,” there was no newness bias (52.9%,  $\chi^2(1) = 0.42, NS$ ). The difference in proportions between the two groups is significant ( $\chi^2(1) = 22.40, p < 0.001$ ).

Among the 153 participants in the order condition, 115 answered the manipulation check question correctly, and 67 chose the newer option (58.3%,  $\chi^2(1) = 2.82$ , *NS*). The difference between day condition (78.3%) and order condition (58.3%) remains significant ( $\chi^2(1) = 12.40$ ,  $p < 0.001$ ).

In this study, we have successfully turned off the newness effect through framing. In order for the new newness effect to occur, people need to gain access to the newness cue. When the same information was framed as “first versus second” as opposed to “yesterday versus the day before yesterday,” the newness cue became less salient and thus had less impact on choices. Although the information contained in the two framings was equivalent, participants’ ease of perceiving the newness was not. With deliberation, participants should be able recognize that the second option is indeed the newer option. However, due to the nature of how heuristics work, and consistent with our empirical finding, participants generally did not capture the difference in newness when the options were framed in order instead of days.

Although the newness effect is a heuristic process that happens quickly, we still wonder what mediators could lie between the newness manipulation and people’s choice during this quick process. In our next study, we further decipher the process underlying this effect.

## ***STUDY 8 CROSSWORD PUZZLE: EXPLORING MEDIATORS***

In addition to test mediation, we adopted a more nuanced framing manipulation in this study to further explore how the newness effect works. From study 7, we know that the easiness to perceive newness is essential for this effect to occur. However, for a dyad of options that are both easy enough to identify which one is newer and which one is older, could there be a framing effect?

Past research found that people made different inferences when the same time span is expressed with different levels of granularity (e.g., one year or 365 days) (Zhang & Schwarz, 2012). LeBoeuf (2006) found that description in terms of time or days can have different effect on people's discount rates. In our current context, we hypothesized that the newness effect from options framed in weekdays (e.g., yesterday versus the day before yesterday) would be stronger than the options framed in dates (e.g., October 13<sup>th</sup> versus October 12<sup>th</sup>). This difference occurs due to the fact that one day's is 1/7 of a natural unit for time keeping (week) and 1/30 of another natural unit (month), so the extent of newness perceived from week days is more than that perceived from monthly dates. As a result, the newer option framed in the former way would seem to be even more newer. In addition to the framing manipulation, we inserted five potential mediators to make an attempt to gain further insights of the psychological mechanism.

### **Methods**

Study 8 used a 2 (mediator before vs. after choice)  $\times$  2 (newness manipulation: date vs. day) between-subjects design, asking students to pick up a crossword puzzle

between two randomly selected crossword puzzles from a static pool of crossword puzzles. We also tested five potential mediators: relevance, interest, excitement, freshness, and difficulty.

Two hundred and sixty-one students were given the same instruction as in study 7 in a lab setting. After the instructions, half the students were asked to make a choice and then answer the mediator questions, while the other half students were asked to answer the mediator questions before making a choice. We ask students to “evaluate their feelings about the two crossword puzzles” on a five-point bipolar scale, from “the crossword puzzle from the day before yesterday feels more relevant/interesting/exciting/fresh/difficulty” to “the crossword puzzle from today feels more relevant/interesting/exciting/fresh/difficulty.”

When prompted to make a choice, half the students were given the choice between puzzles from “yesterday” and “the day before yesterday,” while the other half were given the choice between the exact two crossword puzzles framed in dates (e.g., Oct 13<sup>th</sup> and Oct 12<sup>th</sup>).

## **Results**

Consistent with previous studies, there is a significant preference for newer options overall (69.7%,  $\chi^2(1) = 39.86$ ,  $p < 0.001$ ). Importantly, we found the predicted difference between the day and the date group: Among the 131 students who chose between yesterday’s and the day before yesterday’s puzzle, 76.3% ( $\chi^2(1) = 35.30$ ,  $p < 0.001$ ) chose the newer option, whereas only 63.1% ( $\chi^2(1) = 8.38$ ,  $p < 0.01$ ) of the 130

students in the date group chose the newer option. The between-group difference in proportions is significant ( $\chi^2(1) = 4.82, p < 0.05$ ).

### *Mediation*

We conducted five mediation analyses of the effect of newness manipulation via five mediators using 10,000 bootstrapped samples. Only relevance (indirect effect = 0.04, 95% CI = 0.01 to 0.08) and freshness (indirect effect = 0.04, 95% CI = 0.01 to 0.07) showed significant mediation, while interest, excitement, and difficulty did not. Because relevance and freshness were highly correlated, we needed to include both mediators in one model to get an accurate total mediation effect estimation. We used Structural Equation Modeling to model the two mediators simultaneously. We identified the sum of indirect effects was 0.047 (95% CI = 0.012 to 0.083).

### **Discussion**

In this study, in addition to the main newness effect, we found that when the same option is framed in dates instead of weekdays, the newness effect was attenuated. From both study 7 and study 8, we can see that the ease of perceiving the newness information and the extent of newness perceived matter in people's choices. The two studies further supported our main hypothesis that the effect was driven by newness per se, instead of other alternative accounts.

Our mediation analysis further shed light on the mechanism of how the mere newness effect works. Out of the 13.3% proportional difference between the date and day group, 4.7% was explained by the mediation, with relevance and freshness explaining 2.9%



and 1.8% respectively. In other words, the indirect (mediation) effect explained 35.3% of the total effect.

One potential weakness of our studies so far is that people always made forced choices between two options, with the only difference between the options being newness. Are people willing to pay for such newness? The next two studies measure people's willingness to pay for newer raffle tickets (Study 9) and books of the month (Study 10).

### ***STUDY 9 WILLINGNESS TO PAY***

#### **Method**

Study 10 adopted a 2 (chance, between-subjects factor)  $\times$  5 (ticket purchase date, within-subjects factor) mixed design. Two hundred and sixty-five undergraduate students reported their willingness to pay (WTP) for each of five identical raffle tickets differing only on when the ticket was purchased (one year ago, one month ago, one week ago, yesterday, or today) all for a raffle to be drawn next week. Importantly, the order of the five ticket dates was randomized within participant and WTP was asked on five separate screens.

Participants read the following scenario:

Imagine that every morning in the last year (366 total, including today), we have purchased a raffle ticket and put it directly into my file cabinet. They are all for the same raffle in which 1 ticket [100 tickets] will be drawn to win \$1000. The drawing for this raffle is

next week and each raffle ticket has the same chance of being chosen in the drawing. Now, imagine that we give you the opportunity to buy one of these raffle tickets. What is the most you would be willing to pay for the raffle ticket we purchased [one year ago/one month ago/one week ago/yesterday/today]?

## Results

Average WTP for the raffle tickets from oldest to newest is shown in Figure 2. Participants' WTP were submitted to a 2 (between-subjects factor: chance)  $\times$  5 (within-subjects factor: ticket purchase date) repeated measure ANOVA. Not surprisingly, more chances to win led to higher WTP ( $F(1, 263) = 8.06, p < 0.01$ ). There was also a significant effect of ticket purchase date on WTP ( $F(4, 1052) = 9.14, p < 0.001$ ), but no interaction effect between chances to win and ticket purchase date ( $F(4, 1052) = 1.80, NS$ ).

If a newer raffle ticket can sell at a premium over an older one, we would expect an upward trend of WTP as the purchase date draws nearer. We tested contrasts in a repeated-measure design by constructing a Lambda Score ( $L$ ) for every participant. The  $L$  score reflects the degree to which a particular participant's repeated measures are consistent with the prediction made in terms of the lambda weights defining it (Rosenthal & Rosnow, 1991)(Rosenthal and Rosnow 1991). To address the question of the degree to which the scores of this sample support an upward linear trend (defined by lambda weights of -2, -1, 0, 1, 2), we computed a one sample  $t$ -test for both low chance ( $t(132) =$

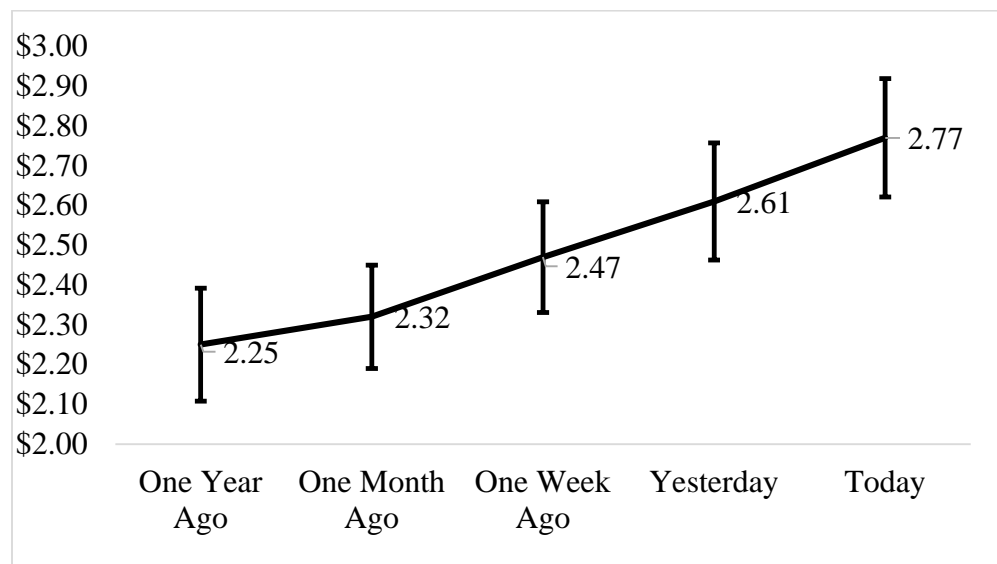
2.38,  $p < 0.05$ ) and high chance groups ( $t(131) = 3.31$ ,  $p < 0.01$ ). Both groups showed a significant linear trend with no difference between groups ( $t(263) = -0.84$ , *NS*).

Next, we analyzed the WTP data for only the first date that each participant saw, which should be unaffected by comparison with other dates. The average WTP in the chronological order of raffle drawing date is 2.18, 2.28, 2.90, 3.00, and 2.53. An ANOVA of WTP on time yielded a non-significant result ( $F(4, 260) = 1.37$ , *NS*).

In order to validate our finding, we tested another Amazon Mechanical Turk sample of 505 participants with the same within-subjects manipulation of time but without the between-subjects manipulation of chance. We found the same pattern that there is a linear trend of WTP within subjects ( $t(504) = 2.63$ ,  $p < 0.01$ ) but not between subjects ( $F(4, 500) = 1.01$ , *NS*).

Figure 2 Average WTP on different raffle ticket purchase dates.

Note: Error bars denote  $\pm$  one standard error of the mean.



## **Discussion**

Even though we told participants explicitly that every raffle ticket was the same and had the same chance to be drawn, they were still willing to pay a premium for a newly drawn raffle ticket over an older one. Our results showed that preferring newness *per se* can lead to a material difference in WTP. We also found that people's newness bias translates into higher WTP only in our within-subjects manipulation but not between-subjects manipulation. This suggests a boundary condition for the newness bias is that it will occur in joint evaluation than in separate evaluation (Hsee, 1996), for chronological newness cues are relative (one week ago is newer than one month ago, but older than today). Without a comparison, it is difficult for participants to get a sense of newness (e.g., how new or old is a 1 week-old raffle ticket?).

Study 10 showed that mere newness can lead to substantive consequences such as higher willingness to pay, but the choices were hypothetical. In our next study, we asked participants to bid for a real physical book and potentially pay real money.

### ***STUDY 10 BOOK BIDDING***

#### **Method**

Two-hundred and thirty undergraduate students read the following cover story:

Every month, the website Goodreads generates an automatic “Book of the Month” for each genre by **randomly** selecting from the top 100 books in each genre as rated by their users, not including newer books from the last 2 calendar years (2015 and 2016).

In this study, we are giving you the opportunity to bid on two highly-rated Business books: the "Book of the Month" randomly selected **last month** and the "Book of the Month" randomly selected **this month**. **We will not reveal the titles of these books until after the study.**

Participants read instructions about the bidding procedure (see Appendix 1), which used the Becker–DeGroot–Marschak method (Becker, DeGroot, & Marschak, 1964), an incentive compatible procedure to measure willingness to pay (Wang, Venkatesh, & Chatterjee, 2007). They were then told that three students would be randomly selected to play out their bids for real.

All students acknowledged that they fully understood the instruction before leaving their email addresses. In the following bidding screen, they were required to move a slider to indicate their willingness to pay for the “Book of the Month” for this month and the “Book of Month” for last month ranging from \$0 to \$20. The order of books was randomized. Finally, as a manipulation check, participants were asked to judge whether the statement is true: *The "Book of the Month" for this month is likely to be published more recently than the "Book of the Month" for last month.*

### **Results and Discussion**

On average participants bid \$9.33 for this month’s book of the month, significantly more than the average bid of \$8.60 for last month’s book,  $t(229) = 3.99$ ,  $p < 0.001$ . Next, we divided participants into two groups based on their answer of the manipulation question. For those participants ( $N = 129$ ) who thought the statement was

false, the newness premium from this month over last month was 0.33\$ ( $t(128) = 1.23$ , *NS*); for participants ( $N = 101$ ) who indicated the statement was true, the newness premium was 1.26\$ ( $t(100) = 5.26$ ,  $p < 0.001$ ).

In this study, we tried to use incentive compatible design to measure the monetary amount mere newness can be translated to. Based on our sample of 230 participants, the total premium of designating one book as this month's "Book of the Month" over previous month's was 168\$. Not surprisingly, the premium for those who inferred that more recently selected book was also published more recently was higher than those who did not make such an inference. Even when we explicitly told participants that the books were all old books (before 2015) and were randomly selected from a static pool, participants nevertheless were willing to pay for newness per se.

## ***GENERAL DISCUSSION***

Consumers are often drawn to new products for their improved design and features. Yet, newer products in many domains are often newer without really being better. For example, the latest diet fad may not necessarily be any more likely to help you lose weight than its predecessors were and newer editions of textbooks often add trivial differences. However, even in these cases where newness is not associated with greater quality, people may nonetheless show a preference for newer options. That is, people seem to prefer newer options just because they are newer. We proposed and found evidence that choosing newer options operates through heuristic processes due to overlearned generalizations of the general trend of consumer products improving with

time. For the newness effect to occur, the easiness to infer the newness information is critical, and the degree of newness inferred determines the magnitude of the effect.

In consumer products market, it is not uncommon to observe people's frenzy toward newly released products, such as mobile phones, movies, shoes and so on. For instance, new iPhone releases can always draw long lines. An Apple fan in South Korea camped four days to get a newly released iPhone 7 (Ramirez, 2016); another fan waited outside Apple store in New York for two weeks to get a newly released iPhone 6 (Bloomberg, 2014). There are many reasons behind the phenomena. In this essay, we offered one underlying reason, among others, for people's preference toward newer options in general. That is, the fact that the product is new (newness per se) is itself a motivation for people to buy it.

### **Relationship to Other Research**

It is important to distinguish how the preference for chronologically newer options is situated relative to existing research on novelty in marketing and psychology. In marketing, the well-studied constructs of consumer innovativeness and novelty-seeking focus on individual differences in consumers' tendency to seek out and adopt new products and information, whereas what we study is a more generalized preference for newer options, particularly among options with the same quality, prior exposure, and familiarity. In addition, whereas newness as studied in the prior work is generally associated with actual improvement or variety, newness here is merely derived a chronological cue with no relationship to quality.

Another major difference between the previous psychological research on novelty seeking and our work is that prior studies examined participants' choices for stimuli that varied in terms of how novel or familiar they were as a function of prior exposure (Förster, 2009; Krebs et al., 2009; Wittmann et al., 2008). That is, their participants made choices *after* exposure to the stimuli, whereas participants in our studies made choices *before* they were exposed to any stimulus, with choice options differing only on a chronological label. We have therefore shown that people seek newer options even before stimuli exposure, as is the case with many experience products such as movies. Indeed, we often observe people's frenzy about a newly released Hollywood blockbuster, and then with a few weeks or months the original frenzy went away. Preference for newness *per se* is likely to be one major reason for that.

#### *Appeal to tradition*

People preferring newness *per se* is similar to the logical fallacy of argumentum ad novitatem—arguing that something is better just because it is new. However, argumentum ad antiquitatem—appeal to tradition or antiquity—is another frequently encountered fallacy with relevance for decision making. That is, people may draw an inference that if something has been around for a long time, it must be good (e.g., traditional medicines). Consistent with this inference, just as new products advertise their newness, older products and companies often advertise their oldness (e.g., Coca-Cola's “since 1886”). Eidelman and colleagues (2010, 2014) documented a “longevity bias”: preference for things that have existed longer. The longevity bias represents people's assumptions about the goodness and rightness of existing and longstanding states of the



world (Eidelman & Crandall, 2014). The apparent contradiction between longevity bias and newness bias may be reconciled by differences in time ranges studied (today versus yesterday or 10 years versus 100 years) and the nature of the stimuli studied. Indeed, none of our studies' stimuli overlapped with theirs (Eidelman et al., 2010). This opposite finding may also be due to different framings of time versus dates (LeBoeuf, 2006). That is, in their studies to show the longevity bias, they used a time span, e.g., an acupuncture that has existed for 250 years versus 2000 years. In our studies, we used a specific time point, e.g., a die rolled today versus one week ago. Further research is needed to understand whether the two effects can both occur to the same pair of options as researchers move their manipulations gradually along the time line. If so, will there be a specific point of preference reversal?

### *Psychological distance*

One potential alternative explanation to an association-based heuristic is that newer options are psychologically less distant. If newness bias is explained by psychological distance, then one might expect that people generally prefer psychologically closer to psychologically further options. For example, we would expect that people also prefer physically less distant options. We examined this possibility in a study with 91 undergraduate students, asking them to bet on two dice rolled at different locations, one closer and one further. We did not find any preference for the closer die roll and results did not change when we controlled for participants' familiarity with and liking toward the two locations. This lack of preference for closer die rolls is consistent

with our association-based story. People have not learned that physically closer options are better than further ones.

#### *Comparison Neglect and Revision Bias*

Sela and LeBoeuf (2016) found that consumers often failed to compare the upgrade option to the status quo option, a phenomenon which they call comparison neglect. Consequently, consumers showed an elevated likelihood of upgrading. Although they did not provide direct explanation as to why comparison neglect occurred, the current essay offers one plausible reason: as the upgraded option is always the newer option, people's preference toward newness *per se* would contribute to their comparison and the elevated likelihood of upgrading. Along a similar vein, John et al. (2016) found that people prefer a revised version of products even though there is no objective quality improvement. We suspect that people's preference toward newness *per se* also contributes to the revision bias.

#### *Contamination*

Another potential explanation for people preferring newer options is that they may seem less contaminated, either literally or metaphorically. Although contamination is an important consideration for foods and even consumer retail goods (Argo, Dahl, & Morales, 2006), it is unlikely to explain our results. In particular, because feelings of contamination based on physical contact fade over time, a contamination mechanism would predict the opposite of what we found in studies 2 and 3. The newer die roll or

coin toss was more recently touched by the “staff member” so a contamination explanation would actually increase preference for the older option.

### *(un)familiarity*

This essay documents the bias for newer options. This effect is over and above people’s (un)familiarity preferences. That is, in all of our studies, familiarity towards the stimuli should be identical. The only difference is the different chronological information associated with two otherwise identical stimuli. Since most of the novelty seeking literature in psychology and marketing are studying novelty caused by familiarity differences, we are actually studying a different and independent construct from traditional novelty seeking.

Although novelty seeking is a fundamental drive in humans (Mesulam, 1998), literature has also documented a seemingly opposite human tendency, the *mere exposure* effect. Mere exposure proposes that repeated exposure to a stimulus enhances people’s attitudes toward it (Zajonc, 1968). To reconcile the opposite effects, Liao, Yeh, and Shimojo (2011) proposed that different contexts (e.g., faces vs. natural scenes) matters in inducing different effects, whereas Förster et al. (2009) also showed how both effects could occur in one context. Future research may help our understanding with comparison between the effects caused by familiarity and effect caused by mere newness.

### **Managerial Implications**

Since quality can only be fully evaluated after usage or purchase for most products or services, particularly for “experience goods” (Nelson, 1974), an

overgeneralized preference for newer options could result in suboptimal purchase decisions when firms release newer products that are either worse (e.g., Windows Vista and New Coke) or no better than older products (e.g., annual new editions of text books). Even if the newer product is actually better, the fact that people prefer newness *per se* may still cause consumers to overpay for newer products (see studies 10 and 11), a process that may contribute to the phenomenon of price skimming (Besanko & Winston, 1990).

Firms can use newness effect to their advantage. According to signaling theory, sellers can send pre-purchase quality signals to consumers when buyers and sellers possess asymmetric quality information (Boulding & Kirmani, 1993). One strategy is therefore to prominently display how new the product model is or how recently it was produced. Perhaps this is why the word “new” features so prominently in the packaging and advertisements for new products, often appearing in bold fonts and colors or as a separate label or sticker (Eelen, Verlegh, & Van den Bergh, 2015). It may also explain why manufacturers print product production dates in small font on the back of the box: A “new” label will always convey newness whereas a product with a longer expected shelf life may still be on the shelf long after its production date indicates that it is new.

Newness may even influence consumer decisions when there is nothing substantively new. Selling “old wine in new skins”—repackaging an old product or updating product appearances (e.g., “New look. Same great taste.”)—can make a product more appealing, especially if the “new” product comes with a new release date. For a few real world examples, just consider the many realtors and AirBnB users that pull and then

repost homes, media websites that update their new stories with trivial changes, and eBay merchants that repeatedly repost the same products in the new auctions.

In addition, even for identical products, managers could use different framings to affect people's preference over the newer or older option, as shown in studies 7 and 8. For instance, framing an older option as "the first of its kind" and the newer option as "the latest" may both whet people's appetites, although in different directions.

### **Future Directions**

Chronological newness information from product release or production dates is readily available for many products in the marketplace. Surprisingly, marketing research has not studied objective chronological cues, despite the extensive literatures on new product development, consumer innovativeness, and consumer novelty seeking. We believe that our findings complement these existing literatures. If such a simple cue can bias people's choices in a predictable way, further studies are needed to explore its antecedents and consequences.

Although we showed that newness *per se* can lead to a material difference in WTP in studies 9 and 10, future research should measure the willingness to pay for newness in more realistic product purchase settings or using a conjoint design. While we believe that such a robust newness effect should persist in many real world domains, it is nonetheless important to compare the strength of this preference with other product dimensions such as brand and country of origin.

Although we have identified a boundary condition of product domains that activate opposing associations between older and better, more research is needed to study other moderators. For example, it is worth exploring whether other factors may reduce or reverse people's association of newer with better, such as culture or age. It is possible that in cultures where people expect things to get better over time (such as the U.S.), the association between newer and better are more perennially activated than in cultures where people do not expect things to become better. Likewise, older people that think their best years are behind them may shy away from new products and experiences. Similarly, among depressed people who hold pessimistic views about the future, the association between newer and better may be weakened. Last but not least, we still have not yet reconciled newness effect with the existence and longevity bias (Eidelman & Crandall, 2014; Eidelman, Crandall, & Pattershall, 2009; Eidelman et al., 2010) in a unified conceptual framework. Future research on a unified framework would shed more light on people's decision making related to the time.

### ***CONCLUSION***

This essay tried to answer a basic question: Do people prefer newness *per se*? Our findings show that they do and that this newness effect is the result of heuristic processes. No matter whether it is choosing between randomly selected jokes or betting on a die roll or a coin, the majority of people chose newer options despite us eliminating rational reason to do so. They were even willing to pay a premium for newer but otherwise

identical products. People seem to intuitively fall prey to the fallacy of *argumentum ad novitatem*.

***ESSAY 2 PREPAYMENT EFFECT: PREPAYING PEOPLE INCREASES  
PARTICIPATION IN HEALTH-RELATED TASKS***

***INTRODUCTION***

Incentives play a central role in economics. A great deal of recent field research suggests that financial incentives can motivate people to do healthy behaviors, such as consumption of healthy foods (Anderson et al., 2001; Dolan, Galizzi, & Navarro-Martinez, 2015; Just & Price, 2013; Raju, Rajagopal, & Gilbride, 2010), gym attendance (Milkman, Minson, & Volpp, 2013), weight loss (Volpp et al., 2008), smoking cessation (Giné, Karlan, & Zinman, 2010; Halpern et al., 2015; Sunstein, 2015; Volpp et al., 2009), and many other preventive behaviors (Jochelson, 2007; Kane, Johnson, Town, & Butler, 2004). A general conclusion from these papers is that financial incentives are effective in facilitating task performance in the short run but not long run (Jochelson, 2007; Kane et al., 2004).

A limitation<sup>3</sup> of this prior health literature is that the participant recruitment process is usually exogenous to the incentive programs under study, or not the focus of

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<sup>3</sup> Charitable giving literature studies donation rate. For example, studies found that method of introducing lottery can increase donation rate to charity (Landry, Lange, List, Price, & Rupp, 2006); introducing default option can increase organ donation (Johnson & Goldstein, 2003). For the use of lottery in health intervention, see the review below. For the usage of default option, it is less clear as to how it can increase participation rate in health intervention. Since charitable giving (make people to pay out of their own



health prevention programs. That is, research has focused on the effectiveness of incentives among people who—by nature of their agreeing to participate in the study before the real study starts—are already willing to take on the tasks. Even when researchers accounted for the intention-to-treat effect, they used statistical techniques to get more accurate *ex post* estimate of program effect (Giné et al., 2010). What about people who did not choose to participate in the first place? How can we turn more people into participants? In this essay, participation is the variable of our major interest. We focus on increasing the initial participation rate *ex ante*.

For many tasks, such as eating healthier or saving for retirement, willingness to participate is a critical first step toward success. In fact, participation rate is arguably the biggest challenge for many tasks such as corporate wellness initiatives (Haisley, Volpp, Pellathy, & Loewenstein, 2012). This essay proposes a solution that increases participation rates without increasing per capita incentive rate. Although this essay focuses on a health-related behavioral task (study 1) and a survey task (study 2), the proposed solution can apply to any task for which we want people to participate.

### **Participation in health intervention programs and prepayment solution**

In a typical health intervention program, researchers approach eligible individuals first, recruit those who are willing to participate (hereafter “participants”), and randomly

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pockets) is different from health intervention (pay people to do certain tasks), the participation problems in the two contexts are different. Nevertheless, we believe prepayment as proposed in this essay may also be effective in certain charitable giving contexts.

assign them to groups of different incentive treatments. Participants are typically paid after program completion or as the program proceeds, with the effectiveness of the program usually assessed by objective measures such as weight loss. Although the potential effectiveness of the intervention program for those who chose to participate could be high, can we also help those people who are initially not planning to participate or who are not responsive to program invitation?

Indeed, participation rate, hereby defined as the rate of the number of people who participated in a task divided by the total number of people invited, varies a lot from program to program and is typically not high. For instance, in a weight loss program (Volpp et al., 2008), 165 out of the 985 people (17.2%) responded to the mailed invitation letter. In a smoking cessation program, participation rates ranged from 5.4% to 15.4% (Volpp et al., 2009). In another study, participation rates ranged from 4.3% to 11.9% (Giné et al., 2010). A nation-wide survey across US showed that participation ranged from 32% for health education programs to 5% for smoking cessation programs (Grosch, Alterman, Petersen, & Murphy, 1998). Even though these health intervention programs were effective, the benefits only applied to those who accepted invitation. If we can increase the pool of participants, more people could benefit.

Program participation is a thorny issue in traditional incentive programs that reward participants if they reach their predetermined goal. It is also a central problem in designing commitment contracts (Ayres, 2010b; Beshears, Choi, Laibson, Madrian, & Sakong, 2011), in which people are punished if they cannot reach their predetermined

goal. In a typical commitment contract, participants need to commit their own money upfront. If they reach their goal, they can get their money back; otherwise, they forfeit their money to others, e.g., a charity they dislike (Ayres, 2010a; Halpern, Asch, & Volpp, 2012). Extant literature shows high effectiveness of commitment contracts in motivating health-related behaviors (Jeffery, Gerber, Rosenthal, & Lindquist, 1983; Jeffery, Thompson, & Wing, 1978). However, this effectiveness is accompanied by high rate of decline to participate (Jeffery et al., 1978), since the upfront financial requirements deter people from making commitments (Harris & Bruner, 1971). The larger the contract is, the more it will discourage participation (Jeffery et al., 1983). As a result, there is a constant tension between a program's effectiveness and people's willingness to participate: The most effective commitment contract also has the lowest participation rate (Ayres, 2010a)

### **Solutions to the Participation Problem**

One straightforward way to motivate people's participation is to spend more money on each person, but this may not always be a viable option if per capita budget is constrained. Even if budget allows, a program organizer cannot increase payment per person *ad infinitum*. Is there a more effective way to motivate people when there is a fixed budget? Can we increase program participation rates without spending more money per capita? For example, lottery-based incentives can redistribute payments without increasing total payments. Extant literature, however, showed mixed results. Some scholars have found that lottery incentives were better than fixed payments in motivating people (Haisley et al., 2012); others have found no difference between lottery and fixed payments (Levitt, List, & Sadoff, 2016; Volpp et al., 2008); and yet others found that

lottery-based incentives were inferior to fixed payments (Halpern et al., 2012; Paolillo & Lorenzi, 1984). Further research is needed to explore more systematically the conditions under which lottery-based incentives could be useful.

In this essay, we propose a different solution, *prepayment*: moving the reward timing in traditional incentive programs to the start of the program or even before the program starts. People are told that acceptance of the prepayment is equivalent to acceptance of the task, which will be carried out sometime after receiving the prepayment. If they are not able to complete the pre-specified task, the prepayment will be clawed back in the future.

Clawing back in extant literature refers to paying people their expected bonus in advance and taking it back if they underperform (Fryer Jr, Levitt, List, & Sadoff, 2012). Previous research has demonstrated the effectiveness of loss aversion and claw-back incentives on people's performance (Fryer Jr et al., 2012; Hossain & List, 2012; Levitt, List, Neckermann, & Sadoff, 2012), and negatively-framed incentives can be more motivating than positively-framed incentives (Goldsmith & Dhar, 2013). In these studies, people were incentivized to do things that they had already committed to (e.g., teaching students, preparing for exams, manufacturing consumer electronics), or those participants were already recruited in the experiments before the negative/positive message manipulation (Goldsmith & Dhar, 2013), resulting in program participation rates of close to 100%. Instead of using claw-back to motivate people who already committed to certain tasks, we extend the use of incentive claw-back to initial participation.

In our proposed prepayment solution, both participation and subsequent effort to fulfill the task are still voluntary; people still have the option of not fulfilling the task even after they have accepted the prepayment. This arrangement of prepayment and claw back can be seen as an informal contract. We have come up with this solution based on two major findings in economics and psychology.

### ***INTEGRATING TEMPORAL DISCOUNTING AND LOSS AVERSION***

#### **Temporal Discounting**

It is a widely accepted fact of human nature that people discount the future. In Fisher's "The Theory of Interest" (1930), he explained in detail why money now is worth more than money in the future, through the lens of people's impatience to spend income and opportunity to invest it. In standard economics, economists model people's choices using discounted utility theory (Fishburn & Rubinstein, 1982; Loewenstein & Prelec, 1992), which assumes exponential discounting and produces time-consistent choices. Time consistent preference means that later preferences "confirm" earlier preferences (Frederick, Loewenstein, & O'donoghue, 2002), whereas time-inconsistent choices is one that would not have been made if it had been contemplated from a removed, dispassionate perspective (Hoch & Loewenstein, 1991),

Empirical studies, however, have shown that the exponentially discounted utility model is insufficient to explain people's choices and have provided increasing support for hyperbolic discounting (Laibson, 1997) that valuation fall rapidly during initial delays but

more slowly as time goes by (Thaler, 1981). Such hyperbolic discounting has been used to explain people's time-inconsistent choices. One consequence of the hyperbolic discounting is that people are present biased (Frederick et al., 2002; O'Donoghue & Rabin, 1999, 2001; O'Donoghue & Rabin, 2002). That is, they excessively favor gratification now at the expense of future gratification. In addition to behavioral findings, neuroscientists have discovered evidences for hyperbolic discounting at the neural level. For example, McClure, Laibson, Loewenstein, and Cohen (2004) identified separate neural systems that correlate with immediate and delayed monetary rewards, whereas Kable and Glimcher (2007) argued that subjective value of potential rewards is explicitly represented in the human brain.

All of this evidence points to the prediction that moving payment from a later time point to an earlier time point can cause great changes in people's neurological state, psychological perception, and behavioral outcome. Present bias is often treated as a culprit of bad behavior in the literature. It is related to a wide range of people's unhealthy behavior such as procrastination (Bisin & Hyndman, 2014; O'Donoghue & Rabin, 1999, 2001; Reuben, Sapienza, & Zingales, 2007) and addiction (O'Donoghue & Rabin, 1999; O'Donoghue & Rabin, 2002). Can we make use of present bias to help people do more good behavior?

Past research found that when activities involve immediate rewards, people will preproperate activities: doing them too soon (O'Donoghue & Rabin, 1999). With this insight, we propose that this effect is likely to be extended from when to accomplish a

task (shall I do it sooner or later?) to the decision of participation in the task itself (shall I do it or not?). When we move the payment type from post-payment to prepayment in a task, we change a “delayed” reward to an “immediate” reward. Therefore, we propose the following hypothesis.

H1: People are more likely to accept a payment to perform a task if the payment is made earlier (prepayment) than later (post-payment) in a payment for effort transaction.

### **Loss Aversion from Prepayment and (Potential) Incentives Claw-back**

Loss aversion refers to the notion that losses have greater impact on preferences than gains, or losses loom large than gains (Kahneman, Knetsch, & Thaler, 1991; Thaler, Tversky, Kahneman, & Schwartz, 1997; Tversky & Kahneman, 1991). An immediate consequence of loss aversion is that the anticipated loss of utility associated with giving up a valued good is greater than the anticipated utility gain associated with receiving it (Tversky & Kahneman, 1991). This discrepancy was termed the “endowment effect” by Thaler and colleagues (Kahneman et al., 1991; Thaler, 1980).

In our proposed solution, once participants accept the prepayment, it becomes their endowment. Accepted prepayment, in the form of endowment, will therefore be valued higher than post-payment (Thaler, 1980), which is not endowed yet. The endowment effect, in turn, will motivate people to keep participating in any pre-committed task in order to avoid the loss of the prepayment. Based on the above analysis, we are proposing the following hypothesis:

H2: Loss aversion after accepting prepayment can motivate people's subsequent task participation and completion.

In the remainder of the paper, we report the results of two quasi-field experiments designed to test the above hypotheses. In study 1, we compare pre and post-payment incentives for motivating fruit consumption. In study 2, we move the prepayment timing up even further and test the role of gain versus loss framing for motivating people to participate in a survey.

### ***STUDY 1: FRUIT CONSUMPTION***

In study 1, we are trying to encourage people to eat more fruit. We chose the domain of promoting healthy eating because of the prevalence of diet-related issues in our society. The results of the 2012 Food and Health Survey (Food Insight, 2012) indicate that 6 out of 10 Americans surveyed reported to have given a lot of thought to their diet; yet, the objective standard of the average U.S. diet remains poor (Wang et al., 2014). When faced with choices between healthy and indulgent options, consumers all too often choose the latter (Chandon & Wansink, 2007; Finkelstein & Fishbach, 2010; Shiv & Fedorikhin, 1999; Wansink & Chandon, 2014). Can we use prepayment to motivate people to pick up healthy foods?

#### **Participants, Experimental Design, and Procedure**

To create a pool of eligible participants for subsequent randomization, a research assistant approached students on the campus of a large public university and offered a



small upfront payment to answer a survey. They were told that the survey would be sent to their email addresses later, along with an email invitation to participate in a free fruit study. It was made clear that the payment they received was only for the survey, and participation in the follow-up fruit program was voluntary.

One hundred sixty-six students accepted the payment and chose to sign up for this survey. The survey measured their fruit consumption frequency, self-control (4 items), desire for control (3 items), optimism (4 items), self-monitoring (2 items), risk (1 item for overall risk attitude, 1 item for stimulating risk taking, 1 item for instrumental risk taking), overconfidence (2 items), intrinsic motivation (1 item) and extrinsic motivation (1 item), need for control (1 item), planning fallacy (1 item), unrealistic control beliefs (3 items), locus of control (3 items) and risk aversion in gamble (1 item) to explore predictors of people's show up behavior.

We randomly assigned the 133 students (72 women, median age = 21) who actually answered the survey to one of three conditions: (1) Prepayment ( $n = 44$ ), (2) Post-Payment ( $n = 44$ ), and (3) Control ( $n = 45$ ). The study was carried out in two phases. In the first 2 weeks, we used financial incentives in the Prepayment and Post-Payment conditions. In the second 2 weeks, we stopped providing any incentives to test for any lasting effect of our intervention programs. The details of the program are outlined as follows:

### *Phase 1*

One day before the program started, we sent an email notifying participants of the starting date, location, and available collection times for payments and/or fruits. We also informed participants about the specifics of the program, depending on the condition.

Prepayment condition participants received the following email:

You have been selected into our rewards group to get fresh fruit. That means not only can you get free fruit, you will be paid \$10 upfront for your 10-day participation. You only need to collect the money on the first day of our program. If you miss your first chance to get fruit and come in on the 2nd day, you will get \$9. By the same logic, if you come in on the 3rd day, you will get \$8, etc. Once you have collected your payment, you commit to collecting your fruit each business day until the program ends on Friday of week two. If you miss a day of fruit, you will need to repay \$1. If you miss your fruit on two days, you will need to repay \$2, etc.

Post-Payment condition participants received the following email:

You have been selected into our rewards group to get fresh fruit. That means not only can you get free fruit, you will also be paid \$1 whenever you collect your fruit. The program starts on

Monday. For the next 10 business days, you can get one portion of free fruit plus \$1 each day.

Control condition participants received the following email:

We will provide free fresh fruit from tomorrow. For the next 10 business days, you can get one portion of free fruit each day.

Every business day, a research assistant prepared three to five different kinds of fruit, presented in plastic cups, and recorded the names of participants who participated and picked up their fruit that day.

### *Phase 2*

On the last day of Phase 1, we informed participants about the extension of our program for another 10 business days (i.e., 2 more weeks). They were told that “Some participants liked our program so much that they were asking for an extension. We are therefore happy to announce that we will, indeed, by extending our program for another TWO WEEKS, beginning tomorrow.” Unlike for Phase 1, participants were informed in Phase 2 that they would receive fresh fruit *only*, and that no monetary compensation would be provided.

## Results

We examined our data in two ways. First, we analyzed aggregated daily show-up frequencies (i.e., the total number of people showing up each day) over the 20-day program period. Second, we used survival analysis to analyze individual level data.

Overall, 27 participants out of 44 (61.4%) showed up at least once in the Prepayment condition, 18 out of 44 (40.9%) in the Post-Payment condition, and 16 out of 45 (35.6%) in the Control condition (Test for Equality of Proportions,  $\chi^2(2) = 6.62, p < 0.05$ ). Ten participants showed up every day in Phase 1, out of whom seven were from the Prepayment condition, two were from the Post-Payment condition, and one was from the Control condition ( $\chi^2(2) = 6.82, p < 0.05$ ). None of the participants showed up every day in Phase 2.

## ANOVA

We analyzed the variance of the show up data over the 10-day period of Phase 1 (see Figure 3a). We treated each group of subjects as one unit of analysis ( $N = 3$ ). Daily show ups of each group had been repeatedly observed for 10 days. A repeated measure ANOVA of the daily show up data with treatment (Prepayment, Post-Payment, Control) as between-unit factor and days as within-unit factor yielded significant main effects ( $F(2, 18) = 104.6, p < 0.001$ ). Post-hoc tests with Bonferroni adjustment (Rosenthal & Rosnow, 1991) indicated that the average number of students showing up during Phase 1 was significantly higher in Prepayment condition than Post-Payment condition

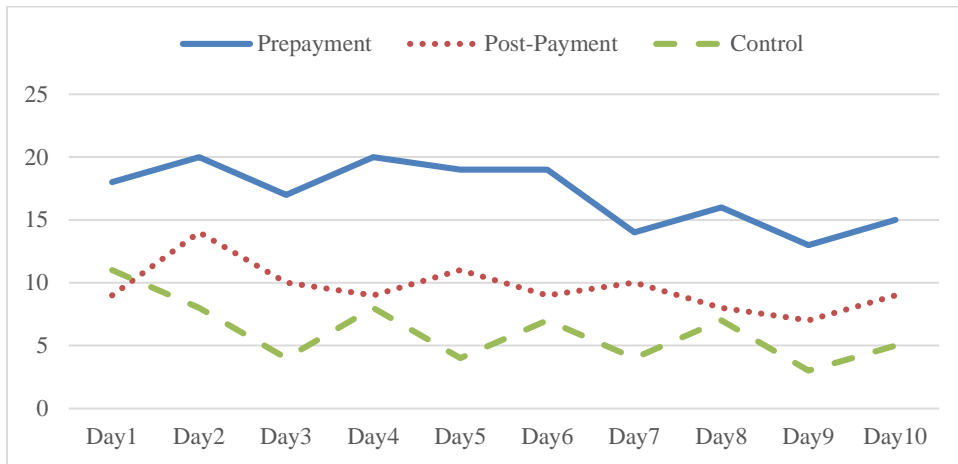
( $M_{\text{Prepayment}} = 17.1$  vs.  $M_{\text{Post-Payment}} = 9.6$ ,  $p < 0.001$ ) or Control condition ( $M_{\text{Control}} = 6.1$ ,  $p < 0.001$ ). Show-up was also significantly higher in Post-Payment condition than in Control condition ( $p < 0.01$ ).

An analogous ANOVA for Phase 2 yields similarly significant treatment effects ( $F(2, 18) = 35.8$ ,  $p < 0.001$ ), although for considerably lower show-up rates (see Figure 3b). Post-hoc tests with a Bonfferoni adjustment indicated that show-up was significantly higher in the Prepayment condition than in the Post-Payment condition ( $M_{\text{Prepayment}} = 4.1$  vs.  $M_{\text{Post-Payment}} = 0.8$ ,  $p < 0.001$ ) or Control condition ( $M_{\text{Control}} = 1.1$ ,  $p < 0.001$ ). Show-up in the Post-Payment condition was not significantly different from the Control condition.

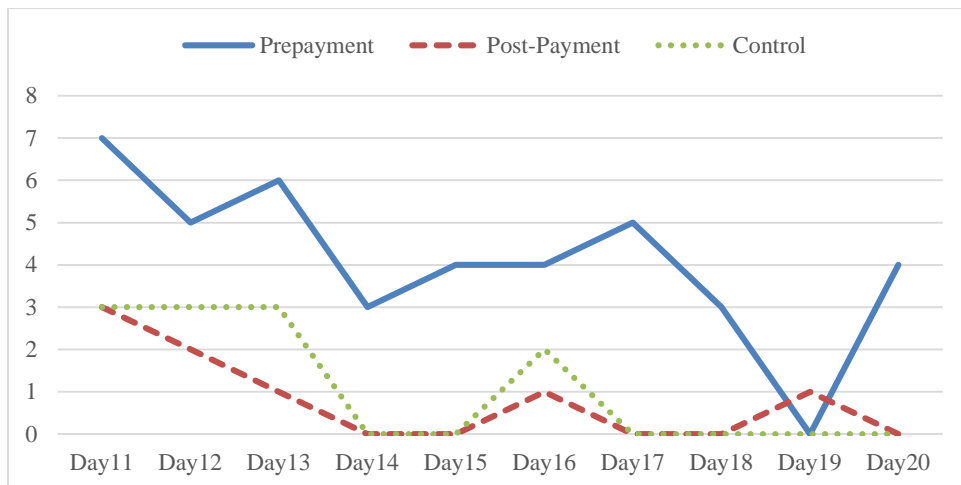
ANOVA results showed that the between-unit factor is mainly driving the show up variance. Post-hoc test confirmed the superiority of Prepayment in motivating show ups. Due to random assignments of individual members, we believe that the between-unit difference can be caused by no other reasons than experimental treatments. Therefore, causal inference in traditional RCTs are still valid; prepayment makes more people show up.

Figure 3: Distribution of total show-ups by condition for Phase 1 (a) and 2 (b)

a



b



### *Survival Analysis*

Next, we explore the dynamic process by using survival analysis. Behaviorally, task participation is measured by initial showing up to get the prepayment, while task fulfillment is measured by the number of days participants persisted before they stopped coming. In order to do survival analysis, we define the failure event to occur for a participant on the last day she collected fruit. That is, if one participant could not persist to day 18 or later, she was considered “dead” on her last day to pick up the fruit.<sup>4</sup> Figure 4 shows the results of the survival analysis graphically. The y-axis is the survival rate, i.e., percentage of surviving participants.

Each participant’s days “survived” was calculated as the number of days before their last collection of fruit (last day included). For instance, if a participant’s last day to pick up fruit is on day 15, then she survived 15 days and was dead on day 15. Most of the participants were “dead” by the end of Phase 2, with only 6 participants surviving. Among them, 5 were from Prepayment condition; 1 was from Post-Payment condition. A survival analysis showed that there was an initial loss of participants on day 1 across all three treatments. Specifically, among 44 participants we sent email to in the Prepayment

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<sup>4</sup> The survival curve can show a dynamic process of subject attrition. It is theoretically possible that subjects will resume picking up fruits after a long period of absence. However, we did not observe any subject would show up again after more than 5-day absence during our 20-day observation window. All our results remained significant and qualitatively unchanged if we define the cut-off day as any day between 15 and 20.

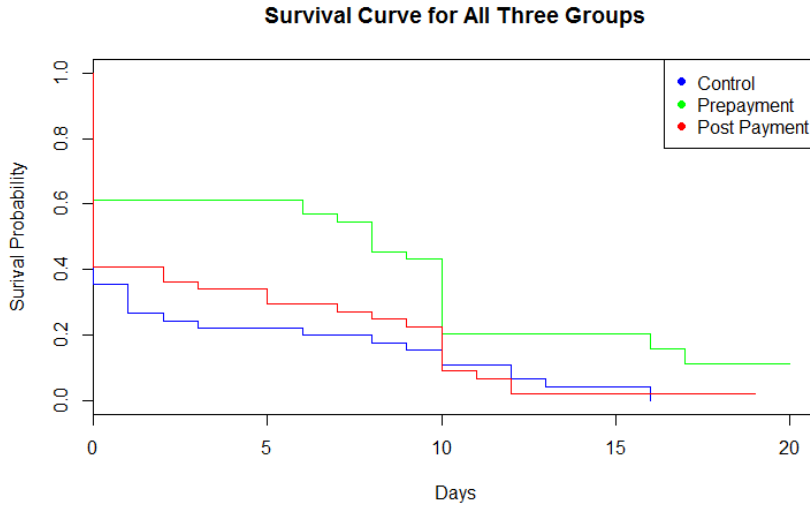
condition, 27 (61.4%) survived the first day. Prepayment program had a significantly higher survival probability than the other two programs, suggesting that the Prepayment program was more successful than the other two programs ( $P_{\text{Prepayment}} = 61.4\%$ ,  $P_{\text{Post-Payment}} = 40.9\%$ ,  $P_{\text{control}} = 35.6\%$ ,  $\chi^2(2) = 6.62$ ,  $p < 0.05$ ). Although there was a sudden drop after day 10, reflecting the removal of the financial incentives in Phase 2, Prepayment still did significantly better than the other programs.

Our survival analysis provided convergent evidence that there were significant differences among the three treatments in motivating task participation and task fulfilment, both for the entire four weeks (Phase 1 and Phase 2, Log-Rank Test,  $\chi^2(2) = 13.4$ ,  $p < 0.01$ ), as well as for the last two weeks (Phase 2,  $\chi^2(2) = 9.2$ ,  $p < 0.01$ ). Pairwise comparisons showed that Prepayment had a significantly higher survival rate than Post-Payment condition ( $\chi^2(1) = 7.2$ ,  $p < 0.01$ , entire four weeks;  $\chi^2(1) = 4.9$ ,  $p < 0.05$ , last two weeks) and Control condition ( $\chi^2(1) = 12.2$ ,  $p < 0.001$ , entire four weeks;  $\chi^2(1) = 7.2$ ,  $p < 0.01$ , last two weeks). There were no significant differences between Post-Payment condition and Control condition (neither for entire four weeks nor last two weeks).

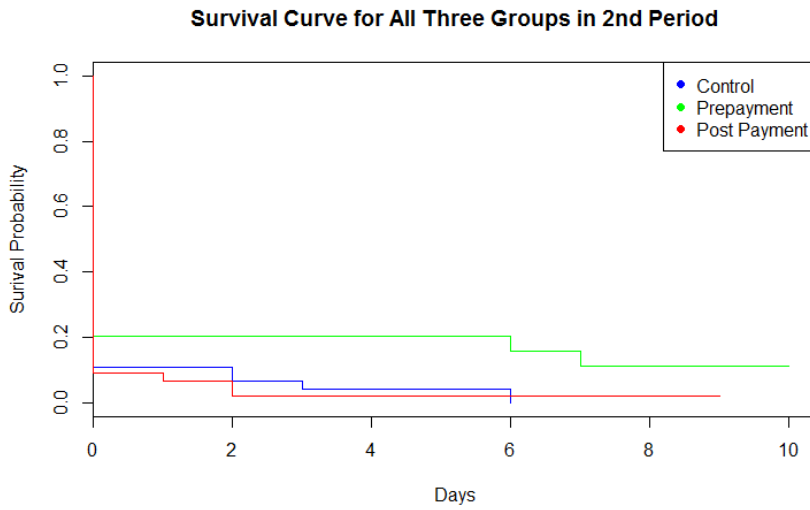


Figure 4 Survival curves by treatment for the entire experimental period 2 (a) and Phase 2 only (b)

a



b.



### *Regression*

We made an exploratory attempt to search for people's behavioral predictors from the constructs measured in the initial survey. Constructs of more than two items were first screened by internal consistency reliability (coefficient  $\alpha_{\text{self-control}} = 0.63$ ,  $\alpha_{\text{desire for control}} = 0.63$ ,  $\alpha_{\text{optimism}} = 0.77$ ,  $\alpha_{\text{self-monitoring}} = 0.63$ ,  $\alpha_{\text{unrealistic control belief}} = 0.65$ ,  $\alpha_{\text{locus of control}} = 0.61$ ). We have created a composite for these constructs. Since the two items of overconfidence have low internal consistency ( $\alpha_{\text{overconfidence}} = 0.41$ ), we did not create a composite score but still included both items in our following exploration.

In addition to the psychological constructs, we also included our treatments as two dummies and fruit consumption frequency in our equation. We ran two logistic regressions to predict whether people show up at least once in phase 1 and phase 2 separately. Regressions show that the Prepayment treatment significantly predicted participation in both phase 1 ( $B = 1.15$ ,  $SE = 0.49$ ,  $p < 0.05$ ) and phase 2 ( $B = 2.07$ ,  $SE = 0.65$ ,  $p < 0.01$ ), while no other variables can significantly predict show up.

Next we ran another two linear regressions of each person's total show up times on the same group of predictors for phase 1 and phase 2 respectively. Again Prepayment treatment could significantly predict the subjects' number of show ups in both phase 1 ( $B = 2.34$ ,  $SE = 0.74$ ,  $p < 0.01$ ) and phase 2 ( $B = 0.67$ ,  $SE = 0.26$ ,  $p < 0.05$ ). In addition, one overconfidence item<sup>5</sup> was negatively associated with show up times in phase 1 ( $B = -0.52$ ,

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<sup>5</sup> The item is "When people disagree with me, they are generally wrong."

SE = 0.25,  $p < 0.05$ ). In phase 2 overall risk attitude<sup>6</sup> ( $B = 0.24$ , SE = 0.12,  $p < 0.05$ ) and stimulating risk taking<sup>7</sup> ( $B = -0.21$ , SE = 0.079,  $p < 0.01$ ) were also significant predictors. Given that each equation has nineteen predictors, the overall preferred alpha level (Rosenthal & Rosnow, 1991; Rosenthal & Rubin, 1983) is about  $p = 0.6$  assuming independence among the predictors. The predictive validity of any of these psychological constructs remained inconclusive. Our two groups of regression analysis showed again that Prepayment treatment was a consistent predictor of both subjects' showing up behavior as well as their total show up times, controlling for all the measured psychological constructs.

## **Discussion**

### *Summary*

In this study, we tested the Prepayment program against a program that offers payments without any repayment commitment, giving participants maximum flexibility, as well as a baseline program without financial incentives. We found increased participation when we prepaid participants a lump sum of money, demonstrating prepayment's effect in motivating people to perform a healthy behavior in a short term. All of analyses, including ANOVA, survival analysis, and regression analysis have validated prepayment's effect in motivating people's participation. More specifically, our

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<sup>6</sup> The item is "I like taking risks."

<sup>7</sup> The item is "I often take risk just for fun."

data have supported both of the proposed hypotheses that (1) prepayment will make more people to accept the payment over post payment and (2) the average show ups of each person in Prepayment group is higher than Post Payment and Control group. In addition, no other psychological constructs we measured can consistently predict people's participation.

#### *Self-selection issue*

We have preselected our subjects for the sake of compiling a subject pool for subsequent randomization based on their willingness to participate in a survey. Those subjects were also aware that there would be a following fruit program, although no further details were provided. Therefore, there may be self-selection effect due to their interest in the survey or fruit program. However, any self-selection effect was controlled for in our following randomly controlled trials. Any difference in actual participation should be due to our experimental manipulation. Our key dependent variable, participants out of all invited students, was low in Control group (35.6%). Prepayment increased participation rate to 61.4%, comparing with a slight increase caused by post payment (40.9%).

#### *Limitations*

Although study 1 found initial support for both hypotheses, there were a number of limitations. First, the Prepayment program mixed the prepayment phase and actual task phase in day 1. That is, when participants showed up in day 1 to get the Prepayment, they also got day 1's portion of fruit. Since our hypotheses distinguished two stages:

prepayment/task acceptance stage and task fulfillment stage, it is desirable if we can decouple the two stages and observe them separately, although in practice the two stages can be mixed. In study 2 we made such an attempt to separate the money acceptance and task fulfillment stages.

Secondly, because some people may intrinsically want to eat fruit, task participation may have been driven by a mixture of intrinsic and extrinsic motivation (Benabou & Tirole, 2003; Davis, Bagozzi, & Warshaw, 1992; Ryan & Deci, 2000). Some participants in the Prepayment condition probably chose to participate mainly because they wanted the money, while others may have chosen to participate mainly for the benefits of daily fruits, such as those in the control group. Among participants in Prepayment program, besides the two motivations mentioned, a few sophisticated participants may accept the prepayment because they wanted to commit themselves in a loss contract (Imas, Sadoff, & Samek, 2016) in order to eat more healthily. Therefore, temporal discounting may not be the only reason that drives their acceptance of the prepayment. If we choose a task that has little intrinsic motivation, we can provide a better support to our hypotheses. In study 2 we have adopted such a task.

Thirdly, the Prepayment condition used a lump sum of money, while Post-Payment condition disbursed the money in equivalent streams of smaller rewards. This difference may lead to “illusion of wealth” (Goldstein, Hershfield, & Benartzi, 2015) by which a lump sum seems more adequate than its equivalent streams of smaller rewards. Such an illusion may lead people to be more likely to accept the lump sum instead of the smaller equivalent streams. However, the reverse effect is also documented in the same

paper (Goldstein et al., 2015), so it is not clear how this factor is playing in the current study. This limitation could be fixed by giving participants the same amount of money as a one-time post payment. In study 2 we have adopted this change.

Finally, participants received the prepayment they first showed up, so it still needs effort from participants' side to pick up the prepayment. What would happen when we offer the prepayment directly to participants so that we can further reduce the cost of payment pickup? Temporal discounting predicts that it will be even more effective in motivating people, since we are moving the payment even closer to them. In study 2 we moved the prepayment directly in front of subjects.

## ***STUDY 2 SURVEY TAKING***

In study 2, we remedied all the above four issues by (1) decoupling the prepayment phase and task phase; (2) choosing a task that was less driven by intrinsic motivation; (3) set both the prepayment and post-payment as a one-shot payment amount; (4) moving prepayment directly in front of subjects.

### **Design and Participants**

Previous research showed that loss-framed incentives can motivate people to pay more efforts (Goldsmith & Dhar, 2013; Hossain & List, 2012; Imas et al., 2016). In addition to the prepayment versus post-payment manipulation, we also included gain/loss message framing (Tversky & Kahneman, 1981) as another factor in our study design so that we can compare two types of losses: loss based on framing versus loss based on

prepayment. Four discussion sections from the same large undergraduate business class were randomly assigned into one of the four conditions in a 2 (payment method: Prepayment vs. Post-Payment)  $\times$  2 (message frame: gain vs. loss) between-subject design. A total of 147 students were exposed to the program announcements.

### **Procedure**

*Phase 1:* One student RA who was blind to our research hypothesis went to each classroom to make an announcement of a paid survey to be taken next week (see Table 1 for the specific announcements) and projected the program details on a screen. They were informed that the survey would be taken at a specific location during a few time slots. It would take 7 minutes to answer the survey. Students did not know about the survey content. The Pre/Post-Payment manipulation was done by allowing them to pick up the payment either at the time of the announcement or when they actually showed up, while the loss/gain manipulation was done by framing the incentive as “getting the payment for showing up” versus “giving up the payment if not showing up.” (see Table 1)

After making the verbal announcement, the RA wrote on the blackboard the survey location and time (9-11 or 3-5, Monday-Thursday the following week) to repeat the key information. In the Prepayment conditions, the RA asked interested students to raise hands, distributed the \$3 payments, and collected receipts. She also counted the number of students in the classroom before she left. In the Post-Payment conditions, the RA made the announcement, wrote the information on the blackboard, and counted the number of students in the class; however, no payments were made.

*Phase 2:* A researcher waited for students to show up at the specified location and time during the following week. When the students showed up with their own mobile phone or laptop, they were asked to indicate whether they had received the payment already, as well as write down their own names and email addresses. An online Qualtrics survey was immediately sent to the email addresses by the researcher. All students were instructed to complete the survey on site. After completing the survey, the students in the Prepayment conditions were dismissed right away while the students in the Post-Payment conditions were paid \$3 before being dismissed. Student's self-reported information (whether they received the money yet; which discussion section they are from) was cross-validated with the information we already collected.

*Phase 3:* In the week following Phase 2, the same RA went to the discussion sections in the Prepayment conditions to collect money from those who had accepted the prepayment but did not show up to take the survey. We were able to recover the prepayments from 11 out of 17 students. The authors will discuss this issue further in next section. We then sent the same online survey to all other students from the four classes. These students answered the survey on a voluntary basis.



Table 1 Experiment Design and Announcements

	Gain	Loss
Prepayment	You've been selected to be paid to answer our survey for \$3. You can pick up your payment now. After picking up the payment, you need to come to Olmsted 2336 at any time between 9-11 or 3-5, Monday to Thursday next week to answer a survey, which takes 7 minutes. If you don't come, an RA will come to the discussion two weeks later and get back the money. It is important to bring your laptop/tablet/smart phone with you.	You've been selected to be paid to answer our survey for \$3. If you don't pick up your money now, you automatically give up your payment. After picking up the payment, you need to come to Olmsted 2336 at any time between 9-11 or 3-5, Monday to Thursday next week to answer a survey, which takes 7 minutes. If you don't come, an RA will come to the discussion two weeks later and get back the money. It is important to bring your laptop/tablet/smart phone with you.
Post-Payment condition	You've been selected to be paid to answer our survey for \$3. You need to come to Olmsted 2336 at any time between 9-11 or 3-5, Monday to Thursday next week to answer a survey, which takes 7 minutes. After you answered our questionnaire, we will pay you \$3. It is important to bring your laptop/tablet/smart phone with you.	You've been selected to be paid to answer our survey for \$3. You need to come to Olmsted 2336 at any time between 9-11 or 3-5, Monday to Thursday next week to answer a survey, which takes 7 minutes. If you don't show up, you automatically give up the payment. After you answered our survey, we will pay you \$3. It is important to bring your laptop/tablet/smart phone with you.

## Results

### *Manipulation Check*

As a check for the payment manipulation, all participants were asked “when they were supposed to get the payment, before (Prepayment) or after (Post-Payment) survey.”

All 35 students who showed up correctly identified their payment type.

To check the success of the gain/loss framing manipulation, we asked students to indicate whether they saw taking the survey as “a chance to gain 3 dollars” or “a chance not to lose 3 dollars.” Correlation between our experiment manipulation of gain/loss and students self-reported perception is 0.07 (*NS*), indicating that our framing manipulation did *not* cause any perception change among participants

*Participation rate*

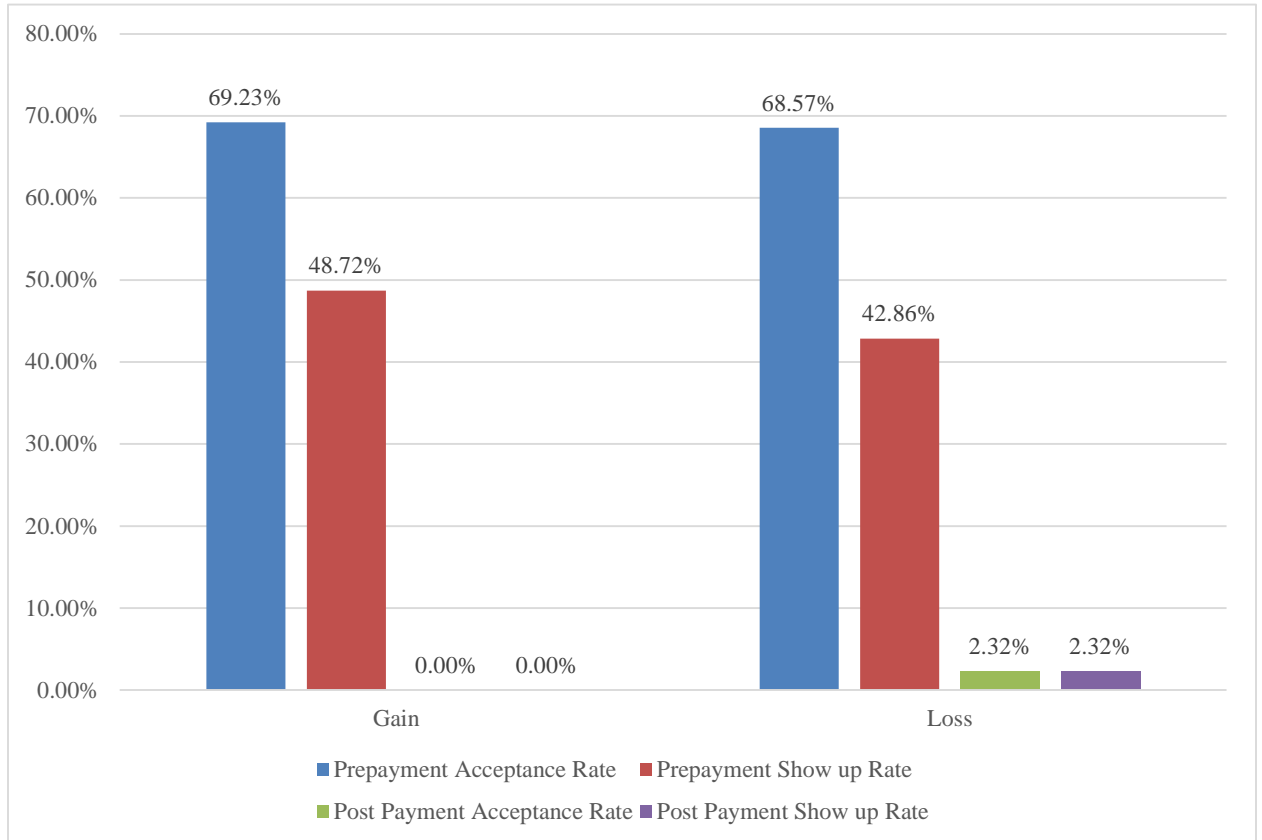
Participation rate was measured as show up rate in this study. In the two Prepayment conditions, out of 74 students who heard the announcement, 51 accepted the prepayment, and 34 eventually showed up to take the survey.<sup>8</sup> In the two Post-Payment conditions, out of 73 students who have heard the announcement, one student showed up to take the survey and got paid (See Table 2 and Figure 5). The difference between the two show up rates was significant (Test of Equal Proportions,  $\chi^2(1) = 37.83, p < 0.001$ ).

Table 2 Show-up Rates Across Four Conditions

	Prepayment	Post-Payment
Gain	39 total, 27 accepted prepayment, 19 showed up	30 total, 0 showed up, 0 accepted post payment
Loss	35 total, 24 accepted prepayment, 15 showed up	43 total, 1 showed up, 1 accepted post payment

<sup>8</sup> Two students in prepayment conditions showed up at the wrong time and were not counted.

Figure 5 Payment Acceptance and Show up Rate Across Conditions



A logistic regression of payment type and payment framing on participant show up yield a significant effect of payment type ( $\beta = 3.45, z = -2.89, p < 0.01$ ), but no effects of either framing ( $p = 0.99$ ) or payment-framing interaction ( $p = 0.99$ ). Furthermore, 21 out of the 34 students (61.76%) who showed up in the Prepayment conditions reported seeing the survey as a chance “not to lose 3 dollars,” providing evidence that loss aversion contributed to their showing up. Only one participant showed up in Post-Payment conditions. This subject saw the survey as “a chance to gain \$3 payment.”

## **Discussion**

In study 2, we have shown that prepayment can significantly increase participation. Average participation rate was 1.37% of all subjects in Post-Payment conditions, comparing with 45.95% in the Prepayment conditions. The acceptance rate of prepaid money conditions was 68.92%. This high acceptance rate of Prepayment condition supported H1's prediction that prepayment will make more people participate. Two-thirds of those who accepted the money in Prepayment conditions subsequently showed up. Participants' self-reported data supported our loss aversion hypothesis that most of the subjects who accepted prepayment saw the survey as a chance "not to lose 3 dollars." This high conversion rate, combined with participants' self-report, supported H2 that loss aversion as a major driving force of task fulfilment (H2).

In contrast, the loss versus gain framing of incentives has little effect on people's participation. This finding is surprising to the author, given that literature showed support for such a framing effect (Goldsmith & Dhar, 2013; Hossain & List, 2012). It is possible that our manipulation failed such that we did not create a loss mindset when the RA was making program announcements. On the other hand, however, this may be due to a major difference between effort and participation. Recall that in their experiments all participants already agreed to the task, while our participants had not made up their mind yet. It is possible that when participants were already in a participation mindset, their sensitivity to message framing was different from when they had not yet decided to participate or not. In addition, there is recent evidence that loss/gain framing does not

always work. For instance, in a field study by Chung and Narayandas (in press), they found a quota-bonus plan and a punitive-bonus plan framed as a penalty for not achieving quota are *not* different in their effectiveness to incentivize sales persons. This topic may merit future research.

### ***STUDY 3: DO PEOPLE ANTICIPATE THE PREPAYMENT EFFECT?***

In studies 1 and 2, we have showed that in a between-subjects design, Prepayment program significantly outperformed the other programs in motivating people's participation. Our next study sets out to explore people's intuition about this effect. Do people anticipate the higher effectiveness of Prepayment program versus other programs? Are they willing to select the Prepayment program when given other choices simultaneously?

According to Imas et al. (2016), a standard behavioral model will yield two predictions (1) People will work harder under loss than gain contracts, as predicted by loss aversion; (2) Anticipating this loss aversion, individuals will have a strict preference for the gain contract. The first prediction was supported by a series of studies mentioned earlier in this essay, as well as their own results. The second prediction, however, was not supported by their empirical results. Instead, they found the opposite pattern: people prefer loss contracts over gain contracts. They explained their finding by proposing that people *intentionally* select into loss contracts as a commitment device to improve performance and solve dynamic inconsistency. The current study tried to test whether this pattern also applies when it comes to program participation decision. If this was true in

our context, we would find that most people prefer the prepayment programs over other programs for similar reasons in Imas et al. (2016). In study 3, we used similar program instruction as in study 1 in a within-subjects design. We only care about which program people prefer *ex ante*, so no actual programs were carried out after they made their choice.

### **Participants, Experimental Design, and Procedure**

One hundred and fifty participants (62 women, aged 20-64 years, MD = 31) recruited from MTurk participated in the study in exchange for \$0.30. Participants read the following scenario about different food programs, which were shown in random order:

“A non-profit organization close to your home is launching a ‘Free Fruit’ Program. The purpose of the program is to encourage people to develop a habit of eating fresh fruit regularly. The organization offers three options to choose from:

1. Prepayment Program: In this program, not only can you get free fruit, you will be paid \$10 upfront for your 10-day participation. By signing up, you commit to collecting your fruit each business day until the program ends. If you miss a day, you will need to repay \$1. If you miss two days, you will repay \$2, etc.
2. Free Fruit Program: In this program, the organization will provide free fresh fruit for the next 10 business days. You can get one portion of free fruit each day.
3. Post-Payment Program: In this program, not only can you get free fruit, you will also be paid \$1 whenever you collect your fruit. The program

runs for the next 10 business days. You can get one portion of free fruit plus \$1 each day.”

After reading about the programs, participants were asked to indicate which *single* program they would like to sign up for. Participants were also asked on a 7-point scale to what extent they agreed with the following statement: “My chosen program helps me to commit to my goal of improving my diet” (1 = strongly disagree, 7 = strongly agree). This item is intended to measure their *predicted* effort they were planning to exert.

### **Results**

Only 21 participants (14%) chose Prepayment program, 23 (15%) chose Free Fruit program, and the vast majority of people, 106 (71%), chose Post-Payment program ( $\chi^2(2) = 94.12, p < 0.001, Cramer's V = 0.79$ ). In terms of participants' prediction about their likely commitment across the programs, the results show no significant difference ( $M_{Prepayment} = 5.57, M_{Post-Payment} = 5.44, M_{Free Fruit} = 5.70; F(2, 147) = 0.40, NS$ ), suggesting that people believed that they would be equally likely to make effort in any of the offered programs.

### **Discussion**

Study 3 showed an opposite pattern to that of Imas et al. (2016). Given that the Prepayment program had additional attraction from prepayment itself, the result is more conservative. Therefore, in study 3 we found (1) no evidence that people had anticipated loss aversion from their answers of the commitment scale (2) no evidence that people would select into a loss framed program over gain framed program (3) no more people

wanted to use prepayment program as a commitment device when they were given other options of equivalent payment.

This discrepancy may be due to the difference between effort and participation, or due to the fact that their participants' preferences were measured by their WTP for the gain/loss contracts in a between-subjects design (Imas et al., 2016), whereas in our study participants' preferences were measured directly by comparing different options in a within-subject design. Such differences in preference elicitation, as well as separate evaluation and joint evaluation (Hsee, 1996), may systematically affect people's perception and choices. In addition, the flexibility of the Post-Payment program may also be a major reason for people to choose it. It is possible that if we change the small streams of post-payment into a lump sum (Goldstein et al., 2015) at the end of the program, the preference for the Post-Payment program will be attenuated. Last but not least, it is likely that most people do *not* like the commitment, contrary to Imas et al. (2016). Instead they valued the flexibility of the Post-Payment program, which offered the same financial incentives as the Prepayment program. Surprisingly, Prepayment were not more appealing than free fruit in our study, which offered no financial incentives at all. This was further evidence that people disliked commitment upfront. In other words, they do not like the idea of tying their own hands.

Although our findings here contradict Imas and colleagues' conclusion (2016) that people will prefer loss contracts, our explanations offered here are speculative. Future work is needed to explain this discrepancy: why people prefer loss framed



contracts sometimes but prefer gain framed contracts other times, and when people are willing to pre-commit themselves and when they are not.

One intuition of the authors is that *before* people made their decision to participate in a task, they generally prefer more flexible options; *after* they self-select into a task, they may prefer options with pre-commitment or loss component to better motivate their subsequent effort. If this is true, it not only explains that in a joint evaluation people would like to choose the Post-Payment program *before* participation (study 3), but when they were actually offered the Prepayment program, they actually made more efforts (study 1 and study 2) *after* they agree to participate. Due to correct anticipation of higher effort and higher return in Prepayment program, they preferred it to Post-Payment Program (Imas et al., 2016).

## ***LIMITATIONS AND GENERAL DISCUSSION***

### **Limitations**

Studies 1 and 2 showed support for our main argument that prepayment can increase task participation due to present bias and loss aversion. There are a few limitations. First of all, our studies used relatively smaller amount of money and in a relatively closed community, whether prepayment could be more effective with larger scale of money in a more open community needs to be tested in future research.

Secondly, in our study 2 we have fixed four issues in study 1, but it introduced another variable: group setting. Since the RA announced the program to an entire class in public, this may lead to herd behavior (Banerjee, 1992; Scharfstein & Stein, 1990) in

Prepayment conditions, which in turn can partly explain the dramatic gap between Prepayment and Post-payment conditions. However, study 1 was communicated in a private setting. Such a herd behavior account cannot explain study 1's result.

A related issue is the interpretation of study 2's results. Our experimental randomization occurs at the class level instead of individual level. Therefore, the logistic regression results can only be interpreted in a class fixed-effect model, which has already accounted for any "herd" effect. In other words, our results support the prediction that the likelihood of any individual's show up will be greatly increased if she is offered prepayment in a group setting, but not in a private setting.

Last but not least, prepayment means default risks for the program organizer. Namely, there is a possibility that money cannot be collected back. If the purpose of a program is mainly to encourage people to participate or the amount of money involved is negligible, not being able to get the money back may not be a major concern for program organizers. For example, if a company could make more employees to do health risk assessment (Haisley et al., 2012) by spending moderate amount of money, money collection may be a lower priority compared with the increased participation and potential reduction of today's increasing health care cost (Grosch et al., 1998). Otherwise potential financial loss should be assessed before adopting a prepayment program.

*Is money collection an issue?*

One major issue of prepaying people is how to enforce money collection if they have not fulfilled the pre-specified task. In both studies, most of the students who

accepted the prepayment participated in the subsequent tasks. For those whose money needs to be collected, the researcher had successfully collected back from the majority of them. Indeed, most of the students voluntarily returned money in study 1<sup>9</sup>. This fact indicated that students treated the prepayment program as an informal contract: once they accepted the prepayment, they knew they should either fulfil the task or return the money in the end. Despite the fact that the contract was not legally binding, it was morally binding, at least to most people. As a result, the worry of “not being able to collect money back” may be an unnecessary concern.

However, as mentioned in the last section, we strongly recommend an assessment of clawing back success before launching a prepayment program. We are still unclear about the clawing-back rates if the prepayment is in a larger scale of amounts. Our intuition is that both the present bias and loss aversion effect will be much stronger when stakes are higher. Therefore, the need for clawing back will be reduced. Nevertheless, there are many factors that can affect this “morally binding” element. For instance, in a relatively stable community in which people care about their reputation, such as a school or a company, this informal binding power would be stronger comparing with a community of high mobility. If the program organizer can obtain contact information of the subjects such as email addresses or phone numbers for follow-up contact, it will also increase the success rate of clawing back.

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<sup>9</sup> In study 2, researchers re-approached students to collect money. Students did not need to return their money by themselves.

## Discussion

This essay has documented the prepayment effect: prepayment can make more people participate in tasks and programs. We have used a fruit consumption task and a survey taking task to illustrate this effect. Theoretically, we have extended the effect of loss vs. gain framed contracts on effort and performance to participation. Interestingly, people's preference for such prepayment program was not strong (study 3). Nevertheless, when people were actually offered the program, they tend to accept the prepayment, and it is very effective in motivating them to fulfil the subsequent task, as shown in studies 1 and 2. Practically, although we have mainly focused on health context, we believe that the prepayment effect can be generalizable to many other contexts, as long as there is a need to recruit people, and task<sup>10</sup> and payment could be temporally decoupled.

In studies 1 and 2, we have successfully made more people participate in a pre-specified task by using prepayment. We believe prepayment has much potential in the field to motivate participation, especially in but not limited to health-related interventions. Although we utilized actual money as our prepayment, other forms of prepayment are possible. For instance, a program utilizing small gifts as prepayment to encourage people's behavior could be tested by researchers in the future.

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<sup>10</sup> Sometimes task participation and task performance is hard to distinguish when participation is equivalent to task performance (study 2), but other times there is a clear line between the two. In traditional health intervention programs, participation and performance are usually easy to disentangle. For instance, in a weight loss program, participation is the proportion of people actually participate out of all people invited. Task performance is usually measured by an index such as average weight loss or body mass index change.

In addition to the proposed solution of prepayment, the authors also want to call more attention to the participation problem of our research community. In an experimental paradigm, researchers are mostly interested in the effect of their proposed manipulation. They often randomize a fixed pool of subjects into pre-specified experimental conditions. In their subsequent study report, subjects' participation is taken for granted. However, in intervention programs outside the lab, such a fixed pool of subjects may not be well defined or even existed. In a typical health-intervention program, for instance, in addition to the goal of establishing effectiveness of an intervention, another major goal is to increase the pool of participants in the first place. It is that end which motivates this research project; it is also toward that end that we proposed a solution utilizing well-established theories in economics and psychology. Since this is only one way that could work, more research is needed to test other ways to increase people's participation.

### ***DISSERTATION CONCLUSION***

In this dissertation we have documented two time-related effects: newness effect and prepayment effect. The former applies to past options. It says that moving an identical product closer to now makes people prefer it more. The latter applies to future payments. It says that moving a future payment closer to now makes people more likely to accept the payment, thus entering into an informal contract. we have also explored the psychological mechanisms underneath the two effects. We found that the first effect is likely to be an overgeneralization of a learned association, and people use the newness as

a heuristic strategy when making choices. They are willing to pay for such newness in joint evaluations. For the latter effect, we found it was driven by two forces: temporal discounting and loss aversion. People do not like the prepayment program when they have other options, but when they *are* offered such a program, a lot of people are willing to accept it, because prepaid (immediate) money is very attractive to them, as suggested by hyperbolic discounting. Once people accepted the prepayment, loss aversion will subsequently drive their continuous effort. As a result, we could motivate more people implement healthy behavioral change.

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## ***APPENDIX 1 INSTRUCTION OF BDM METHOD FOR BOOK AUCTION***

You will be bidding on two Business books randomly selected as "Book of the Month." **We will not reveal the titles of these books until after the study.**

**How to bid:** To bid for a book, you will specify the maximum dollar amount that you are willing to pay for that book, between \$0 to \$20 ( $X$ ). Your bid has nothing to do with other people's bids. **For three randomly selected students from this study, these bids will be for real!** Each of these three lucky students will first receive a **\$20 cash reward** in person at Anderson Hall, and then play out their bids, as described below.

**How book prices are determined:** The price for each book will be randomly determined by rolling a fair 20-sided die in person. If your bid ( $X$ ) is greater than or equal to the price rolled for that book, you will pay the rolled price and receive the book. Otherwise, you keep your money and do not buy the book.

In case you don't know, 20-side dice look like the picture below, with the numbers 1-20 on the 20 sides of the die. All numbers between 1-20 are equally liked to be rolled.



Note that the chance of buying the book will increase if your bid is higher. For instance, if you bid 1\$, you have a 5% chance to buy the book (only if the roll is 1); if you bid \$20, you have a 100% chance to buy the book at a randomly rolled price between \$1 to \$20.

**Optimal bidding:** The best strategy is for you is to **bid the highest amount of money you are willing to pay for each book.** No more, no less.