Journal of Economic Behavior & Organization 107 (2014) 582–594

Contents lists available at ScienceDirect

Journal of Economic Behavior & Organization

journal homepage: www.elsevier.com/locate/jebo

Keeping your gains close but your money closer: The prepayment effect in riskless choices

Guy Hochman[a,b,*], Shahar Ayal[b], Dan Ariely[a]

[a] Duke University, Durham, NC, USA
[b] Interdisciplinary Center (IDC) Herzliya, Israel

ARTICLE INFO

Article history:
Received 26 February 2013
Received in revised form 22 January 2014
Accepted 26 January 2014
Available online 3 February 2014

Keywords:
Loss aversion
Prepayment
Money representation

ABSTRACT

Although research on loss aversion now spans more than three decades, researchers are still debating whether (or in which cases) the finding holds true for money. We contribute to this debate by exploring how prepayment affects financial decisions. In one set of experiments, we show that when faced with a tradeoff between post- and prepayment, participants overvalue prepaid money, and sometimes even prefer it over objectively higher gains. Importantly, this effect was more pronounced when prepayment was more distant from its pure representation in dollars and cents (Experiment 1A), as well as when potential losses were directly linked to specific options (Experiment 1B). As far as the processes involved, our results suggest that prepayment leads to increased personal commitment to prepaid options (Experiment 1C). In a second set of experiments, we show that even when the tradeoff element is eliminated, participants are more motivated and engaged in a task that is prepaid rather than post-paid (Experiments 2A and 2B). Based on our findings, we discuss how firms can use prepayment mechanisms to get more out of their agents, and how individuals can be motivated to better utilize their money.

1. Introduction

Imagine you are the CEO of a company that sells financial services (e.g., insurance). Intermediaries sell your products alongside those of competing companies, and you want your products to be promoted above others. To beat the competition, you could pay intermediaries a higher commission for selling your products – but this could get pricey. Alternatively, imagine that you paid the standard commission for your products but you did so in advance – the same amount of money, only paid up front rather than after the sale. Under this *prepayment* approach, intermediaries are required to pay you back the prepaid commission for any of your products that they fail to sell. With this type of tradeoff scenario, in which they must choose between your prepaid products and other (post-paid) products, would they promote your products over others? Would they do so even if time discounting would play no role and if they would clearly earn less overall? Or when faced with both yours and the competitors' products, would they invest more time and effort in selling your products to try and avoid losing the prepaid commission? Of course, if the intermediaries adhere to the principle that "money is money," or "all dollars are born equal," they should treat prepaid money in exactly the same way as their standard post-paid commission and promote the products that maximize their payoffs. However, if the intermediaries violate the principle of fungibility, they might be reluctant to give up the prepayment – even when it would be economically inefficient.

* Corresponding author at: Duke University, 2024 W. Main Street, Durham, NC 27708, USA. Tel.: +1 919 681 5922.
E-mail address: guy.hochman@duke.edu (G. Hochman).

0167-2681/$ – see front matter © 2014 Elsevier B.V. All rights reserved.
http://dx.doi.org/10.1016/j.jebo.2014.01.014

Although the timing of the payment should, in principle, have no effect on how it is valued, decision making literature suggests that the question of whether prepayment will have an effect on individuals' choices and effort depends on whether money (in the form of prepayment or otherwise) is perceived as a loss or as a foregone gain (Novemsky and Kahneman, 2005). According to the theory of loss aversion (Kahneman and Tversky, 1979), individuals are more sensitive to the possibility of a loss than they are to the potential for an equivalent gain. Thus, if prepayment is perceived as a loss, this should lead to the overvaluation of prepaid money and an over-eagerness to sell prepaid contracts in order to avoid the loss of the received payment. In contrast, however, overvaluation of prepaid money would not be expected in cases where this money is not perceived as a loss.

In choices under risk, the accumulated evidence supports the former interpretation. For example, Davis et al. (2010) found that providing a show-up fee for participants at the end of the experiment (post-payment) leads to a house money effect (Thaler and Johnson, 1990) whereby individuals are more risk-seeking because they do not yet consider the money to be theirs. In contrast, when a show-up fee is administered as a prepayment (before the beginning of the experiment), participants are risk-averse because they are more reluctant to lose their "own" money. Rosenboim and Shavit (2012) further supported these findings by showing that when the prepayment is given to participants two weeks before the experiment, the willingness to take risk decreases since the participants are even more strongly tied to the prepaid money. In riskless choices, however, the picture is much more nuanced.

A straightforward reading of reference-dependent preference theory (which models loss aversion in riskless choices; Tversky and Kahneman, 1991) suggests that the act of giving up money (e.g., forfeiting a prepaid commission for a post-paid commission) is construed as a foregone gain rather than a loss (see also Idson et al., 2000; Kahneman and Sugden, 2005; Kőszegi and Rabin, 2006). Thus, a riskless choice between payment before or after the delivery of goods or services merely reflects a comparison between the gain of the post-payment and the (foregone) gain of the prepayment. From this perspective, no special effect of prepayment should be expected. In line with this approach, Novemsky and Kahneman (2005) attempted to define clear boundary conditions for loss aversion. One of their conditions suggests that goods providing the same benefits are not perceived as losses, and can thus be exchanged without subjection to loss aversion (e.g., selling an old car to buy a new one). Similarly, a second boundary condition suggests that when goods are intended for exchange from the start, they are not perceived as losses (for related arguments, see Ariely et al., 2005; Kahneman et al., 1990, 1991; Kőszegi and Rabin, 2006; Thaler, 1980). Thus, these boundary conditions imply that intermediaries – who expect to be compensated for their services in order to purchase other goods and commodities – would readily forfeit their prepaid commissions in order to obtain similar or higher amounts of money in post-paid commissions.

However, a different outcome is predicted by Bateman et al. (1997) who propose an alternative reference-dependent model. According to Bateman et al., any reduction in the status quo (even in the form of money that is given up in routine transactions) is considered a loss, and thus leads to loss aversion. Accordingly, since individuals should be more sensitive to losses than to equivalent gains, prepayment should be overvalued and personal commitment and engagement to the prepaid option should increase. Recently, an adversarial collaboration has formed in an attempt to reconcile these competing models (Bateman et al., 2005). The authors surmise that loss aversion for money could occur under specific conditions (e.g., in cases where individuals perceive themselves as having no budget reserves), and highlight the need to further examine the moderators of loss perception for money (see also Novemsky and Kahneman, 2005). The aim of the current paper is to contribute to this venture by examining the conditions under which loss aversion for money in riskless choices is stronger or weaker.

Similar to Rosenboim and Shavit (2012), we propose that one central moderator determining when prepaid money is treated as a loss can be derived from the rationale of mental accounting (Thaler, 1980, 1999). Mental accounting suggests that individuals evaluate their assets differently depending on how these assets are mentally labeled, leading to violations of the principle of fungibility (Thaler, 1999). Heath and Soll (1996) extended this theoretical framework to budgeting and consumer behavior (i.e., mental budgeting). Here, we posit that when money is represented in its pure form as cash, the principle of fungibility will *not* be violated, resulting in no effect of prepayment. However, in many real-life situations, money is labeled or framed in a multitude of shapes and flavors (see Levav and McGraw, 2009), many of which distance it from its pure cash representation and instead associate it with more tangible resources. For example, Mazar et al. (2008) used tokens as a substitute for money to examine unethical behavior. Although in this experiment the tokens were exchanged for cash almost immediately, participants cheated to a higher degree when receiving tokens than when they received money. In a similar vein, field research shows that consumers make more purchases when they use credit cards (a more distant representation of money) than when they use cash (Feinberg, 1986; Hirschman, 1979; Prelec and Simester, 2001; Raghubir and Srivastava, 2008; Soman, 2003). These results suggest that distancing money from its pure cash representation affects the way it is perceived and ultimately utilized. Under the same logic, we argue that when money is mentally represented as something more tangible than its dollar amount, it is more likely to be perceived as a loss. Under these conditions, we predict that with such distance, people will exhibit more commitment and engagement in prepaid tasks to avoid losing the prepaid money, even when it is clearly irrational to do so.

To examine these research questions, we conducted two sets of experiments. In the first set (Studies 1A, 1B, and 1C) we focus on settings in which individuals make a tradeoff between selecting a prepaid option and returning the prepaid sum in favor of a postpaid option. In these types of situations, individuals make a direct tradeoff between keeping their prepaid money and losing it in exchange for a different payment. In the second set of experiments, we focused on situations with no tradeoff, in which individuals can work under both types of payments (or just one) at the same time, and thus do not need to

Fig. 1. (A) A sample low-spade trial from the close representation condition of the card game. The red frame represents the prepaid highlighted card and the green frame represents the highest card in that trial. (B) A sample low-spade trial from the distant money representation condition of the card game. The red frame represents the prepaid spade card and the green frame represents the highest card in that trial. The financial value of each card is presented beneath it (and was only shown to participants in the moderate representation condition). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of the article.)

sacrifice one payment for the other. The experiments in this set enabled us to directly examine whether prepayment increases motivation and engagement in a task, relative to a post-payment scheme. These two sets of experiments complement one another, as they examine different implementations and implications of a prepayment scheme.

2. Experiments 1A–1C: a trade-off between post- and prepayment

In the first set of experiments, we focus on situations in which participants face trade-offs between selecting a prepaid option in order to avoid losing a prepaid amount and forfeiting the prepaid sum to obtain a smaller, equal or greater amount at a later stage. The reason for this focus was to examine how prepayment affects individual choices, and to examine its underlying cognitive mechanism.

2.1. Experiment 1A – the effects of prepayment and monetary representation on choice

To test how various representations of prepaid and post-paid money affect individual choices, we developed a card game where the representation of money was somewhat distanced from its pure state as cash. In this card game, participants were presented with 4 cards on each trial and asked to select one card as their payment for that trial. Participants were told that each card was worth its point value in either dimes or quarters (depending on the suit of the card), and instructed to select the card with the highest value to receive the maximum payment. To endow participants with money, we paid them for specific cards up front and required them to return the payment for these cards if they were not selected. In half of the trials, the prepaid card yielded the highest monetary value, and in the remaining trials the payout was maximized by a non-prepaid card (that is, by getting paid for a different card and returning the prepaid amount for the forgone card). In order to create three levels of monetary representation, we manipulated the association between the cards and the prepayment such that some conditions included cards with close, moderate or distant representations of the cash value. In the close representation condition, the cards directly represented an amount of money, while in the distant representation condition the cards were represented as playing cards (with a translation of points-to-money that was known up front). Finally, the

moderate representation condition was a combination of the two, in which cards were depicted as playing cards but also included a label containing their monetary value (see Fig. 1 for an example).[1]

2.1.1. Method

2.1.1.1. Participants. Two hundred and six Duke University undergraduates (142 female and 64 male; mean age = 25.3, SD = 9.1) participated in the experiment for a maximum payment of $7.25 (based on their choices in the game).

2.1.1.2. Design and procedure. The experiment included a computerized card game that was presented in a $2 \times 3 \times 2$ factorial design. In each of 7 trials, participants were presented with a set of 4 face-up cards and instructed to select one, with their selections determining their payment.[2] Participants were randomly assigned to one of 6 (2×3) between-subjects conditions that differed in the payment timing (prepayment vs. post-payment) and the level of money representation (close, moderate, or distant).

In the post-payment condition, the experimenter paid participants for the cards they selected (according to their point value) at the end of the game. In the prepayment condition, the value of the four spade cards was prepaid before the start of the game (a total amount of $3.25). Participants in the prepayment conditions were then told that they would be paid for their selections, but if they did not select the four spade cards, they would be required to pay back the monetary value of these unselected cards. At the end of the game, the experimenter paid them for the cards they selected and collected money back for the unselected spade cards.

Each of these 2 payment timing conditions included 2 types of within-subjects trials that differed in the financial value ranking of the *target* (prepaid) card relative to the other cards in the set. In the high-spade trials, the financial value of the target card (i.e., the spade card) was higher than the financial value of all the other non-target cards (i.e., the non-spade cards). In the low-spade trials, the financial value of the target card was lower than the financial value of the highest non-target card. We used 2 high-spade trials and 2 low-spade trials as the basis for the experiment (and for data analysis) in addition to 3 filler trials (which did not include a target card) for a total of 7 trials.

Finally, the experiment included 3 between-subjects money representation conditions. To retain the purest possible representation of money within the constraints of this game, participants in the close condition were presented with amounts of money denoting the cards' value in US dollars. In this close condition, the prepaid spades were replaced with highlighted cards, equivalent in magnitude (as seen in Fig. 1A). To shift money farther from its pure representation in the moderate and distant conditions, participants were presented with playing cards (see a sample trial in Fig. 1B) and were told that club, heart, and diamond cards were worth their point value in dimes (e.g., a 3 of hearts was worth 3 dimes, or 30 cents), whereas spades were worth their value in quarters (e.g., a 3 of spades was worth 3 quarters, or 75 cents). In the moderate condition, labels stating the monetary value of each card were noted underneath each card, whereas in the distant condition the representation of payment had to be calculated with the point value of the cards. Pretesting confirmed that participants had no trouble calculating the value of the cards in all three conditions. Finally, in both the moderate and distant conditions, participants were told that four spade cards (Ace, Three, Four, and Five of spades) were randomly placed in the sets of cards. While all participants saw the same combinations of cards in each trial, the order of the trials and the order of the cards in each trial were randomly assigned to each participant.

Our primary interest was in whether the tendency in the prepayment condition to select the target cards would remain robust even in the low-spade trials when it was sub-optimal to do so, and how this tendency would be affected by the level of monetary representation. In other words, would participants hold on to their prepaid cards even when they could gain a higher payout by choosing a different card in the set, and would this tendency increase as the representation of the cards moved farther from their pure numerical state?

2.1.2. Results and discussion

To examine the prepayment effect, we calculated the maximizing rate for each participant, which was computed as the proportion of selecting the card with the highest expected value in each set. In the post-payment condition, the average maximizing rate in the low spade trials was 0.99 (SD = 0.06) in the close representation condition, 1.0 (SD = 0.0) in the moderate representation condition, and 0.98 (SD = 0.10) in the distant representation condition. Similarly, in the high spade trials, maximizing rate was 0.95 (SD = 0.15) in the close representation condition, 0.96 (SD = 0.14) in the moderate representation condition, and 0.95 (SD = 0.15) in the distant representation condition. A similar pattern was observed in high spade trials of the prepayment condition, with maximizing rates of 1.0 (SD = 0.0), 0.98 (SD = 0.09) and 0.98 (SD = 0.10) in the close, moderate, and distant representation conditions, respectively. By contrast, in low spade trials, maximizing rate was high only in the close representation condition (0.95, SD = 0.15), but participants tended to prefer the lower prepaid card in the moderate representation (0.90, SD = 0.20) and even more so in the distant representation (0.66, SD = 0.39). These results are summarized in Fig. 2.

[1] It could be argued that the manipulation of the distance in monetary representation introduced additional complexity to the task that might confound the results. We address this alternative interpretation in Experiment 1C.

[2] The full instructions (as well as the instructions for all the experiments in this paper) are presented in the Supplementary Material.

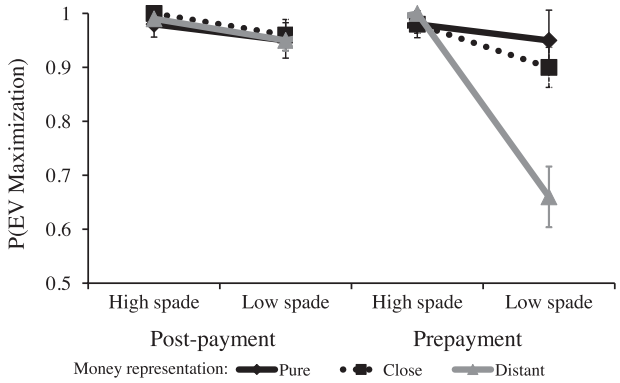

Fig. 2. Maximizing rate (selecting the card with the higher financial value) in Experiment 1A, as a function of the type of trial and the payment timing in each of the three money representation levels. Vertical lines depict standard error.

A $3 \times 2 \times 2$ repeated-measures ANOVA was conducted to test the effects of monetary representation (close, moderate, and distant), payment timing (pre- vs. post-payment), and the type of trial (high- vs. low-spade) on maximizing rate. This analysis revealed a significant main effect for both monetary representation ($F[2,200] = 5.675$, $p < 0.004$), payment timing ($F[1,200] = 9.351$, $p < 0.003$) and type of trial ($F[1,200] = 24.869$, $p < 0.0001$). In addition, significant interactions were found between monetary representation and payment timing ($F[2,200] = 5.635$, $p < 0.004$), as well as between monetary representation and the type of trial ($F[2,200] = 9.238$, $p < 0.0001$).

These results support our hypothesis that the level of monetary representation serves as a moderator for loss aversion. Rosenboim and Shavit (2012) suggest that when prepayment is given just before an experiment, it might be considered "house" money. However, in contrast with this idea and similar to Davis et al. (2010), our results show that prepayment is considered personal money even if it is given just before conducting the task. Because participants seem to have treated prepaid money as a loss, and this tendency increased as the prepayment was distanced from its pure representation, we can argue that the prepaid money was not taken as house money.[3] Still, based on Rosenboim and Shavit (2012), it could be argued that the house money effect still played a role in our experiments. If this is the case, then the prepayment effect showed robustness, as it was found even in the presence of an opposing force. Moreover, if house money did play a role here, then we would expect the prepayment effect to be even stronger if the money was provided several days (or weeks) before the experiment.

In contrast, when money was represented in its pure form, participants did not perceive it as a loss and no difference was obtained between the pre- and the post-payment conditions. Thus, in the close representation condition the fungibility principle was not violated, and the non-target card with the highest monetary value was preferred even when participants were required to forgo the prepaid options (i.e., the target cards).

2.2. Experiment 1B: boundary conditions for the prepayment effect

Experiment 1A showed that prepayment has an effect on individual choices in cases where money is distanced from its pure representation as cash. In Experiment 1B, we focused on the distant condition to test the boundary conditions of the observed prepayment effect. First, we examined whether the effect we found in Experiment 1A is an immediate consequence of the tendency to overvalue losses compared to equivalent gains (Kahneman and Tversky, 1979), or whether it is a unique effect resulting primarily from the reluctance to pay back prepaid funds that were given for a specific choice. If the prepayment effect simply stems from overvaluation of losses compared to gains, then any presence of a loss frame – and not just a loss that is directly linked to a prepaid option – should produce a similar effect. Alternatively, if the effect is more directly linked to the possibility of losing money that was given for a specific option, then the loss of prepayment for *specific* options should have a stronger effect than a *general* loss of prepayment.

Second, we examined an alternative explanation whereby the effect of prepayment could be explained by an effect of compliance, that is, participants simply do what they are paid to do. Since they were paid for selecting spades in Experiment 1, they may have selected spades – even in cases where it was not optimal – merely because they assumed that they were instructed to do so by the experimenter. If this is the case, the effect of prepayment would be expected even if people are not required to pay back the prepaid money in cases where they do not select the prepaid options. In contrast, no effect should be observed in these cases if prepayment indeed stems from loss perception that is linked to a specific option.

[3] In line with this claim, we also analyzed the number of participants in the prepayment condition who preferred the lower prepaid card over the highest card in the trial under the different levels of money representation. This analysis revealed that less than 10% of the subjects in the close representation condition did not select the highest value card at least once, as compared to 20% in the moderate representation and 48% in the distant representation. Thus, the tendency to prefer the (lower) prepaid card indeed increased as the money was farther away for its pure representation as cash.

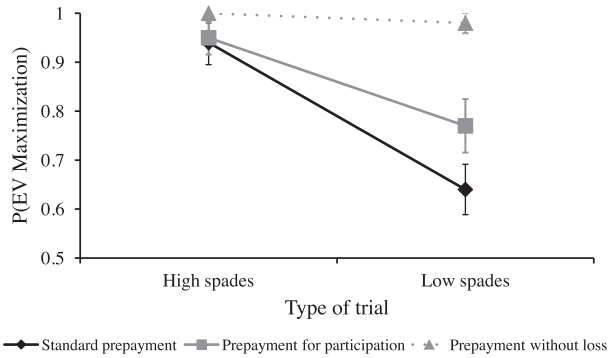

Fig. 3. Maximizing rate in Experiment 1B, as a function of the type of trial (high- or low-spade) in the standard prepayment, prepayment for participation, and prepayment without losses conditions. Vertical lines depict standard error.

To test for these potential boundary conditions, we created two additional variations of the prepayment condition and compared them to a condition replicating the original prepayment condition from the distant condition in Experiment 1A. In the "prepayment for participation" condition, participants were prepaid a general amount for participating in the experiment, not linked to specific cards. This manipulation was created to differentiate between losses of prepayment for specific choices and losses in general. In the "prepayment without losses" condition, participants were told that they would be prepaid for specific cards. However, in contrast to the other prepayment conditions, participants in this third condition were told that they would not be required to pay back the value of the unselected prepaid cards at the end of the game. Instead, participants were told that if they selected a card higher in magnitude than the prepaid spade (i.e., as in the low-spade trials), they would be paid the difference at the end of the experiment. This manipulation enabled us to explore the effect of prepayment when it is not associated with perceived losses.

2.2.1. Method

2.2.1.1. Participants. Sixty-eight undergraduates from Duke University (43 female and 25 male; mean age = 23.6 years, SD = 6.9) participated in the experiment for a maximum payment of $7.25. Participants were randomly assigned to one of three conditions: prepayment ($n = 24$), prepayment for participation ($n = 20$), and prepayment without losses ($n = 24$).

2.2.1.2. Design and procedure. Participants were presented with the same computerized card task described in Experiment 1A. The game included 3 between-subjects conditions. The "standard prepayment" condition was identical to the prepayment condition in Experiment 1 (where the value of the five spade cards was prepaid before the start of the game, and participants were required to pay back the value of the unselected spade cards at the end of the game). In the "Prepayment for participation" condition, participants were told that they would receive a participation payment of $3.25 (an amount equal to the value of all the spade cards), and would be paid an additional amount based on their selections. This participation payment was paid up front but was not matched to specific cards. In addition, to create a loss frame, participants were told that if they chose spades they would get nothing, but if they did not select the five spade cards, they would be required to pay a penalty equal to the worth of the card at the end of the game. This was done to mirror the loss component in the prepayment condition, but without the loss being associated with specific prepaid choice alternatives. Finally, the "prepayment without losses" condition was identical to the standard prepayment condition with the exception that participants were told that they would not be required to pay back the value of the unselected spade cards at the end of the game. Instead, if they selected a higher-value card (e.g., in the low-spade trials), they would be paid the difference at the end of the game, but if they selected a lower-value card (e.g., in the high-spade trials), they would not be required to pay back the difference between the selected card and the unselected prepaid card.

2.2.2. Results and discussion

A 3×2 repeated measures ANOVA was conducted to test the effects of the conditions (prepayment, prepayment for participation, and prepayment without losses) and the type of trial on maximizing behavior as the dependent variable (see Fig. 3). This analysis revealed a significant main effect for condition ($F[2,65] = 12.015$, $p < 0.0001$), and for the type of trial ($F[1,65] = 32.955$, $p < 0.0001$). In addition, a significant interaction between the two variables was found ($F[2,65] = 7.958$, $p < 0.001$).[4]

[4] Note that Experiment 2 also included a replication of the post-payment condition in Experiment 1 ($n = 25$). The results of this condition in both experiments were virtually identical. For conciseness, we have omitted this condition from the analysis. However it should be noted that no difference was observed between the post-payment and prepayment without losses conditions ($t[47] = -1.436$, $p = 0.16$), further suggesting that prepayment without losses has no effect on choices.

Simple effects analyses were conducted to further explore these main effects. An independent-sample *t*-test revealed a marginally significant effect where the maximizing rate for low-spade trials was lower in the regular-prepayment condition than in the prepayment for participation condition ($M_{\text{regular}} = 0.64$, $M_{\text{participation}} = 0.77$; $t[42] = -1.7$, $p < 0.08$). Similarly, a significant effect was observed between the standard prepayment and prepayment without losses conditions ($M_{\text{without losses}} = 0.98$; $[46] = -6.004$, $p < 0.0001$), and between the prepayment for participation and prepayment without losses conditions ($t[42] = -3.882$, $p < 0.0001$). In addition, between the two types of trials (low- and high-spade), a paired-sample *t*-test revealed a significant reduction in maximizing rate for the standard prepayment ($t[23] = -5.286$, $p < 0.0001$) and the prepayment for participation ($t[19] = -2.728$, $p < 0.02$) conditions, but not for the prepayment without losses condition ($t[23] = 1.0$, $p = 0.33$).[5]

In line with a substantial body of research, this pattern of results shows that participants are more sensitive to losses than they are to gains. They preferred to avoid losses, despite the fact that embracing these losses in low-spade trials would have led them to higher overall payoffs (Thaler et al., 1997). More importantly, however, the fact that the maximization rate in the low-spade trials was lower in the standard prepayment condition than that in the prepayment for participation condition shows that coupling prepayment with specific options is a crucial factor in producing the full range of the prepayment effect. Thus, unlike the traditional rationale of loss aversion and mental accounting, it seems that the prepayment effect is not simply the result of an increased valuation of losses compared to gains (cf. Yechiam and Hochman, 2013), but that it is also dependent on the prepayment being linked to a specific action. Finally, since no reduction in the maximization rate was found in the prepayment without losses condition, it could be argued that the possibility of losing prepaid money is a prerequisite for the prepayment effect. This eliminates the possibility that the results in Experiment 1A could be explained by compliance to the experimenter's directions.

2.3. Experiment 1C – examining the underlying cognitive processes

The results of Experiments 1A and 1B show that when people are faced with a tradeoff between two payment schemes, prepayment (and prepayment for specific options, in particular) has an effect on their choices, and individuals tend to forfeit potential gains in order to avoid losses —even when they sacrifice a higher payment in the process. Still, it could be argued that this pattern of results merely captures a reduction in performance in the prepayment condition due to the fact that this condition is more complex and requires additional information processing (e.g., calculations of losses vs. gains). As previous research suggests, this may lead to impaired decision making (e.g., Ayal and Zakay, 2009; Grosskopf et al., 2006) and may explain why prepayment has no effect when it is not associated with losses (as is seen in the prepayment without losses condition in Experiment 1B). Thus, in Experiment 1C we examined how mental effort (indexed by response time) affects performance in order to understand the processes underlying the effect of prepayment. Specifically, we looked at whether prepayment increases information processing (via longer response times), and if so, whether this increase results in an increase in judgment errors.

To test this, we created a new prepayment condition in which all cards in all trials of the game were prepaid, and participants were told that they would get to keep the value of the selected card in each trial but would have to pay back the value of every unselected card at the end of the experiment. This condition was compared to the original post-payment condition, in which we paid participants at the end of the game based on their selections. This manipulation was used as a way to obtain a better measure of choice error by creating an objectively identical task in both conditions (i.e., selecting the card with the highest value), that is framed either as pertaining only to potential gains (the postpaid condition) or to potential losses (the prepaid condition). Since these conditions represent an objectively identical task, we would expect longer response times and more judgmental errors in the prepayment condition if prepayment leads to increased judgmental errors (via increased information processing).

2.3.1. Method

2.3.1.1. Participants. Fifty undergraduates from the Technion – Israel Institute of Technology (23 female and 27 male; mean age = 23.5 years, SD = 2.35) participated in the experiment for a maximum payment of 31 Israeli Shekel (which was equivalent to $7.25 at the time). Participants were randomly assigned to one of two conditions: post-payment ($n = 25$) and overall prepayment ($n = 25$).

2.3.1.2. Design and procedure. Participants were presented with the same computerized card task used in Experiments 1A and 1B. The game included 2 between-subjects conditions of payment timing. The post-payment condition was identical to the distant representation condition in Experiment 1A. However, in the overall-prepayment condition, the value of all the cards in all the trials was prepaid, and participants were told that at the end of the game they could only keep the value of the selected card, and would have to refund the value of all the unselected cards. Importantly, participants in the overall prepayment condition were informed that the software would automatically calculate the amount to be refunded in each

[5] In line with the results of Experiment 1A, we found that less than 5% of the participants in the prepayment without losses condition selected the prepaid card in the low-spade trials at least once, compared to 40% in the general prepayment condition and 62% in the standard prepayment condition. This pattern of results further suggests that our results do not stem from a few outliers, but rather from the increased tendency to prefer prepaid options.

trial, and they should not worry about these calculations. Participants in both conditions were instructed to make their selections as quickly as possible, and told that response time would be measured by the software.

2.3.2. Results and discussion

The average response time per trial was 4.24 s (SD = 1.8) in the post-payment condition, and 6.42 s (SD = 4.9)[6] in the overall prepayment condition, a difference that was found to be significant in an independent-samples *t*-test ($t[48] = 2.023$, $p < 0.05$). Since response time is considered an indicator of the amount of mental effort exerted (e.g., Ariely and Zakay, 2001; Ayal and Hochman, 2009; Hochman et al., 2010; Payne et al., 1993) this pattern of results suggests that participants in the overall-prepayment condition invested more effort (i.e., deliberate reasoning) in the task compared to participants in the post-payment condition.

More importantly, analyzing the choice data revealed that the maximizing rate was 0.83 (SD = 0.24) in the post-payment condition and 0.92 (SD = 0.15) in the overall prepayment condition. An independent *t*-test revealed that this difference was not significant ($t[48] = 1.577$, $p = 0.12$), suggesting that performance in the overall-prepayment condition was not impaired. If anything, performance even improved (in terms of expected value) relative to the post-payment condition, although this improvement was only close to marginal significance.[7] Since response time was longer in the overall-prepayment condition, these results suggest that the motivation to avoid losing endowed money was greater than the motivation to earn unpaid fees. Importantly, in line with findings where losses increase performance (see review in Yechiam and Hochman, 2013), prepayment did not lead to more judgment errors; instead, it focused participants on the task and motivated them to perform better. Presumably, as suggested by Yechiam and Hochman (2012), a global frame of losses increases the global attention allocated to a task, resulting in increased performance. However, it could be that a local loss frame (as in Experiments 1A and 1B) only increases selective attention, which under certain situations might lead to suboptimal behavior.

3. Experiments 2A and 2B: prepayments without trade-offs

In the first set of experiments, we showed that when people face a tradeoff between selecting a prepaid and a postpaid option, they tend to stay with the prepaid option even when it is suboptimal to do so. In addition, we showed that this prepayment effect might increase motivation and engagement in the task. In the second set of experiments, we further investigated the prepayment effect in settings where no tradeoff is required between the prepaid and the postpaid options, and people can give up the prepaid amount without gaining any post-paid fees in return, or when gaining post-payment can be obtained together with prepayment fees. These settings enabled us to test whether people are more engaged and invest more time and effort in prepaid tasks in a more natural environment.

3.1. Experiment 2A: online questionnaires

Experiment 2A allowed us to explore whether prepayment affects motivation and involvement while participants engaged in both the prepaid and the postpaid tasks. In this experiment, we emailed participants two links on every day of one week, and asked them to complete the questionnaires in exchange for (prepaid and post-paid) compensation. For the post-paid questionnaires, participants were told that they would be paid at the end of the week for each day they completed the questionnaire. For the prepaid questionnaires, participants were paid up front for completing all seven questionnaires, but were told that if they chose to not to complete any of the prepaid questionnaires, they would be required to return the payments for those incomplete questionnaires at the end of the week. For example, if a participant chose to answer only 3 (out of the 7) post-paid questionnaires and 5 (out of 7) prepaid questionnaires, she would be paid for those 3 post-paid questionnaires at the end of the experiment, and would be required to pay back the money for two prepaid questionnaires. Since there were no direct tradeoffs between completing the two types of questionnaires, participants could choose to complete both types on any given day.

3.1.1. Method

3.1.1.1. Participants. Twenty-five undergraduates from the Technion (12 females and 13 male; mean age 24.1 years, SD = 2.62) participated in the experiment for a maximum payment of 112 New Israeli Shekel ($1 ≈ 4.31 NIS).

3.1.1.2. Design and procedure. Participants were presented with two web-based questionnaires. Questionnaire A included 26 questions that were randomly selected from the short 24-item Rational-Experiential Inventory (Pacini and Epstein, 1999)

[6] In light of the relatively high SD in the prepayment condition, we examined the effect of possible outliers on the pattern of results. Indeed, we found one participant (Participant 11) whose response time was 2.5 SD higher than the average, and one participant (Participant 4) whose response time was more than 1 SD lower than the average response time. When we removed these two possible outliers, the difference in response time between the two conditions reduced by about half a second (and the standard deviation reduced to 2.30 s), but the pattern of results remained the same and the difference remained significant ($p < 0.05$).

[7] The marginally significant difference in performance between the two conditions, in favor of the prepayment condition, may reflect a ceiling effect for performance given the simplicity of the task. This difference would be expected to increase in more complex tasks that would not be prone to a ceiling effect.

and the generalizable scale of propensity to plan (Lynch et al., 2010), while Questionnaire B included 26 questions that were randomly selected from the self-monitoring of expressive behavior scale (Snyder, 1974), and the battery of lottery gambles from a risk aversion scale (Holt and Laury, 2002). In an independent pilot study, the two types of questionnaires were found to be virtually identical in terms of the amount of time required to complete them.

Participants received links to two questionnaires via email each day for a week, and were told that they should complete both types on each day. For the Type A questionnaires, they were paid 7 NIS for each day they completed the questionnaire, while for the Type B they were paid 9 NIS. However, the seven payments for the 7 NIS questionnaire were paid up front and the payments for the 9 NIS were paid at the end of the experiment. Importantly, the two types of questionnaires were counterbalanced such that half of the participants were prepaid for the first questionnaire and post-paid for the second questionnaire, while for the other half this order was reversed. Thus, the only differences between the questionnaires were the content of the questionnaire (which was counterbalanced), and the amount and timing of the payment.

Participants received 49 NIS for the prepaid questionnaires in a preliminary meeting that took place a few days before the experiment began, and were informed that they would be required to return the money for questionnaires they failed to complete. They were also informed that they would receive the payment for the post-paid questionnaire at a debriefing session that took place a few days after the experiment was completed. Participants signed a contract specifying the terms of the experiment and provided their email address.

Links to both questionnaires were sent by email to participants via the Technion experiments website. The email also included basic instructions and explained the purpose of the questionnaires. Participants were asked to answer the questions to the best of their ability. A few days after the experiment ended, participants were invited to the lab for a debriefing session, in which they were asked to pay back the money for incomplete prepaid questionnaires and received compensation for the post-paid questionnaires they completed. Finally, to get some insight into the effect of prepayment, participants were asked to complete a short survey about the two questionnaires that was comprised of three questions: (a) Which questionnaire (pre- vs. post-paid) was harder to fill out, (b) under which payment condition they were more motivated to work and invest more effort, and (c) how interesting each survey was (on a scale of 1 to 7, where 1 is not at all interesting and 7 is highly interesting).

3.1.2. Results and discussion

All participants chose to complete both questionnaires on each day, presumably to maximize their total earnings. Thus, in contrast with our initial hypothesis, it appears that participants did not prefer to work under a prepaid contract more than under a post-paid contract. Still, as in Experiment 1C, the results showed that participants invested more time in the questionnaires that were prepaid. On average, participants invested 9.97 min (SD = 3.80) filling out the prepaid questionnaires, but only 7.26 min (SD = 2.80) on the post-paid questionnaires. A paired-sample t-test revealed a significant difference ($t[25] = 3.174$, $p < 0.005$); that is, participants invested 2.71 more minutes on average (an increase of about 38%) to complete the questionnaires that were prepaid, even when they were paid more for post-paid questionnaires, and even when both questionnaires were identical. Notably, this effect persisted despite the fact that participants were only paid to complete the surveys (and not for specific responses or the amount of time or effort that they invested completing them).

The results of the debriefing survey provided important insights into the psychological effects of prepayment. Seventy nine percent of the participants (SD = 41.5) found the prepaid questionnaire to be harder to complete than the post-paid questionnaire, regardless of whether this survey was Questionnaire A or B. A one-sample t-test revealed that this percentage is significantly different from chance, or 50% ($t(23) = 3.44$, $p < 0.005$). Similarly, and in line with our response time results, 71% (SD = 46.4) of the participants reported that they were more motivated and invested more effort in completing the prepaid questionnaire, irrespective of whether they were prepaid for Questionnaire A or B ($t(23) = 2.20$, $p < 0.05$). Finally, participants found the prepaid questionnaire to be more interesting ($M_{\text{rating}} = 4.5$, SD = 1.5) than the post-paid questionnaire ($M_{\text{rating}} = 3.5$, SD = 1.8), suggesting that the extent to which a certain task is perceived as interesting might be directly affected by the payment scheme, or indirectly as a result from the fact that more effort is invested in the task (cf. Olivola and Shafir, 2013). A paired-sample t-test revealed that this difference is significant ($t(23) = 2.26$, $p < 0.05$).

In Experiments 1A and 1B, we showed that prepayment affects choices and preferences. Here, we found that even without a direct tradeoff, participants invested more time and effort in completing the prepaid questionnaires. It appears that introducing prepayment may have profound effects on motivation and engagement in a task, as well as on the way people perceive that task (both in terms of difficulty and interest). Arguably, cognitive dissonance could provide an alternative explanation. For example, Festinger and Carlsmith's (1959) classic experiment showed that when asked to rate a boring task, participants who received \$1 to convince others that the task was interesting rated it as more enjoyable than participants who received \$20. According to the authors, the participants who received little compensation experienced dissonance between the conflicting cognition of telling others that the task was interesting while they had actually experienced it as boring, and their statements could not be justified by a measly payment (whereas participants who received \$20 could claim that they just said it for the money). As a result, the low-paid participants changed their opinion about the task so that their beliefs would align with their previous actions. Similarly, it could be argued that in the current task, participants rated the prepaid questionnaire as more interesting and engaging as an internal justification to explain why they were willing to work for a smaller amount. However, this interpretation is not supported by the increased response time to the prepaid questionnaires, which serves as an objective measure of effort and engagement and thus could not be explained post hoc as an internal justification.

3.2. Experiment 2B: signature contractors

In Experiment 2B, we went out to the streets and asked people to work as signature contractors for half an hour. For each signature they gathered, we paid them 50 cents (with a maximum of 20 signatures in the allotted time). In the post-paid condition, they first collected signatures and were paid afterward according to the number collected. In the prepaid condition, we paid them $10 up front and told them that they would be required to return 50 cents for every signature they failed to acquire out of the 20 for which they were paid. Thus, we were able to see whether prepayment increases motivation to perform in order to avoid the loss of the prepaid money in a real-life setting.

3.2.1. Method

3.2.1.1. Participants. One hundred forty one adults (73 females and 68 male; mean age 28.0 years, SD = 10.80) from Durham, NC participated in the experiment for a maximum payment of $10. Selection of participants was based on their presence at the experiment location.

3.2.1.2. Design and procedure. Participants were individually approached by the experimenter in a public setting and were asked to help collect petition signatures for half an hour, for which they would be paid 50 cents per signature.

Two petitions were used. The "green schools" petition highlighted the importance of eco-friendly environment, and called for the government of the United States to ensure that reconstructed schools conform to the U.S. Green Building Councils Leadership in Energy and Environmental Design (LEED) for Schools standards. In contrast, the "global warming" petition argued against the disadvantages of global warming, and called the United States government to reject the global warming agreement that was written in Kyoto, Japan in December, 1997, and other similar proposals. Note that because most of Durham's local passersby were educated individuals with high environmental awareness, the green schools petition was thought to be favorable whereas the global warming petition was considered more controversial. The two petitions were designed to be highly similar in length and appearance, differing only in content. Both petitions contained a paragraph at the top of the page explaining their aim, followed by 20 lines for signatures and personal information.

The two types of petitions were administered under two between-subjects payment conditions. In the post-payment conditions, the experimenter told participants that she would meet them after 30 min had elapsed to count the number of collected signatures, and would then pay them 50 cents for each signature they collected. In the prepayment condition, participants received $10 from the experimenter, and were told that in half an hour they would meet to count the number of signatures they collected. For each signature short of the 20 possible signatures, they would pay her back 50 cents. The allocation of the petition type and payment condition was random. This random mechanism resulted in 39 participants (21 females and 18 male; mean age 27.3 years, SD = 9.91) in the post-payment condition and 32 participants (23 females and 9 male; mean age 29.6 years, SD = 11.30) in the prepayment condition of the green schools petition, and 37 participants (14 females and 23 male; mean age 26.50 years, SD = 11.40) in the post-payment condition and 33 participants (15 females and 18 male; mean age 27.90 years, SD = 9.70) in the prepayment condition of the global warming petition.

The experimenter followed the same wording of her request for all participants, and handed out petitions in a random order that was determined before interacting with participants. After the data had been collected, participants were informed about the true nature of the petitions.

3.2.2. Results and discussion

For the (favorable) green schools petition, we found no significant difference between the percentage of signatures collected (out of the possible 20 signatures) between the post-payment (78%, SD = 26) and prepayment conditions (77%, SD = 30). However, for the (more controversial) global warming petition, we found that people in the post-payment condition collected 74% (SD = 31) of the signatures on average, while 92% (SD = 20) of the signatures were collected in the prepayment condition (see Fig. 4). A 2 (petition type) × 2 (payment condition) ANOVA revealed a significant interaction between the two variables ($F(1,141) = 4.05$, $p < 0.05$). In addition, planned contrasts revealed a significant difference between the two payment conditions in the controversial petition ($t(76) = 2.983$, $p < 0.005$), but not for the favorable petition ($t(76) = -.116$, $p = 0.908$). Thus, our results suggest that even when no tradeoff exists between the two payment schemes, prepayment improves rather than impedes performance. Participants in the prepayment condition were much closer than those in the post-payment condition to meet their goal and collect more signatures. However, this effect was more prominent (and only significant) for the controversial petition. Potentially, this difference could result from an increase in motivation to avoid losing the prepaid money and increased involvement in the task.

This pattern of results suggests that the prepayment effect might be more robust in tasks that require effort and where the primary motivation to perform is monetary (as might be expected when collecting signatures for a more controversial petition (cf. Garbarino and Edell, 1997; Kivetz, 2003; Rosenboim et al., 2012). Thus, it seems that prepayment should be particularly effective in more mundane and banal situations in which motivation is primarily external (Centers and Bugental, 1966).

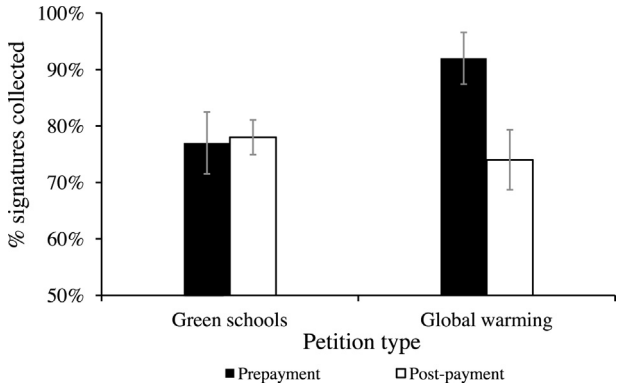

Fig. 4. Percentage of signatures collected out of the 20 possible signatures in Experiment 2B, as a function of the petition type and the payment timing. Vertical lines depict standard errors.

4. General discussion

Despite three decades of fruitful research on the effects of losses in risky choices (see, for example, Benartzi and Thaler, 1995; Birnbaum and Bahra, 2007; Camerer, 2005; Erev et al., 2008; Hochman and Yechiam, 2011; Kahneman and Tversky, 1979; Rabin, 2000; Thaler, 1980; Thaler et al., 1997; Tversky and Kahneman, 1991) and in riskless choices (Carmon and Ariely, 2000; Casey, 1995; Johnson et al., 2007; Kahneman et al., 1990; Morewedge et al., 2009; Thaler, 1980), scholars are still debating the conditions under which money exhibits loss aversion (e.g., Bateman et al., 2005; Novemsky and Kahneman, 2005). These conditions are important to better understand how individuals handle their money and make financial decisions. In this paper, we contribute to the debate by both examining the psychological processes of the prepayment effect and exploring important moderators of loss aversion with money.

In Experiment 1A, we showed that prepayment is perceived as a loss (and thus affects individual choices) when it is more distant from its pure representation as cash money. When participants were required to select cards that represented stacks of money (i.e., closest to money in its numeric form), the possibility of losing the prepaid money had no effect on choice behavior. This suggests that money that is mentally represented as cash is not subject to loss aversion. However, when the exact same earnings were presented in ways that were perceived as more distant from their physical form in dollars and cents, the effect of prepayment increased – suggesting that loss aversion for money increases with the dilution of its pure representation.

Experiment 1B showed that the prepayment effect was completely eliminated when prepayment was not associated with losses, demonstrating that the prepayment effect directly results from the fact that the money is perceived as a loss that should be paid back, and not merely as a consequence of compliance. In line with this assertion, framing the prepayment as an overall loss (e.g., a prepaid participation wage) shifted the reference point such that participants were reluctant to part with their prepayment despite the fact that in some cases it would have led them to higher gains (for related results in risky choices see Davis et al., 2010; Rosenboim and Shavit, 2012). However, when the prepayment (and the loss) was associated with a specific option, the shift in reference point was much more extreme and the prepayment effect was stronger. This result suggests that the prepayment effect is not simply a direct implication of the fact that losses increase the subjective evaluation of outcomes. Rather, losses affect other aspects of the decision process, such as the amount of attention allocated to the task (Hochman and Yechiam, 2011; Yechiam and Hochman, 2012) and the weight that individuals give to different aspects of the decision (e.g., Carmon and Ariely, 2000; Casey, 1995; Johnson et al., 2007; Morewedge et al., 2009).

Finally, in Experiments 1C and 2A, we looked at response times and maximizing behavior to demonstrate that the prepayment effect is not a result of judgment error. Rather, whether prepayment required a tradeoff between the prepaid and postpaid option, or whether the two payment schemes were administered concurrently, prepayment increased motivation and investment in the task, and was directly related to mental effort. Similarly, Experiments 2A and 2B showed that individuals invest more effort working under a prepayment scheme, especially when the work was less desirable.

4.1. Methodological issues and limitations

As in many social science experiments, the compensation amounts used in the current experiments were limited. Thus, although participants in the prepayment conditions in all experiments obtained a significantly lower payment (about 20–30%) than those in the post-payment conditions, these differences did not amount to a high difference (in USD) in absolute terms. It would be interesting to see in future research how higher amounts of money might interact with the prepayment effect, particularly because household income and expenses generally include greater amounts of money. In particular, it would be interesting to see how sensitive the prepayment effect is to differences between prepaid and postpaid fees under different payment magnitudes.

Another limitation of the current paper is the fact that in the second set of experiments, we did not observe any difference in individual choices. Rather, we focused on how prepayment affects involvement and engagement in a task. It would be beneficial for future research to force participants to choose one type of payment scheme and examine their preferences as well as the scheme's effectiveness over time.

Despite these limitations, our results bare methodological implications. Specifically, previous research suggests that prepaying participants in the lab affects their risk-taking behavior (e.g., Davis et al., 2010; Rosenboim and Shavit, 2012). We extend these findings and show that prepayment schemes may affect not only risk behavior, but also a variety of behavior in riskless domains. Thus, similar to these lines of research, our results highlight the importance of the timing of payment within the general duration of the experiment in the experimental design, especially when researchers aim to create a more natural environment in their financial decision making studies (cf. Rosenboim and Shavit, 2012).

In a similar vein, our finding that prepayment increases involvement and engagement in a task suggests that prepayment can be an effective tool to motivate participants in tasks that require effort and increased engagement (Kivetz, 2003; Rosenboim et al., 2012).

4.2. Prepayment in finance

Our results demonstrate that there is no simple answer to the question of whether loss aversion exists for money. Money that is not associated with a specific option and remains close to its pure cash representation is treated as a fungible resource and is thus not subject to loss aversion. Presumably, this is due to the fact that this form of money is implicitly categorized for the sole purpose of transaction (cf., Novemsky and Kahneman, 2005). However, money that is distanced from its pure representation and linked to specific options is categorized as a more concrete and tangible good. As a result, the reduction of money in these situations is perceived as a reduction from the status quo, and is therefore subject to loss aversion. This effect is consistent with the rationale of mental accounting (Thaler, 1980) and mental budgeting (Heath and Soll, 1996), and provides yet another case where representational form moderates how money is both perceived and treated (cf., Mazar et al., 2008).

The moderating effect of representation on the effect of monetary losses has important implications for the financial world. Returning to our earlier example of the CEO of a financial services company, it seems that rather than merely writing checks for prepaid commission, you could bolster the attachment to your products by distancing payment from its pure form and offering rewards that are symbolic of cash but slightly different – in the form of "vouchers" for example, that could be traded for money or prepaid gift-cards, which effectively serve as a more distant representation of money (Spiller, 2011).

Additionally, our results suggest ways in which payment structures can be used to help individuals better manage their finances. For example, the advantage of developing savings programs to aid financially challenged individuals (such as matching accounts) was demonstrated by Grinstein-Weiss and colleagues (e.g., Grinstein-Weiss and Sherraden, 2006; Loibl et al., 2010). In line with this view, consider the following "savings inducement plan," designed to help individuals allocate more money to their savings accounts by giving them a matching fund of up to $3000, for example, if they save $3000 in six months. In a typical (post-payment) matching program, people first save $3000 and are then matched with an additional $3000. However, in the prepayment version of such a plan, people could be endowed in advance with a sum of $3000, deposited into a "matching account." During the same 6-month period of personal savings, each dollar they save is matched by the matching account. However, if they fail to save an amount equivalent to the sum deposited in their matching account, they would be required to pay back the difference. Our results suggest that prepaying the matched savings, and requiring a refund if people fail to save an equivalent amount, might lead individuals to save more money and think more carefully about their spending.

It should be noted, however, that our results also suggest that endowing individuals with money can irrationally lead to suboptimal behavior, and can prevent their maximization of utility. Thus, caution should be exercised when utilizing this "power of prepaid money."

Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jebo.2014.01.014.

References

Ariely, D., Zakay, D., 2001. A timely account of the role of duration in decision making. Acta Psychologica 108, 187–207.
Ariely, D., Huber, J., Wertenbroch, K., 2005. When do losses loom larger than gains? Journal of Marketing Research XLII, 134–138.
Ayal, S., Hochman, G., 2009. Ignorance or integration: the cognitive processes underlying choice behavior. Journal of Behavioral Decision Making 22, 455–474.
Ayal, S., Zakay, D., 2009. The perceived diversity heuristic: the case of pseudodiversity. Journal of Personality and Social Psychology 96, 559–573.
Bateman, I., Kahneman, D., Munro, A., Starmer, C., Sugden, R., 2005. Testing competing models of loss aversion: an adversarial collaboration. Journal of Public Economy 89, 1561–1580.
Bateman, I., Munro, A., Rhodes, B., Starmer, C., Sugden, R., 1997. A test of the theory of reference-dependent preferences. Quarterly Journal of Economics 112, 479–505.
Benartzi, S., Thaler, R.H., 1995. Myopic loss aversion and the equity premium puzzle. Quarterly Journal of Economics 110, 73–92.

Birnbaum, M.H., Bahra, J.P., 2007. Gain–loss separability and coalescing in risky decision making. Management Science 53, 1016–1028.
Camerer, C., 2005. Three cheers – psychological, theoretical, empirical – for loss aversion. Journal of Marketing Research 42, 129–133.
Carmon, Z., Ariely, D., 2000. Focusing on the forgone: why value can appear so different to buyers and sellers. Journal of Consumer Research 27, 360–370.
Casey, J.T., 1995. Predicting buyer–seller pricing disparities. Management Science 41, 979–999.
Centers, R., Bugental, D.E., 1966. Intrinsic and extrinsic jon motivations among different segments of the working population. Journal of Applied Psychology 50, 193–197.
Davis, L.R., Joyce, B.P., Roelofs, M.R., 2010. My money or yours: house money payment effects. Experimental Economics 13, 189–205.
Erev, I., Ert, E., Yechiam, E., 2008. Loss aversion, diminishing sensitivity, and the effect of experience on repeated decisions. Journal of Behavioral Decision Making 21, 575–597.
Feinberg, R.A., 1986. Credit cards as spending facilitating stimuli: a conditioning interpretation. Journal of Consumer Research 13, 348–356.
Festinger, L., Carlsmith, J.M., 1959. Cognitive consequences of forced compliance. Journal of Abnormal and Social Psychology 58, 203–210.
Garbarino, E.C., Edell, J.A., 1997. Cognitive effort, affect and choice. Journal of Consumer Research 24, 147–158.
Grinstein-Weiss, M., Sherraden, M., 2006. Racial differences in performance in a matched saving program. Journal of Income Distribution 13, 98–111.
Grosskopf, B., Erev, I., Yechiam, E., 2006. Foregone with the wind: indirect payoff information and its implications for choice. International Journal of Game Theory 34, 285–302.
Heath, C., Soll, J., 1996. Mental budgeting and consumer decisions. Journal of Consumer Research 23, 40–52.
Hirschman, E.C., 1979. Differences in consumer purchase behavior by credit card payment system. Journal of Consumer Research 6, 58–66.
Hochman, G., Yechiam, E., 2011. Loss aversion in the eye and in the heart: the Autonomic Nervous System's responses to losses. Journal of Behavioral Decision Making 24, 140–156.
Hochman, G., Ayal, S., Glöckner, A., 2010. Physiological arousal in processing recognition information: ignoring or integrating cognitive cues? Judgment and Decision Making 5, 285–299.
Holt, C.A., Laury, S.K., 2002. Risk aversion and incentive effects. American Economic Review 92, 1644–1655.
Idson, L.C., Liberman, N., Higgins, E.T., 2000. Distinguishing gains from nonlosses and losses from nongains: a regulatory focus perspective on hedonic intensity. Journal of Experimental Social Psychology 36, 252–274.
Johnson, E.J., Häubel, G., Keinan, A., 2007. Aspects of endowment: a query theory of value construction. Journal of Experimental Psychology: Learning, Memory and Cognition 33, 461–474.
Kahneman, D., Sugden, R., 2005. Experienced utility as a standard of policy evaluation. Environmental and Resource Economics 32, 161–181.
Kahneman, D., Tversky, A., 1979. Prospect theory: an analysis of decision under risk. Econometrica 47, 263–291.
Kahneman, D., Knetsch, J.L., Thaler, R.H., 1990. Experimental tests of the endowment effect and the Coase Theorem. Journal of Political Economy XCVII, 1325–1348.
Kahneman, D., Knetsch, J.L., Thaler, R.H., 1991. The endowment effect, loss aversion, and status quo bias. Journal of Economic Perspectives 5, 193–206.
Kivetz, R., 2003. The effects of effort and intrinsic motivation on risky choice. Marketing Science 22, 477–502.
Kőszegi, B., Rabin, M., 2006. A model of reference-dependent preferences. Quarterly Journal of Economics 121, 1133–1165.
Levav, J., McGraw, P., 2009. Emotional accounting: how feelings about money influence consumer choice. Journal of Marketing Research 46, 66–80.
Loibl, C., Grinstein-Weiss, M., Zhan, M., Red, B.B., 2010. More than a penny saved: long-term changes in behavior among savings program participants. Journal of Consumer Affairs 44, 98–126.
Lynch Jr., J.G., Netemeyer, R., Spiller, S.A., Zammit, A., 2010. A generalizable scale of propensity to plan: the long and the short of planning for time and money. Journal of Consumer Research 37, 108–128.
Mazar, N., Amir, O., Ariely, D., 2008. The dishonesty of honest people: a theory of self-concept maintenance. Journal of Marketing Research 45, 633–644.
Morewedge, C.K., Shu, L.L., Gilbert, D.T., 2009. Bad riddance or good rubbish? Ownership and not loss aversion causes the endowment effect. Journal of Experimental Social Psychology 45, 947–951.
Novemsky, N., Kahneman, D., 2005. The boundaries of loss aversion. Journal of Marketing Research XLII, 119–128.
Olivola, C.Y., Shafir, E., 2013. The martyrdom effect: when pain and effort increase prosocial contributions. Journal of Behavioral Decision Making 26, 91–105.
Pacini, R., Epstein, S., 1999. The relation of rational and experiential information processing styles to personality, basic beliefs, and the ratio-bias phenomenon. Journal of Personality and Social Psychology 76, 972–987.
Payne, J.W., Bettman, J.R., Johnson, E.J., 1993. The Adaptive Decision Maker. Cambridge University Press, Cambridge.
Prelec, D., Simester, D., 2001. Always leave home without it: a further investigation of the credit-card effect on willingness to pay. Marketing Letters 12, 5–12.
Rabin, M., 2000. Risk aversion and expected-utility theory: a calibration theorem. Econometrica 68, 1281–1292.
Raghubir, P., Srivastava, J., 2008. Monopoly money: the effect of payment coupling and form on spending behavior. Journal of Experimental Psychology: Applied 14, 213–225.
Rosenboim, M., Shavit, T., 2012. Whose money is it anyway? Using prepaid incentives in experimental economics to create a natural environment. Experimental Economics 15, 145–157.
Rosenboim, M., Shavit, T., Cohen, C., 2012. Do bidders require a monetary premium for cognitive effort in an auction? Journal of Socio Economics 42, 99–105.
Soman, D., 2003. The effect of payment transparency on consumption: quasi-experiments from the field. Marketing Letters 14, 173–183.
Spiller, S.A., 2011. Opportunity cost consideration. Journal of Consumer Research 38, 595–610.
Snyder, M., 1974. Self-monitoring of expressive behavior. Journal of Personality and Social Psychology 30, 526–537.
Thaler, R.H., 1980. Toward a positive theory of consumer choice. Journal of Economic Behavior and Organization 1, 39–60.
Thaler, R.H., 1999. Mental accounting matters. Journal of Behavioral Decision Making 12, 183–206.
Thaler, R.H., Johnson, E., 1990. Gambling with the house money and trying to break even: the effects of prior outcomes on risky choice. Management Science 36, 643–660.
Thaler, R.H., Tversky, A., Kahneman, D., Schwartz, A., 1997. The effect of myopia and loss aversion on risk taking: an experimental test. Quarterly Journal of Economics 112, 647–661.
Tversky, A., Kahneman, D., 1991. Loss aversion in riskless choice: a reference-dependent model. Quarterly Journal of Economics 106, 1039–1061.
Yechiam, E., Hochman, G., 2013. Losses as modulators of attention: review and analysis of the unique effects of losses over gains. Psychological Bulletin 139, 497–518.