Self Control and Liquidity: How to Design a Commitment Contract

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Abstract: If individuals have self-control problems that lead them to spend money that they had previously planned to save, they may take up commitment contracts that restrict their spending. We experimentally investigate how the demand for commitment contracts is affected by contract design. Each experimental subject receives an endowment of money and divides that money between a liquid account, which permits unrestricted withdrawals, and a commitment account, which imposes a penalty on early withdrawals. The features of the liquid account are the same for all subjects, but the features of the commitment account are randomized across subjects. The commitment account that disallows early withdrawals—the most illiquid commitment account—attracts *more* money than any other commitment account. We extend the theoretical work of Amador, Werning, and Angeletos (2006) to show that the pattern of experimental commitment account allocations arises in a leading model of intertemporal choice under natural assumptions.

Keywords: quasi-hyperbolic discounting, present bias, sophistication, naivite, commitment, flexibility, savings, contract design

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In 2011, U.S. households collectively held more than $9 trillion in assets in defined contribution savings plans (such as 401(k) plans) and IRAs.[1] An important feature of these financial accounts is that they are illiquid. Withdrawals before the age of 59½ incur a 10% tax penalty (in addition to normal income taxes).[2] This particular instance of illiquidity is the result of a policy design choice, but there is little empirical research on how different types of illiquidity influence household decisions.

According to standard economic theory, illiquidity is (privately) undesirable because it reduces an agent's flexibility. If people have self-control problems, however, an illiquid account may have the benefit of serving as a commitment device that helps them avoid the temptation to spend money when they had previously planned to save it. In this paper, we adopt an experimental approach to ask how the demand for such commitment devices is affected by their design features, especially their degree of illiquidity.

Previous research indicates that people indeed suffer from self-control problems – that is, they intend to make choices that carefully weigh both short-run and long-run costs and benefits, but in the decision-making moment they place disproportionate weight on immediate costs and benefits.[3] If individuals are sophisticated in the sense that they correctly anticipate their future self-control problems (Strotz, 1955; O'Donoghue and Rabin, 1999), they may be willing to take up commitment devices. Today, they may agree to forgo payments or other rewards if they fail in the future to execute their current long-run plans. By agreeing to such a system, they create

[1] Source: Investment Company Institute *2012 Investment Company Fact Book*.

[2] However, it is often possible to access 401(k) account balances by taking a penalty-free loan. In addition, the penalty on withdrawals is sometimes waived. For example, for IRA accounts no penalty is charged when the account holder (i) is permanently or totally disabled; (ii) has medical expenses exceeding 7.5% of their adjusted gross income; (iii) uses the withdrawal (≤$10,000) to buy, build, or rebuild a home, and has not owned a home in the previous two years; (iv) uses the withdrawal to pay higher education costs; (v) uses the withdrawal to make a back tax payment to the IRS as the result of an IRS levy; (vi) uses the withdrawal to pay health insurance premiums (if unemployed for more than 12 weeks); (vii) receives distributions in the form of an annuity; (viii) uses the withdrawal to make a distribution to an alternate payee under a QDRO (Qualified Domestic Relation Order); or (xi) has been affected by certain natural disasters (e.g., Hurricanes Katrina and Sandy generated exceptions).

[3] For evidence on self-control problems, see Read and van Leeuwen (1998), Wertenbroch (1998), Read, Loewenstein, and Kalyanaraman (1999), Angeletos et al. (2001), O'Connor et al. (2002), McClure et al. (2004), DellaVigna and Paserman (2005), Oster and Scott Morton (2005), Shapiro (2005), DellaVigna and Malmendier (2006), Laibson, Repetto, and Tobacman (2007), McClure et al. (2007), Milkman, Rogers, and Bazerman (2009, 2010), Reuben, Sapienza, and Zingales (2009), and Meier and Sprenger (2010). From a theoretical perspective, self-control problems can be derived from declining intertemporal discount rates (Strotz, 1955; Loewenstein and Prelec, 1992; Laibson, 1997), planner-doer models (Thaler and Shefrin, 1981; Fudenberg and Levine, 2006), visceral factors and cue-conditioned impulsivity (Loewenstein, 1996; Bernheim and Rangel, 2004), the internal conflict between "should" and "want" urges (Bazerman, Tenbrunsel, and Wade-Benzoni, 1998; Milkman, Rogers, and Bazerman, 2008), and temptation preferences (Gul and Pesendorfer, 2001).

incentives for themselves to follow through on their intended course of action.

The demand for commitment devices has been documented in the domains of completing homework assignments for university courses (Ariely and Wertenbroch, 2002), quitting cigarette smoking (Gine, Karlan, and Zinman, 2010), avoiding repeated temptations in a laboratory environment (Houser et al., 2010), achieving workplace goals (Kaur, Kremer, and Mullainathan, 2010), reducing time spent playing online games (Chow, 2011), performing an unpleasant task (Augenblick, Niederle, and Sprenger, 2012), and going to the gym (Milkman, Minson, and Volpp, 2012; Royer, Stehr, and Sydnor, 2012). This paper is most closely related to the work of Ashraf, Karlan, and Yin (2006), who offered a financial commitment device to Philippine households in the form of a savings account that restricted withdrawals. In this context, self-control problems may manifest themselves as account withdrawals that fund immediately gratifying current consumption at the expense of long-term financial goals, and the commitment account allowed individuals to partially constrain such impulsive withdrawals. The commitment account that Ashraf, Karlan, and Yin offered was taken up by 28% of households and increased savings among households that were offered the account relative to a randomized control group.

We build on the findings of Ashraf, Karlan, and Yin by offering commitment accounts to U.S. households in a pair of internet-based experiments. Our key contribution is that we vary the characteristics of the commitment accounts, particularly their degree of illiquidity, to investigate which contract design features are attractive to consumers.

In our two experiments, subjects are endowed with an initial sum of money ($50, $100, or $500) and asked to allocate the money between a liquid account, which does not limit withdrawals, and one or more commitment accounts, which impose penalties on early withdrawals, restrict early withdrawals, or disallow early withdrawals altogether. Early withdrawals are defined as withdrawals that are requested prior to a commitment date chosen by the subject. All subjects have access to the same liquid account, but the characteristics of the commitment account(s) vary across subjects. Any balances, including accumulated interest, remaining in the accounts at the end of the months-long experiments are disbursed to subjects automatically.

Several findings emerge from the experiments. First, when subjects have access to a commitment account with the same interest rate as the liquid account, nearly half of the money given to subjects is allocated to the commitment account. Even when the interest rate on a

commitment account is less than that on the liquid account, the commitment account receives one-quarter of the money. When commitment dates are set, they are non-trivial—that is, they are typically several months later than the earliest possible commitment dates.

Second, and most importantly, when a commitment account and the liquid account have the same interest rate, the fraction of dollars allocated to the commitment account is increasing in the degree of account illiquidity. The commitment account with a 10% early withdrawal penalty receives less money than the commitment account with a 20% early withdrawal penalty, which in turn receives less money than the commitment account that prohibits early withdrawals entirely. The no early withdrawal account attracts larger allocations than the 10% penalty account both when comparing across subjects who are offered one account or the other and when comparing within subjects who are offered both accounts simultaneously. The *between-subject* result that increasing illiquidity leads to higher commitment account allocations implies that the relationship is not an artifact of experimenter demand effects.

Finally, the amount of money allocated to a commitment account is increasing in the interest rate on the commitment account, and when a commitment account has a higher interest rate than the liquid account, the relationship between commitment account allocations and account illiquidity no longer holds (that is, allocations are approximately equal for different degrees of illiquidity).

These experimental results may seem surprising, but we extend the theoretical work of Amador, Werning, and Angeletos (2006) to show that the results are explainedby their model. Amador, Werning, and Angeletos consider a three-period setting (t = 0, 1, 2) in which an agent's consumption-savings problem is modeled as an intrapersonal game played by a sequence of temporally situated selves with quasi-hyperbolic discounting(Phelps and Pollak, 1968; Laibson, 1997; O'Donoghue and Rabin, 1999). There is a tradeoff between commitment and flexibility. A fixed endowment must be divided between period 1 consumption c_1 and period 2 consumption c_2, and the period 0 self prefers lower c_1 than the period 1 self because preferences are present-biased. The period 0 self therefore has a motive to commit the period 1 self to lower consumption. However, a taste shock that affects the marginal utility of c_1 is not observable in period 0 but privately observable in period 1. Thus, the period 0 self has a motive to provide flexibility to the period 1 self, as the period 1 self can condition the consumption-savings decision on information that is unavailable in period 0. The period 0 self balances the

commitment motive and the flexibility motive when choosing a budget set from which the period 1 self must select consumption pairs. Amador, Werning, and Angeletos study the case in which the period 0 self can choose any budget set that satisfies the overall resource constraint. We study the case in which the period 0 self can choose any budget set that satisfies the overall resource constraint and that is not steeper than $-(1+\pi)$ in (c_1, c_2) space, where $\pi \in [0, \infty]$.[4]

Under natural assumptions regarding utility function curvature and the taste shock distribution function,[5] we show that the budget set optimally chosen by the period 0 self can be implemented by dividing the endowment between two accounts, a liquid account with no penalty on period 1 withdrawals and an illiquid account with a penalty π on period 1 withdrawals, where π is the maximal penalty available. Thus, the accounts offered to subjects in our experiments mirror accounts that can be used to implement the theoretically optimal mechanism in a much wider class of mechanisms. Furthermore, the model predicts the experimental result that the no early withdrawal commitment account receives larger allocations than the 10% penalty commitment account when the two accounts are simultaneously offered along with the perfectly liquid account.[6]

Under the further assumption that the agent has log per-period utility, we show that the optimal allocation to the illiquid account with penalty π on early withdrawals is non-decreasing in π. The log utility case therefore predicts our between-subjects experimental finding that offering a commitment account with a higher penalty on early withdrawals leads to a higher allocation to the commitment account.

In addition, it is possible to explain the result that giving the commitment account a higher interest rate than the liquid account attenuates the relationship between commitment account illiquidity and commitment account allocations. The higher interest rate on the commitment account makes it attractive to agents who do not correctly anticipate their time-inconsistent preferences (or who do not have time-inconsistent preferences), and these agents find the commitment account less attractive as its degree of illiquidity increases. Of course,

[4] Note that the $\pi = \infty$ case is the case studied by Amador, Werning, and Angeletos. See Section III for details.
[5] The per-period utility function is constant relative risk aversion, and the taste shock distribution function satisfies a condition that is met by most common classes of single-peaked continuous distribution functions. See Section III.
[6] The fact that the 10% penalty commitment account receives *some* money does not contradict the theory. The 10% penalty may be sufficiently high that it effectively disallows early withdrawals for some individuals. Also, the experiment permitted subjects to choose different commitment dates for the no early withdrawal account and the 10% penalty account, a feature that cannot be captured in the simple three-period model.

because the log utility case is special, our experiments are not a robust test of the theory. Nonetheless, the empirical patterns cannot be accommodated by a standard economic model featuring agents with time-consistent preferences, so our experiments help to distinguish between competing models in the sense of Card, DellaVigna, and Malmendier (2011).

Our analysis sheds light on the nature of the demand for commitment. Theoretically, we provide sufficient conditions for the optimal commitment mechanism to take a strikingly simple form that can be implemented using the accounts available in our experiments. As mentioned above, our empirical findings extend the work of Ashraf, Karlan, and Yin (2006) by asking not only whether commitment is valued but also which forms of commitment are more valued. The results offer some insight as to why previous researchers have found that the fraction of individuals who take up commitment devices is generally low, often in the range of 10%–35%.[7] Allocations to our experimental commitment accounts drop significantly as their interest rate decreases relative to the interest rate on the liquid account, and the more illiquid commitment accounts attract larger allocations, suggesting that take-up of external commitment may be highest when the commitment is firm and comes at a low cost.

Our results also complement recent evidence that an important component of the value of financial accounts with commitment properties is their mental accounting features. In a field experiment in Kenya conducted by Dupas and Robinson (forthcoming), subjects in one treatment group received a savings technology with both a mental accounting component and a commitment component (a lockbox for depositing money to which a research officer held the key), while subjects in another treatment group received a savings technology with only a mental accounting component (a lockbox to which the subject held the key). Take-up rates for the two technologies were approximately equal, suggesting that mental accounting is a primary motivation for lockbox take-up. Our experimental design is similar to the design of Dupas and

[7] Ashraf, Karlan, and Yin (2006) find take-up of 28% for their savings product. Gine, Karlan, and Zinman (2010) find take-up of 11% for their smoking cessation program. Houser et al. (2010) find take-up of 20% at the first opportunity to adopt the commitment device to complete their laboratory task. Kaur, Kremer, and Mullainathan (2010) find take-up of 35% for their commitment contract to attain workplace goals. Chow (2011) finds that 10% of subjects are willing to pay for a commitment device designed to reduce online game playing. Royer, Stehr, and Sydnor (2012) find take-up of 12% for their gym attendance commitment contract. One exception is the case of assignments for university courses—Ariely and Wertenbroch (2002) document that 73% of students choose a deadline for turning in assignments that is earlier than the latest possible date. Another exception is reported by Augenblick, Niederle, and Sprenger (2012), who find that 59% of their subjects take up a commitment device when the commitment device is free. Milkman, Minson, and Volpp (2012) find that 61% of their subjects are willing to pay for a commitment device that makes gym attendance a condition for accessing a "page-turner" audio novel.

Robinson in that our key treatments hold the mental accounting properties of the available financial accounts fixed and vary the degree of illiquidity, enabling us to isolate the effect of commitment and document that a stronger form of commitment can increase the attractiveness of an account.[8]

The paper proceeds as follows. Section I describes the design and the results of our first experiment, and Section II describes the design and the results of our second experiment. Section III presents our extension of the Amador, Werning, and Angeletos (2006) modeling framework and discusses the relationship between our theoretical results and our experimental findings on the demand for illiquid financial accounts. Section IV concludes.

I. First Experiment

A. Design

(i) Subject recruitment

We recruited subjects in early 2010 from the RAND American Life Panel (ALP), a panel of respondents of age 18 years or older who are selected to be broadly representative of the U.S. adult population. ALP respondents participate in approximately two half-hour surveys per month over the internet, and respondents who do not have their own internet access are provided with a device that enables them to access the internet through their televisions.

RAND sent an email to 750 panel members inviting them to participate in a year-long experiment on financial decision-making that would provide at least $40 in compensation. The email contained a link to the ALP website, where the experiment was conducted. Panel members are accustomed to receiving emails of this nature from RAND, and they are comfortable clicking on survey links embedded in these emails. When accessing the ALP website, panel members log in with a username and password, enabling researchers to link an individual's responses across surveys.

After panel members clicked on the survey link and logged into the ALP website, they were taken to an informed consent page that gave a brief description of what would happen in

[8] Dupas and Robinson (forthcoming) find that their lockbox featuring only mental accounting was more effective at encouraging deposits than their lockbox featuring both mental accounting and commitment, but these results are not necessarily contradictory to ours. Their lockbox featuring only mental accounting is closest to the liquid account in our experiments. Their results suggest that a liquid account may be more valuable than an illiquid account if only one account is to be offered. Our results suggest that conditional on both types of accounts being offered, it may be valuable to increase the strength of the commitment available from the illiquid account.

the study: study participants would be given money to divide between two accounts, would have the ability to withdraw money from the accounts over the course of a year, and would receive weekly emails reminding them of their account balances. Out of the 750 panel members invited to take part, 495 consented to participate and completed the study.[9]

Data on the demographic characteristics of the 495 subjects, which were collected by RAND in other surveys, are summarized in Table 1. For the sake of comparison, Table 1 also summarizes the demographic characteristics of the 550 subjects in the second experiment, who were recruited in a similar fashion (see Section III). In both experiments, 43% of the subjects are male, and their ages are distributed fairly evenly across ten-year age categories. Nearly two-thirds have at least some college education. Less than 10% of subjects have annual household income below $15,000, while 17% of subjects have annual household income of at least $100,000. Two-thirds are married, and more than 60% are currently working. Approximately 80% of subjects are White/Caucasian; approximately 10% are Black/African American; and the others are American Indian, Alaskan Native, Asian, Pacific Islander, or of unknown race. Finally, the median subject has one other household member. In fact, 41 subjects in the first experiment are in the same household as at least one other subject in the first experiment.[10] In the second experiment, 23 subjects are in the same household as a subject in the first experiment.[11] No subject in the second experiment is a subject in the first experiment, and no two subjects in the second experiment are from the same household.

(ii) Randomization

After consenting to take part in the experiment, subjects were randomly assigned to one of seven treatment conditions. In all conditions, subjects allocated their experimental endowment between a liquid account and a commitment account. The features of the liquid account were constant across the seven conditions – balances in the liquid account earned a 22% interest rate compounded daily, and withdrawals from the liquid account were allowed at any time starting one week from a subject's initial participation in the experiment. The features of the commitment account were the only variables that differed across treatment conditions. There were three possible interest rates for the commitment account: 21%, 22%, and 23%. There were

[9] Of the remaining 255 panel members, 170 did not respond to the invitation, 36 responded to the invitation but did not complete the consent process, 40 responded to the invitation but declined to consent to participation, and 9 consented to participate but did not start the experiment.

[10] Our results remain unchanged if these 41 subjects are dropped from the sample.

[11] Our results remain unchanged if these 23 subjects are dropped from the sample.

also three possible degrees of illiquidity associated with the commitment account. Early withdrawals, defined as withdrawals requested prior to a commitment date specified by the subject at the outset of the experiment, were subject to a 10% penalty, subject to a 20% penalty, or disallowed altogether.[12] Instead of having a full factorial design involving nine conditions, the experiment omitted conditions offering a commitment account with a 21% interest rate and a 20% early withdrawal penalty or no possibility of early withdrawals.

The experimental design is summarized in Table 2, which also gives the number of subjects in each treatment condition.

Two design decisions deserve further comment. First, the interest rates on the liquid account and the commitment accounts are quite high. They were chosen to be higher than typical credit card interest rates so that most subjects would not find it advantageous to allocate money to the liquid account in order to withdraw the money immediately and use it to pay down credit card debt. Second, the design omits two treatment conditions offering a commitment account with a 21% interest rate. We anticipated that commitment accounts with a 21% interest rate would not attract large allocations, so we did not want to devote much of our sample to those conditions. At the same time, we hoped to compare commitment account allocations when the commitment account interest rate was lower than, equal to, and higher than the liquid account interest rate. Therefore, we included one condition offering a commitment account with a 21% interest rate, providing sufficient statistical power to permit direct comparisons while holding the degree of illiquidity fixed.

(iii) Initial allocation

Subjects in the experiment were not expressly informed of the overall experimental design, but they were given all information that was pertinent to their treatment condition. Following the informed consent page, subjects clicked through a series of screens describing the details of their participation. They would receive $50, $100, or $500, with the outcome determined by a random number drawn as part of a national lottery. Their immediate task was to make three allocation decisions: for each possible monetary endowment, they would divide the money between two accounts, a liquid account and a commitment account. They would receive weekly emails displaying their account balances and providing a link to the webpage where

[12] As in the case of the liquid account, no version of the commitment account permitted withdrawals during the first week of the experiment. A 10% penalty implied that an early withdrawal of W dollars reduced the subject's account balance by $(1.1 \times W)$ dollars. The 20% penalty was defined analogously.

withdrawals could be requested, but they could log in at any time to view their account balances and request account withdrawals. Transfers between the two accounts would be impossible after the initial allocation, and withdrawal requests would result in a check being mailed to the subject within three business days.

The experimental website then described the details of the two accounts. Throughout the experiment, the liquid account was labeled the "Freedom Account," and the commitment account was labeled the "Goal Account." These labels were meant to help subjects both remember the rules associated with the accounts and understand the purposes for which the accounts were designed.

The description of the liquid account emphasized that it permitted flexibility and mentioned that partial withdrawals were possible, with interest accruing at a 22% rate on the balances in the account at any given point in time. Appendix Figure 1 shows an image of the screen explaining the liquid account.

The description of the commitment account emphasized that it could help subjects reach their savings goals. Subjects using the commitment account would have to select a commitment date (labeled the "goal date"), and this date might be associated with a gift purchase, a vacation, another special event, or no particular purpose. Withdrawals before the commitment date were restricted in the manner prescribed by the treatment condition. Partial withdrawals were possible, and the balances in the account at any given point in time would earn the interest rate for that condition. Appendix Figure 2 shows an example of a screen explaining a commitment account.

After learning about the details of the two accounts, subjects learned about the details of the randomization procedure that would assign them $50, $100, or $500. The randomization procedure was based on Powerball, a national lottery with jackpots of many millions of dollars. Twice a week, six integers between 1 and 39, inclusive, are randomly drawn without replacement, and one of these numbers is designated as "the Powerball." All numbers have an equal likelihood of being the Powerball. Subjects were informed that they would choose four numbers. If the next drawing's Powerball was the first or second number chosen, the subject would receive $500. If the next drawing's Powerball was the third or fourth number chosen, the subject would receive $100. In all other cases, the subject would receive $50. The money would

then be allocated between the two accounts according to the subject's stated wishes for the given monetary amount.

Subjects were then asked to make their three allocation decisions. All subjects completed the $50 allocation decision first, followed by the $100 decision second and the $500 decision third. When subjects allocated any money to the commitment account, they were required to choose a commitment date no later than one year from the current date, and they were invited to type in the goal associated with the commitment account. A screen shot for an example allocation page is shown in Appendix Figure 3.

Finally, subjects chose their four Powerball numbers. After the following Powerball drawing, they received emails indicating the dollar amount they were assigned and reminding them of the allocation of that amount. All 495 subjects made their initial allocation decisions between February 1, 2010, and February 11, 2010.

(iv) Withdrawals

During the year after the initial allocation decision, subjects received weekly emails indicating their current account balances and offering a link to the experimental website where withdrawals could be requested. For the text of a sample e-mail, see Appendix Figure 4. Subjects could also log into the website at any time during the year to access the same information. Appendix Figure 5 shows an image of this summary page. When a subject requested a withdrawal, a message asked the subject to confirm the withdrawal amount and the amount by which the account balance would be reduced. If the withdrawal was confirmed, a check for the withdrawal amount was mailed to the subject within three business days.

If subjects withdrew all money from their accounts before a year had elapsed, they were asked to complete an exit questionnaire asking whether any parts of the study were confusing and whether they would have changed any of their decisions in the experiment with the benefit of hindsight. If subjects still had money in their accounts a year after their initial allocation decisions, the account balances were automatically disbursed to them, and they were asked to complete the same exit questionnaire.

B. Results

We first examine the initial allocation decisions of subjects. We treat each of a subject's three allocation decisions as a separate observation, and for each observation we calculate the fraction of the endowment directed to a commitment account. We perform statistical inference

using standard errors clustered at the subject level.[13] Panel A of Table 3 shows the mean fraction allocated to a commitment account by treatment condition. Our main results are immediately apparent.

First, about half of initial balances are allocated to a commitment account when it has the same interest rate as the liquid account (22%), and about one-quarter of initial balances are allocated to a commitment account when it has a lower interest rate than the liquid account. Thus, it seems that subjects place some value on commitment.

Second, when the interest rate on a commitment account is equal to the interest rate on the liquid account, the fraction allocated to the commitment account increases in the account's degree of illiquidity. Increasing the illiquidity from a 10% early withdrawal penalty to a 20% early withdrawal penalty to a prohibition on early withdrawals increases the mean fraction allocated to the commitment account from 39% to 45% to 56%. The first and second means are not statistically significantly different from each other, but the first and third as well as the second and third are. This result gives us some confidence that the value subjects place on commitment is not purely an artifact of experimenter demand effects. Such demand effects could explain our result only if they intensify as the commitment account becomes more illiquid. While possible, this pattern of experimenter demand is not necessarily one that would be predicted ex ante.

The effect of increasing the commitment account's degree of illiquidity can be benchmarked against the effect of increasing the commitment account's interest rate. Comparing across conditions with a 10% early withdrawal penalty, the commitment account with a 21% interest rate attracts a mean allocation of 28%; the commitment account with a 22% interest rate attracts a mean allocation of 39%; and the commitment account with a 23% interest rate attracts a mean allocation of 58%. The differences across these three conditions are statistically significant. Thus, relative to a commitment account with a 10% early withdrawal penalty and a 22% interest rate, increasing the degree of illiquidity by prohibiting early withdrawals has approximately the same effect on commitment account allocations as increasing the interest rate to 23%.

[13] Commitment account allocations generally increase as the initial endowment amount increases, but our results are otherwise qualitatively similar if we separately examine all $50 allocation decisions, all $100 allocation decisions, or all $500 allocation decisions.

Third, when the interest rate on a commitment account is higher than the interest rate on the liquid account, the relationship between commitment account allocations and the degree of illiquidity disappears. Commitment accounts with a 23% interest rate attracted mean allocations of approximately 60% in the 10% penalty, 20% penalty, and no early withdrawal cases.[14]

Panel B of Table 3 is similar to Panel A, except it contains regression-adjusted mean allocations to the commitment account by treatment condition. We perform an ordinary least-squares regression of the fraction allocated to a commitment account on treatment group indicator variables and the demographic variables summarized in Table 1. For each demographic variable, we construct indicator variables for the categories listed in Table 1.[15] We demean these indicator variables, drop one category for each demographic variable, and omit a constant term from the regression so that the coefficients on the treatment group indicators, displayed in Panel B of Table 3, are maximally comparable to the corresponding entries in Panel A of Table 3. As expected in a randomized experiment, controlling for demographic characteristics does not change our results.

To give a sense of the distribution of the fraction of balances allocated to a commitment account, Figure 1 displays cumulative distribution functions by treatment condition. The results are in accord with the results from Table 3. When two conditions have mean fractional allocations that are statistically significantly different, the corresponding distributions display the empirical analog of a first-order stochastic dominance relationship. In particular, differences in the fraction of subjects who allocate all money to the liquid account and differences in the fraction of subjects who allocate all money to the commitment account align with the differences in mean fractional allocations.

When subjects allocate money to a commitment account, they are required to specify a commitment date, which is the date before which early withdrawal restrictions apply. Therefore, a comprehensive measure of a subject's chosen extent of commitment must take into account both the amount of money devoted to a commitment account and the amount of time before the commitment date arrives. For each of a subject's three allocation decisions, we calculate the dollar-weighted days to commitment date, which is the fraction of balances allocated to the

[14] A formal test of this interaction effect is provided in Appendix A.

[15] We combine some categories. For education, the no high school diploma and high school graduate categories are combined, and the associate's degree and bachelor's degree categories are combined. For marital status, job status, and race, all categories other than the majority category are combined.

commitment account multiplied by the number of days between the date of the allocation decision and the commitment date. Panel A of Table 4 displays the unadjusted mean dollar-weighted days to commitment date by treatment condition, while Panel B of Table 4 displays the regression-adjusted mean using a regression specification analogous to the specification for Panel B of Table 3. Differences across conditions here are similar to the differences in mean fractional commitment account allocations but are slightly weaker statistically. Comparing conditions offering commitment accounts with a 22% interest rate, the mean dollar-weighted days to commitment date increases from 82 to 101 to 132 as the degree of illiquidity increases from a 10% early withdrawal penalty to a 20% early withdrawal penalty to a prohibition on early withdrawals. Comparing conditions offering commitment accounts with a 10% penalty on early withdrawals, the mean dollar-weighted days to commitment date increases from 64 to 82 to 130 as the interest rate increases from 21% to 22% to 23%. Among conditions offering commitment accounts with a 23% interest rate, the means of dollar-weighted days to commitment date are statistically indistinguishable as the degree of illiquidity increases.[16]

Figure 2 displays withdrawal patterns for the seven treatment conditions. For each subject and for each day during the year-long experiment, we calculate the sum of the liquid account and commitment account balances that the subject would have had if no withdrawals had been requested. This hypothetical total balance takes as given the subject's initial allocation between the liquid account and the commitment account, which can have different interest rates, and it uses the allocation decision that applies to the ex post realization of the endowment amount ($50, $100, or $500). We then calculate the ratio of the subject's actual balance to the hypothetical total balance, and we use this ratio as our measure of withdrawal activity. For each treatment condition, we plot the mean of our measure against the number of days since the initial allocation decision. Withdrawals take place slightly earlier in the experiment when the interest rate on the commitment account is lower. Withdrawal patterns do not seem to systematically vary according to the commitment account's degree of illiquidity. Of course, it is important to note that differences in withdrawal patterns across treatment conditions are somewhat difficult to interpret. For instance, when comparing a condition offering a 22% interest rate commitment

[16] A subject who is offered a commitment account with a 23% interest rate might allocate the entire endowment to the commitment account but choose the earliest possible goal date in order to earn the higher interest rate while avoiding commitment. We see little evidence of this behavior. Of the 214 subjects who had access to the 23% interest rate commitment account, four subjects selected goal dates within the first two weeks after the initial allocation decision.

account to a condition offering a 23% interest rate commitment account, it is unclear whether any differences in withdrawals are the direct result of the difference in interest rates or are the result of differences in commitment account allocations and commitment dates, which are themselves influenced by the difference in interest rates. The breakdown of withdrawals into liquid account withdrawals and commitment account withdrawals further complicates matters. We examine withdrawals more closely in our second experiment.

We linked the data from our experiment with other subject data available from the RAND American Life Panel, and we examined correlations between commitment account allocations in the experiment and variables such as credit card usage. Taking multiple hypothesis testing adjustments into account, we did not identify any clear correlations.

II. Second Experiment

A. Design

(i) Subject recruitment

The recruitment procedure for the second experiment was very similar to that for the first experiment. In early 2011, RAND emailed 737 ALP members inviting them to participate. When panel members logged into the ALP website, they were informed that the experiment involved allocating $100 among different accounts. The study would end on September 1, 2011, at which point subjects would receive checks for any balances remaining in the accounts, but withdrawals could be requested before the end of the study, with withdrawal checks mailed within three business days of the withdrawal request. As in the first experiment, subjects would receive weekly emails stating their account balances.

After receiving this information, panel members could consent to participate in the experiment. Out of the 737 invited ALP members, 550 gave their informed consent and completed the study.[17] Their demographic characteristics, summarized in Table 1, are similar to those of the participants in the first experiment. There is no overlap between the subjects in the first experiment and the subjects in the second experiment. Furthermore, no subject in the second experiment is in the same household as another subject in the second experiment, but 23 subjects in the second experiment are in the same household as a subject in the first experiment.

[17] Of the remaining 187 panel members, 107 did not respond to the invitation, 26 responded to the invitation but did not complete the consent process, 46 responded to the invitation but declined to consent to participation, and 8 consented to participate but did not start the experiment.

(ii) Randomization

Subjects were randomized into four treatment conditions. In all conditions, subjects had access to a liquid account that was similar to the liquid account in the first experiment. The liquid account had a 22% interest rate and allowed penalty-free withdrawals. In contrast to the first experiment, the second experiment permitted withdrawals immediately instead of allowing withdrawals only if one week had elapsed since the initial allocation decision. The commitment accounts varied across conditions but earned a 22% interest rate in all cases. Two treatment conditions mimicked conditions in the first experiment: subjects had access to one commitment account, and withdrawals from the commitment account prior to the commitment date chosen by the subject incurred a 10% penalty or were prohibited altogether. The third treatment condition offered subjects two commitment accounts, one with a 10% penalty on early withdrawals and the other with no possibility of early withdrawals. Subjects in this condition could allocate money to the two commitment accounts simultaneously, and each commitment account could be assigned its own distinct commitment date. Finally, the fourth treatment condition offered a commitment account with a "safety valve" – early withdrawals from this account were not allowed unless a subject indicated that the funds were needed for a financial emergency. Financial emergencies would not be verified, but subjects were asked to indicate honestly whether or not they were experiencing a financial emergency.

After subjects indicated their desired allocations, they were randomly assigned to receive $100 divided according to their wishes or $100 allocated entirely to the liquid account. At the time they indicated their allocation preferences, they knew there was a 50% probability that their choices would be implemented and a 50% probability that the experimenters would choose their allocations, but they did not know what allocations the experimenters would choose.

Table 5 indicates the number of subjects assigned to each treatment condition broken out into the number who received allocations according to their wishes and the number who received all of their funds in the liquid account. We did not stratify by treatment condition when randomly assigning subjects to receive their chosen allocations or the 100% liquid account allocation, so the distribution of subjects within a treatment condition appears somewhat imbalanced. However, the overall imbalance is negligible.

The second experiment allows us to investigate some questions that follow naturally from the first experiment. First, given subjects' apparent preference for commitment accounts with a

greater degree of illiquidity, it is puzzling that such commitment products are rarely observed in the market. It is possible that highly illiquid commitment accounts are attractive when placed in contrast to a completely liquid account but unattractive when a less illiquid commitment account is added to the choice set, since the latter comparison makes the highly illiquid account seem like an extreme option (Simonson, 1989). Furthermore, the complexity of choosing from a set including multiple commitment accounts may make individuals favor the simple liquid account (Redelmeier and Shafir, 1995). The treatment condition that offers subjects the liquid account, the 10% penalty commitment account, and the no early withdrawal commitment account simultaneously helps us address these possibilities.

Second, it is interesting to ask whether it is possible to design a commitment account that is even more attractive to subjects than the commitment account that prohibits early withdrawals. The advantage of an account with no possibility of early withdrawals is that it provides subjects with a strong form of commitment, but the drawback of the account is that is does not permit subjects to access their funds even when a financial emergency arises. The treatment condition that offers subjects a "safety valve" commitment account may provide a superior combination of commitment and flexibility. Instead of imposing a monetary penalty on early withdrawals or disallowing early withdrawals altogether, the "safety valve" commitment account requires subjects to certify that they are experiencing a financial emergency if they wish to make an early withdrawal. Because we make no attempt to verify whether or not a financial emergency has actually occurred, the "safety valve" commitment account only imposes the psychological cost of lying on subjects if they wish to make an early withdrawal when they are not experiencing a financial emergency. There is probably no psychological cost associated with an early withdrawal during a true financial emergency, so the "safety valve" commitment account may be able to create a state-contingent cost of early withdrawals that does not discourage withdrawals in financial emergencies but does discourage withdrawals in other cases.

Finally, the second experiment allows us to examine whether commitment accounts indeed seem to provide commitment by discouraging withdrawals. Approximately half of the subjects indicated their desired allocations but received all of their money in the liquid account. The withdrawal decisions of these subjects can be compared to the withdrawal decisions of subjects who received their desired allocations.

(iii) Initial allocation

After subjects gave their informed consent and were randomly assigned to treatment conditions, they were informed of the details relevant to their respective conditions. They would be given $100 and would allocate the money to a liquid account (again labeled the "Freedom Account") and one or more commitment accounts (again labeled "Goal Accounts"). The experimental website would display balances and allow withdrawal requests at any time, and weekly emails would also display balances and provide a link to the withdrawal webpage. Deposits into and transfers between the accounts would not be allowed, and withdrawal checks would be mailed within three business days of the withdrawal request.

The description of the liquid account, the description of the 10% penalty commitment account, and the description of the no early withdrawal commitment account were the same as the descriptions used in the first experiment except for appropriate adjustments to relevant dates. When the 10% penalty commitment account and the no early withdrawal commitment account were offered simultaneously, they were labeled "Goal Account A" and "Goal Account B," respectively, and were explained concurrently instead of consecutively (see Appendix Figure 6). Subjects learned that the two commitment accounts could be assigned distinct commitment dates (again labeled "goal dates"). In the case of the "safety valve" commitment account, subjects were informed that early withdrawals were possible only when a financial emergency occurred. Subjects would be the sole judges of whether or not an emergency was actually occurring (see Appendix Figure 7).

Subjects were then told that they would receive their chosen allocation with 50% probability and would receive an allocation selected by the experimenters with 50% probability. They did not know that the allocation selected by the experimenters would place all of the money in the liquid account. A computer rather than a public randomizing device was used for this randomization procedure.

Finally, subjects made their allocation decisions. When appropriate, they selected one or more goal dates and were given the option to describe the goals associated with the commitment accounts. After making these decisions, subjects were informed whether they were randomly assigned to receive their chosen allocation or the 100% liquid account allocation.

Subjects completed this initial phase of the experiment between February 14, 2011, and March 2, 2011. The experiment ended for all subjects on September 1, 2011.

(iv) Withdrawals

As in the first experiment, subjects in the second experiment could see their balances and request withdrawals by logging into the ALP website at any time. They also received weekly emails displaying current balances and a link to the experimental website.

All subjects who requested withdrawals were asked to confirm their requests. In addition, subjects who wished to make early withdrawals from the "safety valve" commitment account were shown the following text:

> We are relying on you to be *honest* in judging whether you have a financial emergency. If you are sure you want to make a withdrawal, please type the sentence below, then click "Next." Otherwise, click "Cancel my withdrawal."

The sentence that these subjects were asked to type was, "I attest that I have a financial emergency." However, the website accepted any entered text.

Similar to the first experiment, the second experiment gave an exit questionnaire to subjects who withdrew all of their money before September 1, 2011. Subjects who had remaining balances on September 1, 2011, automatically received checks for their balances and received emails with links to the same exit questionnaire. The exit questionnaire gave subjects the opportunity to identify confusing aspects of the experiment. However, in contrast to the first experiment, subjects in the second experiment were not asked to explain anything that they would have done differently in retrospect. Also, whenever subjects in the second experiment made any withdrawals (including partial withdrawals) before September 1, 2011, they were given the option to provide their reasons.

B. Results

Panel A of Table 6 gives mean fractional allocations to a commitment account by treatment condition. For the condition offering both the 10% penalty commitment account and the no early withdrawal commitment account, we report the mean of total fractional allocations to either commitment account as well as mean allocations to each of the commitment accounts separately. Standard errors are robust to heteroskedasticity.

The results replicate and extend some of the main findings from the first experiment. Commitment account allocations represent a meaningful fraction of endowments and are similar in magnitude to allocations in the first experiment. When subjects are offered the liquid account

and the 10% penalty commitment account only, the mean commitment account allocation is 46% of the endowment, roughly similar to the 39% mean allocation observed in the first experiment. When subjects are offered the liquid account and the no early withdrawal commitment account only, the mean commitment account allocation is 54%, approximately the same as the 56% mean allocation in the first experiment and statistically significantly different at the 5% level from the 46% mean allocation among subjects in the 10% penalty commitment account only condition.

The commitment account that disallows early withdrawals is desirable even when it is offered in the same choice set as the commitment account that imposes a 10% penalty on early withdrawals. The no early withdrawal commitment account attracts a mean allocation of 34%, while the 10% penalty commitment account attracts only a mean allocation of 16%, a difference that is highly statistically significant in a paired t-test. Thus, it does not seem that the direct comparison of the no early withdrawal commitment account with the 10% penalty commitment account makes the no early withdrawal commitment account an extreme, unattractive option. Interestingly, making two commitment accounts available simultaneously does not lead to higher mean overall allocations to commitment accounts when compared to making only the no early withdrawal commitment account available. In the condition offering two commitment accounts, total commitment account allocations have a mean of 50%, which is smaller than but not statistically significantly different from the mean allocation of 54% in the condition offering only the no early withdrawal commitment account. It is possible that the availability of two commitment accounts makes the allocation decision more complex and therefore leads subjects to view the simple and distinct liquid account as slightly more desirable (Redelmeier and Shafir, 1995).

The "safety valve" commitment account receives a mean allocation of 45%, approximately the same as the 10% penalty commitment account and statistically significantly less than the no early withdrawal commitment account. It may be that the psychological cost of lying about a financial emergency in order to make a withdrawal is sufficiently low that the "safety valve" commitment account does not serve as an effective commitment device, but there are many other potential explanations for this result.

Panel B of Table 6 is analogous to Panel B of Table 3. We perform an ordinary least-squares regression of the fraction allocated to a commitment account on treatment condition indicator variables and demeaned indicator variables for the demographic categories in Table 1,

omitting a constant term.[18] Panel B of Table 6 reports the coefficient estimates on the treatment condition indicator variables, along with the associated heteroskedasticity-robust standard errors. The results are similar to the results in Panel A of Table 6. Note that we do not perform a regression-adjusted comparison of 10% penalty commitment account allocations and no early withdrawal commitment account allocations among subjects who were offered both commitment accounts, since the within-subject comparison already controls for demographic characteristics.

We also report the distribution of commitment account allocations. Figure 3.A displays cumulative distribution functions by treatment condition for the fraction allocated to a commitment account. For the condition offering two commitment accounts, we display the distribution of total allocations to either account. As in the first experiment, commitment account allocations in the treatment condition offering only the no early withdrawal commitment account have the empirical analog of a first-order stochastic dominance relationship with allocations in the condition offering only the 10% penalty commitment account. Comparisons to the condition offering two commitment accounts and the condition offering the "safety valve" commitment account are less clear, although the condition offering only the no early withdrawal commitment account generally exhibits higher commitment account allocations. Figure 3.B focuses on the condition offering two commitment accounts and separately displays the cumulative distribution functions for the fraction allocated to each commitment account. Subjects exhibit a clear preference for the commitment account that disallows early withdrawals. Only 25% of subjects allocate no money to this commitment account, while 44% of subjects allocate no money to the 10% penalty commitment account. Furthermore, 8% of subjects allocate all of their money to the no early withdrawal commitment account, while only one subject allocates all of his money to the 10% penalty commitment account.

We also calculate each subject's chosen dollar-weighted days to commitment date. For the three treatment conditions offering only one commitment account, we simply multiply the fraction of balances allocated to the commitment account by the number of days separating the subject's allocation decision from the subject's chosen commitment date. In the case of the condition offering two commitment accounts, we multiply the fractional allocation by the number of days until the commitment date for each of the commitment accounts, and we add

[18] Again, we merge some categories. The category groupings here are the same as the groupings used in Panel B of Table 3.

these two products together. Note that these calculations use subjects' desired allocations, even though approximately half of the subjects were randomly assigned to receive all of the money in the liquid account. Panel A of Table 7 gives the unadjusted means of the dollar-weighted days to commitment date for the four treatment conditions, and Panel B of Table 7 gives the regression-adjusted means. The results are in line with the differences in fractional commitment account allocations across treatment groups. The condition offering only the no early withdrawal commitment account has a mean dollar-weighted days to commitment date of 75 days, which is slightly higher than the mean of 71 days in the condition offering two commitment accounts. The condition offering only the 10% penalty commitment account and the condition offering the "safety valve" commitment account have lower means of 64 days and 62 days, respectively, although the differences relative to the condition offering only the no early withdrawal commitment account are not statistically significant. The magnitudes are difficult to compare to the magnitudes from the first experiment, since the first experiment took place over the course of a full year instead of slightly more than half a year.

Figure 4 shows the withdrawal patterns for the four treatment conditions. We adopt the same procedure for analyzing withdrawals that we used for the first experiment. We calculate the hypothetical balance that a subject would have had on a given day if no withdrawals had been requested. For each condition, we plot the mean ratio of actual balance to hypothetical balance against the number of days since the initial allocation decision. Because subjects made initial allocation decisions on different dates but completed the experiment on the same date (September 1, 2011), some subjects participated in the experiment for slightly longer periods of time than others. The figure displays only the first 183 days since initial allocation decisions so that the sample remains constant. Withdrawal patterns do not differ markedly across the four conditions, although withdrawals are slightly lower in the condition offering both the 10% penalty commitment account and the no early withdrawal commitment account.

To examine whether the commitment accounts indeed provide a binding form of commitment that reduces withdrawals, we separately examine subjects who were randomly assigned to receive their chosen commitment account allocations and subjects who were randomly assigned to receive the entire endowment in the liquid account. Figure 5 replicates the analysis of Figure 4 but breaks out each treatment condition into these two groups. The withdrawal patterns diverge noticeably within the 10% early withdrawal penalty condition and

within the no early withdrawal condition. As a formal test of this divergence, we perform t-tests comparing means of the withdrawal measure for those who received their chosen allocations relative to those who received the entire endowment in the liquid account. For selected points in time during the experiment, Table 8 reports the t-statistics from these tests for each of the four treatment conditions and for the four treatment conditions pooled. The results for the pooled sample suggest that the commitment accounts do reduce withdrawals.

Withdrawals among subjects who receive the entire endowment in the liquid account provide a measure of ex post self-control problems. It would be interesting if there were a correlation between these withdrawals and chosen commitment account allocations, but we do not find such a correlation in our sample. Of course, it is difficult to draw strong conclusions from this analysis because withdrawals also reflect liquidity shocks and are therefore at best a noisy measure of self-control problems.

III. Theory Section

Theory section goes here.

1. MODEL

To analyze the decisions in our experiment we use a generalization of the framework developed by Amador, Werning and Angeletos (2006; hereafter AWA). AWA's framework has three conceptual ingredients:

1. *Present-biased preferences: the discount function is* $\{1, \beta\,\delta, \beta\,\delta^2, ...\}$. Because these preferences are dynamically inconsistent, the agent has a reason to constrain the actions of future selves.

2. *Short-run taste shocks.* Such taste shocks generate a reason to give future selves some flexibility in choosing the consumption path.

3. *A general commitment technology.*

We enrich the analysis of AWA by including a bound on the strength of the commitment technology. We vary this bound and analyze the implications for equilibrium behavior. These comparative statics enable us to compare the model's predictions with the behavior of our experimental subjects.

We now describe the details of the model.

Timing and Preferences AWA adopt a timing structure that is as simple as possible, while still generating a tradeoff between commitment and flexibility.

Period 0. An initial period in which a (savings/consumption) mechanism is set up by self 0 but no consumption occurs.

Period 1. The taste shock $\theta \in \Theta = \left[\underline{\theta}, \overline{\theta}\right]$ is realized and privately observed. Consumption c_1 occurs.

Period 2. Final consumption c_2 occurs.

Preferences are defined at dates 0, 1 and 2:

$$\begin{array}{rcccc} V_0 & = & \tilde{\theta}\,\beta\,\delta\,U_1(c_1) & + & \beta\,\delta^2\,U_2(c_2) \\ V_1 & = & \tilde{\theta}\,U_1(c_1) & + & \beta\,\delta\,U_2(c_2) \\ V_2 & = & & & U_2(c_2) \end{array}$$

Here U_t is the utility function at time t. The distribution function of the taste shock θ is F. We assume that: $U_t : [0, \infty) \to [-\infty, \infty)$; $U_t' > 0$ and $U_t'' < 0$ on $(0, \infty)$; and $U_t'(0+) = \infty$.[1] We introduce our assumptions on F below.

Since present-biased preferences are dynamically inconsistent (if $\beta \neq 1$), this environment is modeled as an intra-personal game (Strotz, 1957; Phelps and Pollak 1968). The game has three players: self 0, self 1 and self 2.

1.1. Budget Set. In period 0 self 0 chooses a budget set B, which is the set of consumption pairs (c_1, c_2) that can be chosen by self 1. Recall that the taste shock is not yet observable in period 0 and that it will only be privately observable in time period 1, so B cannot be conditioned on the realization of the taste shock. FigureA plots an illustrative budget set.

Let $y > 0$ be the household's exogenous budget, and let the "ambient budget set" A be the set of all consumption pairs (c_1, c_2) such that $c_1, c_2 \geq 0$ and $c_1 + c_2 \leq y$. In particular, take the gross interest rate to be unity. Furthermore, fix $\pi \in [0, \infty]$. Then the budget set B chosen by self 0 must satisfy the following constraints:

Constraint 1. B is a non-empty compact subset of A.

Constraint 2. The local slope of B is no steeper than $-(1 + \pi)$.[2]

In other words, self 0 is free to place essentially any constraints she pleases on the budget set B, subject only to the requirement that the slope of B is no steeper than $-(1 + \pi)$. While our main focus is on the case $\pi < \infty$, we also include the case $\pi = \infty$, which is the case studied by AWA.

Figure A shows a budget set that satisfies Constraint 2 if $\pi = 0.5$ but not if $\pi = 0.1$. Indeed, the slope of the budget set at the encircled point is -1.3, which is steeper than -1.1 and less steep than -1.5.

If a budget set satisfies Constraints 1 and 2 for a particular π, then we refer to it as a *feasible budget set with consumption penalty at most* π. We use this naming

[1] It is not necessarily the case that $U_t(0) = -\infty$. Indeed, in the case of constant relative risk aversion, $U_t(0) = -\infty$ iff $\rho \geq 1$, where ρ is the coefficient of relative risk aversion.

[2] "The local slope of B is no steeper than $-(1 + \pi)$" means that, for all $(c_1, c_2) \in B$ and all $\widetilde{c}_1 \in \left[c_1, c_1 + \frac{1}{1+\pi} c_2\right]$, there exists $\widetilde{c}_2 \geq c_2 - (1 + \pi)(\widetilde{c}_1 - c_1)$ such that $(\widetilde{c}_1, \widetilde{c}_2) \in B$. In other words, if we take any point (c_1, c_2) in B, and if we draw a line segment of slope $-(1 + \pi)$ from (c_1, c_2) to the point $\left(c_1 + \frac{1}{1+\pi} c_2, 0\right)$ at which it crosses the c_1 axis, then, associated with every point $(\widetilde{c}_1, \widehat{c}_2)$ on that line segment, there is a point $(\widetilde{c}_1, \widetilde{c}_2)$ in B that either coincides with $(\widetilde{c}_1, \widehat{c}_2)$ or else lies vertically above it.

Figure A: Illustrative budget set

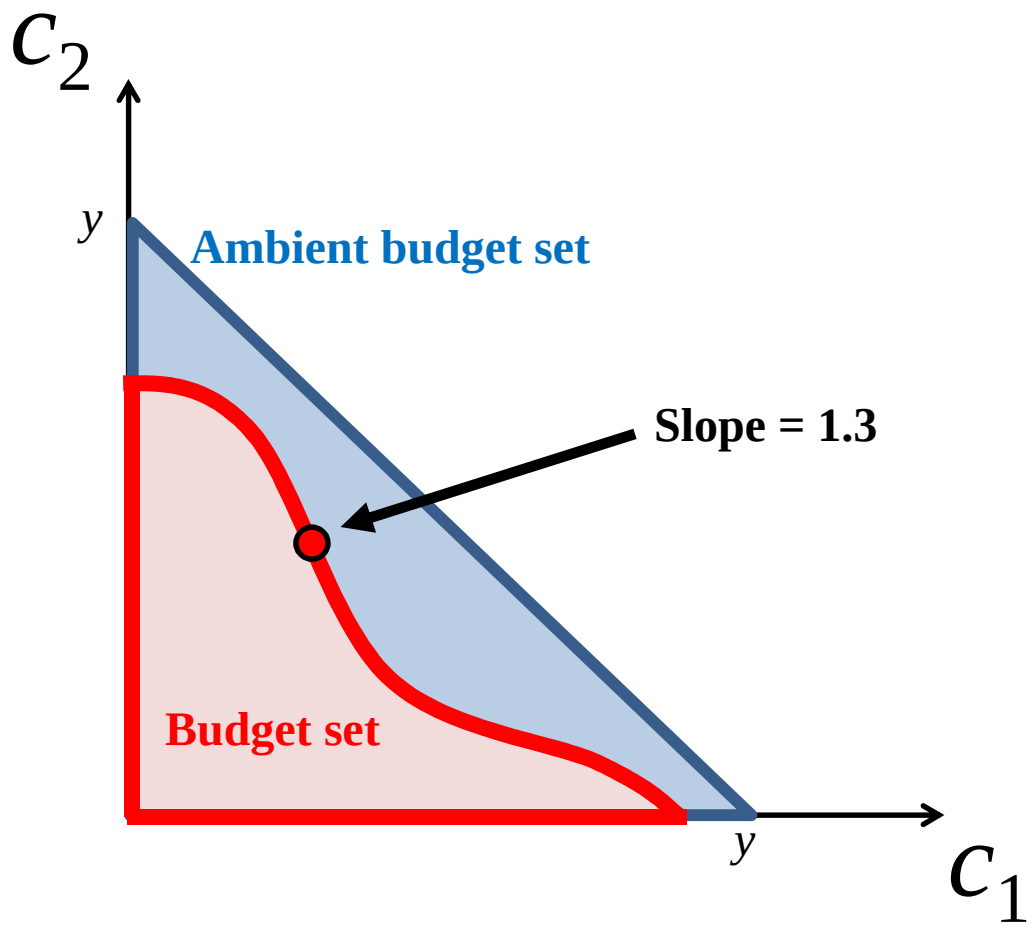
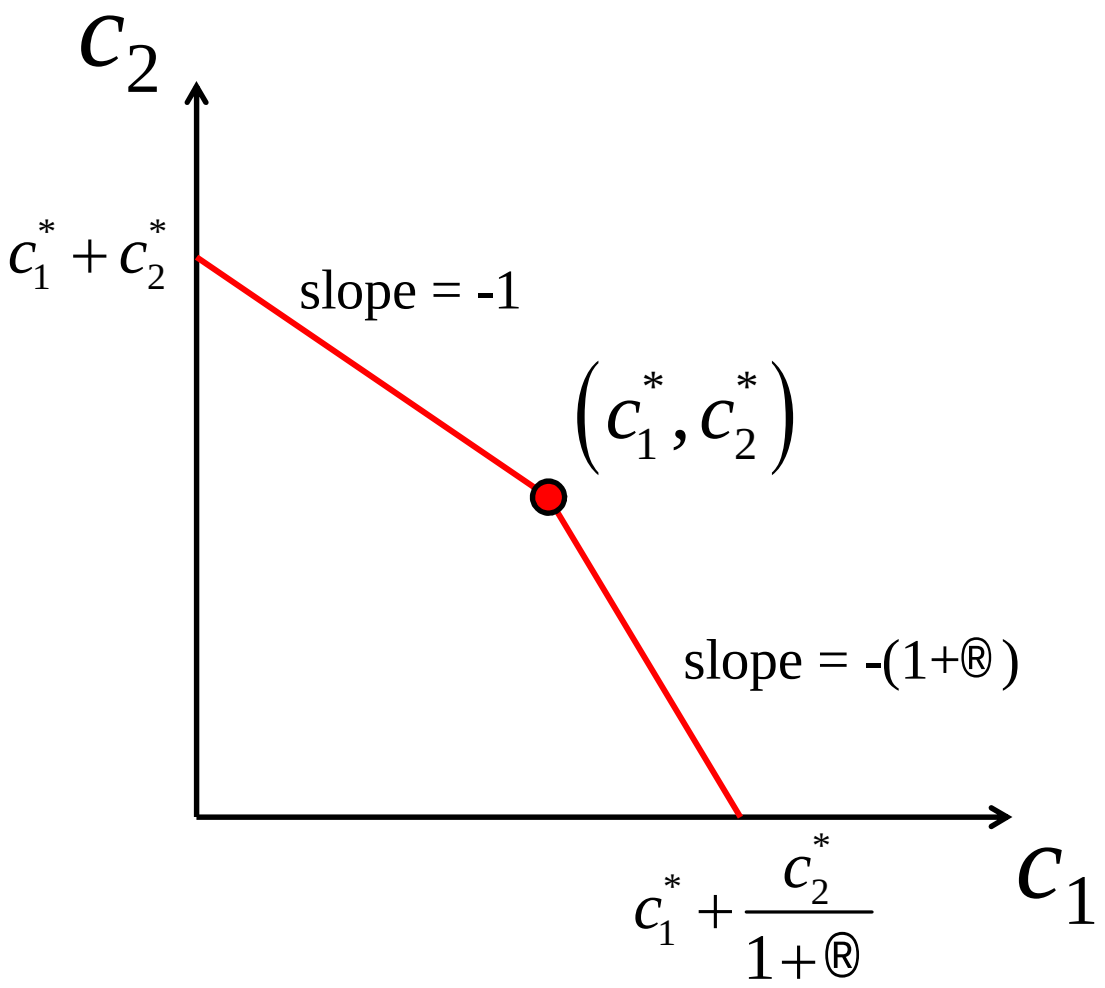


Figure B: Two-part budget set



convention because the local steepness of the budget constraint is 1 plus the penalty of consuming a marginal unit at time 1 instead of waiting until time 2.

The results that follow illustrate the special role played by budget sets that have a particular piecewise linear form: slopes of -1 and $-(1+\pi)$ to the left and right of a kink at (c_1^*, c_2^*). We call such budget sets *two-part budget sets*. The budget set shown in Figure B is an example: starting from the point $(0, c_1^* + c_2^*)$ on the c_2 axis, the frontier of this budget set is linear with slope -1 until it reaches the kink at (c_1^*, c_2^*); it is then linear with slope $-(1+\pi)$ until it reaches the c_1 axis at the point $\left(c_1^* + \frac{1}{1+\pi}\, c_2^*, 0\right)$.

We now describe an equivalence property that applies to two-part budget sets. Assume that self 0 chooses a two-part budget set with slopes -1 and $-(1+\pi)$ to the left and right of a kink at (c_1^*, c_2^*). Then (irrespective of θ) self 1 will choose the same consumption pair that self 1 would have chosen if self 0 had instead handed self 1 two *separate* accounts: a completely liquid account with balance c_1^*; and a second account with balance c_2^* and an early withdrawal penalty π.[3]

The equivalence between the two-part budget set and the two-account commitment mechanism arises because the two-account mechanism will lead self 1 (who prefers more consumption to less consumption in both period 1 and period 2) to withdraw funds from the liquid account first. In other words, self 1 will not withdraw funds from the illiquid account in period 1 unless the liquid account is already exhausted. Accordingly, the two-part budget set – with kink point (c_1^*, c_2^*) – produces the same equilibrium behavior as the two-account commitment mechanism – with respective balances c_1^* and c_2^*. We will show that two-part budget sets arise naturally in the economic setting that we study.

1.2. Distribution of the Taste Shock. AWA show that their problem can be reduced to a problem in the class of optimization problems identified and analyzed by Luenberger (1969).[4] We follow AWA's lead. We make the following assumptions on the distribution function F of the taste shock θ.

A1 Both F and F' are functions of bounded variation on $(0, \infty)$.

A2 The support of F' is contained in $\left[\underline{\theta}, \overline{\theta}\right]$, where $0 < \underline{\theta} < \overline{\theta} < \infty$.

[3] By saying that self 1 pays a penalty π for withdrawals from the second account, we mean that if she consumes Δ from the second account then that account is debited $(1+\pi)\,\Delta$.

[4] David G. Luenberger, 1969, "Optimization by Vector Space Methods", Wiley.

A3 Put $G(\theta) = (1-\beta)\,\theta\,F'(\theta) + F(\theta)$. Then there exists $\theta_M \in [\underline{\theta}, \overline{\theta}]$ such that: (i) $G' \geq 0$ on $(0, \theta_M)$; and (ii) $G' \leq 0$ on (θ_M, ∞).[5]

We now comment on each of these assumptions, beginning with A1. A function $f : (0,\infty) \to \mathbb{R}$ is of bounded variation iff it is the difference of two non-decreasing functions $f_1, f_2 : (0,\infty) \to \mathbb{R}$ such that $-\infty < f_i(0+) \leq f_i(\infty-) < \infty$. Since F is a distribution function, it is automatically a function of bounded variation. The substance of A1 is therefore the requirement that F has a density F', and that this density is itself a function of bounded variation.

Assumption A2 implies that $F' = 0$ on $(0,\infty) \setminus [\underline{\theta}, \overline{\theta}]$. Notice that F' need not be continuous. In particular, it can jump up at $\underline{\theta}$ and down at $\overline{\theta}$.

Assumption A3 implies that G is first increasing and then decreasing. A3 also implies that the support of F' is connected. A3 is preserved under truncation: if a distribution satisfies A3, then so too does the distribution obtained by truncating it at $\underline{\theta}$ and $\overline{\theta}$. A3 is satisfied by many of the distributions that one encounters in practice. In order to illustrate this final point, we made an exhaustive list of all the distributions occurring in two widely-used statistics textbooks.[6] This list contains eighteen distributions. All but one satisfy A3 for all parameter values (including $\underline{\theta}$ and $\overline{\theta}$). More precisely, we have:

Remark 1. *Suppose that D is one of the Beta, Burr, Chi-squared, Exponential, Extreme Value, F, Gamma, Gompertz, Log-Gamma, Log-Normal, Maxwell, Normal, Pareto, Rayleigh, t, Uniform and Weibull distributions. Then, for any $\underline{\theta}, \overline{\theta} \in (0,\infty)$ such that $\underline{\theta} < \overline{\theta}$, the distribution function F of the distribution obtained by truncating D at $\underline{\theta}$ and $\overline{\theta}$ satisfies Assumption A1.*

The exception is the Cauchy distribution: A3 is violated if the distribution is located too far to the right.[7]

[5] Assumption A3 is stronger than the analogous assumption in AWA ("Assumption A"). But A3 is easier to state and it is easier to check whether a given distribution satisfies it.

[6] Hogg, McKean, and Craig: Introduction to Mathematical Statistics, 6th Edition; Rice: Mathematical Statistics and Data Analysis, 2nd Edition.

[7] The probability density function of the Cauchy distribution is proportional to

$$\frac{1}{\sigma\left(1+\left(\frac{x-\mu}{\sigma}\right)^2\right)},$$

Theorems and Relationship to Experimental Results

Theorem 2. *Suppose that $U_1 = U_2 = U$, and that U has constant relative risk aversion $\rho > 0$. Suppose further that the maximum consumption penalty is π. Then self 0 will choose a two-part budget set with slopes -1 and $-(1+\pi)$ to the left and right of a kink at (c_1^*, c_2^*), and with $c_1^* + c_2^* = y$. This budget set is equivalent to creating two accounts: a liquid account with completely flexible withdrawal rights in both period 1 and period 2; and an illiquid account with a penalty π for withdrawals in period 1, and no penalty for withdrawals in period 2.*

In other words, Theorem 1 says that self 0 will choose to give self 1 a two-part budget set (as in Figure B), with the maximal allowable steepness to the right of the kink: slope $-(1+\pi)$.

Theorem 3. *Suppose that $U_1 = U_2 = U$, and that U has constant relative risk aversion $\rho = 1$ (i.e. that $U = \log$). Then the amount of money deposited in the illiquid account is non-decreasing in π.*

Theorem 2 provides a sufficient condition for a monotonic relationship between the amount placed in the illiquid account and the maximal penalty π. Theorem 2 matches our empirical results. Specifically, as π rises exogenously across our experimental arms (from 10% to 20% to ∞), the allocation to the illiquid account rises. Note that this is not a robust test of our theory, since log utility is a knife-edge case. However, the log utility case is a leading empirical case.[8]

where $\mu \in \mathbb{R}$ is a location parameter (the location of the mode of the distribution) and $\sigma > 0$ is a scale parameter. For our purposes, only the ratio $\frac{\mu}{\sigma}$ matters. If

$$\frac{\mu}{\sigma} \leq \sqrt{\frac{1-(1-\beta)^2}{(1-\beta)^2}},$$

in particular if $\frac{\mu}{\sigma} \leq 0$, then, for any $\underline{\theta}, \overline{\theta} \in (0, \infty)$ such that $\underline{\theta} < \overline{\theta}$, the distribution function F of the distribution obtained by truncating the Cauchy distribution at $\underline{\theta}$ and $\overline{\theta}$ satisfies Assumptions A1-A3. On the other hand, if

$$\frac{\mu}{\sigma} > \sqrt{\frac{1-(1-\beta)^2}{(1-\beta)^2}},$$

then, for some choices of $\underline{\theta}, \overline{\theta} \in (0, \infty)$, it can happen that G is first increasing (at $\underline{\theta}$), then decreasing, then increasing again and finally decreasing again (at $\overline{\theta}$).

[8] The following studies use large stakes gambles to measure the level of risk aversion. XXX When researchers use the equity premium to infer the level of risk aversion, far higher levels of risk aversion are imputed (e.g., Mehra and Prescott 1987).

Now we turn to the case $\pi = \infty$. In other words, we study the case of unbounded penalties. For this case, we do not restrict the two utility functions to be equal, nor do we restrict them to be in the constant relative risk aversion class. We only impose the properties of monotonicity, concavity, and marginal utility that goes to infinity as consumption goes to zero.

Theorem 4. *Suppose that the maximum consumption penalty is* ∞*.Then self* 0 *will set up a budget set that is equivalent to creating two accounts: a liquid account with completely flexible withdrawal rights in periods* 1 *and* 2 *and an illiquid account with no withdrawal rights in period* 1 *(i.e. infinite withdrawal penalty in period* 1*) and no penalty for withdrawals in period* 2*.*

Corollary 5. *Suppose self* 0 *has access to three accounts, a liquid account with completely flexible withdrawal rights in periods 1 and 2, an illiquid account with early withdrawal penalty* $\underline{\pi} \in (0, \infty)$ *in period* 1 *and no withdrawal penalty in period* 2*, and another illiquid account with withdrawal penalty* $\bar{\pi} = \infty$ *in period* 1 *and no withdrawal penalty in period* 2*. Then self* 0 *will divide all of her resources between the liquid account and the illiquid account with withdrawal penalty* $\bar{\pi} = \infty$*.*

This corollary is closely related to our empirical results in the arm in which subjects had access to three accounts: a liquid account, an illiquid account with a 10% penalty for withdrawals before their goal date, and an illiquid account with no opportunity for withdrawals before their goal date. The Corollary predicts that subjects will allocate all of their illiquid savings to the perfectly illiquid account (and hence none to the illiquid account with the 10% penalty). In our experiment, we find that the perfectly illiquid account attracts twice as many deposits as the 10%-penalty illiquid account.

IV. Conclusion

This paper studies the demand for illiquid financial accounts as commitment devices that help individuals avoid spending money that they had planned to save. We experimentally examine the features of commitment accounts that make them attractive by offering subjects the ability to allocate an endowment between a liquid account and one or more commitment accounts, which place restrictions on early withdrawals. We find that subjects allocate meaningful fractions of their endowments to commitment accounts. When the commitment account and the liquid account have the same interest rate, commitment account allocations are increasing in the account's degree of illiquidity, but the pattern disappears when the commitment account has a higher interest rate than the liquid account. We extend the theoretical framework of Amador, Werning, and Angeletos (2006) to show that our experimental results can be explained by a model in which time-inconsistent agents face a tradeoff between commitment and flexibility. In fact, under a natural set of assumptions, the optimal commitment mechanism in the model can be implemented using financial accounts that map directly to the accounts available to our experimental subjects.

Our findings have implications for the design of the institutional environment in which households make financial decisions. For example, time-deposit financial products such as certificates of deposit generally impose early withdrawal penalties equal to the amount of interest earned over the previous few months. Our results suggest that there may be a role for deposit products with larger penalties on early withdrawals. For another example, many defined contribution retirement savings plans impose some degree of illiquidity on individuals' account balances—in the case of 401(k) plans, withdrawals before the age of 59½ generate a 10% tax penalty.[19] One study found that 46% of workers who have 401(k) accounts and who leave their jobs receive their 401(k) balances as lump-sum withdrawals (Hewitt Associates, 2009), and retirement savings plan managers worry that this "leakage" can reduce retirement wealth (Steyer, 2011). It may be beneficial to create a second type of 401(k) account that is used alongside the existing type of 401(k) account but that features greater illiquidity. Our results suggest that such an account type would be attractive to individuals and would limit "leakage." It would be valuable for future research to test whether the results from our experiments generalize to settings such as the market for time-deposit products and the domain of retirement savings plans.

[19] See footnote 2.

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Table 1. Subject Characteristics

This table summarizes subjects' demographic characteristics in the first experiment (n = 495) and the second experiment (n = 550).

	First	Second		First	Second
Percent male	43%	43%	Marital status		
			Married	68%	66%
Age			Separated/divorced	11%	14%
≤ 25	8%	8%	Widowed	5%	5%
26-35	17%	19%	Never married	16%	15%
36-45	21%	18%			
46-55	22%	22%	Job status (has overlap)		
55-65	16%	15%	Working now	63%	60%
≥ 66	16%	17%	Unemployed	8%	9%
			Temporary layoff	1%	1%
Education			Disabled	4%	6%
No high school diploma	3%	5%	Retired	19%	19%
			Homemaker	10%	11%
High school graduate	32%	29%			
Some college	20%	23%	Race		
Associate's degree	7%	12%	White/Caucasian	80%	81%
Bachelor's degree	24%	19%	Black/African American	8%	10%
Graduate degree	13%	12%			
			American Indian or Alaskan Native	1%	1%
Annual household income					
< $15,000	6%	9%	Asian or Pacific Islander	4%	2%
$15,000 - $34,999	19%	20%			
$35,000 - $49,999	16%	16%			
$50,000 - $74,999	27%	22%	Size of household		
$75,000 - $99,999	15%	16%	0	48%	40%
≥ $100,000	17%	17%	1	19%	20%
			2	15%	21%
			≥ 3	15%	19%

Table 2. Sample Size in Each Treatment Condition: Experiment 1

This table reports the number of subjects who were assigned to each treatment condition in Experiment 1 (2/01/2010-2/13/2011).

Withdrawal restrictions on commitment account prior to commitment date	Commitment account interest rate		
	21%	22%	23%
10% early withdrawal penalty	72	66	78
20% early withdrawal penalty	0	79	68
No early withdrawals	0	64	68

Table 3. Fraction of Endowment Allocated to Commitment Account: Experiment 1

For each treatment condition, this table reports the mean fraction of endowment allocated to a commitment account. There are three observations for every subject, one observation for each possible endowment amount. Standard errors clustered at the subject level are in parentheses. The table also gives t-statistics from tests of the equality of means as indicated. Panel A reports unadjusted means, and Panel B reports regression-adjusted means (see text).

Panel A: Unadjusted means

Withdrawal restrictions on commitment account prior to commitment date	Commitment account interest rate			Test of equality of means	
	21%	22%	23%	21% vs. 22%	22% vs. 23%
10% early withdrawal penalty	0.276 (0.028)	0.389 (0.034)	0.582 (0.034)	-2.576	-4.014
20% early withdrawal penalty	--	0.448 (0.034)	0.611 (0.034)	--	-3.418
No early withdrawals	--	0.560 (0.041)	0.599 (0.036)	--	-0.727
Test of equality of means					
10% penalty vs. 20% penalty	--	-1.231	-0.615		
10% penalty vs. no early w/d	--	-3.207	-0.360		
20% penalty vs. no early w/d	--	-2.123	0.242		

Panel B: Regression-adjusted means

Withdrawal restrictions on commitment account prior to commitment date	Commitment account interest rate			Test of equality of coefficients	
	21%	22%	23%	21% vs. 22%	22% vs. 23%
10% early withdrawal penalty	0.274 (0.028)	0.388 (0.034)	0.575 (0.032)	-2.54	-3.93
20% early withdrawal penalty	--	0.441 (0.033)	0.620 (0.035)	--	-3.73
No early withdrawals	--	0.570 (0.040)	0.600 (0.035)	--	-0.57
Test of equality of coefficients					
10% penalty vs. 20% penalty	--	-1.11	-0.94		
10% penalty vs. no early w/d	--	-3.44	-0.54		
20% penalty vs. no early w/d	--	-2.48	0.39		

Table 4. Dollar-Weighted Days to Commitment Date: Experiment 1

For each treatment condition, this table reports the mean dollar-weighted days to commitment date. Dollar-weighted days to commitment date is defined as the fraction of endowment allocated to a commitment account multiplied by the number of days separating the allocation decision and the commitment date. There are three observations for every subject, one observation for each possible endowment amount. Standard errors clustered at the subject level are in parentheses. The table also gives t-statistics from tests of the equality of means as indicated. Panel A reports unadjusted means, and Panel B reports regression-adjusted means (see text).

Panel A: Unadjusted means

Withdrawal restrictions on commitment account prior to commitment date	Commitment account interest rate			Test of equality of means	
	21%	22%	23%	21% vs. 22%	22% vs. 23%
10% early withdrawal penalty	64.3 (7.3)	81.8 (9.1)	129.6 (10.6)	-1.498	-3.433
20% early withdrawal penalty	--	100.5 (10.9)	127.0 (12.3)	--	-1.616
No early withdrawals	--	131.8 (13.9)	117.9 (11.2)	--	0.781
Test of equality of means					
10% penalty vs. 20% penalty	--	-1.323	0.161		
10% penalty vs. no early w/d	--	-3.012	0.763		
20% penalty vs. no early w/d	--	-1.777	0.549		

Panel B: Regression-adjusted means

Withdrawal restrictions on commitment account prior to commitment date	Commitment account interest rate			Test of equality of coefficients	
	21%	22%	23%	21% vs. 22%	22% vs. 23%
10% early withdrawal penalty	65.6 (7.6)	79.9 (8.9)	125.8 (10.3)	-1.23	-3.34
20% early withdrawal penalty	--	98.7 (10.7)	128.0 (12.0)	--	-1.82
No early withdrawals	--	134.8 (13.7)	120.8 (11.2)	--	0.79
Test of equality of coefficients					
10% penalty vs. 20% penalty	--	-1.36	-0.14		
10% penalty vs. no early w/d	--	-3.31	0.33		
20% penalty vs. no early w/d	--	-2.07	0.44		

Table 5. Sample Size in Each Treatment Condition: Experiment 2

This table reports the number of subjects who were assigned to each treatment condition in Experiment 2 (2/14/2011-9/01/2011). All accounts in Experiment 2 had a 22% interest rate.

	Endowment allocated	
Withdrawal restrictions on commitment account prior to commitment date	According to subject's choice	All in liquid account
Safety valve (withdrawals only in financial emergencies)	85	65
10% early withdrawal penalty	54	46
No early withdrawals	60	90
Two commitment accounts: 10% early withdrawal penalty and no early withdrawals	70	80

Table 6. Fraction of Endowment Allocated to Commitment Account: Experiment 2

For each treatment condition, this table reports the mean fraction of endowment allocated to a commitment account. For the condition offering two commitment accounts, mean fractional allocations are also reported for each individual commitment account. Standard errors robust to heteroskedasticity are in parentheses. The table also gives t-statistics from tests of the equality of means as indicated. Panel A reports unadjusted means, and Panel B reports regression-adjusted means (see text). All accounts in Experiment 2 had a 22% interest rate.

Panel A: Unadjusted means

Withdrawal restrictions on commitment account prior to commitment date		Test of equality of means, vs. no early withdrawals
Safety valve (withdrawals only in financial emergencies)	0.453 (0.027)	-2.379
10% early withdrawal penalty	0.458 (0.029)	-2.131
No early withdrawals	0.537 (0.023)	
Two commitment accounts: 10% early withdrawal penalty and no early withdrawals	0.501 (0.027)	-1.016
10% early withdrawal penalty	0.162 (0.014)	-6.105
No early withdrawals	0.339 (0.024)	

Panel B: Regression-adjusted means

Withdrawal restrictions on commitment account prior to commitment date		Test of equality of coeff., vs. no early withdrawals
Safety valve (withdrawals only in financial emergencies)	0.473 (0.026)	-1.75
10% early withdrawal penalty	0.445 (0.030)	-2.37
No early withdrawals	0.534 (0.023)	
Two commitment accounts: 10% early withdrawal penalty and no early withdrawals	0.493 (0.027)	-1.16

Table 7. Dollar-Weighted Days to Commitment Date: Experiment 2

For each treatment condition, this table reports the mean dollar-weighted days to commitment date. When one commitment account is offered, dollar-weighted days to commitment date is defined as the fraction of endowment allocated to a commitment account multiplied by the number of days separating the allocation decision and the commitment date. When two commitment accounts are offered, dollar-weighted days to commitment date is obtained by calculating this product for each account and taking the sum. Standard errors robust to heteroskedasticity are in parentheses. The table also gives t-statistics from tests of the equality of means as indicated. Panel A reports unadjusted means, and Panel B reports regression-adjusted means (see text). All accounts in Experiment 2 had a 22% interest rate.

Panel A: Unadjusted means

Withdrawal restrictions on commitment account prior to commitment date		Test of equality of means, vs. no early withdrawals
Safety valve (withdrawals only in financial emergencies)	62.0 (4.6)	-2.007
10% early withdrawal penalty	64.4 (5.5)	-1.493
No early withdrawals	74.8 (4.4)	
Two commitment accounts: 10% early withdrawal penalty and no early withdrawals	71.3 (4.8)	-0.543

Panel B: Regression-adjusted means

Withdrawal restrictions on commitment account prior to commitment date		Test of equality of coeff., vs. no early withdrawals
Safety valve (withdrawals only in financial emergencies)	64.0 (4.7)	-1.64
10% early withdrawal penalty	63.1 (5.5)	-1.65
No early withdrawals	74.7 (4.3)	
Two commitment accounts: 10% early withdrawal penalty and no early withdrawals	70.3 (4.8)	-0.68

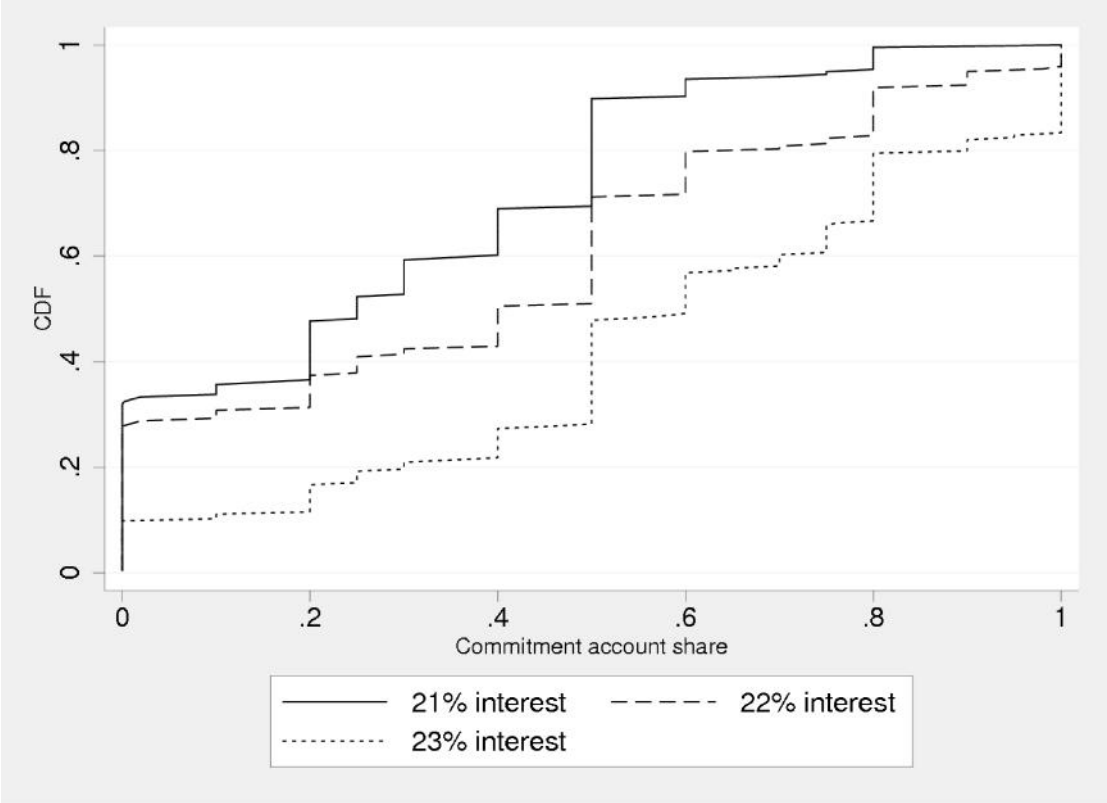
Table 8. Mean Withdrawal Measure for Own versus All Liquid Allocation: Experiment 2
For each subject and for a given number of days since the start of the experiment, we calculate the ratio of actual balances in experimental accounts to hypothetical balances in experimental accounts had the subject not made any withdrawals. For each treatment condition and for the treatment conditions combined, this table reports t-statistics from tests of the equality of means for this withdrawal measure comparing subjects who were randomly assigned to receive their chosen allocations and subjects who were randomly assigned to receive their entire endowment in the liquid account.

	Test of equality of means t-statistic: Own allocation vs. all in liquid account				
Withdrawal restrictions on commitment account prior to commitment date	Days since start of experiment				
	20	60	100	140	180
Safety valve	1.64	-0.09	0.04	0.33	-0.39
10% penalty	2.07	1.74	1.95	2.30	1.60
No early withdrawals	1.99	2.89	2.18	1.38	1.61
Two commitment accounts	-1.22	0.62	0.49	0.61	1.04
Combined	2.44	2.74	2.33	2.36	1.92

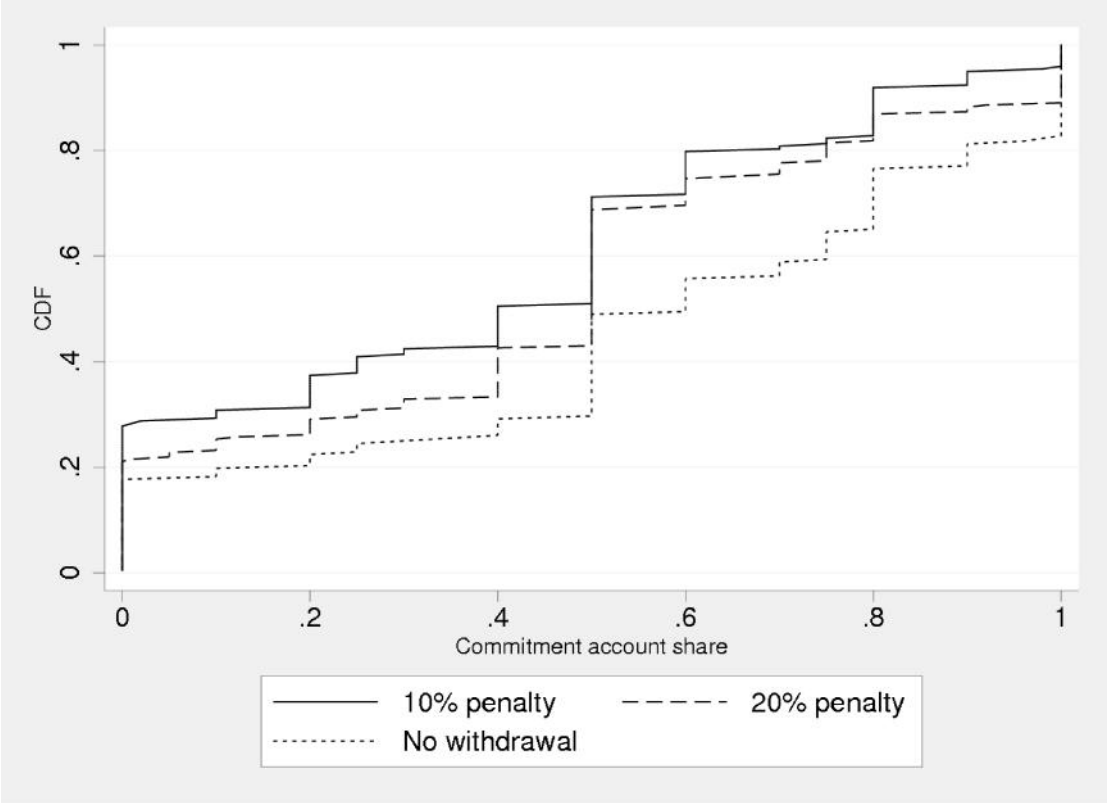
Figure 1. Distributions of Fraction of Endowment Allocated to Commitment Account: Experiment 1

These figures show cumulative distribution functions, by treatment condition, of fraction of endowment allocated to a commitment account. There are three observations for every subject, one observation for each possible endowment amount.

A. 10% early withdrawal penalty, by interest rate



B. 22% interest rate, by early withdrawal restriction



C. 23% interest rate, by early withdrawal restriction

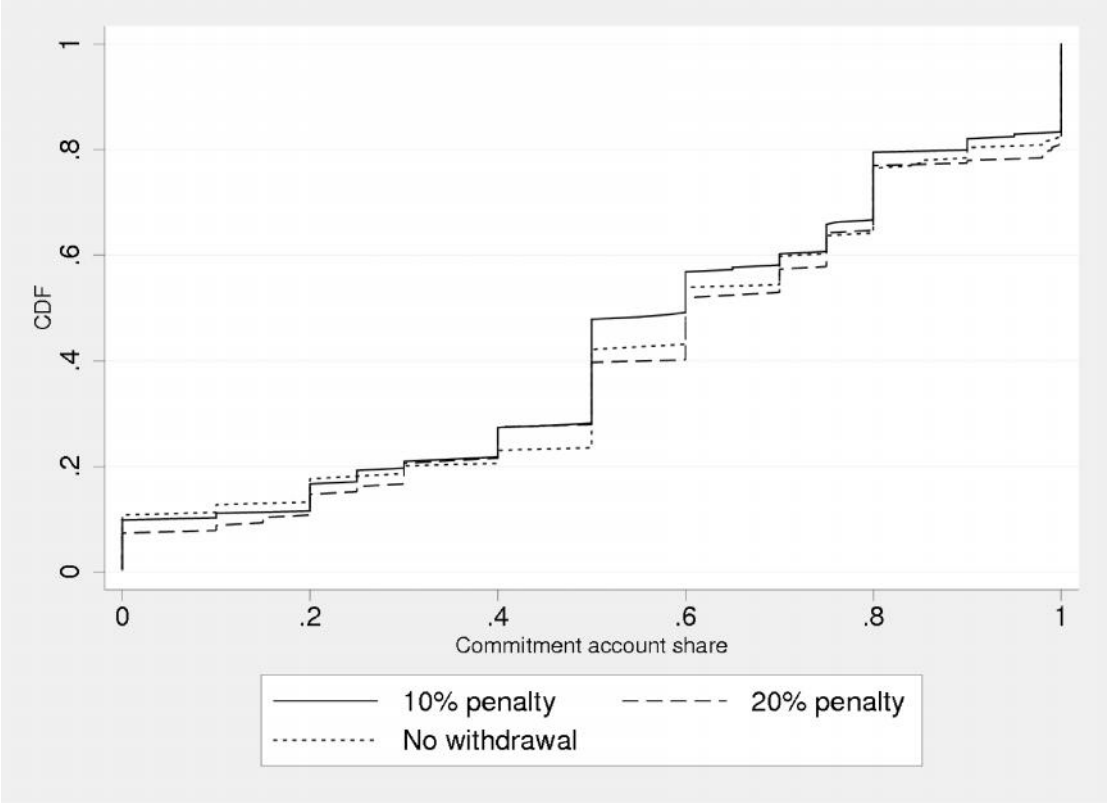


Figure 2. Withdrawal Patterns by Treatment Condition: Experiment 1

For each treatment condition, these figures show withdrawal patterns over the course of the experiment. For each subject and for each day, we calculate the sum of the liquid account and commitment account balances that the subject would have had if no withdrawals had been requested. This hypothetical total balance takes as given the subject's initial allocation between the liquid account and the commitment account, and it uses the allocation decision that applies to the ex post realization of the endowment amount ($50, $100, or $500). We then calculate the ratio of the subject's actual balance to the hypothetical total balance, and we plot the mean of this ratio against the number of days since the initial allocation decision.

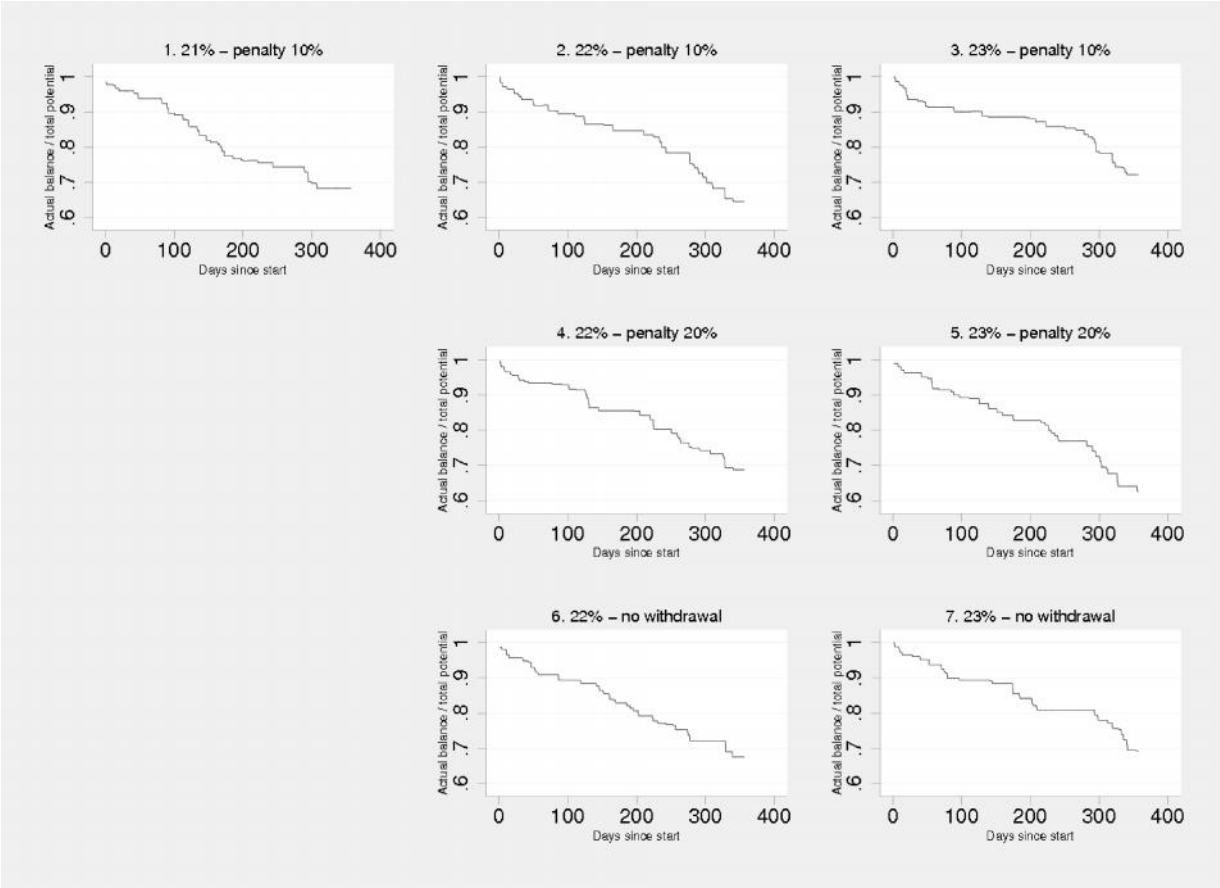
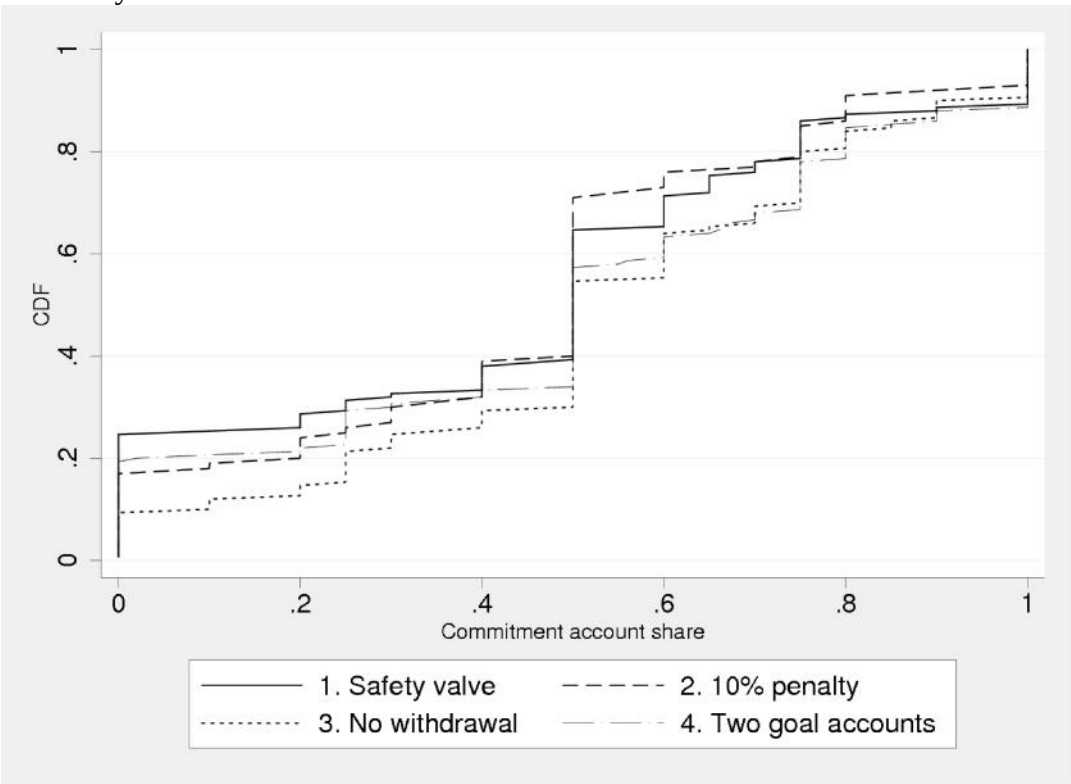


Figure 3. Distributions of Fraction of Endowment Allocated to Commitment Account: Experiment 2

These figures show cumulative distribution functions of fraction of endowment allocated to a commitment account.

A. By treatment condition



B. Within condition offering two commitment accounts, by commitment account type

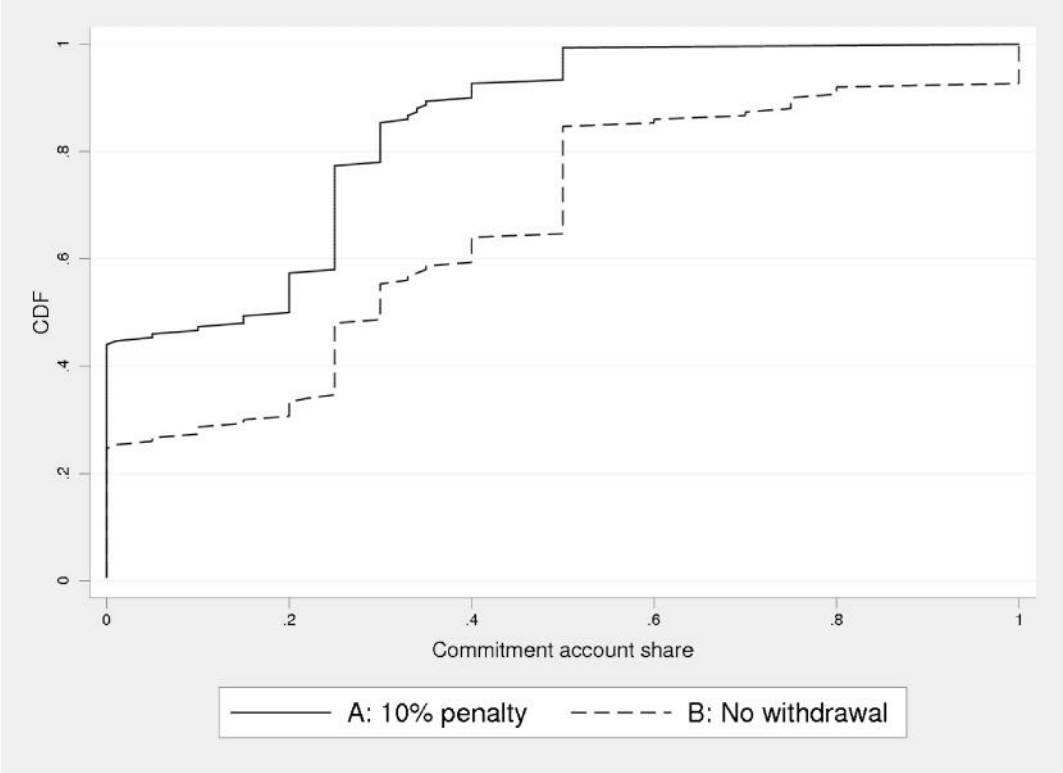


Figure 4. Withdrawal Patterns by Treatment Condition: Experiment 2

For each treatment condition, these figures show withdrawal patterns over the course of the experiment. For each subject and for each day, we calculate the sum of the liquid account and commitment account balances that the subject would have had if no withdrawals had been requested. We then calculate the ratio of the subject's actual balance to this hypothetical total balance, and we plot the mean of this ratio against the number of days since the initial allocation decision.

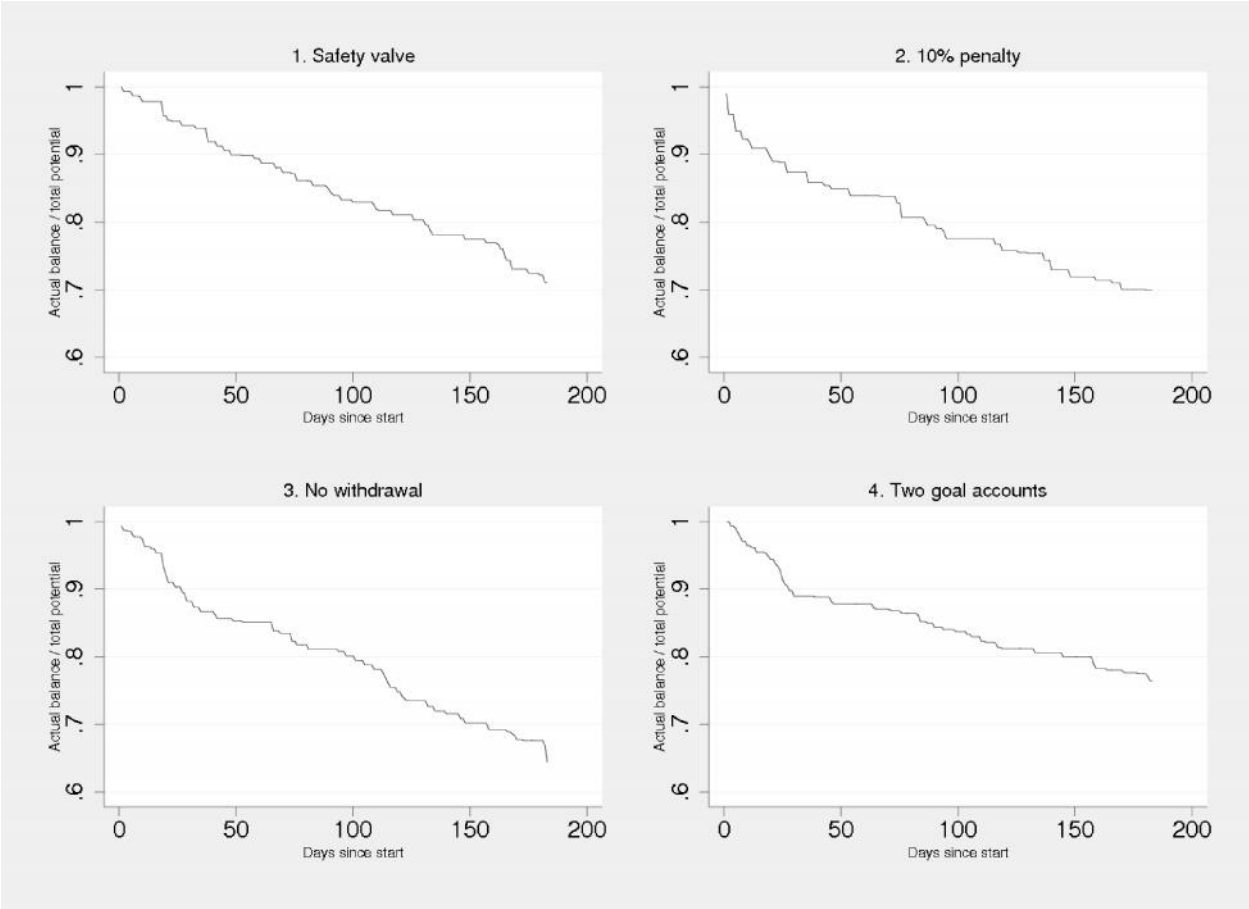
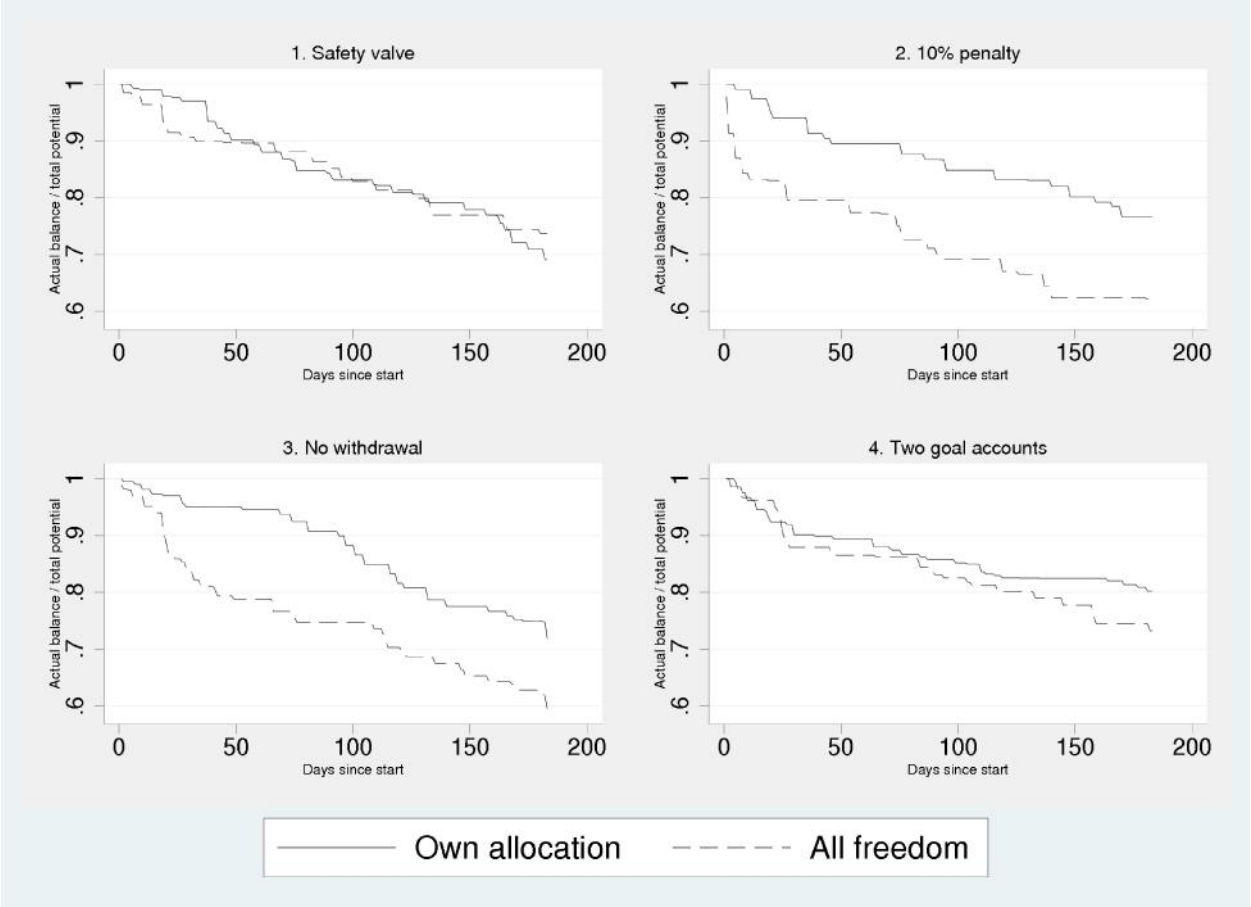


Figure 5. Withdrawal Patterns for Own versus All Liquid Allocation: Experiment 2
For each treatment condition, these figures show withdrawal patterns over the course of the experiment for subjects who were randomly assigned to receive their chosen allocations and for subjects who were randomly assigned to receive their entire endowment in the liquid account. For each subject and for each day, we calculate the sum of the liquid account and commitment account balances that the subject would have had if no withdrawals had been requested. We then calculate the ratio of the subject's actual balance to this hypothetical total balance, and we plot the mean of this ratio against the number of days since the initial allocation decision.

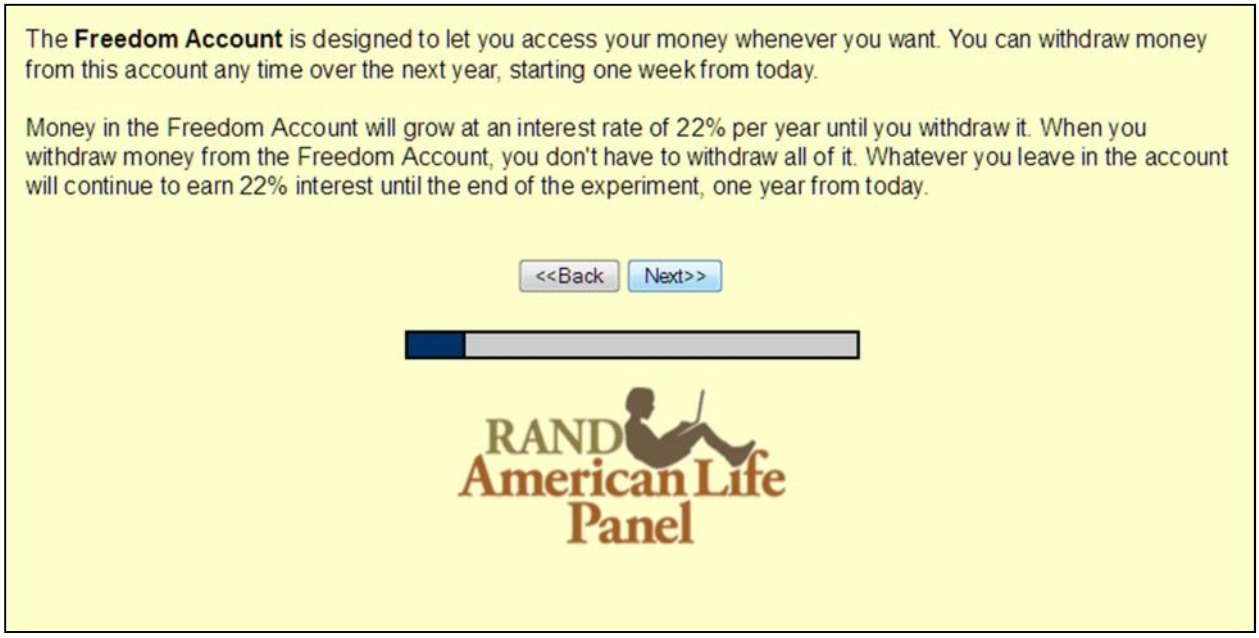


Appendix Table. Regression Analysis of Fraction of Endowment Allocated to Commitment Account and Dollar-Weighted Days to Commitment Date: Experiment 1

This table reports coefficients and t-statistics from ordinary least-squares regressions where the outcome variable is fraction of endowment allocated to a commitment account or dollar-weighted days to commitment date. Dollar-weighted days to commitment date is defined as the fraction of endowment allocated to a commitment account multiplied by the number of days separating the allocation decision and the commitment date. There are three observations for every subject, one observation for each possible endowment amount. Standard errors are clustered at the subject level. *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

	Fraction of endowment allocated to commitment				Dollar-weighted days to commitment date			
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
20% penalty	0.059	(1.233)	0.053	(1.108)	18.708	(1.325)	18.788	(1.360)
No withdrawal	0.171**	(3.213)	0.182***	(3.442)	50.017**	(3.018)	54.858***	(3.312)
21% interest	-0.113*	(-2.580)	-0.114*	(-2.539)	-17.415	(-1.501)	-14.276	(-1.231)
23% interest	0.193***	(4.021)	0.187***	(3.931)	47.815***	(3.438)	45.886***	(3.340)
23% X 20%	-0.029	(-0.435)	-0.008	(-0.120)	-21.317	(-0.992)	-16.600	(-0.785)
23% X no w/d	-0.154*	(-2.125)	-0.156*	(-2.200)	-61.743**	(-2.734)	-59.862**	(-2.685)
$100			0.013	(1.665)			4.597*	(2.543)
$500			0.044***	(4.119)			18.670***	(6.289)
Male			0.008	(0.283)			-3.558	(-0.420)
Age 26-35			-0.136*	(-2.400)			-17.471	(-1.022)
Age 36-45			-0.075	(-1.352)			-5.097	(-0.291)
Age 46-55			-0.116*	(-2.022)			-6.640	(-0.353)
Age 56-65			-0.155*	(-2.452)			-4.634	(-0.229)
Age > 65			-0.131*	(-2.138)			-18.947	(-0.920)
Some college			-0.010	(-0.268)			-7.878	(-0.640)
Assoc or BA			-0.067	(-1.898)			-18.274	(-1.621)
Higher degree			-0.069	(-1.445)			-12.562	(-0.835)
Inc 15-35k			0.098	(1.794)			60.215***	(3.733)
Inc 35-50k			0.118*	(2.030)			48.562**	(2.823)
Inc 50-75k			0.107	(1.882)			55.365**	(3.268)
Inc 75-100k			0.125	(1.964)			43.453*	(2.201)
Inc > 100k			0.075	(1.090)			42.392*	(2.012)
Married			0.027	(0.905)			10.853	(1.159)
Working now			-0.003	(-0.091)			1.047	(0.102)
White			-0.047	(-1.427)			-6.695	(-0.675)
0 hhold mem			0.068	(1.685)			24.915*	(2.086)
1 hhold mem			0.052	(1.239)			9.896	(0.806)
2 hhold mem			-0.018	(-0.371)			-1.944	(-0.140)
Constant	0.389***	(11.371)	0.390***	(5.199)	81.761***	(9.012)	27.645	(1.202)
Observations	1485		1485		1485		1485	

Appendix Figure 1. Description of the Liquid Account



Appendix Figure 2. Description of the 22% Interest Rate, 10% Early Withdrawal Penalty Commitment Account

The **Goal Account** is designed to help you save. You can withdraw money from this account without penalty any time after a goal date that you pick. Setting a goal for yourself and picking the right goal date can help you avoid the temptation to spend your money too soon.

Money in the Goal Account will grow at an interest rate of 22% per year, both before and after the goal date, until you withdraw it. When you withdraw money from the Goal Account, you don't have to withdraw all of it. Whatever you leave in the account will continue to earn 22% interest until the end of the experiment, one year from today.

As explained earlier, if you withdraw money from the Goal Account before your goal date, you will incur a penalty equal to 10% of the amount you withdraw.

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To use the Goal Account, you will need to pick a goal date. You might want to pick a date based on something you want to save money for, like a birthday gift, holiday presents, vacation, or any other special purchase that you plan to make. You can also use the Goal Account as a way to help you save, even if you don't have a special purchase in mind.

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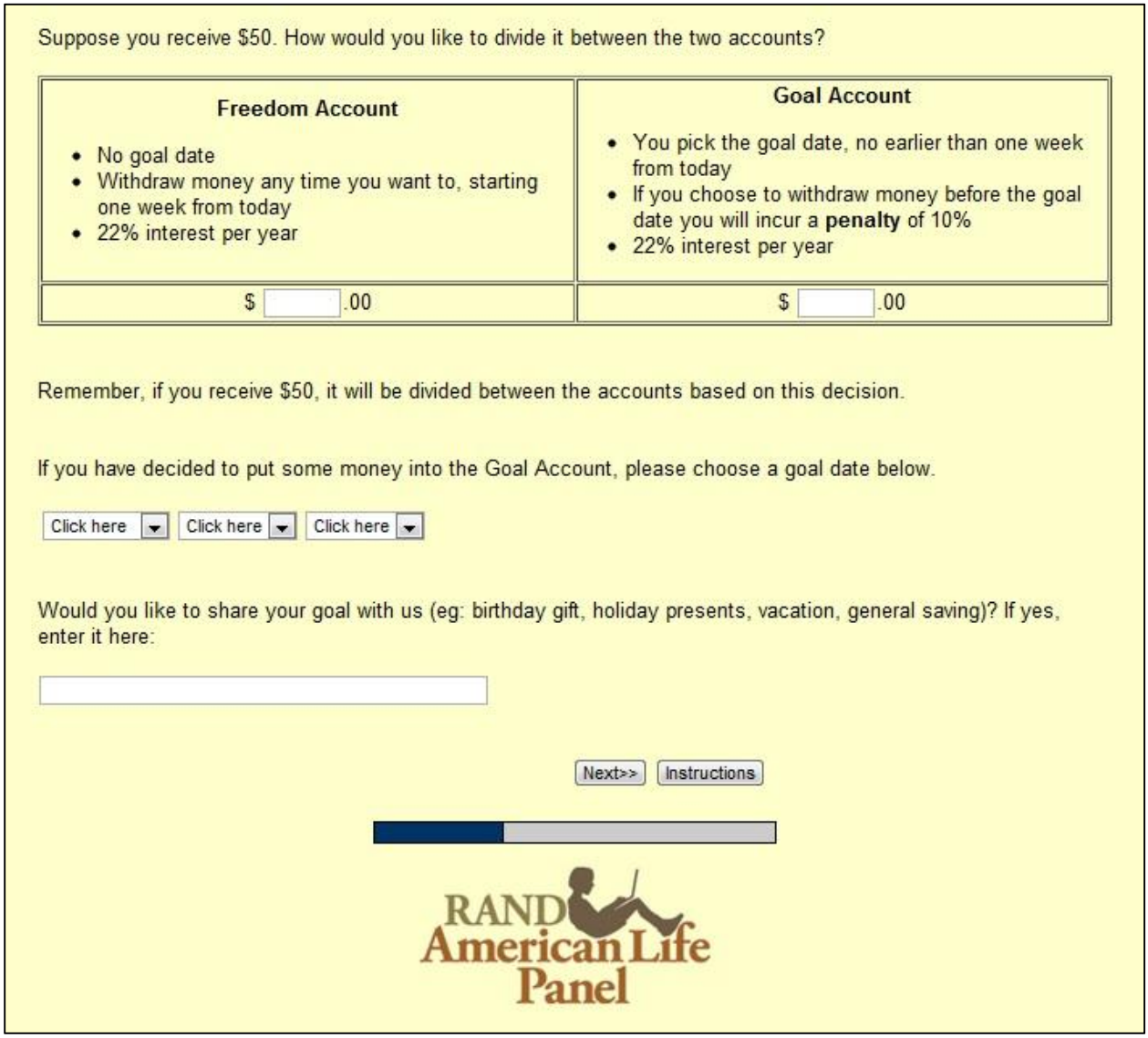
Appendix Figure 3. Example Allocation Page



Appendix Figure 4. Sample Weekly Email to Subject

Dear Subject,

This is a breakdown of your current balances:

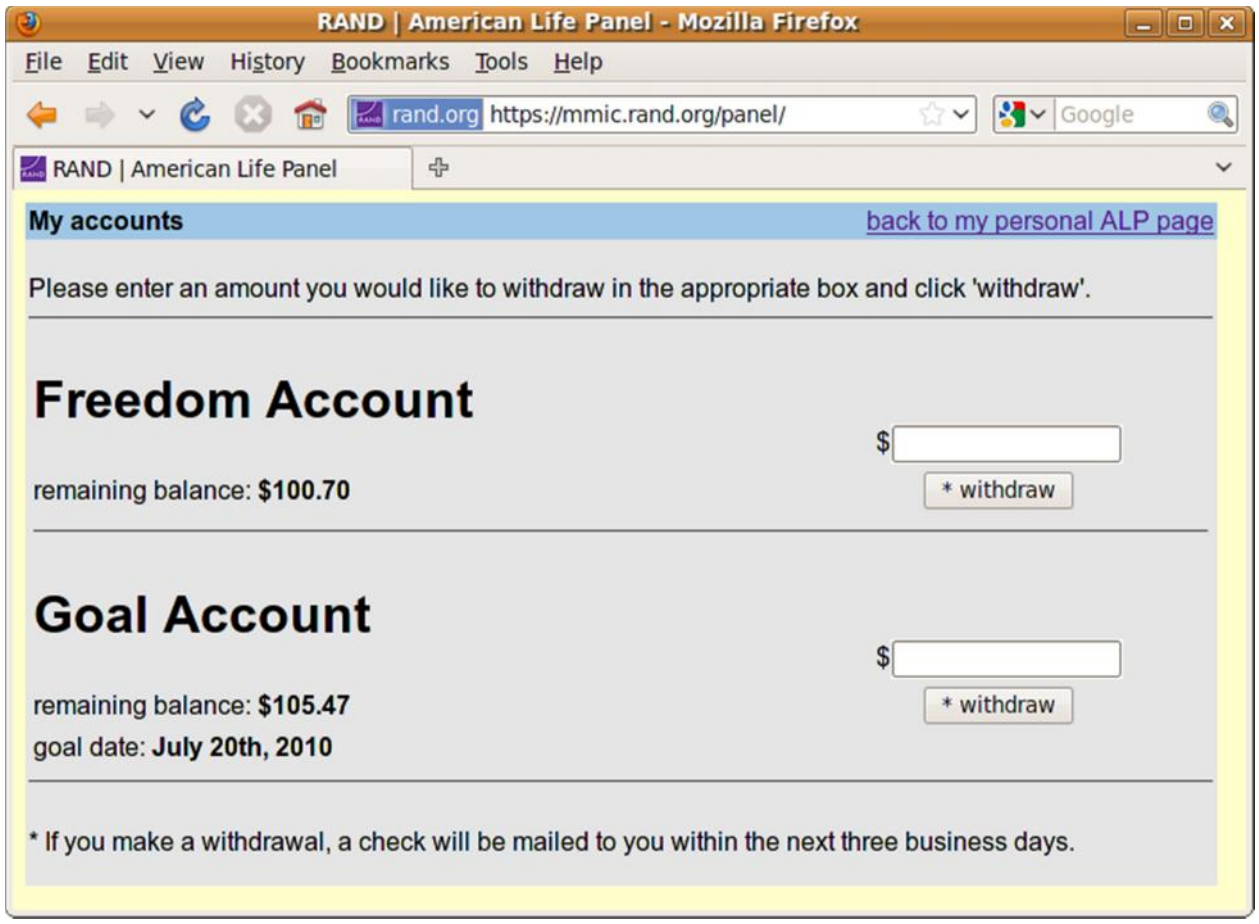
Freedom Account: $24.25
Goal Account: $53.18
Goal Date: July 20th, 2010

If you wish to withdraw any money from your accounts, please go to your panel pages and click on the "Savings Game" button: https://mmic.rand.org/panel

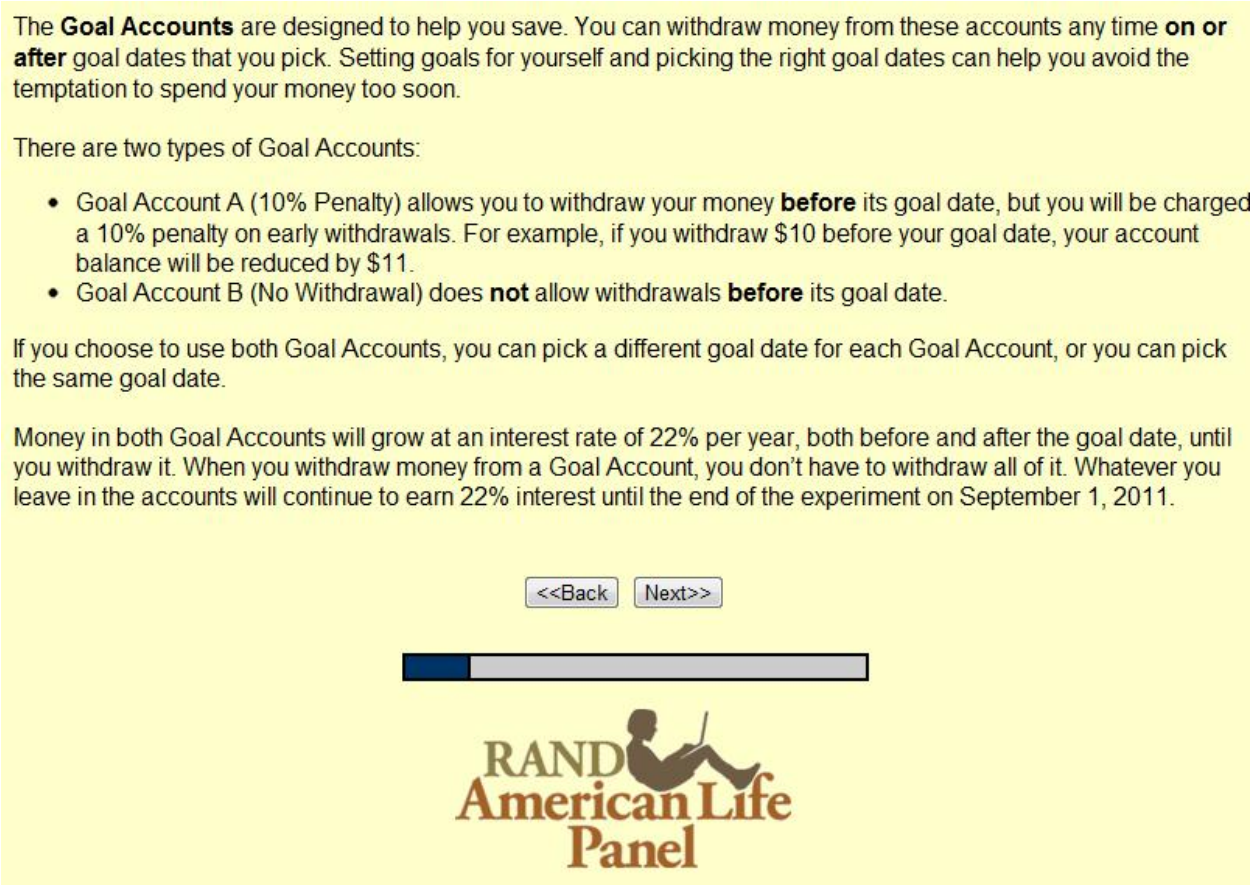
If you have any questions about this game or your accounts, please feel free to contact us at webhelp@rand.org or 866.591.2909

Thanks!
www.rand.org/alp

Appendix Figure 5. Withdrawal Interface



Appendix Figure 6. Description of Two Commitment Accounts Offered Simultaneously



Appendix Figure 7. Description of the Safety Valve Commitment Account