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RAFAEL MORAES BATISTA

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To my parents, Vera and Guilherme Batista, my partner, Megan Kang, and my advisors, Sendhil Mullainathan and Abigail Sussman— who gave me space to wonder.

"Science is about a great many things, says Doyne Farmer. It's about the systematic accumulation of facts and data. It's about the construction of logically consistent theories to account for those facts... But at heart, he says, science is about the telling of stories—stories that explain what the world is like, and how the world came to be as it is. And like older explanations, such as creation myths, epic legends, and fairy tales, the stories that science tells help us understand something about who we are as human beings, and how we relate to the universe."

— Doyne Farmer, quoted in M. Mitchell Waldrop,
Complexity: The Emerging Science at the Edge of Order and Chaos (1992)

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INTRODUCTION

This dissertation examines how individuals think about and manage their personal finances, with a particular focus on decisions regarding assets and debt. The work is organized as three distinct but conceptually related essays on consumer financial decision-making.

The first essay, "Self-Other Differences in Perceptions of Wealth," explores how people evaluate wealth differently when considering their own financial situation versus that of others. Through a series of experiments, this work demonstrates that individuals focus disproportionately on debt when thinking about their own wealth compared to others' wealth. This differential focus affects both future financial planning and present consumption decisions. The findings extend the literature on self-other differences while offering new insights into how social comparisons shape financial behavior.

The second essay, "Disclosing the costs of co-holding liquid assets and high-interest debt has limited impact on behavior," describes the nature of "co-holding"—simultaneously maintaining liquid assets and revolving credit card debt—using a novel, transaction-level dataset from a large retail bank in Australia. It then attempts to change co-holders' behavior through a large-scale randomized controlled trial. Specifically, the study is designed to test whether increasing awareness of co-holding behavior affects debt repayment. Despite the seemingly irrational nature of this behavior, the experiment reveals that simply making consumers aware of their co-holding status and its costs does not meaningfully change repayment behavior. These findings challenge conventional assumptions about the role of attention and awareness in financial decision-making. The findings also suggest that simple information disclosure, even when carefully designed and delivered through trusted channels, may not effectively address costly financial behaviors.

The third essay, "Keeping Cash and Revolving Debt: How Consumers' Preference for Spending on Debit versus Credit Influences Their Decision to Co-Hold," develops a theoretical framework linking payment method preferences to co-holding behavior. Building

on transaction-level data and survey evidence, this work demonstrates that consumers who prefer using debit cards for everyday expenses are more likely to engage in co-holding. The analysis reveals how mental accounting practices and payment preferences can lead to seemingly suboptimal financial outcomes. This essay advances our understanding of co-holding by identifying a novel psychological mechanism driving the behavior.

ESSAY 1

SELF-OTHER DIFFERENCES IN PERCEPTIONS OF WEALTH

This essay is adapted from Batista, Rafael M., Abigail B. Sussman, and Jennifer S. Trueblood (2023), "Self-Other Differences in Perceptions of Wealth," Journal of Experimental Social Psychology, 104, 104420.

1.1 Introduction

Picture a colleague driving into work with a brand-new luxury car. Seeing the shiny rims and the leather seats, you might be left feeling a bit of envy, wishing you were as wealthy as them. After all, consumption tends to signal greater wealth (Veblen, 1899). But then try to imagine yourself with that new car. Assuming you do not already own one, what would it take for you to arrive at work tomorrow with the exact same model? It may become apparent that expensive cars often come with hefty car loans. Together, these observations reveal something basic about how people perceive their fortunes when compared to others'. Debt appears to be more prominent in a person's evaluation of their own wealth relative to others' wealth.

This paper examines how people evaluate their wealth in contrast to how they assess the wealth of others. Differences in how people think about assets and debts for themselves and others shape how they plan for the future and how they perceive consumption today. In particular, people focus on their debt more than they believe others do when judging wealth. Focusing on debt when thinking of one's finances leaves people more motivated to pay off what they owe while also feeling worse than their peers today. In brief, these self-other differences shape both future beliefs and present assessments of one's own wealth relative to others.

Across six experiments, we find evidence that people tend to focus more on debt when thinking of themselves than others, despite being provided with complete information about assets and debt. First, people predict they will be wealthier than others in the future (Studies

1-3). Beliefs that one's own debt will shrink more than others' largely drive this difference. Second, when asked to expand on their predictions, people more frequently referred to paying down debt (vs. growing assets) when thinking of their own strategies than when thinking of others' (Study 2). Third, people appear more motivated to repay their debt than they believe their peers are, allocating more of a hypothetical windfall to their debt than they think others would (Study 3). Fourth, when presented with information about a recent purchase, participants imagined others would feel wealthier, even controlling for the price of the good and the amount used to finance it (Studies 4-6). This suggests that not only does focusing on debt impact how people plan for the future, it also shapes perceptions of present consumption. Finally, participants appear to implicitly recognize the gap between self and others, weighing their own debt less when asked to consider their own situation from another person's point of view (Study 6).

One's wealth (i.e., net worth) is determined objectively by their assets minus whatever debt they owe. Still, how wealthy a person is *perceived* to be is more malleable. Perceptions tend to be shaped by what draws the perceiver's attention (Morewedge, Holtzman, and Epley, 2007; Sussman and Shafir, 2012; Thaler, 1999). Whether assets or debts stand out as a signal of wealth depends on whether total net worth is positive or negative (Sussman and Shafir, 2012). For example, two people could be equally wealthy with the same positive net worth, but the person with less debt will be perceived as wealthier. Furthermore, how wealthy one perceives themselves to be can affect subsequent spending decisions (De La Rosa and Tully, 2022; Sussman and Shafir, 2012).

Nevertheless, research in this area has tended to separately examine questions about one's perception of themselves or others. The following section reviews the literature on self-other differences, suggesting that a closer examination of wealth perceptions along this dimension is warranted.

1.1.1 Self-Other Differences

People tend to believe that others "have more money" and "have more disposable income" (Matthews, Gheorghiu, and Callan, 2016) than they themselves do and that this leaves others willing to pay more for everyday items than they would themselves (Frederick, 2012).[1] What remains unclear is whether people perceive others' wealth differently than their own, even after holding constant the target's (self; other) assets and debts.

Researchers have long examined gaps in self-other perceptions (e.g., Brown and Gallagher, 1992; Feather and Simon, 1971). Early accounts of self-other differences attributed these discrepancies to asymmetric information or informational disparities (e.g., Kuiper and Rogers, 1979; Prentice, 1990; Ross and Sicoly, 1979). For example, in Ross and Sicoly's (1979) studies, participants were ready to assume more responsibility on a project or in a game than others gave them credit for. An effect that diminished when others' contributions were made more salient.

Nevertheless, even when others are on equal footing to oneself in terms of information provided, attentional biases may lead to a gap in perceptions. In other words, people attend to different features when evaluating their own situation versus that of others. For example, Williams and Gilovich (2012, , Study 3) show that people rely on their own peak performance when predicting how they will do on a future exam. However, people instead rely on an average of past performances when predicting how others will do. In another set of papers, Kahneman and colleagues (Kahneman, 2006; Schkade and Kahneman, 1998) argue that differential attention—which they term a "focusing illusion"—leads people to believe that certain features are more important in others' lives than their own. Consistent with literature on goal-directed attention, differences in goals for the self versus others may contribute to this differential attention (e.g., Corbetta and Shulman, 2002). For example, a given individual's

1. Galesic, Olsson, and Rieskamp (2012) and Dawtry, Sutton, and Sibley (2015) offer evidence that is largely in support of this, and suggest social sampling may play a role.

motivation to maximize their own outcomes in comparison to another person's could shift both attention and subsequent actions. In this paper, we home in on differential attention as a contributing factor to self-other differences in perceptions of wealth.

In this paper, we seek to explore whether self-other differences extend to how people evaluate wealth:

> **Hypothesis 1.** Holding information about assets and debt constant, people will evaluate wealth differently for self versus others. They may either (a) focus on their successes (i.e., assets) more than the successes of others or (b) focus on their hardships (i.e., debt) more than the hardships of others.

Earlier research provides competing predictions about what features one might attend to for the self versus others. On one hand, the work by Williams, Gilovich, and Dunning (2012), mentioned above, suggests that people might focus on different components of wealth for self versus others. If people overweight appealing features, such as one's assets, for themselves, they may perceive themselves to be better off financially than others. This finding would be largely consistent with literature on "better-than-average" effects (for a recent review, see Zell et al., 2020). This literature would suggest that, to the extent people perceive their own wealth differently than others, people would consider themselves as better off, corresponding to Hypothesis 1a.

On the other hand, more recent work has highlighted a tendency for people to focus on their own effort and obstacles while overlooking those of others (e.g., Davidai and Gilovich, 2016; Kruger and Savitsky, 2009; Schroeder, Caruso, and Epley, 2016). For example, when people are explicitly asked about challenges, they can more quickly recall obstacles in their own life while often failing to recognize the struggles of others (Davidai and Gilovich, 2016). From this perspective, people might overweight debt for themselves more than they do for others, causing them to see others as financially better off even at similar levels of assets and debt. This pattern would correspond to Hypothesis 1b.

Furthermore, we anticipate that a differential focus, consistent with either Hypothesis 1a or 1b is likely to have downstream consequences for perceptions and behaviors. For example, if people focus more on their assets than their debts, they may neglect outstanding balances (and the accompanying interest rates) when thinking about their future wealth. Conversely, if debt is more salient when thinking about one's own wealth versus others', one might see themselves as worse off financially, even at similar levels of wealth. Therefore, contingent on finding evidence in support of Hypothesis 1a or 1b, we propose:

> **Hypothesis 2.** Differences in how people think about assets and debts for themselves versus others will influence both how people plan for the future and how they perceive current consumption.

1.1.2 Social Components of Wealth Perception

People care about where they stand relative to others (e.g., Frank, 1985; Hagerty, 2000; Walasek and Brown, 2015, 2016). When it comes to finances, people want to be wealthier than their peers (Bottan and Perez-Truglia, 2020; Boyce, Brown, and Moore, 2010; Perez-Truglia, 2020). They also want to be *perceived* as wealthier than their peers (e.g., Charles, Hurst, and Roussanov, 2009; Clingingsmith and Sheremeta, 2018; Han, Nunes, and Drèze, 2010; Wilcox, Kim, and Sen, 2009). Wealth itself, however, is not easily observed. People instead rely on consumption (e.g., Charles, Hurst, and Roussanov, 2009; Ordabayeva, Lisjak, and Jones, 2022; Veblen, 1899) to signal their own wealth and infer the wealth of others. Yet, people increasingly consume using borrowed money (Bricker et al., 2017). [2] While consumption is conspicuous, borrowing is not (Georgarakos, Haliassos, and Pasini, 2014).

When spending requires borrowing, people may find themselves torn between two images: their social image and their self-image. *Social image* is the image others have of the target

2. Bricker et al. (2017) note that though the proportion of households with debt has increased, the debt burden per household has generally decreased.

individual or, from the target's perspective, the image they believe others have of them. In the latter sense, social image is a belief about beliefs, or a second-order belief. Concerns about social image affect many behaviors, including voting, effort in the workplace, engagement in the classroom, and investment decisions (for review, see Bursztyn and Jensen, 2017). People may, therefore, choose to consume using borrowed money to keep up social appearances. Conspicuous consumption through inconspicuous debt creates a rather perverse incentive to "keep up with the Joneses" on credit. Research by Georgarakos, Haliassos, and Pasini (2014) shows that social image concerns lead to borrowing and an increased chance of ending up financially distressed.

In contrast, one's *self-image*, or personal identity, is the image one has when they thinks of themselves (Oyserman, Elmore, and Smith, 2011). Self-image can include personal traits, characteristics, and goals, often decontextualized of one's social role (Oyserman, 2009). People often come to know themselves partly by observing their own circumstances (Bem, 1972). What one possesses can be a source from which one's self-image is both constructed (Ahuvia, 2005; Belk, 1988) and projected (Berger and Heath, 2007). One's assets can, therefore, bolster one's self-image. Nevertheless, assets acquired through borrowing can leave people feeling inauthentic (for related work, see Goor et al., 2020).

Social-image and self-image can often complement each other. Sivanathan and colleagues, for example, find that individuals experiencing threats to their self-image were more likely to seek high-status goods (Sivanathan and Pettit, 2010) and less likely to seek out low-status goods (Pan et al., 2014). Conversely, Bursztyn and colleagues (2018) provide experimental evidence that a boost in self-esteem reduces demand for status goods.

Despite being averse to debt (e.g., Amar et al., 2011; Gal and McShane, 2012; Prelec and Loewenstein, 1998), people may choose to borrow as a means to keep up social appearances (Georgarakos, Haliassos, and Pasini, 2014) and alleviate threats to self-image (Pettit and Sivanathan, 2011). Yet, over time debt can also leave people feeling worse off (Greenberg

and Mogilner, 2021). Debt, therefore, introduces a wedge between how one sees themselves and how they imagine others see them, suggesting a third hypothesis:

> **Hypothesis 3.** People will evaluate their own wealth differently when considering their self-image versus their social image. They may either (a) focus on their successes (i.e., assets) more when considering their self-image than when considering their social image or (b) focus on their hardships (i.e., debt) more when considering their self-image than when considering their social image.

This paper attempts to bridge these streams of research, exploring self-other differences in how wealth is perceived and the role these differences play in social interactions.

1.1.3 Overview of Current Research

This article reports the results of six studies that examined self-other differences in perceptions of wealth. Across all of our studies, we investigate whether people evaluate wealth differently for the self versus others, holding constant information about assets and debt. In every study, whether asking about the self or someone else, we present participants with complete information regarding wealth (i.e., assets and debt). Our study materials, preregistrations, data, and code are available on the Open Science Framework (osf.io/3q8tn). For all studies, we exclude from the analysis participants who did not complete the surveys in their entirety. We report all measures, manipulations, and exclusions.

In Study 1, we examined people's expectations for the future while holding current wealth constant for self and others. Participants appeared to think of wealth differently for themselves versus others, imagining different futures for themselves than for others when provided with present-day assets and debts. Specifically, participants believed that their debt would shrink more than other people's over one year. Since this belief about debts was not offset by an equivalent difference in beliefs about assets, it corresponded to predictions that

people would be wealthier in the future than they expected others to be. In Study 2, we investigated the origins of these expectations by analyzing participants' text responses. Participants tended to focus on paying off debt for themselves and growing assets for others. In Study 3, we tested for motivational differences by asking participants to allocate a windfall gain either towards repaying debt or growing assets. Participants allocated more money towards repaying the debt when considering the self than thinking of others.

If a focus on debt shapes how people plan for the future, it may also affect how they perceive their wealth today. Studies 4 and 5 explored how focusing on debt might shape perceptions of overall wealth for self and others when provided with full information about a large purchase. We varied the price of a house and car and the debt used to finance the purchase. Perceptions of wealth for the self were primarily shaped by the relative amount of debt, with less weight placed on the product's price. In contrast, others' wealth was evaluated based on both debt and the asset's price. In Study 6, we replicated the findings of Studys 4 and 5 while adding a third condition, which asked participants to consider how others might see them. Perceptions of wealth moved differently when thinking about others' judgments. When asked to evaluate their wealth from another person's point of view, they rated themselves as wealthier across the different debt levels. To the extent that people care about how others come to judge them, these differences introduce a conflict between one's self-image and their social image. Furthermore, people see others' differently from how they imagine others are seeing them.

1.2 Study 1

Study 1 investigated beliefs about future wealth. Specifically, it varied present assets and debts and asked participants to imagine how wealthy they expected themselves or someone else to be in a year. The study asked about, and included variation in, assets and debts because previous research suggests the makeup of wealth shapes perceptions of wealth

(Sussman and Shafir, 2012). What remained unclear is whether these perceptions differed when thinking about the self versus another person. Study 1 examined these differences by peering into beliefs about how wealth changes over time.

1.2.1 Method

Participants

495 participants (202 women, 293 men, age: M = 34.9, SD = 10.1) completed the experiment, which was approved by the Institutional Review Board at a Midwestern University. Participants were recruited via Amazon Mechanical Turk using the TurkPrime platform (Litman, Robinson, and Abberbock, 2017, ; precursor to the CloudResearch platform) in March 2019 with the intent of having approximately 125 participants per condition, prior to exclusions. The experiment had four between-participant conditions. The sample size was based on similar studies where participants generated predictions about future assets and debts (Trueblood and Sussman, 2021). The sample size was determined prior to starting recruitment and the data was analyzed only after all data had been collected. Participants were paid $1.00 for completing the experiment, which took an average of 13.9 minutes to complete.

Materials

The experiment had a 2 (between: self vs. other) by 2 (between: positive vs. negative net worth) by 2 (within: small vs. large asset size) mixed design. The stimuli consisted of 40 wealth profiles, which made explicit the total amount of assets and debts (Sussman and Shafir, 2012; Trueblood and Sussman, 2021). Of the 40 profiles, 20 were of positive net worth and 20 were of negative net worth. Participants were presented either the positive or negative set, depending on their assigned condition. Within each of the two sets, half of the

profiles had relatively small assets and debts (e.g., assets = $2,500 and debt = $1,300 in the positive net worth set) and the rest had relatively large amounts in assets and debts (e.g., assets = $323,440 and debts = $100,000 in the positive net worth set). A complete list of the wealth profiles used in this experiment are in the Supplementary Materials.

To provide participants with realistic wealth profiles, we generated the 40 profiles used in the experiment from asset and debt amounts calculated from the Panel Study of Income Dynamics (PSID; noa, 2019). The PSID is a longitudinal panel survey of American families that measures a number of different factors including wealth, income, health, employment, and many more topics. The PSID conducts their main interview every two years. We used data from the 2013 PSID survey to generate the profiles. First we calculated the total amount of assets and debts for the PSID respondents. The PSID has seven questions relating to assets, asking people to report the value of their checking and savings accounts, business/farm assets, vehicles, stocks, annuity/IRA, other real estate assets, and other assets. There are eight questions relating to debt, which ask people to report their debt related to credit cards, business/farm, medical debt, student loans, legal debt, family loans, other real estate, and other debt. We also included home equity, that is, home value minus mortgage, in our calculations. Next, based on our calculations of respondents' assets and debts, we calculated their total net worth and defined "small profiles" to be between the 0.05 and .25 quantiles of the net worth distribution. Likewise, we defined "large profiles" to be between the 0.75 and 0.95 quantiles of the net worth distribution. We calculated the quantiles separately for positive and negative net worth profiles. We then randomly selected 10 respondents who had positive net worth and "small profiles", 10 respondents who had positive net worth and "large profiles", 10 respondents who had negative net worth and "small profiles", and 10 respondents who had negative net worth and "large profiles". This generated our set of 40 profiles for the experiment.

Procedure

At the start of the experiment, participants were randomly assigned to one of four conditions in a 2 (Self; Other) by 2 (Positive; Negative Net Worth), between-participants design. Participants were told that they would be shown a financial profile—either another individual's or, hypothetically, their own—and that their task would be to predict what the financial situation would look like in one year. They were told that they would be asked to provide an estimate for both future assets and future debt. In the "other" condition, participants were told that they would see a different individual on each trial. Once participants read the instructions, they were asked to complete a guided practice. As part of the guided practice, participants were instructed to type a number into the text box provided. This was done to ensure participants knew not to include dollar figures or special characters and that all responses should be in terms of absolute value, so that no negative symbols were used for debts. Upon completing the guided practice, participants were each presented with 20 profiles in random order. Altogether, each participant reviewed 10 *small* profiles and 10 *large* profiles. For example, a participant might have been asked to "Imagine that [you have / Mr. G has] $2,500 in assets and $1,300 in debt." For each profile, participants were then asked to predict how much they (or this other person) would have in assets and in debt in one year; specifically, "In 1 year, what do you estimate [your / Mr. G's] assets will be?" and "In 1 year, what do you estimate [your / Mr. G's] debt will be?" Respondents were free to write in any numeric value between 0 and 4,000,000 for assets and between 0 and 400,000 for debts. The upper bounds for the text boxes were set based on the .99 quantile of the PSID data. In the 'other' condition, the names of the individuals were different on each trial and followed the format "Mr. G" where "G" was a single letter.[3]

Once participants completed the responses for each of the 20 profiles, they were asked

3. In Studies 1 and 2 and 4, "Mr." was always used; Studies 3, 5, and 6 had half the profiles referring to "Mr." and half referring to "Ms."

to report on their actual financial situation and about their demographics. For example, participants were asked how wealthy they felt, how much money they believed they could come up with in case of an emergency, and a set of questions assessing financial literacy (Lusardi and Mitchell, 2008) and their propensity to take on risk in the domains of gains and losses. [4]

1.2.2 Results

We excluded participants who failed an attention check or had multiple outlier predictions. We followed the exclusion criterion from Trueblood and Sussman (2021) where participants were excluded if they had 10 out of 40 responses (or 25%) that were three scaled median absolute deviations away from the median response for a given question (Leys et al., 2013). Exclusions were necessary because the text box response mode resulted in very noisy data as participants could enter almost any number they wished. This removed 113 participants, leaving 382 for the data analyses. The Supplemental Materials includes an additional table where we estimate similar models without any exclusions and controlling for participant demographics; the results are consistent with those reported here.

First, we examined whether participants' future predictions were generally larger or smaller than the stated present values using Wilcoxon signed rank tests. For assets in positive net worth profiles, participants' predictions were larger than present values for both self (Z = 18.47, $p < .001$) and other (Z = 16.87, $p < .001$). This was also true for assets in the negative net worth profiles for both self (Z = 13.11, $p < .001$) and other (Z = 13.62, $p < .001$). For debts in the positive profiles, participants' predictions were generally smaller than present values for both self (Z = -23.09, $p < .001$) and other (Z = -14.35, $p < .001$). This also held for debts in the negative profiles for both self (Z = -16.11, $p < .001$) and other

4. We had no specific plan for analyzing these variables and therefore do not report them in our main analysis. However, we describe them here for transparency (e.g. Simmons, Nelson, and Simonsohn, 2011, p. 1363). Material and data can be found on the OSF page linked above.

(Z = -2.26, $p = .024$). Thus, participants were generally optimistic about the future for both self and other, thinking that assets would grow and debts would shrink over the course of one year.

To further investigate participants' predictions about the future value of assets and debts, we calculated the growth rate for assets and debts for each profile separately:

$$\text{growth rate} = \frac{\text{Future Value - Present Value}}{\text{Present Value}} \tag{1.1}$$

where "Present Value" is the given asset or debt amount and "Future Value" is the participant's prediction. Figure 1.1 shows the growth rates for different types of profiles for participants in the self and other conditions.

To examine how growth rates varied by the characteristics of the wealth profiles and self versus other framing, we fit two robust linear mixed-effects models to the growth rates for assets and debts separately with three fixed effects: self-other (reference level = "self"), asset amount (in dollars), and debt amount (in dollars).[5] We allowed for by-subject random intercepts. Robust regression was used because the asset growth rates were heteroscedastic as determined by Levene's test for equality of variances ($p < .001$). We note that the assumption of equal variances did hold for debt growth rates (Levene's test with $p = .646$); however, for consistency we used robust regression for the debt growth rates as well. Robust regression was performed in R using the "robustlmm" package (Koller, 2016).

Results from the two regressions are shown in Table 1.1. For the asset growth rates, both the value of assets and debts were significant predictors of participants' growth rates. In particular, participants' growth rates were higher for small asset and debt values (assets: $b_{Assets} = -.00002$, $t = -2.333$, $p = .020$; debt: $b_{Debt} = -.0006$, $t = -14.597$, $p < .001$), implying small profiles were predicted to grow faster than large profiles. However, self-other was not a significant factor ($b_{Other} = -.022$, $t = -1.522$, $p = .129$) for asset growth rates.

5. In estimating the regressions we divided the asset and dollar amount by 1000

Figure 1.1: Study 1 results: The panels show the mean of the median growth rates for assets and shrinkage rates for debts for different types of profiles along with error bars equivalent to two standard errors. Median growth rates were calculated at the individual level for assets and debts, separately for the small and large profiles. The median was used at the individual level to handle outlier estimates.

For the debt growth rates, all three predictors were significant. In this case, growth rates were negative, which implies shrinking. Participants predicted debts would shrink faster for profiles with large assets than small ones (b_{Assets}) = −.0001, t = −9.716, $p < .001$). They also predicted that debts would shrink faster for profiles with small debts than large debts (b_{Debt} = .001, t = 24.782, $p < 0.001$). Importantly, unlike the asset growth rates, self-other was a significant predictor of debt growth rates (b_{Other} = .103, t = 3.718, $p < .001$). Participants in the "self" condition predicted debts would shrink faster than participants in the "other" condition.

Table 1.1: Coefficients of robust linear mixed effects regression models of asset and debt growth rates for Study 1

	Dependent variable:	
	Asset Growth Rate	Debt Growth Rate
	(1)	(2)
Constant	.117***	−.240***
	p = .000	p = .000
Other Condition	−.022	.103***
	p = .129	p = .000
Assets	−0.00002*	−0.0001***
	p = .020	p = .000
Debt	−.0006***	.001***
	p = .000	p = .000
Observations	7,640	7,640
R^2 Marginal	.030	.058
R^2 Conditional	.369	.601
Note:	*p<.05; **p<.01; ***p<.001	

1.2.3 Discussion

Results from Study 1 show that people are generally optimistic about the future, believing that assets will grow and debts will shrink for themselves and others. This optimistic view of the future holds for both positive and negative net worth profiles. While people made similar directional predictions for themselves and others, they tended to predict that their own debt would shrink faster than someone else's. However, there were no significant differences in predictions regarding assets. This evidence is consistent with the hypothesis that people evaluate wealth differently for the self versus others, holding constant information about assets and debt. The significant effect for debt but not for assets suggests that people might be focusing on their hardships (i.e., debt) more than the hardships of others.

1.3 Study 2

Study 1 shows that people believe their own debts will shrink faster over the course of one year as compared to the debt of others. But why do people show an asymmetry in their beliefs about future debt when thinking about themselves versus others? Study 2 examines this question by asking participants (1) to make predictions about the future value of assets and debts, similar to Study 1, and (2) to share in words why they made the predictions they did. If predictions between self and others differ because people focus more on debt when thinking about themselves, we should see debt repayment as central to their own strategy (versus what they believe others will do).

1.3.1 Method

Participants

501 participants (244 women, 257 men, age: M = 37.0, SD = 11.6) completed the experiment, which was approved by the Institutional Review Board at a Midwestern University.

Participants were recruited via Amazon Mechanical Turk using the TurkPrime platform in March 2019 with the intent of having approximately 125 participants per condition, prior to exclusions, and the experiment had four between-participant conditions. Like Study 1, the sample size was based on similar studies where participants generated predictions about future assets and debts (Trueblood and Sussman, 2021). The sample size was determined prior to starting recruitment and the data was analyzed only after all data had been collected. Participants were paid $1.00 for completing the experiment which they completed in 9.4 minutes, on average.

Materials

The stimuli consisted of eight wealth profiles (four positive net worth and four negative net worth where two of each were "small" profiles and two were "large" profiles, as above). These wealth profiles were drawn from Study 1. The Supplemental Materials includes a list of the profiles used.

Procedure

At the start of the experiment, participants were randomly assigned to one of four conditions in a 2 (Self; Other) by 2 (Positive Net Worth; Negative Net Worth) between-participants design. Each participant saw two of the eight profiles, depending on their assigned condition. The instructions mirrored those in Study 1, including a page for guided practice. Participants were told the current asset and debt levels and asked to estimate the asset and debt levels in one year. Unlike in Study 1, participants were provided with a text box after each set of predictions to explain why they made the judgment they made. Specifically, participants were reminded of their forecast, "On the previous question you thought [your / Mr. G's] assets would change from $19,000 to $X" where X was the value they predicted. Then, "In a few sentences, please explain why you made this judgment." Participants were then asked

about their debt forecast in a similar format.

Once participants responded to the two wealth profiles, they were asked about their actual financial situation and demographics. Participants were also asked how wealthy they felt, how much money they believed they could come up with in case of an emergency, and to respond to a set of questions assessing financial literacy and their propensity to take on risk.

1.3.2 Results

For the analysis below, participants were excluded if they failed the attention check or responded with two or more extreme predictions (out of four). Specifically, as in Study 1, we defined extreme predictions as responses that were three scaled median absolute deviations away from the median response for a given question. 409 participants remained for the data analysis. In the Supplemental Materials, we have included the primary models estimated without any exclusions which show qualitatively similar results.

Similar to Study 1, we first examined whether participants' future predictions were larger or smaller than the stated present values. For assets in positive net worth profiles, participants' predictions were larger than present values for both self ($Z = 3.42$, $p < .001$) and other ($Z = 6.04$, $p < .001$). This was also true for assets in the negative net worth profiles for both self ($Z = 3.32$, $p < .001$) and other ($Z = 3.11$, $p = .002$). For debts in the positive profiles, participants' predictions were smaller than present values for both self ($Z = -8.07$, $p < .001$) and other ($Z = -5.33$, $p < .001$). This also held for debts in the negative profiles for self ($Z = -7.08$, $p < .001$), but not other ($Z = -.68$, $p = .496$). Thus, participants were generally optimistic about the future for both self and other, except in the case of others' debt with negative net worth.

Next, we calculated the growth rate for assets and debts for each profile separately and fit two robust linear mixed-effects models to the growth rates with three fixed effects: self-other

(reference level = "self"), asset amount (in dollars), and debt amount (in dollars). As before, we allowed for by-subject random intercepts. Robust regression was performed in R using the "robustlmm" package (Koller, 2016).

Results from the two regressions are shown in Table 1.1. Similar to Study 1, we observe that self-other was not a significant factor ($b_{Other} = .003$, $t = .164$, $p = .870$) for asset growth rates. However, it was a significant predictor ($b_{Other} = .110$, $t = 5.363$, $p < .001$) of debt growth rates. Participants in the "self" condition predicted debts would shrink faster than participants in the "other" condition, replicating Study 1.

Table 1.2: Coefficients of robust linear mixed effects regression models of asset and debt growth rates for Study 2

	Dependent variable:	
	Asset Growth Rate	Debt Growth Rate
	(1)	(2)
Constant	.107***	−.290***
	p = .000	p = .000
Other Condition	.003	.110***
	p = .870	p = .000
Present Assets	−.0001*	−.0003***
	p = .010	p = .000
Present Debt	−.0008**	.003***
	p = .005	p = .000
Observations	824	824
R^2 Marginal	.028	.119
R^2 Conditional	.028	.119
Note:		*p<.05; **p<.01; ***p<.001

To analyze the text responses we first partitioned the responses into bigrams and examined frequency counts using a bag-of-n-grams model in Matlab. As shown in Figure 1.2, bigrams referring to "paying off" or "paying down" debt were the most frequent, followed

by the bigram "increase asset." This suggests that participants were thinking about simple strategies to improve their financial situation when making predictions in the task.

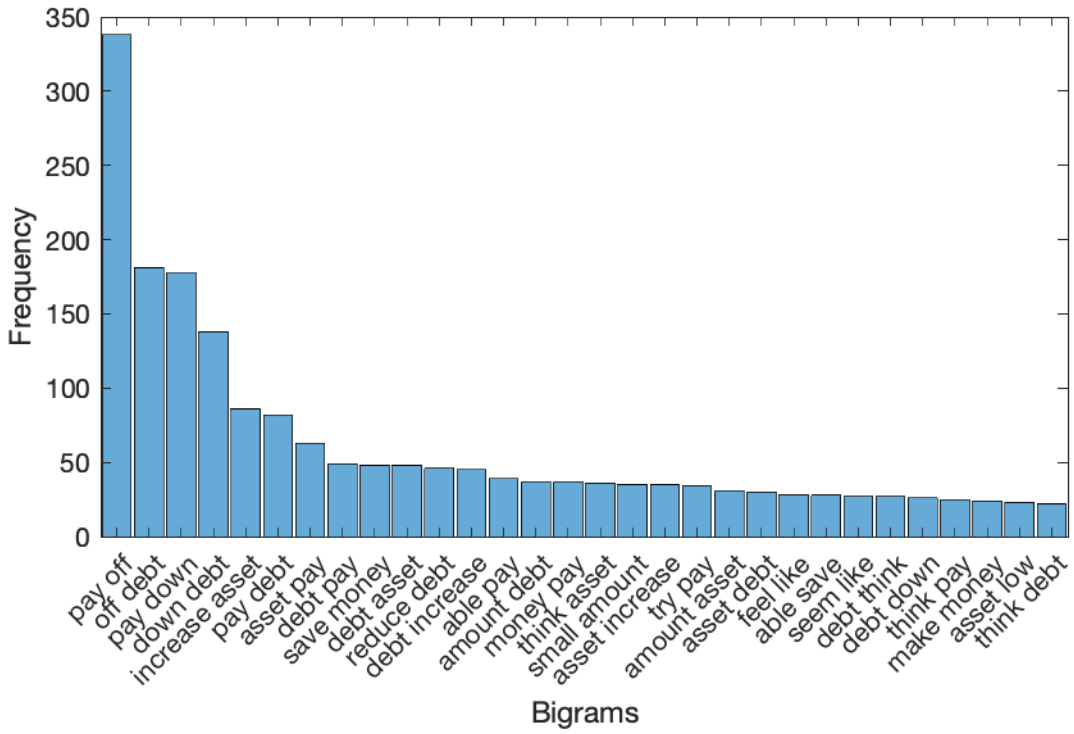


Figure 1.2: Frequency of the top 30 bigrams mentioned in Study 2 across both assets and debts.

Next, using the results from the bag-of-n-grams model, we defined three main strategies ("pay off debt," "pay down debt," and "grow assets") based off of the most frequently mentioned bigrams, shown in Table 1.3. We treat "pay off debt" and "pay down debt" separately because participants mentioned paying off debts more frequently for positive net worth profiles and paying down debt more frequently for negative net worth profiles ($\chi^2 = 11.61$, $p = .003$). This difference suggests that these terms may represent different strategies to participants. For example, participants may have used "pay off debt" to describe paying off all debt and used "pay down down" to describe paying off part of the debt.

We then combined the two text entries for a single profile for each participant (i.e., combined asset explanation and debt explanation for each profile for each person). After

Table 1.3: Strategies from text responses in Study 2

Strategy	Bigrams
Pay off debt	pay off; off debt; debt off
Pay down debt	pay down; down debt; debt down
Grow Assets	increase asset; asset increase; grow asset; asset grow; save money; make money; able save

combining the text entries, 59.5% of participants mentioned at least one of the three strategies (i.e., mentioned one of the bigrams in the Table 1.3). Further, most people only mentioned a single strategy (46% of all participants). Using this subset of participants, we analyzed how strategies varied across self / other conditions. We found that participants focused more on repaying debts when thinking about themselves than when thinking about others ($\chi^2 = 15.67, p < .001$). In the self condition, 52.6% of the strategies belonged to the "pay off debt" category, 31.4% of the strategies were from the "pay down debt" category, and 16.0% of the strategies were from the "grow asset" category. In comparison, in the other condition, 40.8% of the strategies belonged to the "pay off debt" category, 25.4% of the strategies were from the "pay down debt" category, and 33.8% of the strategies were from the "grow asset" category. This shows that there is a shift in thinking about debts to assets when changing the framing from self to others.

Finally we ran a sentiment analysis in Matlab, which categorized participants' subjective statements as positive, negative, or neutral. Overall, people were more negative when discussing debts as compared to assets, $t(1593) = 14.55, p < .001$. Interestingly, we did not find evidence of differences between self and other in either responses about assets (F(1, 380) = .255, $p = .614$) or debts (F(1, 390) = .055, $p = .815$). This suggest that people do not view others more positively or negatively than themselves in this domain. Rather, differences between self and other conditions are likely due to differences in thinking about financial strategies to shrink debt and grow assets.

1.3.3 Discussion

Together these findings help explain why people's future predictions of their own situation differ from those of others. In thinking about the future, people tend to focus on strategies to improve their financial situation by reducing their debt. In considering what others' approaches might be, plans to reduce debt do not feature as prominently.

In all cases, it is interesting that people home in on relatively simple strategies—paying down debt and growing assets. There are few mentions of more complex approaches, such as investing in the stock market or other assets, and almost no mention of external influences such as getting a promotion or the effects of compounding interest or fluctuations in the broader economy.

These findings further suggest that people may focus on different columns of the ledger when thinking about their own versus others' finances. This experiment provides further evidence that focusing on one's own hardships drives self-other differences in how wealth is perceived.

1.4 Study 3

The previous experiment sheds light on people's reasoning for their predictions. In particular, it offers one potential explanation for why the differential focus leads to different predictions when thinking about the self versus others. If debt weighs heavier on the mind for self, people might be more motivated to pay off this debt. In contrast, people may believe others are less motivated to pay off their debt. Study 3 probes further into this possibility by replicating the design in Study 2 and adding on a dependent measure intended to examine motivation.

1.4.1 Method

Participants

498 participants (244 women, 245 men, 3 transgender, 1 self-identified, and 5 not reported; age: M = 38.8, SD = 13.5; 68.5% white, 8.6% Black, 6.2% Latin American, 6.0% East Asian, 10.6% all others) completed the experiment. Participants were recruited via Prolific (www.prolific.co) in April 2022 with the intent of having approximately 125 participants per condition, prior to exclusions. The experiment had four between-participant conditions. The sample size was selected to be consistent with the earlier studies and pre-registered on AsPredicted.org (#89700) before any data was collected. Participants were paid $1.25 for completing the experiment that took them approximately 8.1 minutes to complete.

Materials

The stimuli included seven of the eight wealth profiles used for Study 2 (four positive net worth and four negative net worth profiles where two of the profiles included small assets and two included large assets). We replaced one of the profiles because the debt amount was under $1,000 (the amount available to allocate across repaying debt and building assets in the experiment scenario). We worried that the tendency for people to want to close debt accounts (e.g., Amar et al., 2011) would bias the results of the motivation measure described below. Tables 5 and 6 in the Supplemental Materials include the list of the profiles.

Procedure

At the start of the experiment, participants were randomly assigned to one of four conditions in a 2 (Self; Other) by 2 (Positive; Negative Net Worth), between-participants design. As in Study 2, participants were told that they would be shown a financial profile—either another individual's or, hypothetically, their own — and that their task would be to predict

what the financial situation would look like in one year. They were told that they would be asked to provide an estimate for both future assets and future debt. In the "other" condition, participants were told that they would see a different individual in each trial. Once participants read the instructions, they were asked to complete a guided practice. As part of the guided practice, participants were instructed to type in numbers into the text boxes provided. This was done to ensure participants knew not to include dollar figures or special characters and that all responses should be in terms of absolute value, so that no negative symbols were needed for debts. Upon completing the guided practice, participants were each presented with four profiles in random order. Altogether, each participant reviewed two small-asset profiles and two large-asset profiles. For example, a participant might have been asked to "Imagine that [you have / Mr. G has] $2,500 in assets and $1,300 in debt." For each profile, participants were then asked to predict how much they (or this other person) would have in assets and in debt in one year; specifically, "In 1 year, what do you estimate [your / Mr. G's] assets will be?" and "In 1 year, what do you estimate [your / Mr. G's] debt will be?" Respondents were free to write in any numeric value between 0 and 4,000,000 for assets and between 0 and 400,000 for debts. The upper bounds for the text boxes were set based on the .99 quantile of the PSID data. In the "other" condition, the names of the individuals were different on each trial and followed the format "Mr. G" where "G" was a single letter and the referenced gender ("Mr." or "Ms.) was balanced across profiles.

New to this experiment was a measure of motivation. Once participants made their predictions, they were presented with a short scenario: "Imagine [you / Mr. G] received $1,000 through a bank promotion, which can be used to pay down debt, deposited as savings, or both. Assuming [you / Mr. G] had [$1,300] in debt and [$2,500] in assets, how much of the $1,000 [would you use / do you believe they would use] to pay down their debt versus grow their assets (i.e., deposit into savings or checking)?" Participants were provided two boxes to input their responses, any numeric value between $0 and $1000, such that the total

summed to $1,000. Participants responded to this question for each profile. While there are other ways to use money (e.g., spend it, invest it, donate it, etc.; see Arkes et al., 1994; Li et al., 2019; Shapiro and Slemrod, 2003a), we were primarily interested in the difference in proportion allocated to debt between self and others. Indeed, in past research, when offered the option to use a windfall to repay debt, nearly half of respondents in America selected it (e.g., Asebedo et al., 2020; Shapiro and Slemrod, 2003b, 2009). The hypothetical bank promotion was designed to offer a plausible restriction to either grow one's assets or pay off one's debt.

As in Studies 1 and 2, once participants had responded to each of the four profiles, they were asked to report on their actual financial situation and demographics. Specifically, participants were asked to estimate their net worth, whether or not they discuss personal finances with family and friends, the value of their car (if they owned one), how much money they believed they could come up with in case of an emergency, and which zip code they resided in. These questions were excluded from the analysis below.

1.4.2 Results

Per our pre-registration, we excluded 74 participants who failed the attention check in the survey and an additional 21 participants who had four or more outlier responses. The outlier exclusion criteria was again three scaled median absolute deviations from the median response, calculated for each profile, separately for Positive and Negative Net Worth conditions, after excluding participants who failed the attention check. 403 participants remained for the pre-registered analysis. A parallel analysis without exclusions and one with exclusions but controlling for failing the attention check or having four or more outliers produces consistent results.

Similar to Studies 1 and 2, we started by examining whether participants' predictions were larger or smaller than the stated present values. For assets in positive net worth profiles,

participants' predictions were smaller than present values for self ($Z = -4.85$, $p < .001$) but larger than present values for other ($Z = 11.77$, $p < .001$). In the negative net worth conditions, assets were predicted to be larger than present values for both self ($Z = 10.33$, $p < .001$) and other ($Z = 8.71$, $p < .001$). In contrast, debt was predicted to be lower in the future than it was today for self in the positive net worth condition ($Z = -13.41$, $p < .001$) and in the negative net worth condition ($Z = -9.85$, $p < .001$). However, for other, debt was predicted to shrink less in the positive net worth condition ($Z = -6.10$, $p < .001$) and remain largely unchanged in the negative net worth condition ($Z = .94$, $p = .347$). We note that some of these results differ from those of the earlier studies. We believe this reflects changes in the economic environment in 2022, such as predictions of an impending recession.[6]

Next, we calculated the growth rate for assets and debts for each profile separately and fit two robust linear mixed-effects models to the growth rates with three fixed effects: self-other (reference level = "self"), asset amount (in dollars), and debt amount (in dollars). As before, we allowed for by-subject random intercepts. Robust regression was performed in R using the "robustlmm" package (Koller, 2016).

Results from the two regressions are shown in Table 1.4. Here we see weak evidence for self-other difference ($b_{Other} = -.039$, $t = -1.78$, $p = 0.076$) for asset growth rates, where people believe assets will grow less for others.[7] The evidence for self-other difference was stronger for debt growth rates ($b_{Other} = .115$, t = 6.44, $p < .001$). Participants in the "self" condition predicted debts would shrink faster than participants in the "other" condition,

6. A look at the front page of the New York Times and the Wall Street Journal highlights a very different economic climate between experiments. In April 2022, Americans were concerned about the invasion of Ukraine and surging inflation rates and the Dow Jones was down 5% from the start of the year. Looking back at March 2019, Americans were reading headlines about trade and foreign policy and the Trump Administration; Dow Jones was up 9% since the start of the year.

7. These results may appear to contradict the z-statistics reported above; further exploration into the data, however, suggests the inconsistency is driven by the aggregation of responses across within-participant conditions, Large and Small profiles. For those in the Self x Positive Net Worth condition, future predictions are larger than present value for Small Profiles ($M_{Diff} = 2,785$, Z = 7.25, $p < .001$) but smaller than present value for Large Profiles ($M_{Diff} = -20,552$, Z = $-.44$, $p = .663$)

replicating Studies 1 and 2.

Table 1.4: Coefficients of robust linear mixed effects regression models of asset and debt growth rates for Study 3

	Dependent variable:	
	Asset Growth Rate	Debt Growth Rate
	(1)	(2)
Constant	$.242^{***}$	$-.203^{***}$
	p = .000	p = .000
Other Condition	$-.039$	$.115^{***}$
	p = .076	p = .000
Assets	$-.00026^{***}$	$-.00002$
	p = .000	p = .434
Debt	$-.0011^{***}$	$.00106^{***}$
	p = .000	p = .000
Observations	1,612	1,612
R^2 Marginal	.052	.091
R^2 Conditional	.329	.613
Note:		*p<.05; **p<.01; ***p<.001

To test whether motivation to repay debt differed between Self and Other conditions, we asked participants to imagine receiving a windfall of $1,000 that could be used to grow one's assets or repay outstanding debt. We then estimated a linear mixed-effects model, regressing the fraction (out of $1,000) allocated towards repaying outstanding debt on three predictors: self-other (reference level = 'self'), asset amount (in dollars), and debt amount (in dollars). Like the models above, we allowed for by-subject random intercepts.

The amount allocated towards repaying debt differed significantly between participants who were asked to consider the self versus someone else ($b_{Other} = -.176$, t = -6.99, p $< .001$, 95% CI [$-.23$, $-.13$]). When thinking of one's own wealth, participants allocated more of the windfall towards their outstanding debt balance (see Table 1.5). This effect is

illustrated in Figure 1.3.

Table 1.5: Coefficients of linear mixed effects regression models of windfall allocation with by-subject random intercepts for Study 3

	Dependent variable:
	Prop Allocated to Debt
Constant	$.70^{***}$ p = 0.00 (.66, .74)
Other Condition	$-.18^{***}$ p = 0.00 (−.23, −.13)
Present Assets	0.0000 p = .98 (−.0001, .0001)
Present Debt	.0002 p = .33 (−.0002, .001)
Observations	1,612
Log Likelihood	−457.24
Note:	*p<.05; **p<.01; ***p<.001

We ran a set of exploratory analyses to examine whether the motivation to repay debt mediated the relationship between thinking about the self (versus other) and predicted growth rates. We used the proportion of the windfall allocated towards debt as the measure of motivation, consistent with the earlier analysis. As reported above, there was a significant relationship between thinking about the self (vs. another person) and motivation to repay debt ($p < .001$). We estimated the mediating effects for Asset Growth Rates and Debt Growth Rates separately (using the "lavaan" package in R, see Rosseel, 2012).

For Asset Growth Rates, we observe weak evidence of a total effect between self-other condition and predicted growth rate ($b = -.408$, $Z = -1.90$, $p = .058$). However, we

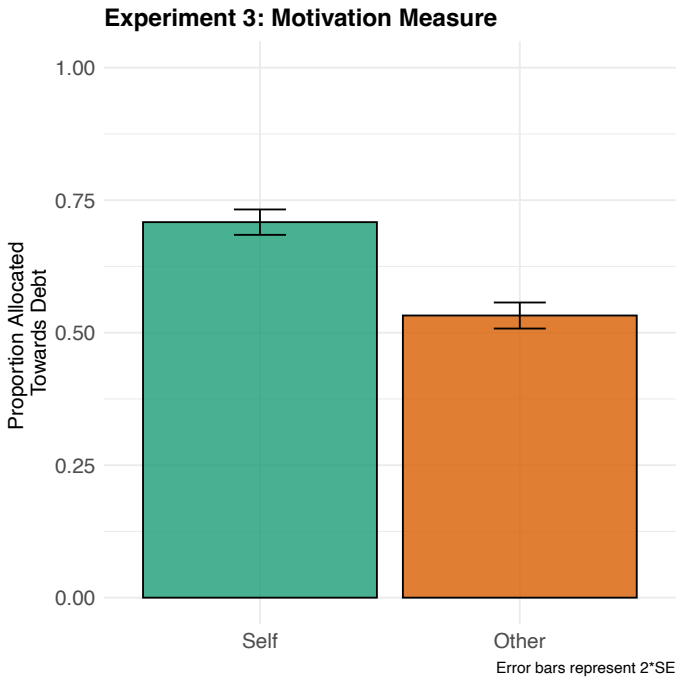


Figure 1.3: Study 3 results: For each profile participants were asked to imagine themselves or [Mr. G] receiving $1,000 before being asked how much would be allocated towards repaying outstanding debt versus growing assets (i.e. deposit into savings or checking). The bar graph displays the proportion allocated towards debt, split by Self versus Other.

do not observe evidence of a relationship between motivation to repay debt and predicted growth rate ($b = -.142$, $Z = -.456$, $p = .649$) nor do we find evidence of an indirect effect $(-.176)(-.142) = .025$, $Z = .46$, $p = .649$.

For Debt Growth Rates, the total effect between self-other condition and predicted growth rate was significant ($b = .151$, $Z = 4.86$, $p < .001$) as was the relationship between motivation to repay debt and predicted growth rate ($b = -.121$, $Z = -2.69$, $p = .007$). In addition, we find evidence of an indirect effect of motivation on predicted growth rate $(-.176)(-.121) = .021$, $Z = 2.61$, $p = .009$. These findings are consistent with motivation as a mediator of the relationship between self-other and predicted debt growth rates.

1.4.3 Discussion

Studies 1-3 provide evidence that people think about wealth differently for themselves versus others. While people uniformly tended to predict that financial conditions would improve, they reported noticeably different strategies when thinking of themselves compared to others. Participants' strategies tended to focus more on repaying debt. In practice, these differences might arise from asymmetrical information, divergent attention, or motivational processes that vary when thinking of the self versus others. Across studies, participants have full information by design.

In this study, however, we focus on whether people might be more motivated to repay their debt than they believe others are. Whereas Study 2 looked at what people said they would do, Study 3 illustrated what participants would choose to do. Participants allocated more funds towards repaying debt than they believed others would. Notably, this difference may play a mediating role in beliefs about the future, especially when it comes to changes in debt.

The three studies presented so far examined perceptions of wealth over time. Yet, it is possible that these differences shape perceptions not only of future wealth but also of wealth

today. In the next set of studies, we vary present-day levels of wealth by manipulating the price of a large and conspicuous product purchase (a new home) and how much of that cost was financed using debt. If debt weighs more heavily for the self than it does on perceptions of others, then we expect perceptions of others to remain relatively stable as debt levels increase compared to perceptions of the self at the same product price and relative level of debt.

1.5 Study 4

Studies 1, 2, and 3 documented that debt appears to be front-and-center in many people's minds, particularly when thinking about one's own finances. In the remainder of this paper, we explore consequences of this difference in focus. Study 4 studies the role of debt in perceptions of wealth directly by asking participants to imagine a recent purchase. If an individual focuses more on debt, the proportion of debt relative to the price of the product should play an out-sized role in how wealthy an individual is seen to be. Given the earlier findings, we expected this effect to be more pronounced when thinking about one's own finances. In other words, how wealthy a person feels will have more to do with the relative amount of debt they hold than the size of the asset they have acquired. Yet when looking towards another person, debt will play a smaller role. That is, in contrast with the self, when others spend more, they will be seen as wealthier (even at similar proportions of borrowing, i.e., more debt in absolute terms). This consistent focus on debt could lead to distinct patterns in which participants expect to pay down their own debt faster (vs. another person), which could lead to better financial outcomes in the future (as in Studies 1-3), but they also see themselves as less wealthy than another when they have more debt in the present.

In Study 4, participants were asked to imagine a recent purchase of a house. Houses are relatively visible products that make up a large expenditure in most households (Heffetz,

2011). They also tend to be products that require borrowing to acquire; among American households, over 40% possess a mortgage (Bricker et al., 2017).

It is important to note that the studies presented here are likely conservative tests for what we might expect outside of a controlled experiment since participants in all conditions were provided with complete information. In practice, others' debt is rarely observed, which would suggest a larger effect in the predicted direction. Nevertheless, here we are interested in examining the differential focus when the same information is available for self and others.

1.5.1 Method

Participants

405 participants (144 women, 261 men; age: M = 37.5, SD = 11.5) completed the experiment, which was approved by the Institutional Review Board at a Midwestern University. Participants were recruited from Amazon's Mechanical Turk using the CloudResearch platform in February 2020. The sample size was selected to be consistent with the earlier studies, approximately 100 participants per between-participant condition. The sample size was determined prior to starting recruitment. Participants were paid $1.00 for completing the experiment; average completion time was 7.0 minutes.

Materials

The stimuli consisted of 40 wealth profiles, made up of a home price and the current outstanding balance of the mortgage (see Supplemental Materials for a list of the profiles). Home price was either relatively High or Low. The value of the High-Priced houses were drawn from a uniform distribution of values between $1.1 million and $1.3 million and rounded to the nearest thousand. These were drawn once and then fixed for every participant in that condition. The same was done for the values used in the Low-Priced condition, only the

distribution ranged from $160,000 to $240,000.[8] The outstanding mortgage balance ranged from 0% to 95% in increments of five percentage points and was displayed to participants in dollars.

Procedure

Participants were randomly assigned to one of four conditions in a 2 (Self; Other) by 2 (Low-Price; High-Price) between-participant design. After agreeing to participate in the experiment, participants were informed that they would be reading "about an individual who has recently purchased a new home" or asked to "imagine that you have recently purchased a new home." For each profile, participants in the Other condition were asked to "estimate how well-off financially this person is" and were told that each profile belonged to a different person. Participants in the Self condition were, instead, asked to "decide how financially well-off you feel."[9] Each participant then saw 20 randomized wealth profiles for either the Low-Price or High-Price condition. For each profile, participants were either asked "How well-off do you feel financially?" or "How well off do you think [Mr. X] is financially?." Response options spanned five points, from "Not at all well-off" to "Extremely well-off." In the "other" condition, the names of the individuals were different on each trial and followed the format "Mr. G" where the referenced gender was always "Mr." and "G" was always a single letter.

As in Study 1, participants were asked to report on their actual financial situation and about their demographics before finishing the survey. In particular, participants were asked how wealthy they felt, their own net worth, how expensive they perceived a $200,000 house and $1 million house to be, whether they owned a home, how much money they believed

8. Note that while the "High-Priced" homes were considerably high, the "Low-Priced" homes represented prices close to the median house price in the U.S. at the time of this study.

9. We recognize the question wording was inconsistent between conditions; Study 6 aligns the language and replicates the main effect.

they could come up with in case of an emergency, and a set of questions assessing financial literacy (Lusardi and Mitchell, 2008) and their propensity to take on risk in the domains of gains and losses.

1.5.2 Results

No participants were excluded in this analysis.[10] To assess the role of price versus debt and self-other differences we estimated an ordinary least squares regression. We treated the scaled response as a continuous variable and regress it on an indicator variable for whether the participant was in the High-Price condition (i.e., reference level = Low-Price), another for whether they were in the Other condition (i.e., reference level = Self), and an interaction term for the two. The regression also adjusts for debt by including a continuous variable ranging from zero to 95 based on the amount borrowed as a percent of the total price. There were no by-subject random intercepts. Table 1.6 provides the full model specification along with the estimated coefficients.

Overall, others were seen as wealthier ($b_{Other} = .22, p < .001$, 95% CI [.15, .29]). Profiles with higher-priced homes, as one would expect, were also seen as better off ($b_{HighPrice} = 0.29, p < .001$, 95% CI [.22, .36]), but this effect appeared larger for when evaluating Others (relative to Self), $b_{Other*HighPrice} = 0.20, < .001$, 95% CI [0.10, 0.30]. Figure 1.4 illustrates these results. These effects also hold when we run the same regression predicting the rank of the response.

1.5.3 Discussion

The effect of price on perceptions of wealth was greater when evaluating others' wealth and had a smaller impact on how wealthy a person saw themselves, adjusting for the relative

10. Unlike Studies 1-3, participants in Study 4 responded on a 5 point scale (not a text box). Thus, the data were much cleaner and removal of outliers was not needed.

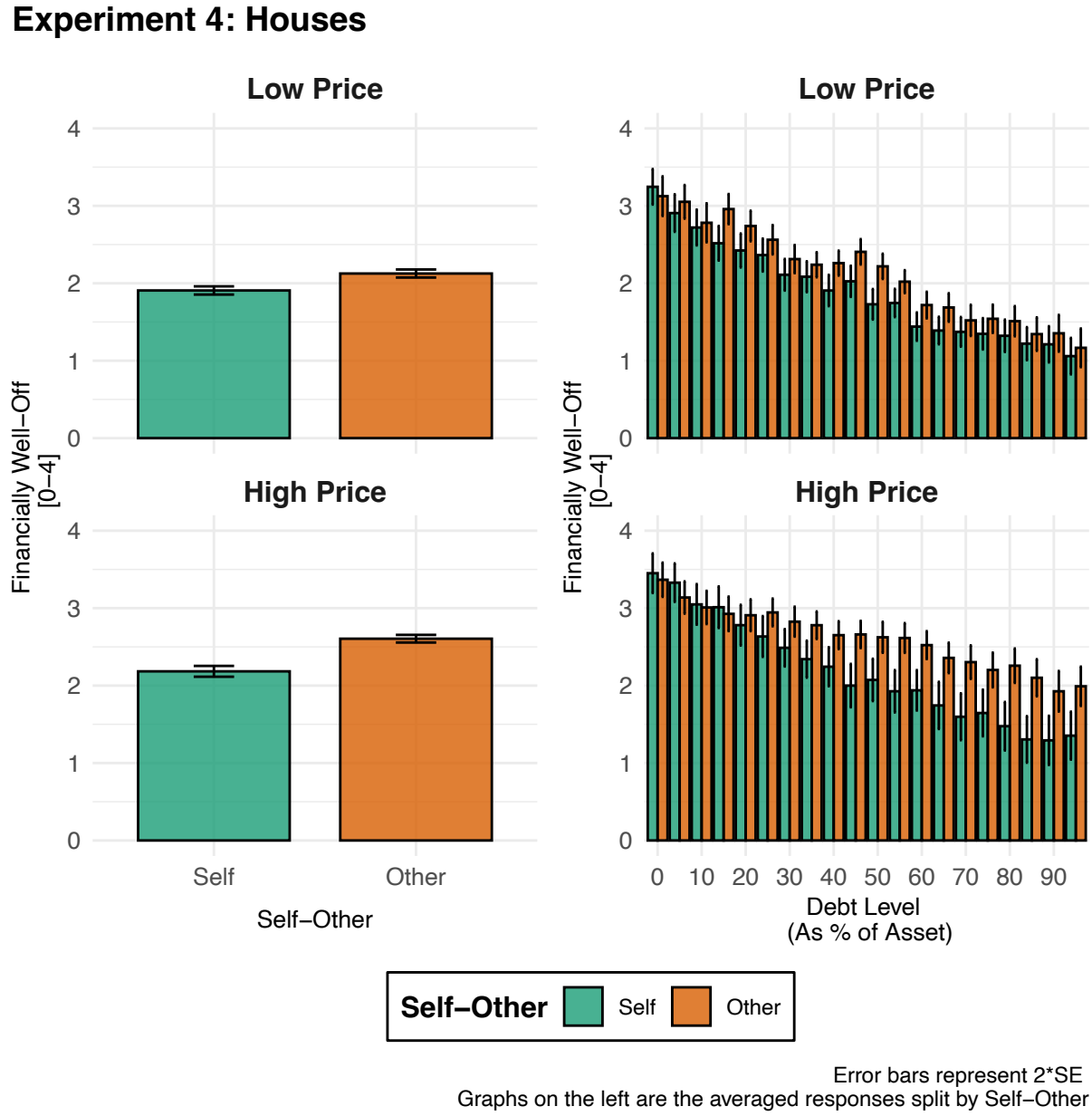


Figure 1.4: Study 4 results: Perceptions of wealth between self and others for Low-Priced and High-Priced homes. The panels on the left collapse responses across debt levels. The panels on the right show the results separately for each debt level. The scale consisted of five points, ranging from "Not well-off at all" to "Extremely well-off." Error bars are equivalent to two standard errors from the mean.

amount of debt. Although appearing somewhat contradictory with findings from Studies 1-3 in which a focus on debt corresponded to beliefs about greater wealth in the future, we propose that the two sets of results are in fact consistent and in support of Hypothesis 1b (i.e., focus on hardships). If people tend to focus more on debt when thinking of their own financial status, we should expect a larger difference in perceptions of wealth for self versus others as price increases. Holding the proportion of debt constant, more expensive homes come with larger liabilities.

Nevertheless, homes are unique on a few dimensions. Home buying involves an arduous process of demonstrating a person can be trusted to pay down their mortgage. Furthermore, buying a home is often seen as an investment, and mortgages are commonly seen as a form of "good debt" (Greenberg and Mogilner, 2021; Peñaloza and Barnhart, 2011). This suggests one limitation of the study, namely that relatively large mortgages, especially for million-dollar homes, might in-and-of-itself be a signal that one earns well, has plenty of money saved up, or is financially savvy. After all, not everyone can afford a million dollar mortgage and relatively few people would even qualify for one. To address this, the next study serves as a conceptual replication with a product (i.e., a car) that can also serve as a conspicuous proxy for wealth, typically requires some amount of borrowing, and, yet, is relatively more attainable.

1.6 Study 5

Having shown a difference in perceptions between self and other in the domain of mortgages, we modified the design to examine the effect for another product. Study 5 explored perceptions of wealth when informed of a recent car purchase. A similar pattern of results for car purchases would suggest the results in Study 4 are not specific to home purchases but, in fact, extend to another highly conspicuous product category at a much lower price-point than a home.

1.6.1 Method

Participants

In January 2021, 401 participants (193 women, 205 men, 2 Self-Identified, and 1 not reported; age: M = 39.2, SD = 12.1; 73% white, 7% Latin American, 7% Black, 6% East Asian) were recruited through CloudResearch's (Litman, Robinson, and Abberbock, 2017) "Approved Participants" list. The sample size was selected to be approximately the same as earlier studies, 100 participants per condition across four between-participant conditions. The sample size was determined prior to starting the experiment and pre-registered on As-Predicted.org (#55513).[11] Participants were paid $1.20 for completing the experiment which took, on average, 6.4 minutes to complete.

Materials

The stimuli consisted of 40 financial profiles, made up of a car price and the current outstanding loan balance (see Supplemental Materials for a list of the profiles). The value of the High-Priced cars were drawn from a uniform distribution of values between $57,999 and $59,999 and rounded to the nearest dollar.[12] The same was done for the values used in the Low-Priced condition, where the range spanned $17,999 and $19,999. These values were drawn once and then fixed for every participant in that condition. The outstanding loan balance ranged from 0% to 95% in increments of five percentage points; participants would then see the corresponding dollar amount.

11. While Study 5 was pre-registered, the hypothesis tested here was not. Study 6 serves as a replication, where the current hypothesis was pre-registered.

12. The range of High-Priced values were meant to approximate the 90th percentile of car prices listed on Kelley Blue Book (kbb.com. Low-Priced values were drawn from the 25th percentile. To avoid any left-digit bias, we rounded the value down where rounding to nearest thousand would have changed the left-most digit.

Procedure

Similar to Study 4, participants were randomly assigned to one of four between-participant conditions in a 2 (Self; Other) by 2 (Low-Price; High-Price) design. Participants were told to imagine themselves or another person owing some amount in car loans; each participant saw 20 profiles presented in a random order. For each profile, participants were either asked "How well-off do you feel financially?" or "How well off do you think [Mr. X] is financially?," depending on their condition. Like Study 3, the referenced gender was balanced across profiles. Response were on a five point scale from "Not at all well-off" to "Extremely well-off."

Prior to completing the study, participants were asked about their own financial situation as well as their demographics. Specifically, participants were asked to estimate their own net worth, whether or not they discuss personal finances with family and friends, the value of their car (if they owned one), how much money they believed they could come up with in case of an emergency, and which zip code they resided in. These measures, however, were not used in the analysis reported here.

1.6.2 Results

In replicating the analysis from Study 4, we estimated an ordinary least squares regression. We regressed perceptions of wealth on indicators for price (reference level = Low-Price) and self-other (reference level = Self) conditions, their interaction, and control for the proportion of debt of each profile. Table 1.6 provides the full model specification along with the estimated coefficients.

After adjusting for relative debt, others were generally seen as wealthier ($b_{Other} = .24, p < .001$, 95% CI [.18, .30]); and even more so when they were said to have purchased an expensive car, $b_{Other*HighPrice} = .34, p < .001$, 95% CI [.25, .44]. Yet, unlike in Study 4, we found no difference between the Low-Priced and High-Priced conditions when participants were

asked to think of their own situation, $b_{HighPrice} = -.01, p = .75$, 95% CI [-.08, .05]. One possibility is that this is due to the type of product used in this experiment. For example, Greenberg and Mogilner (2021), find that different types of debt are associated with different levels of life satisfaction and this may offer one explanation for the different findings. Figure 1.5 illustrates the results of this experiment. These effects remain statistically meaningful when we run the same regression predicting the rank of the response.

Table 1.6: Coefficients of OLS regression model for Studies 4 (Houses) and 5 (Cars)

	Dependent variable: Financially Well-Off	
	Houses	Cars
	(1)	(2)
Constant	2.80***	2.49***
	p = 0.00	p = 0.00
	(2.74, 2.86)	(2.43, 2.55)
Other Condition	.22***	.24***
	p = 0.00	p = 0.00
	(.15, .29)	(.18, .30)
High Price Condition	.29***	−.01
	p = 0.00	p = .75
	(.22, .36)	(−.08, .05)
Debt (as percentage of asset)	−.02***	−.02***
	p = 0.00	p = 0.00
	(−.02, −.02)	(−.02, −.02)
Other x High Price	.20***	.34***
	p = .0001	p = 0.00
	(.10, .30)	(.25, .44)
Observations	8,243	8,000
R^2	.23	.27
Adjusted R^2	.23	.27
Residual Std. Error	1.12	1.05
F Statistic	614.28***	725.40***
Note:		*p<0.05; **p<0.01; ***p<0.001

1.6.3 Discussion

Studies 4 and 5 explored whether a differential focus led people to perceive wealth differently for themselves versus others in the present and as a function of conspicuous purchases rather than overall wealth. We estimated people's sensitivity to asset prices as a signal of wealth. By varying the value of the asset as well the percentage of debt, we found that, for others, more expensive assets are a sign of financial wellness. However, when considering one's own financial status, the product price plays a less significant role after adjusting for the relative amount of debt. This effect suggests that the amount owed plays a role in determining one's self-image. Study 6 explores whether people evaluate wealth differently when thinking of their self-image versus their social image.

1.7 Study 6

In Studies 4 and 5, we found that the price paid for the car or the house had little effect on how wealthy one felt, what mattered was the level of debt. In contrast, when thinking about others, both the price of the product and the level of debt were important such that those with more expensive goods were seen as wealthier, even when they held more dollars in debt.

Study 6 extended the paradigm used in Studies 4 and 5 to examine second-order beliefs; that is, beliefs of others' beliefs. People care about how they are perceived by those around them and this can affect spending behavior (Bursztyn and Jensen, 2017; Ordabayeva, Lisjak, and Jones, 2022). Importantly, absent explicit feedback from others', individuals are left to infer others' perceptions when making everyday decisions.

We sought to understand whether participants would correctly infer the gap in perceptions found in the earlier studies, or would instead believe that others' saw them as they saw themselves. To study this, Study 6 replicated Studies 4 and 5 while adding on another set of "social image" conditions which asked participants how they imagine "others would see

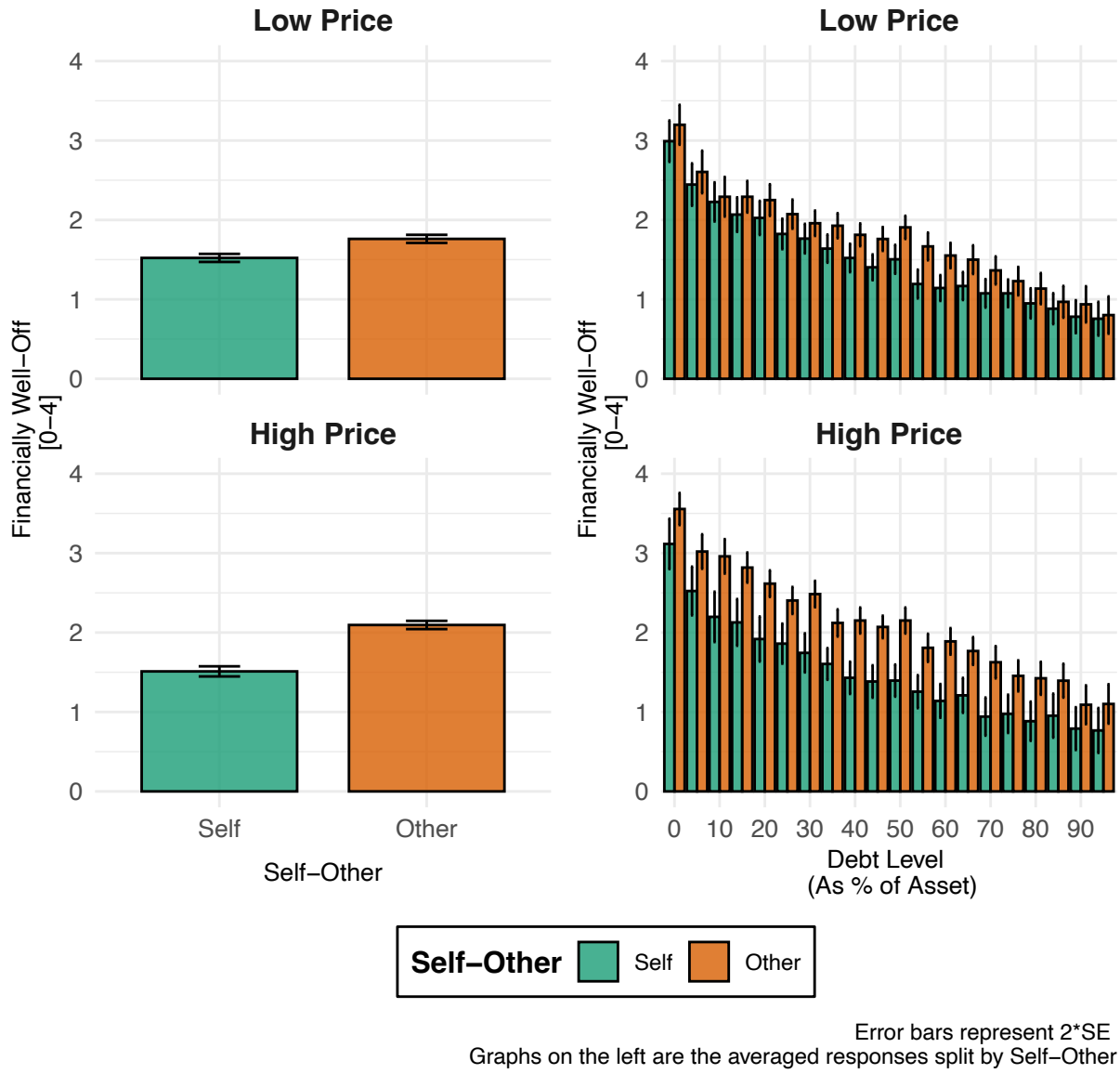


Figure 1.5: Study 5 results: Perceptions of wealth between self and others for Low-Priced and High-Priced cars. The panels on the left collapse responses across debt levels. The panels of the right show responses across all debt levels. The scale consisted of five points, ranging from "Not well-off at all" to "Extremely well-off." Error bars are equivalent to two standard errors from the mean.

them" (holding constant the assets and debt across conditions).

A social image that resembles one's image of others would suggest that people recognize the gap in perceptions. A social image that differed from self-image would suggest that individuals would have to choose between appearing wealthier (improved social image) while holding more debt (worsened self-image) and appearing poorer (worsened social image) with less debt (improved self-image).

We note that when we conducted Studies 4 and 5, we uncovered the reported effects through exploratory analyses. Study 6 pre-registered the new hypothesis with the aim of replicating the earlier findings.

1.7.1 Method

Participants

In March 2022, 1,501 participants (723 women, 749 men, 6 Transgender, 23 Self-Identify; age: M = 38.2, SD = 14.1; 67.8% white, 9.1% Black, 7.3% Latin American, 5.7% East Asian, 10.1% all others) were recruited through Prolific ((www.prolific.co)). We intended to recruit approximately 250 participants per condition, across six conditions. The sample size per condition was selected to be able to detect an earlier finding at 80% power (see Supplemental Material for details on the earlier study) and was pre-registered prior to starting recruitment on AsPredicted.org (#88981). Participants were paid $1.00 for completing the study which took approximately 6.8 minutes to complete.

Materials

The stimuli consisted of 40 wealth profiles. The profiles in this study are a subset of those generated for Studies 4 and 5. Specifically, we draw every other profile starting with 0% owed through 90% owed (e.g., 0%, 10%, 20%... 90%), along with the corresponding price of

the asset. Altogether, there were 20 profiles referencing house purchases and 20 referencing car purchases and within each product set, ten profiles reflected High-Priced items and ten Low-Priced items.

Procedure

Study 6 borrowed the setup from the previous two experiments. This time, participants were assigned to one of six conditions in a 3 (Self; Other; Social Image) by 2 (Low-Price; High-Price) between-participants design. Instructions for the Self and Other conditions were similar to the previous experiments. For those in the "Social Image" condition, participants were asked to "imagine that you have recently purchased a new home" like in the Self condition; however, the task now involved deciding "How well-off do *others think you feel* financially?" (emphasis added). Participants in this study were shown 20 wealth profiles, spanning both products, first the ten profiles related to one product and then ten related to the other. The product order and the profiles within each product set were randomized between participants. Unlike the earlier studies, there were only 10 wealth profiles per product, where debt levels varied between 0% and 90% in increments of ten percentage points. For each profile, participants were asked about perceptions. In this version, unlike Studies 4 and 5, all conditions aligned the question wording to ask about "feeling"—self: "How well-off do you feel financially?"; other: "How well-off do you believe [Mr. G] feels financially?" For those in the Social Image condition they were asked "How well-off do others think you feel financially?" The same five point scale was used. As in Studies 3 and 5, the referenced gender in the "other" condition was balanced across profiles.

Participants were later asked about their own financial situation as well as their demographics. In particular, participants were asked to estimate their own net worth, whether or not they discuss personal finances with family and friends, the value of their car (if they owned one), how much money they believed they could come up with in case of an emer-

gency, and which zip code they resided in. None of these measures were used in the analysis below and are reported here for transparency (Simmons, Nelson, and Simonsohn, 2011).

1.7.2 Results

We once again regressed perceptions of wealth on an indicator for the High-Priced condition and for the Other condition, the interaction term between the two, and adjust for the relative amount of debt. To account for the new Social Image condition, we maintain the Self condition as the reference group and estimate the ordinary least squares regression with an additional indicator and an interaction term for Social Image and High-Priced condition assignment.

Again, others were seen as wealthier (House: $b_{Other} = .14, p < .001$, 95% CI [.08, .20]; Car: $b_{Other} = .13, p < .001$, 95% CI [.07, .18]). Participants also perceived those with more expensive houses to be better off financially ($b_{HighPrice} = 0.12, p < .001$, 95% CI [0.05, 0.18]) and this relationship was more pronounced when thinking about others relative to one's self ($b_{Other*HighPrice} = 0.24, p < .001$, 95% CI [0.15, 0.33]). When considering cars, there appears to be no difference between Low- and High-Priced conditions when thinking of the self ($b_{HighPrice} = -.03, p = .38$, 95% CI [−.08, .03]), yet there is a significant interaction ($b_{Other*HighPrice} = .19, p < .001$, 95% CI [.11, .27]. These results replicate those of Studies 4 and 5.

When asked about social image (i.e., how wealthy others would believe you felt), participants believed others would see them as more well-off than they tended to see themselves when in the Low-Priced condition (Car: $b_{SocialImage} = .04, p = .001$, 95% CI [−.02, .10]; House: $b_{SocialImage} = .10, p < .001$, 95% CI [.04, .16]). Again the effect is more pronounced in the High-Priced condition (Car: $b_{SocialImage*HighPrice} = 0.35, p < .001$, 95% CI [0.27, 0.43]; House: $b_{SocialImage*HighPrice} = 0.49, p < .001$, 95% CI [0.40, 0.58]). Notably, when compared to those in the Other condition, participants imagined others

would see them differently to how they saw others, even at similar levels of wealth (Car: $b_{SocialImage} = -.09, p = .004$, 95% CI [−.14, −.03]; House: $b_{SocialImage} = -.04, p = .21$, 95% CI [−.10, .02], see Supplemental Materials table of coefficients). Figures 1.6 and 1.7 illustrate the results. Regressing the rank of the response on the same predictors produces consistent responses for both cars and houses.

1.7.3 Discussion

Study 6 presents a pre-registered replication of Studies 4 and 5. It also extends the paradigm to explore the social image people have of themselves; that is, beliefs of how others would, in turn, see them. People believed others would see them as wealthier than they saw themselves, and this gap widened for more expensive items. This gap between one's self-image and their social image could bias people to consume more expensive items to appear wealthier to others. We also see a gap when comparing the Social Image condition to the Other condition. This gap between social image and the image of others is important because, on average, we might expect these to be the same. That is, it is reasonable to believe that people's judgment of others should be equivalent to others' judgments of those people, especially when the information available is identical, as it was in these studies. While this equivalence appears true in the Low-Priced condition for houses, there is a significant difference between the image of others and one's social image for cars and for both products in the High-Priced condition (see Table 18 in the Supplemental Materials). This difference suggests that one's perceptions of their social image are potentially biased, deviating from what we might expect. Understanding these beliefs is particularly important in the context of conspicuous purchases.

Table 1.7: Coefficients of OLS regression model for Study 6

	Dependent variable: Financially Well-Off	
	Houses	Cars
	(1)	(2)
Constant	2.85^{***}	2.79^{***}
	p = 0.000	p = 0.000
	(2.80, 2.90)	(2.74, 2.83)
Other Condition	$.14^{***}$	$.13^{***}$
	p = .0000	p = .0001
	(.08, .20)	(.07, .18)
Social Image Condition	$.10^{***}$	.04
	p = .001	p = .17
	(.04, .16)	(−.02, .10)
High Price Condition	$.12^{***}$	−.03
	p = .0003	p = .38
	(.05, .18)	(−.08, .03)
Debt (as percentage of asset)	$-.02^{***}$	$-.02^{***}$
	p = 0.000	p = 0.000
	(−.02, −.02)	(−.03, −.02)
Other x High Price	$.24^{***}$	$.19^{***}$
	p = .0001	p = .000
	(.15, .33)	(.11, .27)
Social Image x High Price	$.49^{***}$	$.35^{***}$
	p = 0.000	p = 0.000
	(.40, .58)	(.27, .43)
Observations	15,010	15,010
R^2	.27	.33
Adjusted R^2	.27	.33
Residual Std. Error	1.10	1.02
F Statistic	927.49^{***}	$1{,}239.11^{***}$
Note:		*p<0.05; **p<0.01; ***p<0.001

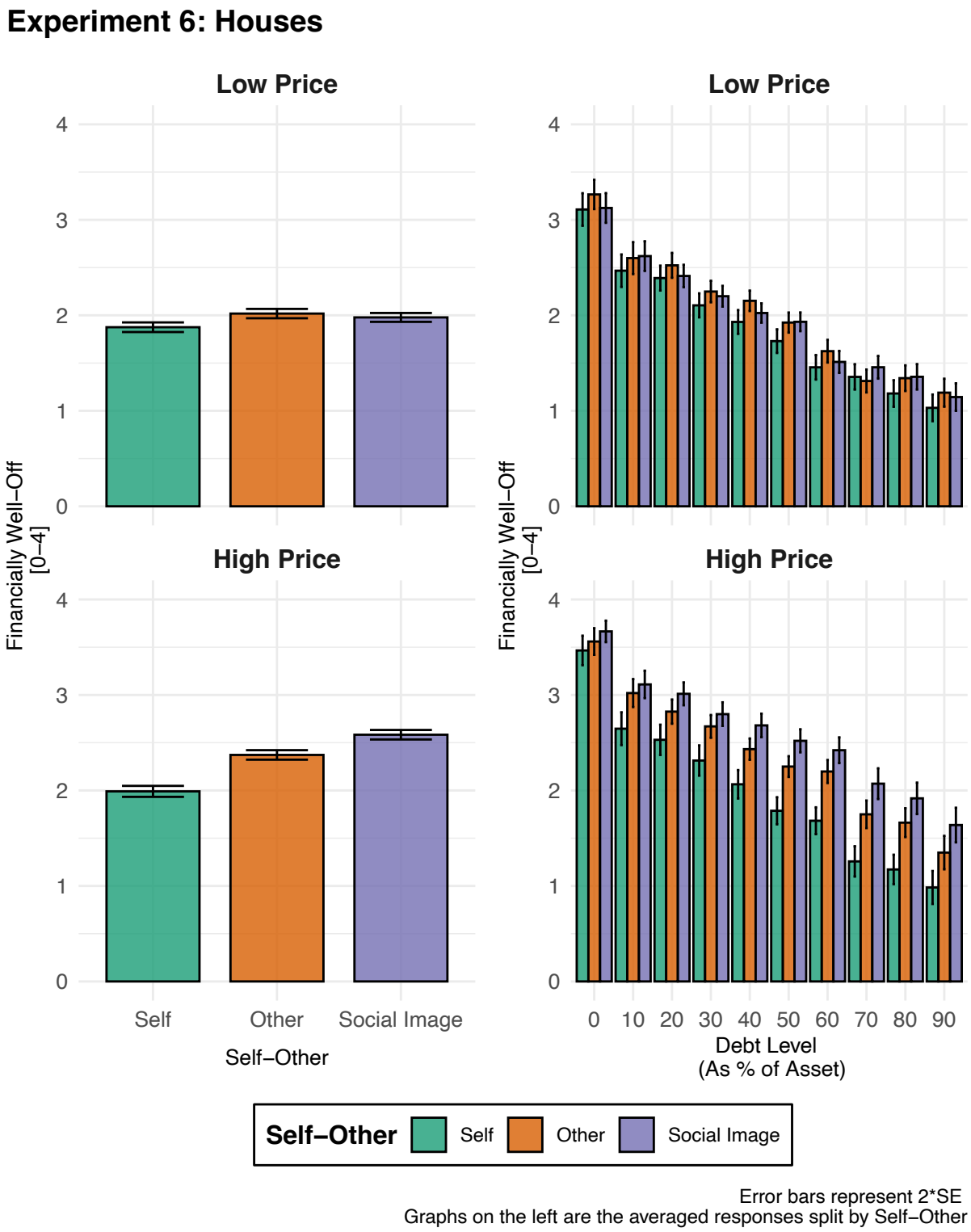


Figure 1.6: Study 6 results for homes: Perceptions of wealth between self, others, and social image for Low-Priced and High-Priced homes. The panels on the left collapse responses across debt levels. The panels on the right show responses across all debt levels. The scale consisted of five points, ranging from "Not well-off at all" to "Extremely well-off." Error bars are equivalent to two standard errors from the mean.

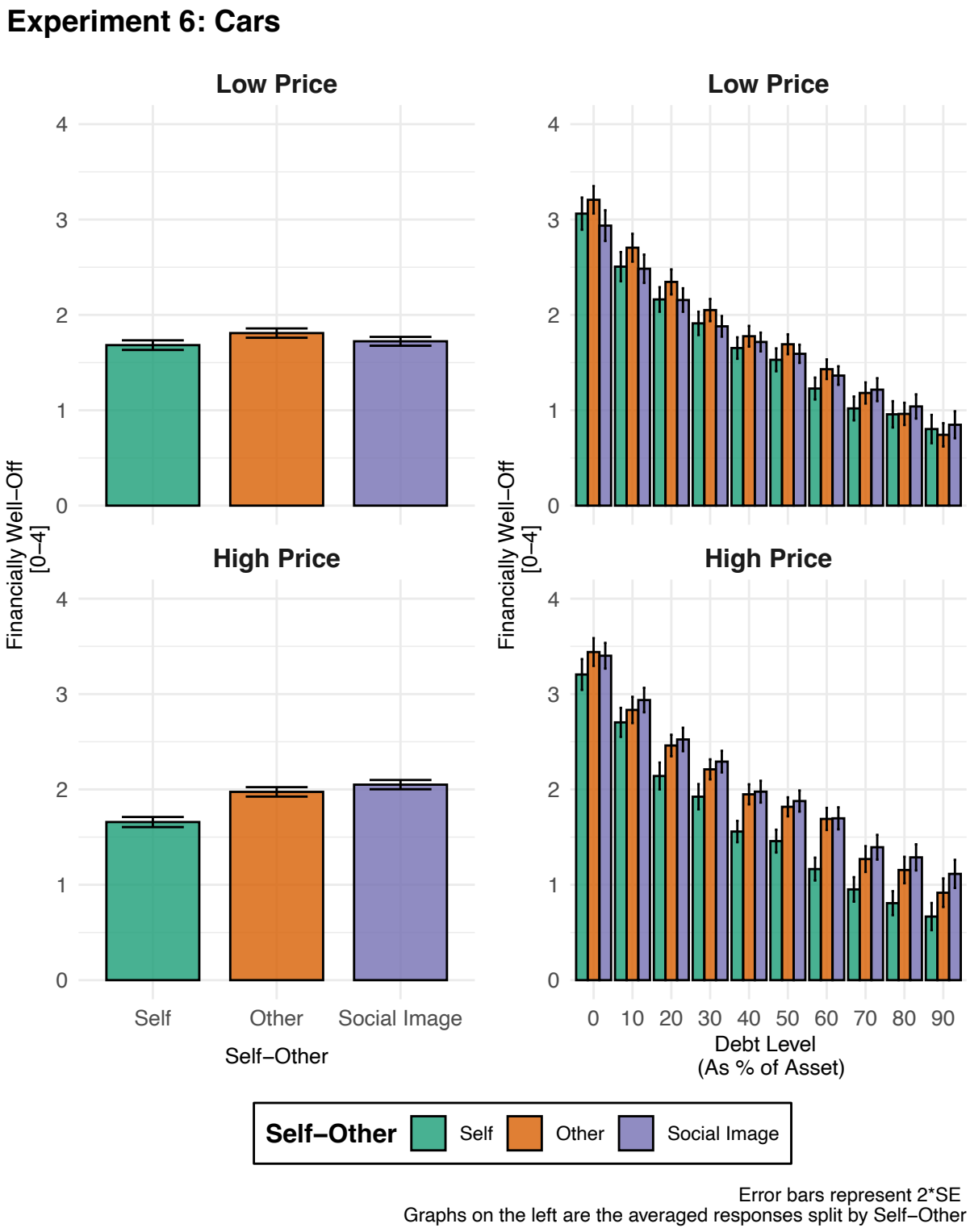


Figure 1.7: Study 6 results for cars: Perceptions of wealth between self, others, and social image for Low Priced and High Priced cars. The panels on the left collapse responses across debt levels. The panels on the right show responses separately for all debt levels. The scale consisted of five points, ranging from "Not well-off at all" to "Extremely well-off." Error bars are equivalent to two standard errors from the mean.

1.8 General Discussion

People think about wealth differently for themselves versus others, even when provided with complete information. Specifically, debt appears to weigh more heavily when evaluating one's own financial situation than when assessing someone else's. Consequently, people appear motivated to pay off what they owe while also believing that they are worse than their peers today.

These findings extend the literature on self-other differences. Previous work has documented patterns of differential attention leading to disparities in how people perceive others relative to the self (e.g., Kahneman, 2006; Williams and Gilovich, 2012). These disparities often lead people to evaluate themselves more favorably than others. Yet, some work has shown that people focus more on their own trials and tribulations (e.g., Davidai and Gilovich, 2016). Taken together, research into self-other differences has offered competing predictions, sometimes beliefs about the self are self-enhancing and other times they are self-deprecating. Why then, when it comes to evaluating wealth, do people perceive themselves as worse off than others?

One account, offered by Galesic, Olsson, and Rieskamp (2012), suggests that differences arise from the way people sample the world (see also Caruso, 2008; Dawtry, Sutton, and Sibley, 2015; Galesic, Olsson, and Rieskamp, 2018; Howard et al., 2022). In cases where the population distribution skews right—as is the case for wealth, where most people are doing worse than the mean—people overestimate the relative frequency of wealthier people and underestimate the relative frequency of poorer people, leading them to believe their own position is worse than it actually is (Galesic, Olsson, and Rieskamp, 2012, p. 1519).

More generally, people tend to have more information about themselves than they do about others (e.g., Kruger et al., 2008; Ross and Sicoly, 1979). This is particularly likely to be true when it comes to household finances, where people have an unbalanced view of assets and debts for themselves versus others. People are constantly being reminded of

their own financial obligations and their earnings, while rarely glimpsing that of others. For example, despite a shared understanding of what might be considered "good debt" and "bad debt" (Greenberg and Mogilner, 2021; Peñaloza and Barnhart, 2011), debt is rarely discussed with others, even with one's partner (Addo and Zhang, 2020). Others' debt also tends to go unseen (Georgarakos, Haliassos, and Pasini, 2014) with few exceptions, including bankruptcies or property repossessions, such as home foreclosures and evictions. In contrast, people tend to incur debt far more frequently than they acquire assets in everyday spending (see Section 2 in Stewart, Chater, and Brown, 2006); a fact they are reminded of each month upon receiving a statement from their bank.

Still, our work suggests that social sampling and informational disparities do not fully account for how people evaluate their own wealth relative to others. Across all our studies, we control for informational disparities by design. Participants had complete information about the assets and debts of the target individual, whether that be themselves or someone else. Indeed, this suggests the findings presented in the paper are likely conservative relative to what we might expect in practice. While this highlights a limitation of the studies—namely that the scenarios are different from what one might experience in the world—by holding information constant, we can explore other factors that may be at play.

Two other mechanisms are worth considering: attention and motivation. In Study 2, we see evidence that paying off debt comes to mind more often when thinking of one's future wealth compared to others' future wealth, suggestive of differential attention. In Study 3, we find evidence that people would allocate more money to paying off their own debt than they believe others would, suggestive of differential motivation. Together this suggests that people attend to debt more when thinking of themselves and are more motivated to repay their debts than they believe others are. Although past work has documented a general aversion to debt (e.g., Gal and McShane, 2012; Kettle et al., 2016; Martinez-Marquina and Shi, 2021; Meissner, 2016), it remains unclear whether debt comes to mind because people

are motivated to get rid of it or if the motivation stems from attention. Consistent with the literature on goal-directed and stimulus-driven attention, it is likely that they both play a role (e.g., Corbetta and Shulman, 2002).

1.8.1 Future directions

In exploring how wealth is perceived when thinking of one's own situation versus another's, this paper attempts to connect work on perceptions of wealth and social interactions. One area of general interest is in the role of conspicuous consumption and the drive to "keep up with the Jones's." Our findings hint at a potential mechanism fueling this drive.

Previous work has documented that relative wealth affects people's well-being (Anderson et al., 2012; Boyce, Brown, and Moore, 2010; Perez-Truglia, 2020) and that people consume in ways to signal that wealth (Charles, Hurst, and Roussanov, 2009). Nevertheless, the asymmetry shown here between the self and others points to a pernicious cycle: if people see others as wealthier, they may decide to spend to "keep up," and signal greater wealth, even if that spending comes with more outstanding debt (Georgarakos, Haliassos, and Pasini, 2014). Yet, if others also experience this self-other discrepancy, then each person will perceive everyone *else* to be getting wealthier while failing to recognize they are all trending in the same direction: each increasingly burdened by debt. This idea is largely consistent with the findings presented by Ordabayeva and Chandon (2011), which suggests that when social status matters, greater equality can fuel *more* conspicuous consumption. We invite researchers to test this hypothesis in future studies.

The findings from Studies 4, 5, and 6 appear robust for cars and houses, both durable products that are relatively costly and conspicuous. It is plausible that the effects reported here are limited to a specific set of consumer products. Future research might explore the boundaries of these results, for example, by investigating whether similar patterns emerge in response to less conspicuous, less expensive, or less durable products.

How might these findings be applied in practice? We conjecture that a better understanding of these perceptions of wealth could help reduce excessive conspicuous consumption. The link between perceptions of wealth and conspicuous consumption is consistent with earlier work examining social effects on one's own consumption. For example, Kuhn et al. (2011, p. 2229) use household data from the Dutch Postcode Lottery (PCL) to show that "having an immediate neighbor win [a car through] the PCL raises the probability that a household will buy a car in the next six months by close to 7 percentage points..." Likewise, Georgarakos and colleagues (2014) provide evidence that "perceived peer income contributes to debt and the likelihood of financial distress among those who consider themselves poorer than their peers." That is, social image concerns lead to increased borrowing. In our attempt to gauge participants' beliefs about beliefs, we find that people have a distorted social image. Participants' social image looked neither like their self-image nor their image of others. People believe others will see them as wealthier than they see themselves, and this effect widens for expensive (relative to inexpensive) products. The core mechanism that leads to this gap is beyond the scope of this paper but the consequences are worth considering. To the extent that people care about their relative standing, beliefs about one's social image might bias individuals towards spending more to keep up appearances, even if that requires additional borrowing. Aligning these beliefs might taper one's motivation to consume beyond their means. Future work should examine why these disparities arise. And, when one's belief about their social image differs from others' actual perceptions, what effect does it have on their behavior?

This paper focuses specifically on perceptions of wealth. Still, the basic psychology here may extend to other domains beyond personal finance. For example, graduate students may find themselves consistently behind their peers—others' success tends to be visible, whereas obstacles are not. As discussed above, others have discussed related psychology in the past. What perhaps is new is the cognitive weight of everyday challenges. It is plausible that

people tend to underestimate how motivated others—who are obese or unemployed or in need of childcare—actually are to lose weight, find a job, or hire a nanny (see Vallone et al., 1990). On the other hand, perceptions of wealth may be unique in that consumption tends to be a conspicuous signal of wealth and, by extension, social status; yet, debt often goes unseen. Broader research exploring the motivation of self versus others could help extend the current insights into other domains.

1.8.2 Conclusion

When assessing wealth, people weigh their own debt more than they do the debt of others. This difference influences both how people plan for the future and how they perceive consumption today. When thinking about the future, people expect they will pay down their debt faster, in part because they believe they are more motivated to reduce debt than their peers. However, by focusing on debt for themselves, people judge themselves feel less well-off than they believe others are after large purchases. This asymmetry points to an insidious cycle, whereby simply "keeping up" with the Jones's could leave people feeling perpetually behind. To the extent people are motivated by social status, a distorted view of wealth will go on to shape people's decisions to borrow and spend.

ESSAY 2

DISCLOSING THE COSTS OF CO-HOLDING LIQUID ASSETS AND HIGH-INTEREST DEBT HAS LIMITED IMPACT ON BEHAVIOR

This essay is adapted from Batista, Rafael M., Ella Mao, Abigail B. Sussman, Neale Mahoney, and Jessica Min (2025), "Disclosing the Costs of Co-Holding Liquid Assets and High-Interest Debt Has Limited Impact on Behavior." Working Paper.

2.1 Introduction

Household debt around the world is at unprecedented levels, with households in many countries now carrying more debt than that country's GDP (Mbaye, Moreno Badia, and Chae, 2018). This trend might suggest that consumers are stretched thin financially. Yet among households carrying high-interest credit card debt, a considerable fraction simultaneously maintain low-yield liquid assets (Greene and Stavins, 2023; Medina and Pagel, 2021). This "co-holding" of assets and revolving debt is puzzling because although consumers earn roughly 1% on their liquid assets, they pay 15-30% interest on their debt.

The costs are substantial. The typical co-holder in our sample maintains about $2,000 in both assets *and* debt, earning $15 annually while paying $300 in interest—a net cost of $285 that could be eliminated if they used their cash to pay down debt. In our field experiment sample alone, this amounts to over $35 million left on the table annually. As we observe, individuals are borrowing money from the same bank where they keep their savings—effectively paying to borrow their own money.

Why do individuals co-hold? Several explanations have emerged, primarily in economics and finance, to rationalize this behavior. For instance, consumers may maintain liquid assets as precautionary savings in case credit becomes unavailable (Fulford, 2015; Gorbachev and Luengo-Prado, 2019), or to ensure access to cash for transactions where cards cannot be used

(Telyukova, 2013). These accounts assume consumers make deliberate, informed decisions despite the financial costs.

More recent work examining the psychology underpinning these decisions suggests co-holding may result from systematic mental accounting processes (Gathergood and Olafsson, 2024; Medina and Pagel, 2021, see also Essay 3). This research proposes that consumers treat different payment methods as distinct accounts and use each for different types of expenses. Co-holding may thus emerge as a byproduct of spending decisions, patterns that would persist even when consumers are aware of the financial costs.

A simpler explanation—though one that remains empirically untested—is that co-holding results from consumers' limited attention (Gabaix, 2019; Weber and Johnson, 2009) and the complexity of interest rate calculations (Brown et al., 2021; Kalaycı and Serra-Garcia, 2016). Consumers may simply be unaware they are co-holding, or they may fail to appreciate the substantial costs involved. Co-holding may arise, for example, if consumers overlook their asset balances when making decisions about debt, and vice versa (Lian, 2021; Read, Loewenstein, and Kalyanaraman, 1999; Thaler, 1999); or if they neglect the interest rates (Agarwal and Karapetyan, 2022; Bertrand and Morse, 2011). Even attentive consumers may fail to appreciate the costs of co-holding due to the complexity of interest rate calculations (Soll, Keeney, and Larrick, 2013; Stango and Zinman, 2009a). If consumers are co-holding due to such oversight rather than deliberate strategy, we would expect their behavior to change after receiving feedback about the behavior and the associated costs (Adams et al., 2022; Bertrand and Morse, 2011; Caflisch et al., 2018; Karlan et al., 2016; Medina and Pagel, 2021).

Using a unique dataset from a major international bank and an online panel, we provide novel empirical insights into co-holding behavior through three complementary analyses. First, we examined 38 months of detailed transaction data from over 2,300 customers, revealing that co-holding is both prevalent and persistent. The typical co-holder maintains

this behavior for most months in our observation period, often in extended periods lasting six consecutive months. Moreover, co-holders show substantial month-to-month variation in asset and debt balances, indicating that these accounts are actively used rather than dormant. Second, we tested whether limited attention drives co-holding by conducting the first large-scale randomized field experiment (n = 125,328) specifically designed to target co-holding behavior. The experiment provided customers who were co-holding clear information about their behavior and its costs through the bank's mobile app where customers could readily act on the information (Fernandes, Lynch, and Netemeyer, 2014). Despite sufficient power to detect economically small effects, we find no meaningful changes in debt repayment amount, though customers did respond in other ways—making more frequent repayments and paying above required minimums. Third, we surveyed co-holders directly through an online panel, further documenting their awareness of their behavior and their stated motivations for maintaining it.

These findings have two key implications with distinct theoretical and policy significance. First, co-holding cannot be explained primarily by limited attention. Combined with banking records showing that co-holding is persistent and systematic, and survey evidence that three out of four co-holders report being aware of their behavior, these results suggest that co-holding reflects a deliberate decision rather than naive behavior (Gathergood and Olafsson, 2024; Gathergood and Weber, 2014; Gorbachev and Luengo-Prado, 2019). Second, and perhaps more consequentially for policy, information disclosure interventions—even when carefully designed and delivered through trusted channels at actionable moments—are insufficient to change this costly financial behavior. For researchers, this points toward the need for better understanding consumers' underlying preferences and decision-making processes. For policymakers, it raises important questions about when information-based interventions are likely to be effective and what alternative approaches might be needed to influence this behavior.

2.2 Study 1

2.2.1 Methods

We partnered with a major Australian retail bank to analyze de-identified banking records. The banking data analysis used two distinct samples: a representative customer sample to establish the prevalence of co-holding and a smaller sample who met a pre-specified set of eligibility criteria and for whom we had detailed transaction-level data.

To establish the prevalence of co-holding, we examined a representative sample of "Main Financial Institution" (MFI, Appendix A) customers (N = 100,000) in January 2020. Approximately 32,400 were credit card users, including around 7,400 who were co-holding. Summary statistics for this representative sample were calculated by the bank's analysts and provided to the research team. Using these statistics we could also assess how our selective sample compares to the overall banking population: the survey sample represents more financially engaged customers, with higher credit limits, account balances, and greater usage of multiple financial products, consistent with the selection criteria (Table 2.1).

For the descriptive analysis, we obtained direct access to individual-level banking records from 2,307 customers (979 co-holders, 1,328 non-co-holders). Eligible customers met a set of key criteria including being the primary holder of a retail or personal credit card, being digitally active on the bank's smartphone application, having no automatic full-balance repayment arrangements, and not being in financial hardship or arrears (see Appendix A for complete eligibility criteria and selection process). The administrative data for this sample included transaction-level records across all account types (checking, savings, and credit cards), daily balances, monthly statement summaries from January 2019 through February 2022 and a demographic snapshot from January 2020. These records enabled us to track co-holding persistence over time, quantify associated financial costs, and examine transactions to distinguish active accounts from dormant ones.

Table 2.1: Sample Characteristics (January 2020)

	Representative		Survey	
	CH	NCH	CH	NCH
N =	*7,438*	*92,562*	*979*	*1,328*
Male (Prop)	.53	.50	.51	.59
Age (Mean)	45	44	48	50
Income (Med)	$59k	$45k	$59k	$59k
Credit (Prop)	.998	.27	.98	.96
Credit Baln (Med)	$3.9k	$933	$4.4k	$737
Credit Rate (p.a.)	12.2%	14.8%	12.3%	15.8%
Credit Util (Med)	79%	12%	55%	12%
Sav Baln (Med)	$101	$246	$198	$752
Sav Rate (p.a.)	.75%	.75%	.75%	.75%
Trans Baln (Med)	$642	$481	$648	$996
PL (Prop)	.16	.03	.22	.07
HL (Prop)	.29	.14	.38	.39

Demographic and financial characteristics of co-holders and non co-holders. Representative sample shows randomly selected customers from the bank's customer base in January 2020. Survey sample includes customers who participated in a survey we conducted in October 2021, for whom we have detailed data (*SI Appendix*). Co-holders simultaneously maintain *revolving* credit card debt and liquid assets above $500 thresholds. CH=Co-Holder, NCH=Non Co-Holder, Balances in AUD; k=thousands. Rep=Representative sample, Survey=Survey sample, Util=Utilization Rate (Balance / Credit Limit), Sav=Savings, Trans=Transaction, PL=Personal Loan, HL=Home Loan. Reported balances only for customers with that product.

We define a *co-holder* as an individual who simultaneously maintains both revolving credit card debt and liquid assets (checking and savings account balances) across three consecutive billing periods. *Co-holding* refers to the act of maintaining both types of balances above specified thresholds. Specifically, customers qualified as co-holding when they maintained minimum revolving credit card balances of AUD$500 and minimum combined liquid asset balances of AUD$500 (Appendix A).

To track co-holding persistence, we created monthly indicators for periods where the co-holding amount was at least $500. Co-holding amount was calculated as the minimum of revolving debt and liquid asset balances. We measured account activity in two ways: measuring month-to-month percent changes in balances and calculating coefficients of variation (CV), which compare the standard deviation of balances over the previous twelve months (including the current month) to the mean over the same period (Farrell, Greig, and Yu, 2019). These measures enabled us to distinguish between accounts with high transaction volumes versus those that remained relatively static. Economic costs were estimated using back-of-the-envelope calculations that compared the interest costs of revolving credit card debt to the potential returns from liquid asset balances, based on account-specific interest rates.

2.2.2 Results

Prevalence and Persistence

Co-holding affects approximately 23% of credit card users at the bank we partnered with.

Most co-holders who participated in our field survey were co-holding most of the time. In fact, co-holding extended well beyond the 3-month window we set to select customers into the sample. Over the 38-month observation period, the median co-holder was co-holding for 23 months (IQR: 13.2-32.0). A closer look at the trends suggests that co-holding for these customers came in fits and starts: the median co-holder experienced three distinct co-holding

periods (IQR: 2.0-5.0), with each period lasting an average of six consecutive months (IQR: 3.1-14.0). In contrast, the median non-co-holder was never observed co-holding (IQR: 0-1.8) during the same period.

Economic Costs

Co-holding is not only prevalent and persistent, it is also costly. In the months where we observe co-holding (i.e., co-holding amount > AUD$0), the median amount concurrently held in assets and debt was AUD$1,976 (25th-75th percentile: AUD$759-$4,432; M = AUD$3,848; SD = AUD$5,830). A back-of-the-envelope calculation suggests that co-holders could have saved upwards of AUD$285 in interest per year by transferring cash to pay off the revolving debt. Furthermore, more than a third of the co-holders had more assets than debt (i.e., "Net Savers"), meaning these customers could eliminate their credit card debt entirely with the money in their asset accounts and still have liquidity left. Extrapolating these costs to all the co-holders at this institution suggests an arbitrage opportunity worth well over AUD$35 million per year.

Account Usage and Additional Characteristics

Our analysis reveals that co-holders actively use both their asset and debt accounts. Co-holders experience substantial variation in both their asset and debt balances, indicating these accounts are actively used. Co-holders show average month-to-month changes of 23.4% in asset balances and 16.9% in credit balances, with average CVs of .43 and .18 respectively. For comparison, non-co-holders show an opposite pattern—more variation in credit card balances (Mean Percent Change: 25.4%; Mean CV: .80) than in asset balances (Mean Percent Change: .3%; Mean CV: .27). This volatility indicates that co-holders regularly deposit and withdraw from savings while continuing to use their credit cards for new purchases.

Co-holders also differ from non-co-holders in other ways. Co-holders maintain higher

credit card balances and lower savings balances than non-co-holders, but also show higher credit utilization rates despite having higher credit limits. They are more likely to have personal loans and home loans. Co-holders possess credit cards with lower interest rates on average and are more likely to have set self-imposed credit limits below the bank's contractual maximum, utilizing a feature marketed as a self-control tool.

Although these patterns reveal that co-holders actively engage with their finances, they leave open the question of whether customers understand the financial implications of their behavior. We therefore conducted a field experiment to test two primary hypotheses: that co-holders are unaware of their behavior, and that they do not fully appreciate its costs.

2.3 Study 2

2.3.1 Methods

We conducted a large-scale randomized controlled trial with 125,328 co-holders, whom we observed to be co-holding $500 or more for three consecutive billing periods as of January 2020. The experiment was designed to test two specific hypotheses: (1) co-holders are unaware that they are co-holding, and (2) co-holders are unaware of the costs of co-holding. The intervention provided salient information about co-holding behavior and its costs at a moment when customers could easily act on this information.

Experimental Design

The study employed a pre-registered, three-arm randomized design. The Control group (n=41,776) received no notification about their co-holding behavior. The Notice group (n=41,776) received an in-app notification reading: "You usually have at least $500 available to pay your credit card debt. Every $100 you pay off could reduce your interest charges." The Notice + Cost group (n=41,776) received the same initial message with an additional

sentence: "... Every $100 you pay off could reduce your interest charges by about $20 per year. Saving $100 only earns about $1 in interest per year..." (Appendix A).

To ensure balanced treatment groups, we used an anticlustering algorithm (Papenberg and Klau, 2021) that balanced condition assignment on key customer characteristics including gender, age band, customer tenure, credit card type, and loan holdings with the bank.

Implementation

The experiment was implemented from February 17 to March 18, 2020, through the bank's mobile application. When customers in treatment groups opened their banking app, they were shown a notification "card" that could be expanded to view the full message. After viewing the message, customers could dismiss the notification, close it, or click "Pay Now" to navigate directly to a money transfer interface. For customers who did not engage with the initial presentation (or dismissed it), the notification reappeared once during a subsequent app login within the experimental period (more details in Appendix A).

Statistical Analysis

We employed an intent-to-treat framework, comparing outcomes across all randomized customers regardless of their engagement with the notifications. We estimated treatment effects using ordinary least squares regression with robust standard errors. Given the minimal difference between the two notification conditions, our reported results collapsed both treatment groups for greater statistical power. Our primary analysis estimated the following model:

$$Y_i = \alpha + \beta T_i + \gamma X_i + \epsilon_i \tag{2.1}$$

where Y_i represents the primary outcome of credit card repayment for customer i, T_i is a binary indicator equal to 1 if the customer was assigned to either notification condition and 0

if assigned to the control condition, and X_i is a vector of customers' baseline characteristics including demographics and financial behaviors. The coefficient β captures the average treatment effect of receiving any notification compared to the control condition.

Pre-registered power calculations were conducted using control group data ($\alpha = 0.05$, power of $1-\beta = .80$) following a two-stage pre-registration process to maintain experimental integrity while ensuring adequate statistical power (see Appendix A). The study was powered to detect a minimum effect of AUD$39 difference in repayment (approximately 3% of the control group mean). We tracked customer engagement with notifications through digital behavioral data, including whether notifications were displayed, expanded, and acted upon through the mobile app interface. Full power analysis procedures and minimum detectable effects for all outcome measures are provided in Appendix A.

Since engagement measures were unavailable for the control group (who received no notifications), we conducted a supplementary matched-pairs analysis. This approach paired treated customers who engaged with the notifications to control customers with similar pre-experiment financial characteristics, enabling a more precise comparison of treatment effects among engaged participants. Detailed matching procedures are provided in Appendix A.

2.3.2 Results

Primary Outcome

We find no evidence that providing information about co-holding behavior led to meaningful changes in debt repayment, our pre-registered primary outcome of interest. Customers who received notifications paid AUD$19.63 (95% CI [−AUD$7.86, AUD$47.12]), or approximately 1.4%, more on average than those in the control group (Treatment Mean = AUD$1,383; Control Mean = AUD$1,364, $p = .162$). This difference was neither statistically significant nor economically meaningful since the average outstanding balance was approximately AUD$6,500. The estimate remains essentially unchanged when we adjust for de-

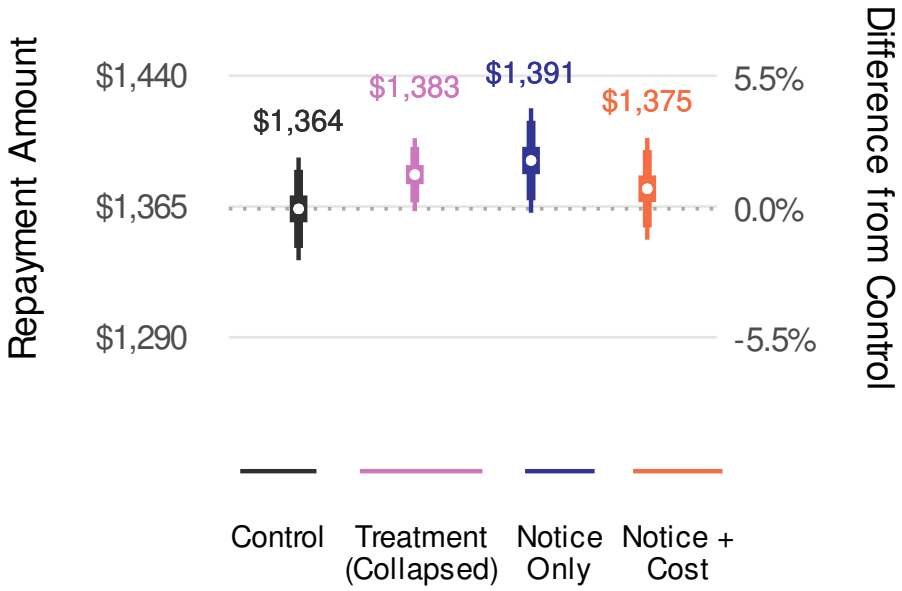


Figure 2.1: Results from Field Experiment. Participants were randomly to assigned to one of three conditions informing them of their co-holding behavior: 1) control (n = 41,424), 2) notice (n = 41,401), 3) notice + cost (n = 41,426). The figure displays repayment amounts for each group, including the two treated conditions collapsed. Notifying co-holders of their behavior had neither a statistically nor economically meaningful effect on repayment. Errors bars represent the 50% (thick line) 95% (medium line) and 99% CIs (thin line). White dot represents mean (also displayed numerically above). To facilitate comparison, the y-axis is zoomed to $1,290 to $1,440 (or approx. -5.5% to $+5.5\%$ relative to Control).

mographics and pre-treatment controls (β = AUD$16.78, 95% CI [-AUD$6.68, AUD$40.24]; Appendix A), suggesting the null effect is robust to observable customer characteristics including age, gender, financial wellbeing scores, and pre-treatment saving and borrowing behavior. Notably, the treatment effect remained consistently null when examined by subgroups, including across gender, age, varying net balance, and the frequency of app logins (Appendix A). A matched-pairs analysis using customers' pre-experiment financial behaviors and demographic characteristics confirms our primary findings: treated customers showed no greater debt repayment than their matched controls (β = AUD$41.78, SE = AUD$72.48, $p = .56$; Appendix A).

Evidence of Engagement

On other measures, however, we do observe statistically significant, though small, effects. Those in the treatment conditions made more frequent payments ($M_{Trt} = 3.21$) compared to the control group ($M_{Ctl} = 3.16$; $Z = 4.48$, $p < .001$) and were significantly more likely to pay amounts above the contracted minimum ($Prop_{Trt}$= 83.8%; $Prop_{Ctl}$ = 83.0%; adjusted $p <$ .001). These findings help contextualize the observed effect on repayment by demonstrating that participants noticed and responded to the intervention, thus ruling out a common concern when interpreting null results—that the message simply went unnoticed.

Secondary Outcomes

Beyond the engagement measures reported above, we found limited evidence of behavioral change. While treated customers were more likely to repay more than their previous three-month average ($Prop_{Trt}$ = .393 vs. $Prop_{Ctl}$ = .382; adjusted $p < .001$), we observed no meaningful differences in credit card balances, interest charges, savings account balances, or net financial benefit (all adjusted ps $> .05$; Appendix A).

The consistency of null effects across outcome measures, subgroups, and analytical approaches, combined with clear evidence of engagement, suggests that limited attention or awareness is unlikely to be the primary driver of co-holding behavior.

2.4 Study 3

2.4.1 Methods

To complement our field experiment and examine co-holders' awareness of their behavior, we conducted an online descriptive survey through the Prolific platform. This survey enabled us to gather detailed information about co-holding behavior beyond the constraints of the

banking partnership, including participants' awareness of their financial situation and self-reported reasons for maintaining liquid assets while carrying debt.

Participants and Screening

The survey employed a two-stage design. In the initial screening stage, participants reported the number of checking, savings, and credit card accounts they held. To obtain accurate balance information, participants were instructed to log in to their primary bank account (through a separate browser tab) and report their actual balances across all accounts. Additionally, participants reported their physical cash holdings and, for those with credit cards, their interest rate and repayment behavior. Based on these responses, participants were classified as either "Co-Holders" (CHs) or "Non Co-Holders" (NCHs). Co-Holders were defined as participants who: (i) had a credit card interest rate above 5%, (ii) did not pay their most recent credit card bill in full, (iii) carried credit card balances of $500 or more, and (iv) held combined checking, savings, and cash balances of $500 or more. Non Co-Holders included participants who owned at least one credit card and either a checking or savings account but did not meet one or more of the other, co-holding criteria. Participants who failed the initial attention check, those without credit cards, those without either a savings or checking account, and those co-holding amounts between $0 and $499 were excluded from further participation.

Main Survey

Participants qualifying as either CHs or NCHs were invited to complete a more detailed survey for an additional US$2.60 compensation. The vast majority chose to proceed (96% of CHs and 93% of NCHs, $p = .15$).

The main survey assessed various aspects of financial decision-making, including self-control (Gathergood and Weber, 2014), risk and time preferences (Falk et al., 2023), debt

aversion (Albrecht and Meissner, 2022; Callender and Jackson, 2005), liquidity constraints (Berman et al., 2016), psychological ownership of (borrowed) money (Sharma, Tully, and Cryder, 2021), payment method preferences (Essay 3), feelings of (ir)responsibility for keeping too little savings or carrying too much debt (Sussman and O'Brien, 2016, also, Essay 3), and one's need for cash. Additional details available in Appendix A.

Crucially, the survey included direct questions about participants' awareness of their co-holding behavior and their reasons for maintaining this financial arrangement.

2.4.2 Results

In the end, the sample included 107 co-holders and 283 non co-holders. When asked directly whether they were aware that they could save money by using their assets to pay down their debt, 76% of co-holders responded "Yes," 17% said "It's Complicated," and only 7% reported being unaware of this opportunity. Critically, when the seven co-holders who were previously unaware were informed about the potential savings and asked whether they would do anything differently, all provided reasons for why they would continue to co-hold. When co-holders were asked directly *why* they choose to maintain both assets and debt, the most common themes included emergency preparedness (e.g., "I try to balance paying down debt while still providing a layer of protection in case of emergency"), peace of mind from having liquid assets available, and deliberate money management strategies that involved separating different income streams or "bucketing" funds for specific purposes. These survey findings reinforce that co-holding behavior is not primarily driven by limited attention or lack of awareness, but rather reflects deliberate decision-making processes (see Appendix A for additional survey findings and detailed qualitative responses).

2.5 General Discussion

Our analysis reveals that co-holding is a prevalent, persistent, and costly behavior exhibited by approximately 23% of credit card users at a major international bank. The median co-holder maintained this pattern for 23 of 38 months observed, holding a median of AUD$1,976 simultaneously in assets and debt, with estimated annual interest costs exceeding AUD$285. Despite these substantial costs, our large-scale field experiment shows that providing clear, actionable information about co-holding and its costs does not meaningfully change debt repayment behavior, our pre-registered primary outcome. We do, however, observe increased payment frequency and higher likelihood of paying above minimum amounts, suggesting that customers engaged with the intervention but ultimately did not alter their patterns of co-holding.

Our findings contribute to a growing literature on when and why information interventions succeed or fail (Adams et al., 2022; Agarwal and Karapetyan, 2022; Bertrand and Morse, 2011). While information disclosure can be effective when behaviors result from genuine oversight or complexity (Alan et al., 2018; Caflisch et al., 2018), our results suggest it may be insufficient to curb co-holding. Even when we explicitly showed customers their behavior and its costs, debt repayment barely changed.

The null result—combined with clear evidence that customers noticed and responded to our messages—provides strong evidence against explanations based on inattention. The robustness of this null effect across multiple outcomes, demographic groups, and analytical approaches, with sufficient statistical power to detect small effects, demonstrates that information alone cannot address this costly behavior. Our banking data and survey responses hint at why: customers appear to co-hold for psychological reasons that outweigh the financial costs, maintaining liquid assets alongside debt even when they are aware of the trade-offs.

Institutions seeking to intervene should, therefore, go beyond simply providing individu-

als with information. For instance, interventions targeting underlying beliefs about spending and payment decisions can influence debt repayment behavior (see Essay 3). Banks could also develop products that allow people access to their liquid assets while offsetting the cost of borrowing. These products both reduce the cost of co-holding for consumers and the risk of lending for the bank. An instructive model for this already exists in the Australian mortgage market, where "offset accounts" allow borrowers to reduce interest costs while maintaining liquid balances. Similar hybrid products could be developed for revolving credit—for instance, checking accounts that "offset" credit card balances at reduced interest rates. Such products could preserve the psychological benefits consumers derive from maintaining liquid assets while minimizing the financial costs of co-holding.

Our field experiment was conducted from February 17 to March 18, 2020, coinciding with the early stages of the COVID-19 pandemic in Australia. While the first case in Australia was documented on January 25, 2020, the first death did not occur until March 1, and most participants received messages during the first week of the experiment. Since most participants received messages before significant pandemic disruptions began, it is unlikely that COVID-19 affected our treatment and control groups differently. Moreover, our analyses of banking data show consistent co-holding patterns before and during the COVID-19 pandemic, supporting the robustness of many of our empirical insights.

We tested our intervention at a single financial institution in Australia during a specific time period. The Commonwealth Bank of Australia is the country's largest bank and serves as the main financial institution for more than one-third of Australians, which enhances the ecological validity of our findings. Nevertheless, replication across different markets and economic conditions would help establish whether these patterns hold more broadly. Additionally, our intervention was relatively light-touch, appearing at most twice per customer during a single month. Although we observed high engagement rates (see Appendix A)—suggesting that limited attention was not the primary barrier—more heavy-handed interventions might

yield different outcomes.

These findings suggest that interventions that simply give people information are not enough to change costly financial behaviors like co-holding. Effective interventions must, instead, be designed to work with the psychological functions this behavior serves in people's lives. Understanding these functions—and designing interventions that respect rather than override them—remains a crucial task for behavioral science and financial services.

ESSAY 3

KEEPING CASH AND REVOLVING DEBT: HOW CONSUMERS' PREFERENCE FOR SPENDING ON DEBIT VERSUS CREDIT INFLUENCES THEIR DECISION TO CO-HOLD

This essay is adapted from Batista, Rafael M., Ella Mao, and Abigail B. Sussman (2025). "Keeping Cash and Revolving Debt: How Consumers' Preference for Spending on Debit versus Credit Influences Their Decision to Co-Hold." Working Paper.

3.1 Introduction

Why would anyone give up $20 in exchange for $1? The answer is not obvious, yet this trade reflects the choice made by millions of people who consistently maintain low-yield asset balances while also holding high-interest revolving debt.

This behavior, known as "co-holding" or the "debt puzzle", is puzzling since co-holders in most cases would be financially better off if they used part of their liquid assets to pay off their revolving debt (Stango and Zinman, 2009b; Varian, 1987). Among credit card users at our partner financial institution, approximately one in five exhibit this behavior. To put the cost co-holding into perspective, for every $100 held in savings and debt, the typical co-holder in our sample is earning $0.75 for their savings while paying nearly $15 on their debt. For a co-holder with a balance of $2,000 (approximately the median amount in our sample), this spread translates to $15 earned but $300 paid in annual interest, not accounting for compound interest. By co-holding at the same institution, co-holders are effectively borrowing back the money they have deposited and are paying to do so.

Financial decisions such as these are core to marketing (Lynch, 2011; Sussman, Hershfield, and Netzer, 2023). Marketing research in financial decision-making examines how consumers save (Reiff et al., 2023; Soman and Cheema, 2011; Sussman and O'Brien, 2016) and repay

debt (e.g., Amar et al., 2011; Donnelly et al., 2023; Kettle et al., 2016; Sakaguchi et al., 2021). Co-holding presents a unique case where consumers regularly choose to save despite having high-interest debt to repay (Medina and Pagel, 2021), and to borrow even though they have cash stored away (Gathergood and Olafsson, 2024). By co-holding month after month, consumers experience compounding costs, affecting their financial well-being with implications for how they spend (Hamilton et al., 2019; Sussman, Hershfield, and Netzer, 2023).

Co-holding is not a new phenomenon. Existing research, using different methods and sampling in different contexts, estimates that between 12% to over 50% of consumers co-hold (Gathergood and Olafsson, 2024; Gathergood and Weber, 2014; Greene and Stavins, 2023; Gross and Souleles, 2002; Medina and Pagel, 2021). Moreover, co-holding is not a one-off choice, but rather persists over time and across many choices (Gathergood and Olafsson, 2024; Medina and Pagel, 2021). Importantly, this persistence occurs even among individuals with substantial liquidity, who could easily eliminate their debt using available cash balances (Gathergood and Olafsson, 2024; Medina and Pagel, 2021). This research (reviewed below) has attempted to rationalize this behavior mainly through the lens of finance and economics.

In this paper, we draw on research from marketing to examine co-holding through the lens of consumer psychology. We examine how preferences for spending on debit versus credit influences decisions to save and borrow. While prior research primarily examines motivations for maintaining asset and debt balances directly in anticipation of the future, our core insight is that preferences around ongoing, daily spending also play a role. Our research contributes to marketing literature in mental accounting (Hirst, Joyce, and Schadewald, 1994; Prelec and Loewenstein, 1998; Thaler, 1985), spending (Soman, 2001; Soman and Cheema, 2002), saving (Soman and Cheema, 2011; Sussman and O'Brien, 2016), and borrowing (Donnelly et al., 2023; Kettle et al., 2016; Tully and Sharma, 2018).

3.1.1 Rational Explanations for Co-Holding

Prior research in economics and finance rationalizes co-holding as a strategic response to anticipated outcomes. Lehnert and Maki (2007), for instance, argues that consumers anticipating bankruptcy strategically preserve their liquid assets instead of paying off debt, allowing them to retain assets under bankruptcy protection. Fulford (2015) offers a second strategy, by which consumers maintain precautionary savings in case they ever lose access to credit (see also Druedahl and Jørgensen, 2018; Gorbachev and Luengo-Prado, 2019). In contrast, Bertaut, Haliassos, and Reiter (2009) suggest consumers maintain high debt balances *to* limit their available credit, thus constraining their future spending impulses. This view presents co-holding as a self-control device (see Gathergood and Weber, 2014; Gross and Souleles, 2002, but also Wilcox, Block, and Eisenstein 2011). Telyukova (2013) offers yet another explanation, proposing co-holders keep liquid assets because they anticipate situations where cash is required (see also Greene and Stavins, 2023).

However, recent large-scale empirical studies challenge the rational explanations for co-holding (Choi and Laschever, 2018; Gathergood and Olafsson, 2024; Medina and Pagel, 2021; Vihriälä, 2025). Medina and Pagel (2021) demonstrates that when 3.1 million customers were nudged to save more, their borrowing remained unchanged—suggesting consumers mentally separate savings and debt accounts rather than optimizing across them. Gathergood and Olafsson (2024) found that consumers with multiple debit cards, linked to different checking and overdraft accounts, allocated their expenses differently. For instance, impulsive expenses like gambling and alcohol were typically charged to accounts with negative balances, while planned expenses like home renovations and books were charged to accounts with positive balances. Gathergood and Olafsson (2024) provides important substantive evidence for mental accounting across multiple debit cards. We extend this research by examining how consumers choose between different payment methods, debit versus credit cards. We also provide experimental evidence showing that manipulating consumers' payment method pref-

erences directly can affect their debt repayment decisions, suggesting that these preferences play a causal role in co-holding behavior.

3.1.2 The Role of Mental Accounting

Implicit in the definition of co-holding is the idea that consumers treat the money they possess in cash differently than the money they borrow (or *can* borrow, in the case of credit lines). Instead of using existing assets to pay down debt or spending their cash before taking on new debt, co-holders maintain high asset *and* high debt balances. We find that they also transact regularly using both debit and credit. By keeping cash alongside a revolving debt balance and spending from different sources, co-holders appear to treat these sources as distinct rather than fungible.

Co-holders' tendency to maintain separate asset and debt balances while using both payment methods aligns with research on mental accounting. This research documents several ways consumers violate the economic principle of fungibility, where all money is assumed to be interchangeable regardless of its source or intended use (Priolo et al., 2023; Thaler, 1985, 1999; Zhang and Sussman, 2018). Mental accounting research highlights how consumers often categorize funds according to where the funds originated (e.g., Lee and Morewedge, 2023; Milkman and Beshears, 2009; O'Curry and Strahilevitz, 2001), where the funds are currently stored (e.g., Hastings and Shapiro, 2013; Reinholtz, Bartels, and Parker, 2015), the intended uses of the funds (e.g., Cheema and Soman, 2006; Heath and Soll, 1996), or the types of expenses considered (e.g., Aydin, 2022; Fei, Bartels, and Zhang, 2025; Quispe-Torreblanca et al., 2019; Tully and Sharma, 2018). Across contexts, this mental accounting helps consumers manage their spending better and track where their money is going (Prelec and Loewenstein, 1998; Thaler, 1999; Zelizer, 1997).

Furthermore, recent work suggests that payment methods themselves may serve as distinct mental accounts. For instance, Agarwal, Bubna, and Lipscomb (2021) found that the

week after receiving a credit card statement, consumers increased their spending by about 15%, but only on credit cards—debit spending was unaffected, which suggests consumers are treating credit and debit cards as separate mental accounts. Similarly, Gelman and Rousanov (2023) demonstrated that when consumers exogenously received new credit cards, they increased total consumption on the new card without reducing spending on existing cards. These findings suggest that beyond categorizing money by its source (e.g., salary vs. windfall) or intended use (e.g., vacation fund vs. groceries), consumers may also partition their spending by payment method.

3.1.3 Consumer Preferences for Spending on Debit and Credit

Different payment methods provide distinct experiences that could drive choices and shape preferences. For instance, consumers may prefer one payment method over another when economic incentives are clear, such as earning rewards (Agarwal, Chakravorti, and Lunn, 2010; Han, 2025) or avoiding surcharges. However, empirical studies find that such price incentives cannot explain the magnitude of observed payment method preferences (Ching and Hayashi, 2010; Stavins, 2018). Instead, psychological factors appear to drive systematic preferences that go well beyond economic reason (e.g., Feinberg, 1986; Hirschman, 1979; Quispe-Torreblanca et al., 2019).

Debit cards offer a sense of control that many consumers find appealing for routine spending. A payment made on a debit card is instantly reflected in one's account (Hernandez, Jonker, and Kosse, 2017; Klee, 2006), and this immediate feedback creates a tighter "coupling" of experiences between paying for something and consuming it (Gourville and Soman, 1998; Prelec and Loewenstein, 1998; Rick, Cryder, and Loewenstein, 2008). As a result, consumers tend to spend less when using debit compared to credit (Prelec and Simester, 2001; Soman, 2001). This spending restraint makes debit cards particularly useful for those seeking to manage their daily expenses (Sprenger and Stavins, 2010).

In contrast, credit cards provide flexibility that can be valuable for larger or unexpected expenses. Credit cards allow consumers to separate the pain of paying from the act of consuming, thereby increasing the enjoyment of their purchases (Prelec and Loewenstein, 1998; Soman, 2001). They also allow consumers to avoid dipping into liquid funds intended for something else (Sussman and O'Brien, 2016). Additionally, credit cards can help consumers cover expenses while they wait for the next paycheck (Agarwal and Qian, 2014) and smooth the cost of large expenses over time (Quispe-Torreblanca et al., 2019).

3.1.4 Drivers of Payment Method Preferences

The distinct experiences described above likely operate through several psychological mechanisms. Existing evidence points to pain of paying (Bechler et al., 2025; Reshadi and Fitzgerald, 2023) and perceived convenience (Boden, Maier, and Wilken, 2020) as particularly important drivers, though other factors such as self-control needs (Choi and Laschever, 2018; Gathergood and Weber, 2014) and responsibility concerns (Sussman and O'Brien, 2016) may also contribute. More importantly, pain of paying and perceived convenience both operate at the moment of purchase, making them particularly relevant for understanding payment method choice.

Pain of paying represents the negative psychological feeling consumers associate with spending (Prelec and Loewenstein, 1998; Reshadi and Fitzgerald, 2023; Rick, Cryder, and Loewenstein, 2008). The intensity of pain varies depending on how obvious it is that one is giving up their money (i.e., "transparency", Soman, 2003) and how tightly it connects to consumption (i.e., "coupling" mentioned above, Prelec and Loewenstein, 1998; Raghubir and Srivastava, 2008). Critically, this pain is often *anticipatory*. Consumers experience it when contemplating purchases, not just when making payments (Rick, Cryder, and Loewenstein, 2008; Sheehan and Van Ittersum, 2018). We posit that anticipatory pain not only shapes decisions of how much to spend (Prelec and Simester, 2001; Sheehan and Van Ittersum,

2018; Soman, 2003; Soman and Cheema, 2002), but can also affect which payment method consumers prefer to use. Prior research showing that cash creates more pain than credit cards (e.g., Bechler et al., 2025; Prelec and Simester, 2001; Soman, 2003) suggests that debit cards (which provide immediate account feedback like cash) should similarly create more pain than credit cards. However, this conclusion rests on studies that typically (i) assign payment methods exogenously rather than elicit preferences (e.g., Prelec and Simester, 2001; Soman, 2003), (ii) focus on cash versus credit while excluding debit (many of these are compiled and replicated in Bechler et al., 2025), and (iii) rarely study the pain of paying broken down by expense (Rick, Cryder, and Loewenstein, 2008; Sheehan and Van Ittersum, 2018). We explore the possibility that some consumers feel more pain paying with credit, especially for routine expenses, and that this shapes their preference for debit.

Beyond pain of paying, consumers may also form payment method preferences based on perceived convenience. Boden, Maier, and Wilken (2020) examines convenience as a novel mediator alongside pain of paying, showing that consumers' willingness to pay increased when they perceived a payment method as more convenient. Though few studies have explicitly studied whether convenience drives payment method choice, broader research demonstrates that small frictions powerfully shape consumer preferences (e.g., Thaler and Sunstein, 2009). For example, consumers might prefer paying with credit if using cash requires withdrawing money from an ATM, or they may prefer paying with cash they have on hand if paying with a card requires entering a PIN or signing a receipt. However, in choosing between two cards (debit versus credit), the friction differences seem especially small. Nevertheless, consumers may *perceive* one as more convenient through repeated association with different expense types (Boden, Maier, and Wilken, 2020). These convenience perceptions could then contribute to payment method preferences that sustain co-holding behavior.

Several additional psychological mechanisms may reinforce these payment method preferences. First, consumers might believe it is more *(ir)responsible* to use one payment method

over another. For instance, consumers might find it irresponsible to borrow (Greenberg, Sussman, and Hershfield, 2020; Peñaloza and Barnhart, 2011; Sussman and O'Brien, 2016) to cover everyday expenses when they have cash available. These feelings of responsibility may make debit cards feel more appropriate for routine expenses, leaving credit to be used in exceptional circumstances. Second, consumers seeking greater *self-control* may also choose to use debit cards over credit cards. Research shows that co-holders tend to be more impulsive than average yet maintain sophisticated self-control strategies (Choi and Laschever, 2018; Gathergood and Weber, 2014). The immediate account feedback from debit cards may serve as a self-control mechanism, helping impulsive consumers monitor and restrict their spending in real-time (Hernandez, Jonker, and Kosse, 2017; Sprenger and Stavins, 2010). These mechanisms—pain of paying, convenience, self-control, and responsibility—may drive consumers to systematically prefer debit cards for routine expenses.

3.1.5 Current Research

We propose that co-holding emerges partly because consumers have systematic preferences for how they pay for different types of expenses. A natural assumption might be that co-holding reflects consumers' preference for spending on credit—after all, Co-Holders are defined by the fact that they carry credit card debt. Under this assumption, co-holding results from one's preference for spending on credit combined with a need to hold liquid assets "in case of an emergency." The liquid assets are precautionary and static, and co-holding emerges from spending on credit.

Instead, we predict that co-holding is associated with consumers' preference for spending on debit. The logic is straightforward: consumers who prefer using debit cards for routine expenses need liquid assets available for their preferred spending method. When such consumers accumulate credit card debt (e.g., if they encounter a large unexpected expense), they face a trade-off: pay off the debt or keep the cash to use for everyday expenses. Choosing to

preserve their cash-based spending strategy while carrying debt would result in co-holding behavior. If this account is correct, then experimentally shifting consumers' preferences toward credit card usage should increase their willingness to repay outstanding debt, as it reduces their need to maintain cash reserves for routine spending. Understanding what drives these payment method preferences—the psychological mechanisms outlined earlier—is crucial for theoretical understanding and practical intervention design.

We test this theory through three core hypotheses:

> ***H1.*** *Co-holders exhibit stronger preferences for using debit cards for small, everyday expenses compared to non-co-holders.*

If payment method preferences contribute to co-holding behavior, then experimentally manipulating these preferences should also affect financial decisions:

> ***H2.*** *Experimentally shifting consumers' payment method preferences toward credit card usage increases their willingness to repay outstanding credit card debt.*

This prediction may seem counterintuitive, but follows directly from our theoretical framework: experimentally shifting consumers toward credit (and away from debit) card usage should increase their willingness to repay outstanding debt, as it reduces their need to maintain cash reserves for routine spending. Given the importance of understanding the underlying psychology driving these preferences, we also test a variety of possible mechanisms including pain of paying, perceived convenience, feelings of responsibility, and self-control. We propose that:

> ***H3.*** *Varying consumers' pain of paying, perceived convenience, feeling of responsibility, and self-control corresponds to changes in consumer payment method preferences.*

In what follows, we test this theory by addressing three questions: Is co-holding associated with a preference for spending using a debit card? Can changing one's payment method preferences affect downstream financial decisions? And what psychological mechanisms account for these effects? Studies 1 and 2 establish the basic relationship using field data. Retail banking records and customer surveys show that co-holders disproportionately prefer debit for everyday expenses. This pattern holds even when we equate total spending and adjust for individuals' characteristics such as demographics and past financial behaviors. Studies 3-5 provide causal evidence by experimentally shifting payment preferences and measuring effects on debt repayment behavior. Study 3 introduces the paradigm and identifies the main effect: shifting consumers' preferences *towards* spending on credit leads them to repay more of their outstanding debt, thereby reducing the amount they co-hold. Study 4 identifies at least two psychological mechanisms that account for payment method preferences, showing that exogenous changes in pain of paying and perceived convenience (but not feelings of responsibility and self-control) correspond to changes in payment preferences and, through a parallel serial mediation, go on to affect co-holding decisions. Study 5 tests whether this relationship is moderated by wealth levels and finds that although greater wealth leads to more debt repayment overall, it does not alter the fundamental relationship between payment preferences and co-holding behavior. This result suggests that the co-holding patterns are tied to underlying preferences that go beyond straight liquidity constraints, consistent with evidence on co-holding from the field. We conclude by discussing the implications of our findings, including potential interventions and important considerations before implementing them.

This finding has several implications for how we think about consumers' decisions to spend, save, and borrow. Theoretically, our work reframes co-holding as a byproduct of spending decisions. Consumers have preferences over how they *spend*, which then informs how much they save and borrow. This represents a departure from studying co-holding

as decisions around asset and debt balances themselves. Methodologically, our results suggest that studying co-holding requires examining active spending patterns alongside static balances to capture how payment preferences manifest in ongoing transaction decisions. Substantively, while extensive research examines how different payment methods affect spending behavior (e.g., Prelec and Simester, 2001; Soman, 2001; Wilcox, Block, and Eisenstein, 2011), our findings reveal that these preferences can be strong enough to sustain costly financial arrangements, demonstrating their fundamental importance for understanding consumer financial behavior.

This paper contributes to consumer financial decision-making research (Greenberg and Hershfield, 2018; Lynch, 2011; Sussman, Hershfield, and Netzer, 2023) by offering a new perspective on the co-holding puzzle. It extends research in mental accounting (Hirst, Joyce, and Schadewald, 1994; Prelec and Loewenstein, 1998; Thaler, 1985) by building on recent work (Gathergood and Olafsson, 2024; Medina and Pagel, 2021) to show that consumers systematically partition spending between debit and credit cards based on expense type and individual preferences. Importantly, our experimental evidence establishes that these payment method preferences causally drive co-holding behavior. We also contribute to research on payment method preferences (e.g., Bechler, Huang, and Morris, 2023; Boden, Maier, and Wilken, 2020; Shah et al., 2016) by documenting heterogeneity in these preferences both across people and within the same person across expense types. Beyond these academic contributions, our findings suggest novel approaches for financial institutions and policymakers seeking to help consumers optimize their financial decisions—rather than focusing solely on encouraging debt paydown or savings accumulation, interventions might more effectively target consumers' spending preferences directly.

3.2 Study 1

We propose that co-holders (vs. non co-holders) prefer using debit cards for routine expenses and are reluctant to deplete the cash reserves to maintain funds for this preferred spending strategy, even when carrying debt. To test this prediction, we partnered with a large Australian financial institution to survey customers about their payment method preferences. We were also provided access to consenting customers' banking records, including asset and debt balances and individual transactions made using both credit and debit cards over three years, giving us a unique view into consumers' stated preferences and actual financial behavior.

This data allowed us to identify co-holding directly using the threshold set by Telyukova (2013, p. 1151), whereby "co-holding" is defined as maintaining $500 or more in high-interest revolving credit card debt and $500 or more across low-yield liquid asset accounts (i.e., savings and transaction or checking accounts).[1] This amount happens to coincide with approximately one week's worth of expenses for the median co-holder in our sample. We find, through a random sample of customers, that approximately 7% of all bank customers and 23% of customers with a credit card were co-holding (as of January 2020). For our field studies (Studies 1 and 2), "Co-Holders" needed to be co-holding ($\geq$ $500 in assets and revolving debt) for three consecutive billing periods. This criteria, while stricter than other papers', serves to exclude individuals who may have accidentally missed a payment one month.

Study 1 tests H1 by examining whether co-holders and non-co-holders possess systematically different payment method preferences across expense categories.

1. By "revolving debt", we are referring explicitly to the portion of debt that incurs interest. If an individual has $200 in unpaid debt from last month, and $1000 worth of new expenses this month, their debt balance is $1200 but their revolving debt is only $200.

3.2.1 Methods

Participants. 2,307 customers (979 co-holders and 1,328 non-co-holders) completed the survey and consented to us linking their responses with their banking data.[2]

To recruit these customers, we invited approximately 950,000 customers to participate in a brief survey through their banking app (see eligibility criteria in Appendix B). 481,897 of these customers were Co-Holders and 546,077 were Non Co-Holders. These customers were all labeled by the bank as being "MFI," or Main Financial Institution, customers, for whom we had broad coverage of their day-to-day financial decisions. Eligible customers could opt into the survey by clicking the "Start survey" button in their app which redirected them to a Qualtrics survey described below.

Design. The survey consisted of eight questions across three pages: The first page was the consent form. The second page included two questions asking participants how much they agreed with a set of statements and three questions asking how they preferred to pay for a general [specific] set of expenses. The third page then asked how they preferred to pay for three specific [general] expenses. The ordering of general and specific expenses was randomized between participants.

The three categories of expenses included (1) *small, everyday*; (2) *large, anticipated*; (3) *large, surprise*. Table 3.1 displays the general and specific expenses used. Specific examples were randomly selected from three options within each category for each participant. Response options included: "Definitely credit card"; "Probably credit card"; "Probably cash / savings"; "Definitely cash / savings"; or "Another payment option (e.g., Afterpay)". Together with our partners, we decided on "Cash / Savings" to represent spending from liquid asset accounts. Colloquially, this was understood to mean debit spending, but also peer-to-peer

2. We provide a table of summary statistics for customers who completed the survey alongside a random sample of 100,000 customers at this bank (see Appendix B). The table provides a snapshot of customers in January 2020; which is before this survey was conducted, and before the start of the COVID-19 pandemic, but one year after the first month of data we have access to.

transfers, cash withdrawals, and writing checks.

The agree-disagree questions were meant to assess whether customers deliberately segregated expenses by payment method, asking, "To what extent do you agree with each of these statements?" The statements included: (1) "I generally use my credit card for certain purchases and my cash or savings for others." and (2) "I don't need to keep money in my savings because I can always use my credit card." Response options were on a 5-point scale from "Strongly Disagree" to "Strongly Agree".

Table 3.1: Category of expenses displayed in field survey

Category	**General**	**Specific**
3*Small, Everyday	33cm*An everyday purchase*	*"Groceries for the week costing $100"*
		"New pair of jeans costing $50"
		"Concert tickets costing $75"
3*Large, Anticipated	33cm*A large expense you were planning for.*	*"A new computer you've wanted for a while costing $1,000"*
		"New furniture that you've had your eye on for a while costing $2,000"
		"Vacation you've been planning for months costing $1,500"
3*Large, Unanticipated	33cm*A large expense you weren't expecting.*	*"Your refrigerator breaks unexpectedly and it'll cost $1,000 to replace it"*
		"Your car breaks down unexpectedly and you spend $1,500 to get it working again"
		"A water pipe bursts unexpectedly and you pay a plumber $1,000 to fix it."

3.2.2 Results

Data Cleaning. Our analysis focuses on payment preferences between credit and debit cards, so we exclude responses of "Alternative Payment option (e.g. AfterPay)". In doing so, we excluded more co-holders than non co-holders (Small: $\text{Prop}_{\text{CH}} = .04$, $\text{Prop}_{\text{NCH}} = .02$, $p = .003$; Anticipated: $\text{Prop}_{\text{CH}} = .16$, $\text{Prop}_{\text{NCH}} = .06$, $p < .001$; Surprise: $\text{Prop}_{\text{CH}} =$

.12, $\text{Prop}_{\text{NCH}} = .06$, $p < .001$). We also compared individuals' responses for General and Specific items and saw no meaningful difference. We, therefore, averaged the two responses per customer to form a single measure for each category.

Payment Method Preferences. To examine customers' overall payment method preference, we estimated a linear mixed-effects model (Pinheiro and Bates, 2000). We regressed the average response by category on an indicator for CH status and included a random intercept to account for variation between customers. In general, CHs were more likely to favor spending using "cash / savings" ($\beta = .39$, $SE = .04$, $p < .001$).

Payment Method Preferences by Expense Type. Consistent with H1, CHs had a greater preference for using debit cards than did NCHs for each expense type examined, see Figure 3.1. Analyzing the average response separately for each expense category, CHs (M = 3.03, SD = 1.04) preferred to pay for small, everyday items using cash or savings, while NCHs preferred to use their credit card (NCH: M = 2.39, SD = 1.17; Cohen's $d = .58$, 95% CI [.50, .67], $p < .001$; middle of the scale is 2.5). For large anticipated expenses, CHs (M = 2.86, SD = .98) preferred to use cash while NCHs tended to be evenly split (M = 2.50, SD = 1.10; Cohen's $d = .37$, 95% CI [.28, .46], $p < .001$). Finally, for large surprise expenses, both CHs and NCHs preferred to use their credit cards overall. However, CHs had a relative preference for spending in cash (CH: M = 2.33, SD = .95; NCH: M = 2.19, SD = .98; Cohen's $d = .16$, 95% CI [.07, .25], $p < .001$). We also find similar results using non-parametric tests.

Mental Accounting Statements. Examining the responses to statements about mental accounting, we, somewhat surprisingly, see little evidence of a difference between CHs and NCHs. When we code the Likert-responses on a scale from -2 to 2 where positive values indicate agreement, we see that both CHs (M = .65, SD = 1.27) and NCHs (M = .64, SD

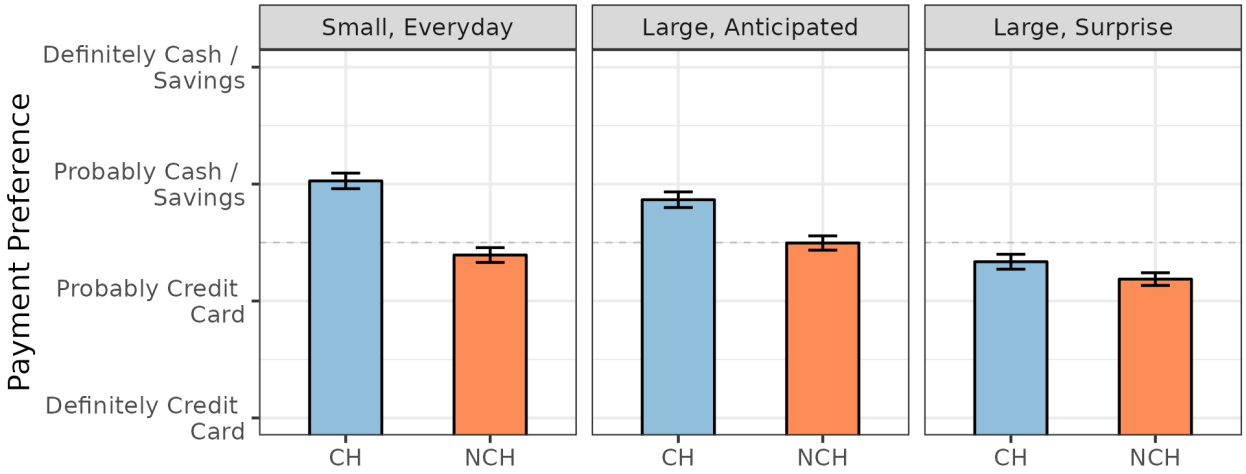


Figure 3.1: Study 1 results: Co-holders report a stronger preference for cash compared to non co-holders. Error bars represent 1.96 * SE; "CH" = Co-Holder; "NCH" = Non Co-Holder; "Another Payment Option" excluded.

= 1.36, Cohen's $d = .006$, $p = .89$) tended to agree with the statement "I generally use my credit card for certain purchases and my cash or savings for others." and disagree with the statement "I don't need to keep money in my savings because I can always use my credit card" (CH: M = −1.06, SD = 1.22; NCH: M = −1.13, SD = 1.25; Cohen's $d = .06$, $p = .20$).

3.3 Study 2

Study 1 establishes a relationship between consumers' co-holding status and their stated payment method preferences. If co-holders do use different payment methods for different types of expenses, this preference should be evident in their actual spending behavior, not just their stated preferences. We test this prediction in Study 2 using administrative banking data from the same customers. Specifically, we examine whether co-holders vary their payment method choice across purchases differently than non-co-holders. The administrative data also allows us to control for demographics and financial characteristics, providing a more rigorous test of H1.

3.3.1 Results

Data Cleaning. We examine credit card and debit card transactions made by each customer in our sample between January 2019 and February 2022. We use the same sample of customers from Study 1 (n = 2,307). We start with transaction-level data and aggregate all transactions to the calendar-month level by individual by product (debit; credit).[3] To capture each customer's payment method preferences, we calculated the proportion of total spending made using debit cards each month (total spent on debit divided by total spent across both payment methods). This proportion serves as our key predictor variable, with higher values indicating stronger revealed preferences for debit card usage.

Correlating Spending Patterns with Survey Responses. Individuals' stated preferences in the survey correlated significantly with the proportion they spent on their debit card that month (Small: $r = .44$; Large, Anticipated: $r = .25$; Large, Surprise: $r = .19$, ps $< .001$), supporting the conclusion that stated preferences reflect actual behavior patterns.

Estimating the Relationship Between Debit Card Spending and Co-Holding. We estimated a series of linear probability models of the following form:

$$CoHolder_i = \alpha + \beta \cdot PropDebit_i + \gamma \cdot X_i + \epsilon_i \tag{3.1}$$

where $CoHolder_i$ is an indicator variable equal to 1 if customer i is classified as a co-holder, $PropDebit_i$ is the proportion of spending made using debit cards, X_i represents various control variables (detailed in Table 3.2), and ϵ_i is the error term.

Consistent with H1, the proportion one spends on their debit card significantly predicts

3. Interest charges and other bank fees were excluded; the remaining transactions were Winsorized at 1% and 99%. If a customer made zero transactions that month, it resulted in a missing value for that month; in these cases, we imputed zero. 39% of CHs and 66% of NCHs ($p < .001$) had at least one month where no transactions were made on their debit card; for credit cards, the pattern reversed (CH: 12%; NCHs: 6%; $p < .001$). Transactions made on different accounts for the same product (e.g., two or more debit cards) were summed together.

Co-Holder status, with one standard deviation increase in the proportion spent on debit corresponding to approximately a 16 p.p. increase in the likelihood a consumer is a Co-Holder ($p < .001$). This relationship is robust across various specifications detailed in Table 3.2, including adjusting for customer demographics, total spending amount, credit card utilization, and one's history of credit limit increases and decreases. That is, the relationship does not appear to be explained by differences in spending levels, nor is it driven by a situation in which CHs have "maxed out" their credit cards and therefore *have* to spend on debit. It is also not accounted for by volatility in one's access to credit at this institution (for related proxies see Fulford, 2015; Gorbachev and Luengo-Prado, 2019). The relationship is consistent whether we examine a single month (Cols 1-3), corresponding to the month the survey was completed by the customer, or an average of all the months between January 2019 and February 2022 (Cols 4-6).

Estimating the Relationship Between Debit Card Spending and Co-Holding by Expense Type. Furthermore, this relationship persists at more granular levels. CHs tend to use their debit cards more across certain subcategories of expenses. For instance, when examining broad merchant categories—such as "Home" goods, "Eating Out", "Groceries" and "Entertainment—CHs use their debit cards more (and credit cards less) compared to NCHs (see Figure 3.2). We see a similar pattern when we relabeled the transactions to include "Small, Everyday", "Large, Anticipated", and "Large, Surprise" expenses (see Appendix B). Appendix B also presents the results from various models broken down by expense type. Together, these analyses support the hypotheses that CHs exhibit stronger preferences for using debit cards than NCHs, especially for small, everyday expenses.

Table 3.2: Estimating the Effects of Debit (vs. Credit) Spending on Propensity to Co-Hold

	Survey Month Only			Average of All Months		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	.428***	.187	.142	.425***	.162	.112
	(.010)	(.244)	(.230)	(.010)	(.244)	(.208)
Prop Spent on Debit (Std)	.162***	.145***	0.103***	.159***	.143***	.063***
	(.010)	(.014)	(.014)	(.010)	(.014)	(.013)
Total Spend (Winsorized; Std)			.007			.057***
			(.013)			(.013)
Average Daily Utilization (Std)			.227***			.433***
			(.032)			(.041)
Max Daily Utilitization (Std)			−.061			−.185***
			(.033)			(.042)
Credit Limit Decreases (Std)			.022			
			(.023)			
Credit Limit Increases (Std)			−.019			
			(.023)			
Customer Controls	No	Yes	Yes	No	Yes	Yes
Observations	2249	2211	2176	2303	2264	2263
Adjusted R^2	.107	.136	.237	.103	.129	.369

Note: Customer Controls include Age Band, Gender, Postcode, State, Tenure with the Bank, and Marital Status as of January 2020. Credit limit increases and decreases represent the total number of months customers experienced an increase or decrease in their credit limit from the previous month, between February 2019 and the survey month. Continuous values have been standardized (Std), $\mathcal{N}(0, 1)$. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

3.3.2 Discussion

Our analysis of survey responses and day-to-day transactions provides converging evidence supporting H1. Co-holders (CHs) use their debit cards more than non co-holders (NCHs), particularly for small, everyday expenses (which comprise a majority of consumers' expenses in both volume and amount of transactions). This pattern holds across various expense categories—including larger, anticipated and surprise expenses—and is robust to controls for demographics, spending levels, and credit utilization.

If consumers were arbitrarily reaching into their wallets, we might expect an even distribution of purchases between credit cards and debit cards. CHs' transaction patterns approach this, but in the survey conducted in Study 1, they consistently reject using credit

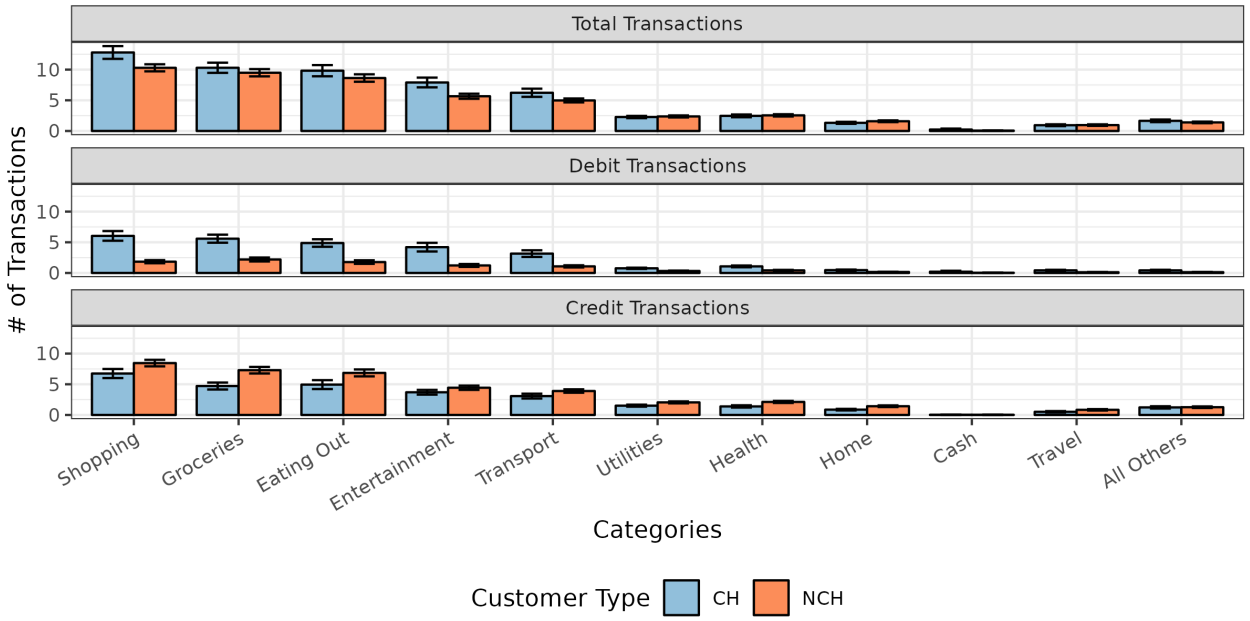


Figure 3.2: Study 2 results: Co-Holders transact more on debit and Non Co-Holders on credit across several categories. # of Transactions represents count made per customer, per merchant category in the month the survey was completed. Error bars represent $1.96 * SE$

cards for all but large, surprise expenses. Instead, our findings are consistent with a pattern in which CHs strategically use different payment methods; debit cards for smaller, everyday expenses while reserving their credit cards for large, unexpected expenses.

3.4 Study 3

Studies 1 and 2 provided converging evidence for our theoretical predictions while establishing strong external validity through field data. However, the correlational nature of these findings limits causal inference. Our theory predicts that experimentally shifting consumers toward credit card usage should increase their willingness to repay outstanding debt with the assets they have, as it reduces their need to maintain cash reserves for routine spending. Study 3 tests this causal prediction (H2) by experimentally manipulating perceptions of credit card usage for everyday expenses.

In addition, the study included exploratory measures of perceived responsibility to help

us understand whether that played a role in participants' responses.

This study was pre-registered on AsPredicted.org (aspredicted.org/xty9-qtq6.pdf).[4]

3.4.1 Methods

Participants. We recruited 360 participants via Prolific; 57 were excluded based on our pre-registered criteria, resulting in a final sample of 303 participants (158 Male, 144 Female; age: M = 41.5, SD = 13.4; 64.7% White, 11.9% Black, 8.9% Latin American, 5.3% Multi-Racial, 9.2% all others).

Design. The study began with participants reporting the number of checking accounts, savings accounts, and active credit cards they had (which we used for pre-treatment screening).

Participants who passed a brief screening indicating that they had both active checking and savings accounts were told they would read a short article containing "tips for improving financial well-being."

Participants were randomly assigned to one of two conditions: Pro-Credit Card or Control. In the Pro-Credit Card condition, the article recommended credit cards as "the wise choice for everyday expenses." The article highlighted benefits such as reward points and fraud protection and contrasted these with spending on a debit card. In the Control condition, the article recommended "diversifying your financial knowledge across multiple areas" without referring to any payment method. Both articles maintained similar structure, language, and length. The articles are provided in Appendix B.

On the next page, participants were asked to "summarize the article, as if describing it

4. In the pre-registration we refer to an earlier study we conducted with similar methodology (this is available in Appendix B). We also indicated that we would focus the primary analyses specifically on co-holders, however, we report results for the full sample to maximize statistical power and because the manipulation effects were consistent across co-holders and non-co-holders (as shown in Web Appendix B.2, Figures B.1 and B.2).

to a friend or family member..." They were then presented with the key measures which appeared on separate pages and in a random order. On one page were a set of payment method preference questions and on another page was a debt repayment scenario.

To assess participants' payment method preference, they were shown the set of three "general" expenses from Study 1 (see Table 3.1). Like the Field Survey, participants were asked how they preferred to pay for *small, everyday*, *large, anticipated*, and *large, surprise* expenses. In this case, the response options ranged from "Definitely Credit Card" (-2) to "Definitely Debit Card" (2), with an "Indifferent" (0) midpoint option. Note that this is different than the Field Survey, which did not include an indifferent option, but instead had a fifth "Alternative Payment" option. The Field Survey also referred to "Cash / Savings" rather than "Debit Card". In our pre-registration, we highlight the small, everyday expenses as the measure of interest.

To assess co-holding decisions directly, participants were given a scenario where they were told to imagine having some amount of outstanding *credit card debt* (e.g., \$1,400) that would start to incur interest if unpaid and some amount of *assets* (e.g., \$2,000) available. They were also told their *average monthly spend* (e.g., "between \$1,200 and \$1,600). Although we sampled the specific stimuli from a set of four asset-debt-spending combinations, we kept the ratio between these constant (debt was 0.7 times the asset amount and the range of average expenses was always 0.6 to 0.8 times assets). Question wording and specific amounts used are provided in Appendix B.

After reading the scenario, participants answered the question "With all this in mind, how much of the money you have saved up (if any) would you transfer to pay off your credit card debt?" by indicating how much of their available funds they would use to pay off their remaining credit card debt (any value between \$0 and the assigned debt amount).

Following these primary measures, participants completed four sets of responsibility perception questions (see Sussman and O'Brien, 2016), designed to assess perceived irresponsi-

bility of: (1) having *less* than $500 in liquid assets, (2) having *more* than $500 in credit card debt, (3) *using* debit cards for everyday purchases, and (4) *using* credit cards for everyday purchases. For more details, see Appendix B.

Participants then reported their typical credit card repayment behavior and self-identified whether they engaged in co-holding (defined as simultaneously paying interest on credit card debt while maintaining money in checking/savings/cash). Participants also read a short description of two "cash flow management strategies" and were asked how well each of these descriptions fit them (see Appendix B).

Finally, participants provided demographic information and completed manipulation checks (Kane and Barabas, 2019), including a subjective manipulation check that asked how financially wise it is to use credit cards for everyday purchases and a factual manipulation check that asked them to recall the main recommendation from the article.

3.4.2 Results

Our primary analyses focused on two key outcomes: (1) payment method preferences for everyday purchases and (2) the proportion of credit card debt participants chose to repay. We first tested whether treatment assignment affected one's reported preference, as intended. We could then test whether shifting preferences also affected the decision to repay debt, consistent with H2. If our theory is correct, then we would expect to see a stronger preference *for* credit cards corresponding with *greater* debt repayment.

Manipulation Checks. The subjective manipulation check suggests the manipulation was successful; participants in the Pro-Credit Card condition (M = .908, SD = 1.11) rated using credit cards for everyday purchases as significantly more financially wise than those in the Control condition (M = −.03, SD = 1.23), one-sided $t(296.4) = 6.94$, Cohen's d = .80, 95% CI [.56, 1.03], $p < .001$. The comprehension check suggests both groups were treated

in similar proportions(81.0% of participants in the Pro-Credit Card condition vs. 81.3% in the Control) answering correctly, $\chi^2(1) = 0, p = 1.000$.

Co-Holding Status. We define co-holding based on two questions: The first asked participants, "When it comes to your credit card, which of the following statements best describes your typical repayment behavior?" Participants could indicate they paid off their balance (a) "in full", (b) more than the minimum, but not in full, (c) the minimum, or (d) less than the minimum. The second question provided participants with a definition of co-holding as: "keeping money in a checking account, savings account, or as cash while also carrying a paying interest [sic] on your credit card debt each month," before asking them to indicate whether they considered themselves to be someone who "co-holds". If participants provided inconsistent responses—for instance, if they said they paid their balance in full (Q1) *and* considered themselves to be co-holding (Q2)—they would see a clarification question that revealed the inconsistency and asked a modified version of the co-holding question.

We classified 52.1% of participants (n=303) as Co-Holders based on these questions. Co-holding status did not differ by condition, $\chi^2(1) = .28, p = .600$. This proportion is noticeably higher than what has been reported in past work (though see Greene and Stavins, 2023). We believe this is partly due to the crude nature of the survey question; for example, we did not ask for exact balance amounts, so we could not exclude those co-holding trivial amounts.

Payment Method Preference. Those assigned to the Pro-Credit Card condition (M = −.78, SD = 1.36) reported a greater preference for credit cards than those assigned to the Control condition (M = .36, SD = 1.51), $t(296.4) = 6.94$, Cohen's d = .80, 95% CI [.56, 1.03], $p < .001$, indicating the manipulation successfully shifted payment preferences (see Figure 3.3, Panel A).

We test whether the manipulation affected Co-Holders differently from Non Co-Holders

using a two-way ANOVA. We find significant main effects for both experimental condition ($F(1, 299) = 51.94$, $\eta_p^2 = .148$, $p < .001$) and co-holding status ($F(1, 299) = 21.16$, $\eta_p^2 = .066$, $p < .001$). We also see a marginal interaction ($F(1, 299) = 3.72$, $\eta_p^2 = .012$, $p = .055$). In other words, participants in the Pro-Credit Card condition showed stronger preferences for using credit cards compared to those in the Control condition, and this shift was particularly pronounced among co-holders, who generally exhibited stronger baseline preferences for debit cards (see Appendix B).

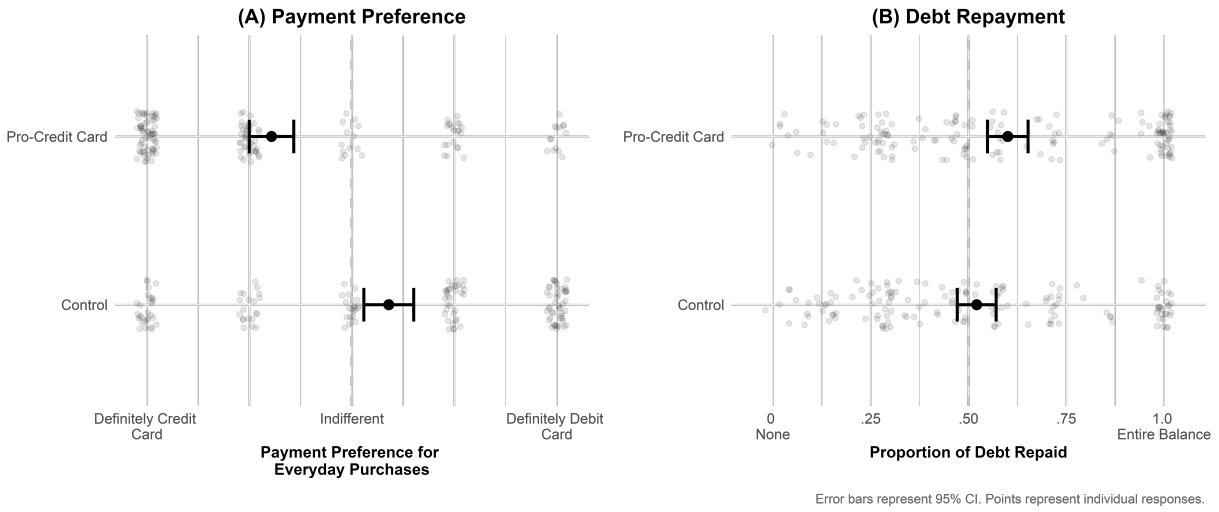


Figure 3.3: Pro-Credit Messaging Affects (A) Payment Method Preference and (B) Debt Repayment Decision. Online Study (N = 721). Error bars represent 1.96 * SE.

Debt Repayment. Consistent with H2, participants in the Pro-Credit Card condition repaid *more* of their debt balance ($Prop_{Pro-CC} = .601$, $SD = .32$) than did those in the Control condition ($Prop_{Ctl} = .521$, $SD = .31$), $t(300.7) = 2.19$, Cohen's $d = .25$, 95% CI [.02, .48], $p = .030$. Figure 3.3, Panel B, displays these results. In the Appendix B, we also display these results split by one's self-reported co-holding status. While the effects are directionally the same for CHs and NCHs, it is stronger for NCHs (Main Effect of Article: $F(1, 299) = 4.90$, $\eta_p^2 = .016$, $p = .028$; Main Effect of Co-Holding Status: $F(1, 299) = 4.90$, $\eta_p^2 = .031$, $p = .002$; Interaction: $F(1, 299) = 4.90$, $\eta_p^2 = .0005$, $p = .684$). This pattern is

consistent with the larger effect of the manipulation on NCHs.

Perceptions of Financial Responsibility. Our exploratory analysis of feelings of irresponsibility revealed distinct patterns based on the financial behavior we asked about (see Figure B.3). For questions asking about perceptions of irresponsibility towards holding high debt balances or low asset balances, we observed no effect based on the treatment assigned ($ps \geq .352$), nor did we see differences between Co-Holders and Non Co-Holders ($ps \geq .344$).

However, perceptions regarding payment methods showed notable differences. For the perceived irresponsibility of using *debit*, we found a significant main effect of experimental condition ($F(1, 299) = 13.53$, $\eta_p^2 = .043$, $p < .001$), with participants in the Pro-Credit Card condition viewing debit use as more irresponsible, with no difference by co-holding status ($F(1, 299) = 1.98$, $\eta_p^2 = .007$, $p = .161$). For perceptions of *credit card* use, we observed significant main effects for both condition ($F(1, 299) = 17.93$, $\eta_p^2 = .056$, $p < .001$) and co-holding status ($F(1, 299) = 30.14$, $\eta_p^2 = .092$, $p < .001$). Co-holders perceived using credit cards as significantly more irresponsible than non-co-holders, regardless of experimental condition (Interaction: $F(1, 299) = 1.25$, $\eta_p^2 = .004$, $p = .265$).

3.4.3 Discussion

This experiment provides causal evidence supporting H2. Exposure to pro-credit card messaging shifted participants' stated preferences toward credit cards and led them to repay more outstanding debt, reducing the co-holding balance. The effect on payment method preferences was particularly pronounced among co-holders, who exhibited stronger baseline preferences for debit cards.

3.5 Study 4

Study 3 established a causal relationship between payment method preferences and debt repayment decisions. This finding raises important questions about the underlying psychological processes: what drives consumers to shift their payment method preferences when exposed to pro-credit messaging, and which psychological factors account for these shifts?

Exploratory analysis in Study 3 revealed that the manipulation affected feelings of irresponsibility toward different payment methods, though this was not a pre-registered hypothesis and requires confirmatory testing. Study 3 was also not designed to test competing hypotheses. Study 4 addresses these limitations by systematically examining multiple psychological factors including responsibility, pain of paying, perceived convenience, and self-control to identify which mechanisms best account for one's payment method preferences (H3).

The study was pre-registered on AsPredicted.org (aspredicted.org/snm5-pz69.pdf).

3.5.1 Methods

Participants. We recruited 562 participants via Prolific; 75 were excluded based on our pre-registered criteria (2 other observations appeared to be duplicates), resulting in a final sample of 486 participants (248 Male, 235 Female, 3 Self-Identified; age: M = 40.3, SD = 13.1; 61.7% White, 13.6% Black, 7% Latin American, 5.8% Multi-Racial, 11.9% all others).

Design. The study procedure followed the same structure as Study 3. Participants first consented to the study and then answered the screening questions. In addition to the questions from Study 3, we also asked participants to write down how much money they typically spent in a month.

Participants were then told they would read a short article which was exactly as in Study 3. Again, the two conditions included a Pro-Credit Card condition and a Control condition.

As before, participants were asked to summarize the article they read.

Participants then completed the same debt repayment task in Study 3. However, this time we included a fifth set of values, "personalized" to the participant. Without explicitly mentioning it to participants, some participants were assigned to a scenario where the asset value was 1.4 times the monthly spending they reported earlier in the study. This ensured that the debt amount was approximately their monthly spending. They were also told they typically spent between 0.6 and 0.8 times the asset amount each month, which in this case included their reported monthly spending amount. All participants indicated how much of their available funds they would use to pay off the remaining credit card debt (any value between \$0 and the assigned debt amount).

Participants were then presented with the payment method preference questions. As in Study 3, participants indicated their preference for using credit versus debit cards for three expense types on a 5-point scale ranging from "Definitely Credit Card" (-2) to "Definitely Debit Card" ($+2$), with "Indifferent" (0) as the midpoint.

Study 4 expanded on Study 3 by systematically measuring four potential psychological mechanisms:

1. *Relative Pain of Paying*: Participants indicated whether they found it more painful to pay for everyday purchases using their credit card or debit card on a 5-point scale from "Much More Painful Using Credit Card" (-2) to "Much More Painful Using Debit Card" ($+2$). The question was asked in the following way: "Imagine needing to make an everyday purchase, such as buying groceries or gas or eating out on your lunch break. Would you find it to be more painful to pay for this expense using your credit card or your debit card?"

2. *Perceived Irresponsibility of Spending on Credit*: Participants rated their agreement with three statements: (i) "I would feel irresponsible paying for everyday items using my credit card." (ii) "Making small, everyday purchases on my credit card would leave

me feeling like a bad person." (iii) "Others would judge me negatively if they learned I had paid for everyday goods using my credit card." (Crobach's $\alpha = .79$). Responses ranged from "Strongly Disagree" (-2) to "Strongly Agree" ($+2$) and were averaged across items to create a composite measure.

3. *Relative Convenience of Debit*: Participants rated their agreement with the statement "In my day-to-day, I find it more convenient to pay using my debit card" on a 5-point scale from "Strongly Disagree" (-2) to "Strongly Agree" ($+2$).

4. *Self-Control*: Participants rated their agreement with two statements (used in Gathergood and Weber, 2014, p. 459): "I am impulsive and tend to buy things even when I can't really afford them" and "I am prepared to spend now and let the future take care of itself" ($r = .46, p < .001$). Responses ranged from "Strongly Disagree" (-2) to "Strongly Agree" ($+2$) and were averaged across items to create a composite measure.

Like in Study 3, we asked participants about their typical credit card repayment behavior and whether they identified as a co-holder. Unlike in Study 3, we did not ask about their cash flow management strategies.

We also replaced the subjective manipulation check with a self-annotation task. Participants were shown the summary of the article they wrote earlier in the survey and were asked whether they mentioned using a "credit card". They could respond "Yes" if they explicitly mentioned it, "Not explicitly, but it is obvious that I am referring to one," or "No" if they did not even refer to it.

Finally, participants provided demographic information and answered the multiple choice reading check from Study 3.

3.5.2 Results

Manipulation Checks. 95.2% of participants in the Pro-Credit Card condition referred to credit cards in their summary of the article. In contrast, only 23.1% of participants did so in the Control, $\chi^2(1) = 261.4, p < .001$. When asked to recall the content of the article, we see approximately the same proportion of correct responses across conditions ($Prop_{Pro-CC} = .786$, $Prop_{Ctl} = .846$, $\chi^2(1) = 2.55, p = .110$).

Co-Holding Status. 57.8% of participants identified as co-holders. This proportion did not differ by condition ($Prop_{Pro-CC} = .579$, $Prop_{Ctl} = .577$, $\chi^2(1) = .00, p = 1.00$).

Payment Method Preference. In replicating the effect from Study 3, we see a significant difference in payment preference by condition. Those in the Pro-Credit Card condition reported a preference for credit (M = −.20, SD = 1.44) while those in the Control condition reported a preference for debt (M = .39, SD = 1.52)—a significant difference, $t(476.5) = 4.42$, Cohen's $d = .40$, 95% CI [.22, .58], $p < .001$.

Comparing the effects by co-holding status, we see a main effect of experimental condition ($F(1, 482) = 20.31$, $\eta_p^2 = .040$, $p < .001$) and reported co-holding status ($F(1, 482) = 16.04$, $\eta_p^2 = .032$, $p < .001$), but no interaction ($F(1, 482) = 2.43$, $\eta_p^2 = .005$, $p = .120$).

Debt Repayment. The debt repayment decision is consistent with H2 and with the finding in Study 3. Those assigned to the Pro-Credit Card condition repaid more of their debt balance ($Prop_{Pro-CC} = .624$, SD = .324) than those assigned to the Control condition ($Prop_{Ctl} = .510$, SD = .317; $t(482.7) = 3.90$, Cohen's $d = .35$, 95% CI [.17, .53], $p < .001$.

Comparing the effect by co-holding status, we see a main effect of experimental condition ($F(1, 482) = 16.22$, $\eta_p^2 = .033$, $p < .001$) and co-holding status ($F(1, 482) = 33.24$, $\eta_p^2 = .064$, $p < .001$) where Non Co-Holders tended to repay more of their debt than Co-Holders, but no interaction effect ($F(1, 482) = .58$, $\eta_p^2 = .001$, $p = .447$), suggesting the treatment had a

similar effect for both Co-Holders and Non Co-Holders.

Psychological Mechanisms. Table 3.3 presents the effects of the Pro-Credit manipulation on all four candidate mechanisms. The manipulation significantly affected three of four mechanisms: Pain of Paying (with Pro-Credit participants finding debit more painful), Convenience of Debit (with Pro-Credit participants rating debit as less convenient), and Self-Control. Irresponsibility of Credit showed no significant difference between conditions despite correlating strongly with payment preferences ($r = .47$, $p < .001$), failing to replicate the exploratory finding in Study 3.

Table 3.3: Effects of Pro-Credit Card Manipulation on Psychological Mechanisms (Study 4)

Psychological Mechanism	**Pro-Credit M (SD)**	**Control M (SD)**	**Cohen's *d* [95% CI]**	***p*-value**
Pain of Paying (Debit more painful)	.19 (1.15)	-.27 (1.29)	.38 [.20, .56]	<.001
Irresponsibility of Credit Spending	-.59 (1.09)	-.44 (1.17)	.13 [-.05, .31]	.156
Convenience of Debit Spending	.19 (1.40)	.54 (1.44)	.24 [.07, .42]	.007
Lack of Self-Control	-.56 (1.15)	-.78 (1.04)	.20 [.02, .38]	.026

Note: N = 486. All scales range from -2 to +2. Pain of Paying: positive values indicate debit feels more painful. Irresponsibility: positive values indicate credit feels more irresponsible. Convenience: positive values indicate debit feels more convenient. Lack of Self-Control: positive values indicate lower self-control.

Parallel Mediation. To systematically examine how the experimental manipulation affected payment preferences and debt repayment decisions, we conducted two types of mediation analyses: parallel mediation to see which of the psychological variables might help explain the effect on payment preference, and a serial mediation, to examine the relationships across the full theoretical model from manipulation to debt repayment.

First, we used a parallel mediation model to test whether the psychological variables mediated the effect of condition on payment preferences (Preacher and Hayes, 2008; Zhao,

Lynch, and Chen, 2010). In this analysis, we only included the variables for which the condition assignment caused a significant difference: Pain of Paying, Convenience of Debit, and Self-Control.

The results revealed that both Pain of Paying and Convenience of Debit significantly mediated the relationship between the experimental manipulation and payment preferences (see Figure 3.4). The total effect of condition on payment preference was $b = -.60$ (indicating a shift toward credit card preference in the Pro-Credit Card condition). Of this total effect, 58.3% was accounted for by the psychological variables: 25.4% through Pain of Paying ($b = -.15$, 95% CI [$-.25$, -0.07]) and 32.2% through Perceived Convenience of Debit ($b = -.19$, 95% CI [$-.34$, $-.06$]). Despite being affected by the manipulation, Self-Control did not significantly mediate the relationship between treatment assignment and payment preferences ($b = -.004$, 95% CI [$-.03$, $.02$]).

Using percentile bootstrapped pairwise comparisons of indirect effects (5,000 resamples; Preacher and Hayes, 2008), we found no significant difference between the Pain of Paying and Convenience pathways ($b_{diff} = .04$, 95% CI [$-.11$, $.20$]), indicating that both mechanisms contribute comparably to the overall effect. In addition, both the Pain of Paying ($b_{diff} = -0.15$, 95% CI [$-.25$, $-.06$]) and the Convenience ($b_{diff} = -.19$, 95% CI [$-.34$, $-.05$]) pathways were significantly stronger than the Self-Control pathway.

Serial Mediation. To examine the full theoretical model linking the manipulation to debt repayment decisions, we conducted a serial mediation analysis using structural equation modeling (SEM) with bootstrapped confidence intervals (5,000 resamples; Preacher and Hayes, 2008; Rosseel, 2012; Zhao, Lynch, and Chen, 2010). This approach allowed us to test whether the manipulation affected debt repayment by sequentially influencing psychological variables and then payment preferences.

The SEM results provided evidence supporting two serial mediation paths that linked the experimental manipulation to debt repayment (see Figure B.4). Specifically, the experimen-

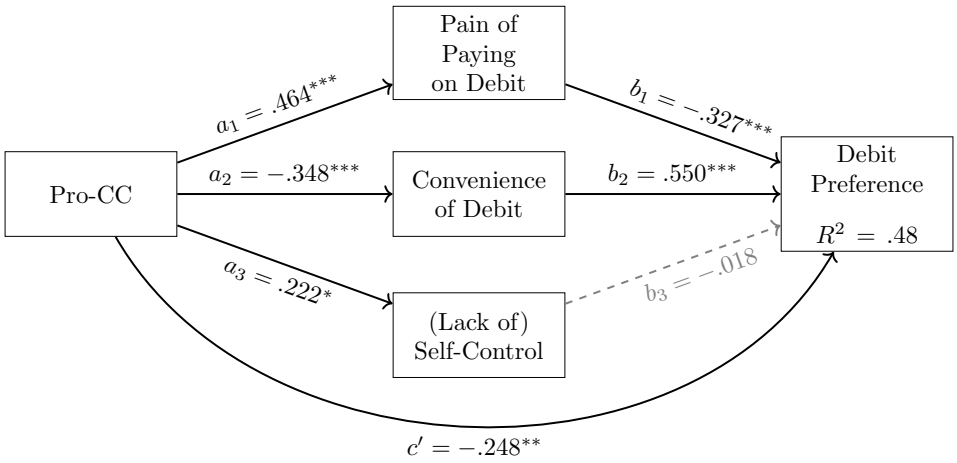


Figure 3.4: Parallel mediation model showing the effect of Pro-Credit Card condition on payment method preference through psychological mediators. Standardized coefficients shown. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dashed line indicates non-significant path.

tal manipulation influenced debt repayment indirectly via changes in: (1) Pain of Paying $\rightarrow$ Payment Preference ($b = .008$, 95% CI [.003, .015], 7.9% of total effect) and (2) Convenience $\rightarrow$ Payment Preference ($b = .010$, 95% CI [.002, .019], 9.9% of total effect). The Self-Control pathway was, again, not significant ($b = .000$, 95% CI [$-.001$, .002]). Together, these serial mediating pathways accounted for 16.8% of the total effect of condition on debt repayment.

Although self-control does not appear to mediate the relationship between the manipulation and payment method preference, we do observe a significant direct effect on debt repayment ($b = -.033$, $p = .009$) independent of payment preferences. That is, lower self-control (higher values represent *lack* of self-control) is associated with lower debt repayment, but does not appear to be connected directly to payment method preferences.

Finally, the serial mediation analysis showed that payment method preferences helped connect the psychological variables to debt repayment decisions ($b = -.050$, 95% CI [$-.074$, $-.026$], $p < .001$).

After accounting for all mediating pathways, the experimental manipulation maintained a significant direct effect on debt repayment ($b = .092$, 95% CI [.035, .151], $p = .002$),

suggesting there are likely to be other mechanisms that are not captured in our model.

3.5.3 Discussion

Study 4 provides systematic evidence for H3 by identifying pain of paying and perceived convenience as the primary psychological mechanisms driving payment method preferences. Through parallel hypothesis testing, these two factors emerged as significant mediators, while responsibility perceptions and self-control did not mediate the experimental effect despite being correlated with payment preferences.

Notably, our responsibility findings differed from Study 3's exploratory results. While Study 3 found that the manipulation affected feelings of irresponsibility towards spending on credit, Study 4's confirmatory test revealed no significant effect of the manipulation on the same measure (despite a strong correlation with payment preferences, $r = .47$). This inconsistency leaves open the possibility that that responsibility concerns influence payment preferences, but likely not in the straightforward manner our studies assumed.

The serial mediation analysis demonstrates that these psychological mechanisms not only shape payment preferences but also indirectly influence decisions to repay debt, providing a complete causal pathway from experimental manipulation to co-holding behavior.

3.6 Study 5

Studies 3 and 4 established that pro-credit messaging increases debt repayment and identified the psychological mechanisms driving this effect. Study 5 explores an important robustness test: do the same patterns hold as available liquidity increases?

Our theoretical framework suggests that payment method preferences may matter most when consumers face trade-offs between maintaining cash for routine spending and paying off debt. When consumers have ample financial resources, they can both pay down debt and maintain substantial cash reserves, potentially reducing the relevance of payment method

preferences for debt repayment decisions. However, it is unclear whether and how consumer's feelings of having enough map onto their actual liquidity. Understanding this boundary condition is particularly important given that many co-holders maintain substantial liquid assets well beyond their debt levels. If payment method preferences only matter when people are liquidity constrained, the theory would have limited explanatory power for the broader co-holding phenomenon.

To examine the robustness of the patterns we have observed thus far, Study 5 manipulates both payment method messaging and liquidity levels. If payment method preferences drive debt repayment because of the economic need to maintain cash reserves for routine spending (e.g., to avoid overdraft fees), we should observe weaker manipulation effects on debt repayment when liquidity is abundant. In contrast, if payment method preferences shape debt repayment decisions through other channels such as increasing the perceived psychological need to hold money in cash without mapping directly onto the economic need, the manipulation effects may persist regardless of liquidity levels.

This study was pre-registered on AsPredicted.org (aspredicted.org/n77t-km43.pdf)

3.6.1 Methods

Participants. We recruited 1,242 participants via Prolific; 196 were excluded based on our pre-registered criteria, resulting in a final sample of 1,046 participants (528 Male, 505 Female, 13 Self-Identify; age: M = 40.4, SD = 13.6; 63.9% White, 13.9% Black, 8.9% Latin American, 13.4% all others).

Design. We used the same procedure as Study 4. In this study, participants who were not screened out were assigned to one of four conditions in a 2 (Pro-Credit Card vs. Control) x 2 (Low Liquidity vs. High Liquidity) between-subjects design. The articles assigned were identical to those in Study 3 and 4 (see Appendix B for wording). As before, participants

were asked to summarize the article they read on the page just after the article.

Participants completed the debt repayment task like in Studies 3 and 4, only this time the *ratio* between assets, debt, and average monthly spend varied by the liquidity condition assigned. Participants assigned to the "Low Liquidity" condition saw an asset value of either \$1,250, \$2,750, or \$4,250 (randomly assigned) and the debt value was always \$3,500 *more* than the assets. Participants were told their total monthly spending ranged from \$1,000 to \$1,500 (when asset was assigned to \$1,250); otherwise, \$2,500 to \$3,000 or \$4,000 to \$4,500. In this condition, participants who chose to use all their assets to pay off their debt would have no cash left over.

Participants assigned to the "High Liquidity" condition saw a *debt* value of either \$1,250, \$2,750, or \$4,250 (randomly assigned) and the asset value was always \$7,000 more than the debt.[5] Total monthly spending in this condition encompassed the debt value \$1,000 to \$1,500; \$2,500 to \$3,000; \$4,000 to \$4,500. In this condition, participants who chose to use their assets to pay off the remaining debt balance would have plenty of cash left over.

Notice that co-holding amounts——or the overlapping debt and and asset amounts—were held constant across liquidity conditions. The set of spending ranges was also fixed.

After the debt repayment task, participants reported their payment method preference. Like in Studies 3 and 4, participants indicated their preference for using credit versus debit cards for three expense types on a 5-point scale ranging from "Definitely Credit Card" (-2) to "Definitely Debit Card" ($+2$), with "Indifferent" (0) as the midpoint.

Unlike in Study 4, we did not include measures to assess Pain of Paying, Convenience, Self-Control, or Irresponsibility of Spending on Credit.

The rest of the survey was the same as in Study 4, with one additional manipulation check that asked participants which amount they remembered being higher in the scenario

5. The asymmetry is due to the fact that in a previous survey, reported in Essay 2, participants noted that having more than \$3,000 would leave them uncomfortable while \$7,000 was the minimum in savings many people felt they needed.

they read: "assets (amount in checking)" or "debt (credit card balance)". Participants could select from three options: (i) Assets much higher than debt, (ii) Debt much higher than assets, and (iii) Assets and debt were about the same.

3.6.2 Results

Manipulation Checks. When asked to recall the content of the article, we see approximately the same proportion of correct responses across conditions ($Prop_{Pro-CC} = .863$, $Prop_{Ctl} = .838$, $\chi^2(1) = 1.08$, $p = .298$). 96.8% of participants in the Pro-Credit Card condition referred to credit cards in their summary of the article. In contrast, only 28.8% of participants did so in the Control, $\chi^2(1) = 517.86, p < .001$.

When asked to recall the balances presented in the scenario, we see a meaningful difference between the two conditions. Those in the High Liquidity group were more likely to be correct ($Prop_{High} = .897$) than those in the Low Liquidity group ($Prop_{Low} = .763$), $\chi^2(1) = 32.39, p < .001$.

Co-Holding Status. 56.6% of participants identified as co-holders. This proportion appeared to differ by condition (Control x Low = .619; Pro-CC x Low = .597; Control x High = .559; Pro-CC x High = .493), $\chi^2(3) = 9.91$, $p = .019$.

Payment Method Preference. We test for the effect of each factor on payment method preference using a two-way ANOVA. We see a main effect of the article assigned ($F(1, 1042) = 93.51$, $\eta_p^2 = .082$, $p < .001$), but no effect of liquidity ($F(1, 1042) = 2.17$, $\eta_p^2 = .002$, $p = .141$) even though the debt repayment scenario preceded the payment method measure. We do not find evidence of an interaction ($F(1, 1042) = .49$, $\eta_p^2 = .0005$, $p = .484$). These results are illustrated in Figure 3.5, Panel A.

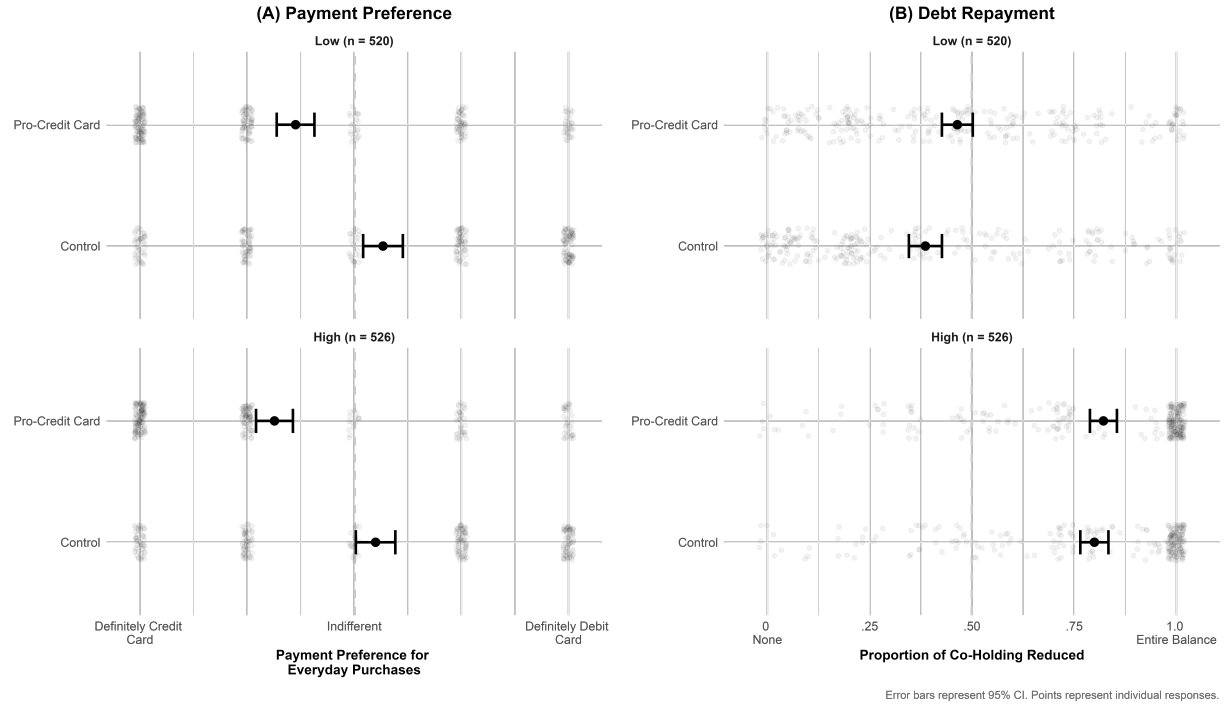


Figure 3.5: Pro-Credit Messaging Affects (A) Payment Preference and (B) Debt Repayment Similarly Across Liquidity Conditions

Debt Repayment. We pre-registered that we would modify the dependent variable to account for the different amount of debt between liquidity conditions. Thus, instead of calculating the proportion of debt repaid (where debt was in the denominator), we calculated the proportion of "co-holding" reduced. The co-holding amount was the intersecting amount, or the minimum value across asset and debt. It was also implied that this was the maximum amount one could repay (in the Low Liquidity condition, this was the maximum amount of assets; in the High Liquidity, this was the maximum amount of debt). By dividing the amount of debt repaid (entered by the participant) by the assigned co-holding amount, the dependent variable remained bounded between 0 and 1.

We tested the effect of each factor on debt repayment using a two-way ANOVA. We observed a main effect of the article assigned ($F(1, 1042) = 8.15$, $\eta_p^2 = .008$, $p = .004$), and liquidity ($F(1, 1042) = 431.95$, $\eta_p^2 = .29$, $p < .001$), but found no effect of an interaction ($F(1, 1042) = 2.28$, $\eta_p^2 = .002$, $p = .131$). These results are illustrated in Figure 3.5, Panel

B. Controlling for whether or not the participant got the manipulation check incorrect does not meaningfully change the pattern of results.

3.6.3 Discussion

Study 5 tested whether the relationship between payment method preferences and debt repayment is robust to liquidity constraints. Our theoretical framework suggested that if payment method preferences drive debt repayment through the need to maintain cash reserves for routine spending, we might observe weaker manipulation effects when liquidity is abundant.

We found that liquidity dramatically affected overall debt repayment (81.1% repayment in high vs. 42.6% in low liquidity conditions). However, the core relationship between payment method preferences and debt repayment persisted even when participants had ample resources both to pay off debt and to maintain cash reserves. Participants exposed to pro-credit messaging consistently repaid more debt regardless of their liquidity constraints.

This finding demonstrates the robustness of the payment method preference effect established in Studies 3 and 4. The consistent experimental effects across three independent studies provide strong evidence for the causal relationship between payment method preferences and co-holding behavior. These results are particularly noteworthy given that many real-world co-holders maintain substantial liquid assets, suggesting that payment method preferences can play an important role in co-holding behavior even when financial constraints are minimal.

3.7 General Discussion

In this research, we examine why consumers simultaneously maintain low-yield liquid assets while carrying high-interest credit card debt. Across five studies, we consistently find that a preference for using debit cards for everyday expenses while reserving credit cards

for larger, unexpected expenses, plays a significant role in explaining this puzzling behavior. Studies 1 and 2 established that co-holders disproportionately prefer and use debit cards for everyday transactions compared to non-co-holders, based on both their stated preferences and actual spending patterns in banking transaction data. Study 3 provided experimental evidence that manipulating perceptions about payment methods affects both stated preferences and hypothetical debt repayment decisions. Study 4 identified specific psychological mechanisms—pain of paying and perceived convenience—that drive payment preferences, offering insights into the underlying psychology. Finally, Study 5 showed that while overall liquidity affects debt repayment, it does not alter the fundamental relationship between payment preferences and co-holding. Together, these findings suggest that co-holding emerges and persists partly because of systematic preferences for how consumers pay for different types of expenses.

Our empirical work reveals several key insights about the psychology underlying co-holding. Most notably, we demonstrate that co-holders systematically prefer debit cards for everyday expenses while reserving credit cards for larger, unexpected expenses, paying interest costs to maintain this strategy. Furthermore, we find a result that appears counterintuitive but follows from our theoretical framework: experimentally shifting consumers' preferences toward credit card use for everyday expenses increases their willingness to repay outstanding debt. This effect proves robust even when consumers have ample liquidity, suggesting that payment method preferences influence financial decisions beyond simple cash constraints.

By linking static balances to dynamic spending, we bridge an important gap in the co-holding literature. Previous research has typically defined co-holding by end-of-month balances (for an approach more similar to ours see Gathergood and Olafsson, 2024). Our approach broadens this understanding by analyzing millions of transactions, revealing that co-holders differ not only by their balances but, but also by their spending patterns. They

engage in both co-holding and *co-spending*, maintaining debt and assets while actively using both payment methods for different expense types. Furthermore, these transaction-level preferences are not arbitrary payment choices but rather are driven by specific psychological mechanisms, pain of paying and perceived convenience. The strong relationship we find between payment preferences and co-holding suggests that to understand this phenomenon fully requires considering these spending patterns and their psychological drivers.

This paper broadens the conversation on the psychology of payment methods and spending to consider the effects on saving and borrowing. Previous research on payment methods has primarily focused on how different payment forms affect purchase likelihood, spending amounts, or product choices (Prelec and Simester, 2001; Soman, 2001; Thomas, Desai, and Seenivasan, 2011). Our research demonstrates that payment preferences can have broader implications for overall financial management, including decisions about repaying debt and allocating assets. The strong association between payment method preferences and co-holding behavior suggests that the psychology shaping spending decisions may affect decisions in other financial domains. By connecting these previously separate research streams, we provide a more integrated understanding of consumer financial behavior that spans spending, saving, and borrowing decisions.

Our findings complement existing economic explanations for co-holding without displacing them. Previous research has proposed various explanations including precautionary savings (Fulford, 2015), liquidity constraints (Telyukova, 2013), and self-control mechanisms (Bertaut, Haliassos, and Reiter, 2009). Our payment method preference account provides an additional psychological mechanism that likely operates alongside some of these economic factors.

Our setting allows us to evaluate several of these alternatives systematically. For instance, to address concerns about access to credit, we control for credit limit fluctuations in our transaction analysis (Study 2). We also focus our transaction analysis on card-based

spending that excludes cash-only merchants, speaking to liquidity constraint explanations.[6] For self-control mechanisms, we find experimentally that self-control does not mediate payment method preferences (Study 4).Additionally, in our field setting, customers had access to a feature that let them set arbitrary limits on their credit spending. While co-holders used this feature more than non-co-holders (48% vs. 28%, $p < .001$), it did not eliminate co-holding altogether. This suggests that while co-holding is associated with self-control differences, self-control tools cannot fully account for the persistence of the behavior.

Our work is most similar to recent findings by Gathergood and Olafsson (2024), who showed that consumers systematically allocate different expense types across overdraft accounts in Iceland. We extend this insight by examining spending across products and eliciting preferences for debit and credit directly, then using controlled experiments to identify specific psychological mechanisms and demonstrate causal effects on debt repayment decisions. Together, this research suggests that payment method mental accounting represents a robust psychological pathway to co-holding across different financial systems and cultural contexts.

3.7.1 Practical Implications

Our findings suggest several actionable approaches for tackling co-holding behavior. If, consistent with our evidence, consumers who prefer using debit for everyday expenses are reluctant to deplete their liquid assets, then changing the behavior requires more than campaigns encouraging debt repayment. Indeed, in Essay 2, we found that directly informing consumers about the opportunity to use their assets to pay down debt (and presenting some of them with the costs) had minimal impact. Rather than focusing solely on the economics, interventions might be more effective if they target underlying beliefs about payment methods.

Co-holders tend to be financially literate (Gathergood and Weber, 2014) and may con-

6. Though we exclude cash withdrawals from the transaction analyses, we checked for difference in withdrawals between CHs and NCHs and observed no meaningful difference.

tinue co-holding despite knowing its costs (Essay 2). Nevertheless, our experimental evidence (Studies 3-5) reveals a surprising result: messaging that promotes credit card use for everyday expenses can *increase* debt repayment. However, promoting credit use could have unintended consequences and put some consumers at risk. This insight must, therefore, be rigorously tested in the field before scaling.

These lessons should also leave us alert to the consequences of *discouraging* credit use (see Choi, 2022), as these may inadvertently *reduce* debt repayment. Policymakers and firms concerned about consumer welfare should consider these psychological factors underlying co-holding when designing interventions that work with, rather than against, consumer preferences.

Beyond messaging interventions, our work points to new products that firms can design to better align with consumers' spending preferences. The Australian mortgage market offers an instructive model through "offset accounts" that allow consumers to reduce home loan interest costs while maintaining liquid balances. Similar hybrid products could be developed for revolving credit; for instance, checking accounts that "offset" credit card balances at reduced interest rates. Such products could preserve the psychological benefits consumers derive from their payment preferences while reducing the financial costs of co-holding. It is also worth considering that from a broader perspective, co-holding may provide genuine economic value. Economic shocks sometimes come in clusters co-hold could help consumers avoid more expensive forms borrowing if they face with consecutive shocks and need access to cash (Desmond, 2023; Farrell, Greig, and Yu, 2019; Morduch and Schneider, 2018). Hybrid products like offset accounts could both insure against these scenarios while reducing the ongoing costs of maintaining this financial buffer. More broadly, firms that offer integrated products designed around consumers' spending preferences could attract and retain customers by incentivizing them to hold multiple products at the same institution.

3.7.2 Limitations and Future Directions

We acknowledge several limitations that suggest directions for future research. First, while our field data provides exceptional ecological validity, the relationship between payment preferences and co-holding in Studies 1 and 2 remains correlational. Although our experimental studies establish causality between payment preferences and debt repayment decisions, they rely on hypothetical scenarios rather than actual financial choices. Future research could implement field experiments that manipulate payment method perceptions and track effects on real co-holding behavior over time.

Second, our field data comes from a single Australian financial institution during a specific time period that included the COVID-19 pandemic, which may limit generalizability. We reran our analyses on only the months preceding the pandemic and find a similar trend as the one reported. Still, replication with data from diverse markets, economic conditions, and time periods would strengthen our conclusions.

Third, our experimental manipulations targeted perceptions about payment methods through several channels rather than addressing the underlying psychological mechanisms directly. This allowed us to explore competing mechanisms and ultimately enabled us to shift preferences, which was core to our thesis. However, there is much we still do not know about the psychology underlying these preferences, which leaves room for additional explanations deserving further study. Most near to the current work is the open question of how payment method preferences form. In our studies, they are correlated with several beliefs including perceptions about convenience, pain of payment, and responsibility. And pain of paying and perceived convenience both accounted for the changes we observed in payment preferences. However, they didn't fully explain it and other results were mixed. Notably, responsibility perceptions showed inconsistent results across studies—emerging as significant in Study 3's exploratory analysis but not replicating in Study 4's confirmatory test. This inconsistency highlights the need for future research to clarify the role of responsibility beliefs in payment

method preferences. This contrasts with convenience and pain of paying, which appear more context-dependent and responsive to intervention. Future research might investigate how these preferences develop over time, how they are shaped by early financial experiences, and the extent to which they reflect stable traits versus malleable attitudes. Understanding the formation of these preferences and developing interventions that target the more malleable mechanisms directly could provide additional insights into how to reduce financially costly co-holding without undermining the psychological benefits consumers derive from their preferred payment strategies.

This paper introduces payment method preferences as a contributing factor to a common, yet puzzling, consumer behavior. Throughout the paper, we have argued that consumers' everyday payment choices—particularly preference to use debit for everyday expenses—play a central role in the emergence and persistence of co-holding. These findings suggest that co-holding is not simply a state that consumers find themselves in, but the result of a broader strategy in how consumers manage their spending. A broader perspective of the phenomenon of co-holding offers new opportunities for interventions and a more complete understanding of how consumers navigate their financial lives.

APPENDIX A

SUPPLEMENT TO ESSAY 2

A.1 Supplement to Study 1

A.1.1 Definitions

- ***Main Financial Institution (MFI)***: This classification is created and used by the bank to identify individuals for whom there is broad coverage of their day-to-day financial decisions. The classification considers a combination of *inflows* (e.g., deposits), *transfers* (between one's accounts within the bank), and *outflows* (e.g., cash withdrawals; purchases). This is similar to other measures used in the literature, which typically focus on outflows (Ganong and Noel, 2019, p. 2389).

- ***Co-Holding***: Refers to the act of maintaining $500 or more in high-interest revolving credit card debt (i.e., portion of balance that incurs interest) *and* $500 or more across low-yield liquid asset accounts (i.e., amount in savings plus amount in checking or transaction accounts(s)). Throughout the paper we default to treating co-holding as a binary measure—either a customer is or is not co-holding at least $500. We set the threshold to $500 in line with past work (Telyukova, 2013); this amount also happens to coincide with approximately one week's worth of expenses for the typical co-holder in our sample. When referring explicitly to co-holding *amounts*, we calculate the amount as the minimum, or intersecting, balance between one's total revolving debt balance and the sum of their liquid assets (Gathergood and Weber, 2014; Gorbachev and Luengo-Prado, 2019). Technical details provided below.

- ***Co-Holder*** (vs. Non Co-Holder): Customers who we observe to be co-holding $500 or more for three consecutive billing periods. For the field experiment, we identify Co-Holders in January 2020. For the comparative analysis, we identified Co-Holders in

August, September, and October 2021 and grouped them together. Individuals selected for the Field Experiment in 2020 were also included in this group. Note that we use another definition for the descriptive survey below, based on self-reported information.

- ***Coefficient of Variation (CV)***: A measure of account activity calculated by dividing the standard deviation of account balances over the prior twelve months (including the current month) by the mean balance over the same period (Farrell, Greig, and Yu, 2019). Formally,

$$CV = SD(Y_{i,m-11,j}, Y_{i,m-10,j}, ..., Y_{i,m,j}) / AVG(Y_{i,m-11,j}, Y_{i,m-10,j}, ..., Y_{i,m,j})$$

where $Y_{i,m,j}$ represents the balance for customer i in month m for account type j. This measure allows us to compare the relative variability of different account types and distinguish between actively used accounts versus those that remain relatively static (Farrell, Greig, and Yu, 2019; Morduch and Schneider, 2018).

A.1.2 Calculating Co-Holding Amounts

We quantify co-holding as the overlapping value between revolving debt and liquid assets, calculated as the minimum of these two balances (see also Table 2, "Definitions" in Gathergood and Weber, 2014). This approach captures the maximum amount a customer could immediately transfer from assets to reduce their interest-bearing debt.

For customers who maintain higher asset balances than outstanding debt ("Net Savers"), the co-holding amount equals their total revolving debt, since they possess sufficient liquidity to eliminate their entire credit card balance. Conversely, for customers with greater debt than assets ("Net Borrowers"), the co-holding amount corresponds to their total liquid asset balance, representing the maximum debt reduction they could achieve through immediate transfer.

As an example, a customer with $1,000 in assets and $700 in revolving debt would have a co-holding amount of $700 (limited by their debt balance). In contrast, a customer with $800 in assets and $1,200 in debt would have a co-holding amount of $800 (limited by their available liquidity).

For our analysis of banking data (n = 2,307), we calculated monthly co-holding amounts by pairing customers' interest-bearing credit card balances at statement due dates with their corresponding liquid asset balances (transaction and savings accounts) on the same date. This methodology specifically captures debt that incurs interest charges, excluding any within-period debt that was repaid before interest accrued.

For customers with multiple credit cards, we summed interest-bearing balances across all cards within each month and paired this total with the average daily liquid asset balance calculated between the earliest and latest credit card due dates in that month. We tested this methodology against alternative approaches, such as using only the balance from the card with the earliest due date or the latest due date, and found strong correlations across methods ($r > .97$, $p < .001$). For customers holding a single credit card, which represents the median customer in our sample, the two approaches yield identical results.

A.1.3 Detailed Banking Data Sample Selection

Individual-level banking data for the comparative analysis was provided for individuals who participated in a survey on payment method preferences (Essay 3). We invited approximately 950,000 eligible customers to participate in a brief survey in October 2021. This pool included 356,569 customers who were co-holding at the time of the survey, 125,328 customers from the field experiment (regardless of their co-holding status in October 2021), and 546,077 non-co-holders who met the remaining eligibility criteria.

Of those invited, 2,307 customers completed the survey and consented to have their responses linked to their banking data, comprising 979 co-holders and 1,328 non-co-holders.

For these customers, we obtained comprehensive banking records including transaction-level variables (debits, credits, and charges across checking, savings, and credit card accounts), daily account balances, monthly statement summaries (average balances, credit limits, interest rates, and statement amounts), and demographic information captured in January 2020.

The demographic snapshot included marital status, estimated annual income, number of personal loans and home loans held, tenure with the bank, age band, gender, and geographic location. This detailed dataset spans January 2019 through February 2022, enabling analysis of co-holding persistence, transaction patterns, and behavioral differences between co-holders and non-co-holders.

Sample Representativeness. While this sample was not randomly selected from the overall customer base, it provides a common base to compare co-holders and non-co-holders within the same institutional and regulatory environment and who shared various characteristics (e.g., credit card access, liquid asset accounts, digital activity). We used the same eligibility criteria for our survey and for the field experiment.

A.2 Supplement to Study 2

A.2.1 Materials

Pre-Registration and Power Analysis

This experiment was pre-registered with the AEA RCT Registry (doi.org/10.1257/rct.5751). Once the experiment period ended, but before selecting our outcome variables of interest, we requested that the bank give us access to the data for only the control group. Using this data, we conducted a power analysis to determine the minimum detectable effects (MDEs) with $\alpha = .05$ and power of $1-\beta = .80$. We therefore submitted two versions of our pre-registration. The first version, before any data was received, outlined the experimental details and noted

the update that would follow once we conducted the power analysis (Banerjee et al., 2020; Olken, 2015). The second version superseded the first, specifying the variables of interest and the respective MDEs (Table A.2). Only after updating the pre-registration with the results of the power analysis did we receive the data for the notification groups.

In the pre-registration we also included a set of engagement indicators, such as which customers saw or interacted with the message. We were unable to conduct a power analysis for these measures because the control group had no message to engage with. These are excluded from the table below.

A.2.2 Methods

Sample

125,328 customers were selected to be part of this experiment based on a set of pre-specified criteria listed in B.1. All of these customers were co-holding as of January 2020 and had been for at least three statement cycles prior.

Our analysis consisted of 124,251 customers. 1,077 (0.09% of the original sample) were dropped from the data set due to attrition beyond our control. These missing customers were evenly distributed between conditions (Control: 352; Notice Only: 375; Notice + Cost: 350). We suspect that the original list was further truncated based on internal policies in the process of launching. It is also possible that some of these customers closed their accounts before the data was shared with us.

Treatment Design

In designing the different treatments, we sought to test two hypotheses:

Hypothesis 1. Co-holders are unaware that they are co-holding.

Hypothesis 2. Co-holders are unaware of the costs of co-holding.

Consumers have limited attention (Gabaix, 2019; Simon, 1955; Weber and Johnson, 2009) and may have overlooked the amount of liquid assets they had when deciding how much of their credit card bill to pay. Furthermore, the banking app allowed customers to "hide" accounts, which would disable the app from displaying information about that account. Anecdotal evidence suggests that some customers would use this feature to facilitate saving, at times combining it with automatic deposits to facilitate a savings strategy of "set and forget". It is also plausible that some customers knew they were co-holding but did not know the cost of doing so.

Treatment Assignment

To test these hypotheses, we randomized the participants into one of three messaging groups. Groups were balanced on key variables — gender, age, tenure with bank, account tenure, credit card type, and whether or not individuals held a home loan or personal loan with the bank — using an anticlustering algorithm (Papenberg and Klau, 2021) (for balance table, see Table A.3). The *Control* group received no notification about their co-holding behavior. The *Notice* group received an in-app notification reading: "You usually have at least $500 available to pay your credit card debt. Every $100 you pay off could reduce your interest charges." The *Notice + Cost* group received an in-app notification in which the first sentence was identical to the Notice condition but included an additional sentence that read: "... Every $100 you pay off could reduce your interest charges by about $20 per year. Saving $100 only earns about $1 in interest per year..." The notifications all included a disclosure statement for regulatory purposes.

Implementation Procedure

The experiment went live to approximately 1% of the eligible sample in early February 2020 before being scaled to the remaining 99% on 17 February 2020. Once the experiment was

live, customers in the two treatment groups were shown a notification "card" within their banking app. Customers were familiar with notifications like these in the app, though the content itself could range from promotional offers to reminders to pay an upcoming bill. Customers could have multiple cards available to scroll through at any point in time. A targeting algorithm determined the order in which they appeared, so some customers may have needed to scroll through other cards to see our notification. However, in planning the experiment, we worked with our partners to ensure that none of the other campaigns released during this period explicitly targeted customers in such a way that it affected one of our conditions but not the other.

Upon seeing a card, customers could choose to "expand" the card or not. If a customer expanded the card, the full message would be displayed. The customers could then choose to (i) close the card by clicking "X", (ii) "Dismiss" the card, or (iii) click to "Pay Now". Clicking on the "Pay Now" button would take customers directly to an existing transfer page in the app, where customers could choose how much money they wanted to transfer from their assets to their credit card balance. For customers who did not expand the message or who clicked the X after expanding it, the notification card would reappear once more on a later login during the duration of the experiment. Customers in the notification group could be exposed to the message up to two times during the month the experiment was live.

Dependent Variables

Our primary outcome of interest was the amount customers paid toward their credit card balance. The study was powered to detect a minimum effect of AUD$39 difference in repayment, or approximately 3% difference in either direction from the control group's mean repayment amount of AUD$1,361 (more details on the power analysis are provided above).

We also specified a series of secondary measures for which we thought we could reasonably detect changes. For example, if a co-holder decided to use their savings to pay off more debt,

we would expect to see both an increase in the amount they repaid and a decrease in their savings balance. To examine these changes, we tested for differences in (i) savings balances; (ii) interest earned from savings accounts; and (iii) the proportion of customers in each group that had a balance higher than their average balance in the three months leading up to the intervention. The longer list of these outcome measures, including the minimal detectable effects (MDEs), can be found in Table A.2.

A.2.3 Analysis

Overall Effect of Notifications

Customers in the notification group paid an average AUD$19.63 (95% CI [−AUD$7.86, AUD$47.12], $p = .162$) more than those in the Control group (M = AUD$1,364.65; SD = AUD$2,326.85), but this effect was neither statistically nor economically meaningful. The estimate remains essentially unchanged when we adjust for demographics and pre-treatment controls (see Table A.4). However, this is expected considering we used most of these same variables in randomizing the treatment assignment. Figure A.2 provides a further breakdown of the treatment effect on repayment amount by subgroups, including gender, age, total net balance, and app logins.

Regressing repayment amount on an indicator of whether the customer received a notice (without cost; i.e., excluding the Notice + Cost group), we see that simply making the co-holding behavior visible had a marginal effect on the amount repaid, β = AUD$27.74 (95% CI [−AUD$4.24, AUD$59.72], $p = .089$). Comparing the Control to the Notice + Cost group alone results in no detectable difference, β = AUD$11.53, 95% CI [−AUD$20.03, AUD$43.10], $p = .474$]. There is also no noticeable difference between the two messaging groups (following a similar model specification where Notice is the reference group, with β = −AUD$16.21 (95% CI [−AUD$48.06, AUD$16.64], $p = .319$)).

Secondary Outcomes

In addition to the total amount repaid, we also tested for a series of pre-registered, secondary measures, including other aspects of credit card repayment, credit card borrowing, savings, and net financial benefit, as shown in Figure A.3.[1] We did not see any difference between the two notification conditions on any of these measures ($ps > .310$), so we pooled the two conditions and report only the effects of receiving a notification.

The messages significantly increased the likelihood of customers paying more than their own previous average and more than the contractual minimum while also reducing the likelihood of ending the billing period with a higher balance. However, these differences did not affect the total credit card balance or interest charges. There was also no evidence that the intervention affected customers' savings balances or whether they paid the full balance. Lastly, the study examined whether customers fared better on net earnings (savings interest earned - credit card interest charges), but the estimated effect was small and not statistically significant. In general, these findings highlight the limited impact of the intervention on various aspects of credit card repayment and savings behavior.

Engagement with Notifications.

To gauge the extent to which customers interacted with the messages presented to them, we examined the digital tags associated with each action in the app. Most customers in the notification groups were exposed to the message. The notification was displayed in the banking app for approximately 83% of the customers in the notification groups. 35% of customers in the notification groups (42% of customers for whom a notification was displayed)

1. In reporting these analyses, we use p-values adjusted to account for multiple outcomes within a domain (e.g., Finkelstein et al., 2012). For example, given three savings-related outcomes and one net benefit outcome, we would adjust the p-value for the savings outcomes but not the net benefit outcome. We calculate the family-wise, or per domain, error rate adjusted p-values using 100,000 permutations of the free step-down resampling method (Westfall and Young, 1993); see also (Anderson, 2008) for more details and (Finkelstein et al., 2012) for application. In our pre-registration, we list the outcomes and specify the domains they each belong to. Standard errors are clustered at the individual level.

expanded the message to see its content. 5% of the customers in the notification groups (14% of those who expanded the message) clicked on the "Pay Now" button, which redirected them to a screen where they could transfer money from one account to the other. While these click-through rates indicate that customers assigned to the notification conditions went on to engage with the message, we have no equivalent digital tags for customers in the control group. Therefore, our ability to analyze the effects of the treatment on treated customers is limited.

Matched-Pair Analysis

As a robustness test—and to address the limitation that control group customers had no notifications to engage with—we employed a matching strategy to compare customers in the treatment groups who engaged with the messages with similar Control customers. We started with the sample of treated customers, who clicked "Pay Now" exactly once and had exactly one active credit card with a payment due between 2 to 30 days after they clicked the Pay Now button. To ensure a like-for-like comparison, we randomly selected one customer from the control group who (i) had a credit card due on the same date; (ii) whose outstanding balance was approximately the same at the end of the statement period (binned intervals of $1,000); (iii) whose total savings balance on the the due date was within the same $1,000 interval; and (iv) whom we could see logged into the app ±7 days of the treated customer. This last criterion was meant to account for cases where treated customers who clicked "Pay Now" had logged in intending to make a payment anyway. It also resembles an attention proxy used in past work (Gathergood and Olafsson, 2024). In the end, we had 4,324 customers (Notification: 2,162; Control: 2,162).

Following the same specification that we used for the main analysis, regressing the amount of debt repaid on an indicator for treatment without additional controls, we again do not see evidence of a difference (β = AUD$41.78, SE = AUD$72.48, p = .56). See Table A.5

(Column 3) below.

Frequency of Payments

Having found no difference in the total amount repaid, we turned to a shorter-term outcome: the number of repayments made in the month of the experiment. We leverage the random assignment of the experiment to estimate a causal effect of being sent a notification and the frequencies of payments a customer made towards their outstanding debt. Using a non-parametric Wilcoxon Mann-Whitney test, we find evidence of a difference. Those assigned to the notification group (Mdn = 2, M = 3.21, SD = 2.83) made more repayments in the month of the experiment than those to the Control (Mdn = 2; M = 3.16; SD = 2.86), $Z = 4.48$, $p < .001$. The effect is even more pronounced when we compare the differences in the matched sample (Treated: Mdn = 3, M = 3.21, SD = 2.44; Matched Control: Mdn = 2; M = 2.98; SD = 2.68; $Z = 6.34$, $p < .001$, see also Figure A.4 in the Web Appendix).

A.3 Supplement to Study 3

A.3.1 Materials

Survey Design

The survey consisted of two parts: an initial set of questions that served to both screen participants for the full survey and classify them as "Co-Holders" or "Non Co-Holders", followed by a longer set of questions.

Screening. The initial screening collected information on the number of savings, checking, and credit card accounts each participant had. To gather the amounts within these accounts, participants were instructed to login to their primary bank account (through a separate tab) and report the respective balances. In addition, participants reported how much physical

cash they had. Finally, participants with at least one credit card reported the interest rate for their primary card, whether they *typically* repay their outstanding balance in full, and whether *last month* they paid their balance in full.

Based on the responses, participants were classified as either "Co-Holder" or "Non Co-Holder". Co-Holders were defined as participants who: (i) had a credit card interest rate above 5%, (ii) did not pay their most recent credit card bill in full, (iii) carried credit card balances of $500 or more, and (iv) held combined checking, savings, and cash balances of $500 or more. Non Co-Holders included participants who owned at least one credit card and either a checking or savings account or both but did not meet one or more of the CH criteria.

CHs or NCHs had the option to proceed to a more detailed survey, which paid an additional US$2.60. Most chose to proceed ($\text{Prop}_{\text{CH}} = .96$; $\text{Prop}_{\text{NCH}} = .93$; $p = .15$). Participants failing the initial attention check, those without credit cards, those without either a savings or checking account, and those who were co-holding balances between $0 and $499 were ineligible to proceed and excluded from the analysis.

Financial Decision-Making. Participants proceeding with the survey answered various individual-level questions (see Appendix for more details; Qualtrics file is available for download on ResearchBox).

Most of the questions were set up as agree-disagree questions, with five response options ranging from "Strongly Disagree" to "Strongly Agree". To measure self-control, we used two items previously used (Gathergood and Weber, 2014) — impulsivity and propensity to plan. To measure risk and time preferences, we modified the single-item questions developed in (Falk et al., 2023). We measured debt aversion using an index of previously used items (Albrecht and Meissner, 2022; Callender and Jackson, 2005; Sharma, Tully, and Cryder, 2021). We included six questions to measure how (ir)responsible participants thought it was to keep *less than* $500 in liquid assets and, separately, to consistently carry *more than* $500 in credit card debt (these were adapted from measures used in Sussman and O'Brien,

2016). Psychological ownership of borrowed money was measured using the three-item scale (Sharma, Tully, and Cryder, 2021). We included one measure to capture a need for cash, "I like to keep some money in liquid assets (in checking, savings, cash) for situations where my credit card may not be accepted." To measure wanting to feel secure, "I feel more secure having liquid assets available (money in checking, savings, cash), even if I have no immediate use for it." To assess whether participants were worried about a credit limit decrease we presented them with, "I worry that my credit card provider may reduce my credit limit without much notice." To assess the extent to which surcharges affect one's payment method, "never use a credit card when a business charges extra for using it." To measure participants' mental accounting strategies we used two items, "I generally use my credit card for certain kinds of purchases and my cash and savings for other kinds of purchases." and "I don't need to keep money in cash or savings because I can always use my credit card."

Beyond the agree-disagree questions, we included various questions to assess liquidity constraints, including a spare money item (Berman et al., 2016, Study 3) and four questions asking them how confident they were they could come up with $1,000 [$1,5000; $2,000; $2,500]. To explore whether debit cards or credit cards were perceived as better products for managing money, participants were asked to select whether debit or credit "Helps you better keep track of where your money is going"; "Makes it easier for you to manage your spending"; "Helps you stay within your budget more effectively"; "Leads to more disciplined spending". Three questions asked about the most important reasons for using (i) a debit card; (ii) a credit card; and (iii) for keeping money in cash or in a bank account.

To understand participants' payment preferences, we asked them how they would pay for three general types of expenses: "An everyday purchase"; "A large expense that you WERE planning for"; "A large expense that you WERE NOT expecting". Response options included four items from "Definitely Credit Card" to "Definitely Cash / Savings" plus a fifth option for "Another Payment Option (e.g., AfterPay)".

We also asked four "threshold" questions, where participants were asked to input a numeric response for (i) the maximum amount of debt they would be willing to take on before they started to get uneasy; (ii) the minimum amount of liquid assets they need in their accounts before getting uncomfortable; (iii) the amount of money they generally like to have available; and (iv) the amount of credit they prefer to have available, which they could borrow if needed.

Finally, co-holders specifically, were told that from their earlier responses, it seemed they had money saved up while also having unpaid credit card debt. To highlight the discrepancy, we borrowed language from the field experiment notifications to tell them the interest they were paying on their credit card debt was probably more than what they were earning on their savings, e.g., "you could pay $15-$20 in interest for every $100 you owe each year, while you only earn about $1-$5 for every $100 you save. By using some of your savings to pay down your credit card debt, you could potentially reduce the amount of interest you pay overall." On the same page, we then asked whether, before starting this survey, they knew they could be saving money by using the cash they had to pay down their debt. Response options included "Yes", "No", and "It's Complicated".

Open-Ended Responses. To gather perspectives on co-holding, all participants were asked to give advice to an imagined person who had some amount of money in savings *and* some amount of outstanding debt. The values were paired in such a way that this imagined person carried either a net positive (Assets: $4,000, Debt: $2,000) or net negative (Assets: $2,000, Debt: $4,000) balance, yet, were always co-holding at least $2,000.

The second open-ended question was presented only to CHs, asking them directly "why do you choose to keep money saved up while also having some credit card debt?". For CHs who reported being unaware that they could be saving money, a third open-ended question asked "Now that you know you could save money by using the cash you have to pay down your debt, is there anything you might do differently?"

A.3.2 Additional Results

Given the exploratory purpose of this survey, The results from this survey are summarized in Table A.6. We highlight some of the key insights below:

Self-Control

While both CHs and NCHs tended to disagree with the statements provided, indicating they were neither impulsive nor ill prepared for the future, CHs tended to be higher on both items than NCHs (impulsivity, $p < .001$; propensity to plan, $p = .002$). This is consistent with the findings from (Gathergood and Weber, 2014).

Debt Aversion

CHs were *less* debt averse than NCHs ($p < .001$). This also appeared in the numeric response item that asked how much debt they would be comfortable taking on; CHs (M = \$4,101; SD = \$3,082) reported higher thresholds than NCHs (M = \$2,252; SD = \$3,211), $p < .001$.

Sense of (Ir)responsibility

CHs had noticeably different beliefs about revolving debt, finding it less irresponsible than NCHs, $p < .001$. However, in considering assets, there was no detectable difference in beliefs, $p = .34$. Beyond a economic reasoning, these results suggest there may also be a moral or identity-based component driving decisions to (not) co-hold.

Psychological Ownership of Borrowed Money

CHs tended to feel *greater* ownership over borrowed money ($p = .007$).

Credit Limit Decrease

Neither CHs or NCHs reported being worried about their credit card provider reducing their credit limit unexpectedly, but NCHs seemed even less worried than CHs ($p = .003$).

Need for Cash and Feeling of Security

Both CHs and NCHs agreed that it is important to keep some money in liquid assets for situations where credit cards might not be accepted ($p = .83$) and that having access to cash gives them a sense of security ($p = .61$).

Liquidity

Across measures, CHs were more constrained ($ps \leq .001$; $p_{Adj} \leq .002$). Only on the question of whether they would be able to come up with $500 was the evidence weaker, although still largely consistent ($p = .08$; $p_{Adj} = .08$).

Debit (vs. Credit) as Money Management Tool

While both CHs and NCHs generally thought debit cards were better for managing money, CHs were more consistent in their responses — *keeping track* of where one's money is going (CH: .79; NCH: .63; $p_{Adj} = .006$); *managing one's spending* (CH: .81; NCH: .67; $p_{Adj} = .006$); *staying within one's budget* (CH: .86; NCH: .75; $p_{Adj} = .016$); and being more *disciplined* about spending (CH: .86; NCH: .76; $p_{Adj} = .019$)

Mental Accounting

CHs were more likely to agree with the statement about segregating payments between debit and credit ($p = .027$). Yet, both CHs and NCHs rejected the idea that keeping cash on hand was unimportant when one had a credit card ($p = .066$).

Payment Preferences

When asked how they would pay for small, everyday expenses and large, anticipated expenses, CHs said they would use their debit cards and NCHs said they would use their credit cards ($ps < .001$). However, both CHs and NCHs reported a preference for credit cards when faced with a large, surprise expense ($p = .64$).

Small Sooner vs. Larger Later

As an exploratory question to gauge the importance of keeping a minimal amount on hand, we provided participants with two hypothetical choices for paying an outstanding bill (shown randomly within-participants). In both choices, participants could choose to pay $600 today or $690 in one year. However, in the "Low Liquidity" condition participants were told they had $900 in assets and the "High Liquidity" condition they had $2900 in assets.

CHs (Prop = .48) were significantly more likely to choose to defer the payment in the Low Liquidity case than NCHs (Prop = .31; $p_{Adj} = .006$). But no more likely to do so in the High Liquidity case ($\text{Prop}_{\text{CH}} = .13$; $\text{Prop}_{\text{NCH}} = .07$; $p_{Adj} = .12$). The weaker evidence of a difference in choice suggests CHs and NCHs may reason about money in similar ways, once a basic need is met.

Reasons People Co-Hold

When CHs were asked explicitly if they knew they could be saving money by using their assets to pay down their debt, 76% of them responded "Yes". Only 7% said they did not know. However, reading through the open-ended responses asking whether any of these latter participants planned to do anything differently, all provided reasons for why they would continue to co-hold.

When asked directly *why* they co-hold, CHs provided even richer insights. Consistent with the results of the field experiment, co-holding for these participants did not arise out

of negligence or oversight. Rather, nearly every participant provided a thoughtful responses that, even when misguided or contradictory, nevertheless, provided an account for how they understood their own decisions (for a discussion on interpreting open-ended self-reports see Gerson and Damaske, 2020).

Many accounts reflected a need for liquid assets to cover emergencies, e.g. "... I try to balance paying down debt while still providing a layer of protection in case of emergency". In fact, CHs appeared especially aware of the costs of debt and anticipated a worse situation still if only had access to credit at the next emergency, e.g., "It makes more sense to me to have money in savings to cover unexpected expenses which I seem to have a lot of. It is more stressful to me to have debt go up and down."

Having cash also left people *feeling* safe and participants seemed ready to acknowledge that, "the funds I loose to interest payments are worth it to me to have the peace of mind [that comes with] having a bit of available funds".

A third common theme suggested co-holding, especially the partitioning of funds, facilitated one's ability to manage their money, "I bucket my money and have a different income stream to pay off my credit card debt. I have a strategy... to pay that debt off over the next few months without having to dip into my savings..." In another example, the participant shared how they kept cash on hand to give to their children "to attend get togethers with friends at places that require an admission fee". If they used their savings to pay off their credit card debt, they'd have nothing, admitting that "we are living beyond our means in the home that we own... we scrape by and use credit cards to make ends meet... We are one major incident away from bankruptcy. Its daunting and stressful and causes many sleepless nights."

Other themes mentioned needing cash on hand for a side business or to steady an unstable income. Some other participants noted how the cash they reported was temporary, earmarked for rent or utilities, and expected to be gone within the week.

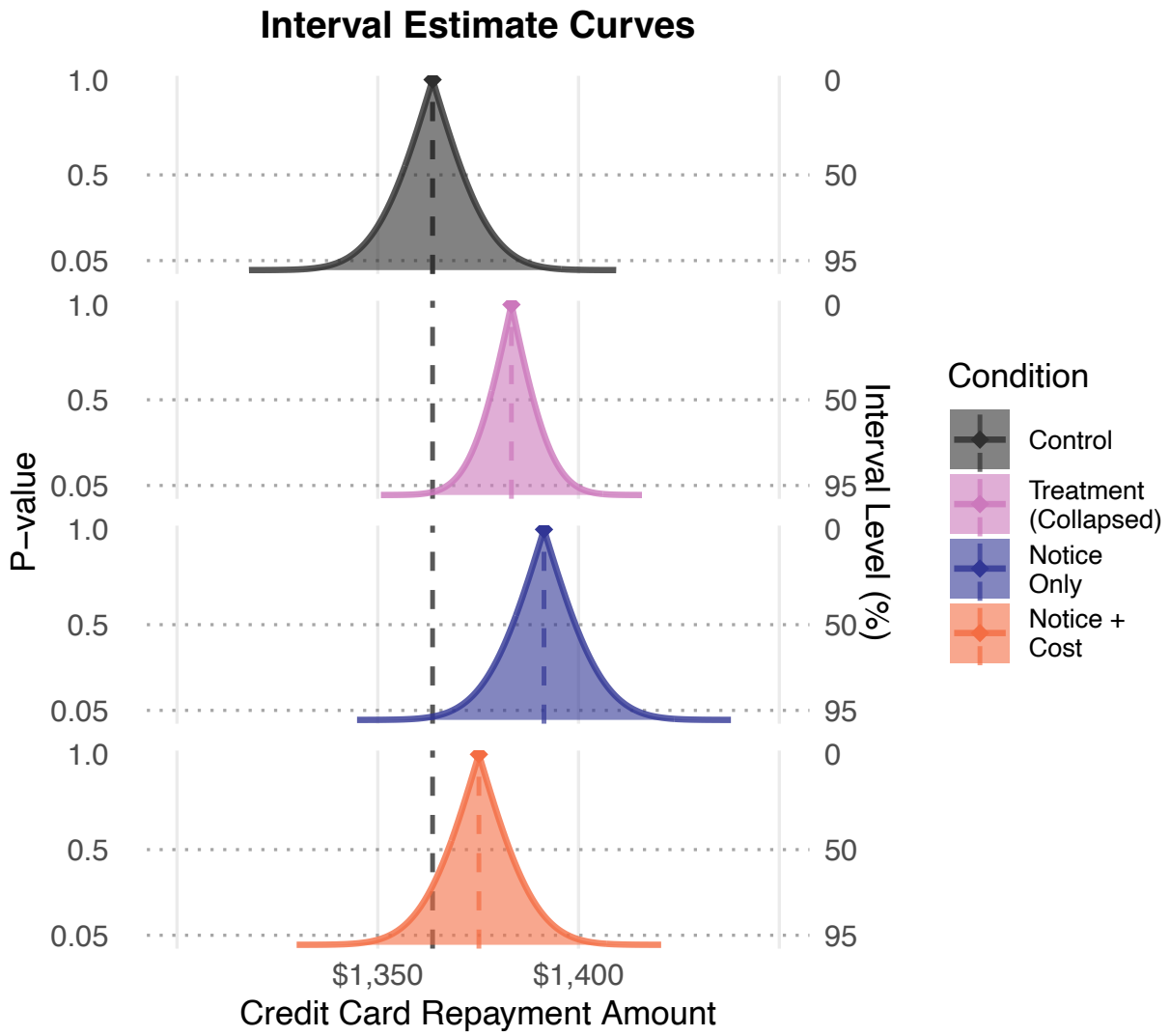


Figure A.1: Field experiment interval estimate curves showing statistical compatibility of credit card repayment interventions. Each panel displays an interval estimate curve for one experimental condition, showing the P-value (left axis) and corresponding interval estimate level (right axis) across potential true credit card repayment amounts McShane et al. (2024). Curves peak at P = 1.0 (observed sample mean) and decrease toward P = 0.0 for effect sizes increasingly incompatible with the observed data. Dashed colored vertical lines indicate observed mean repayment for each condition. Dashed gray vertical lines show the Control condition mean ($1,364) for comparison across all conditions. Horizontal dotted lines mark $p = .05$ (95% interval level) and $p = .50$ (50% interval level) reference points. Sample sizes: Control (N = 41,424), Treatment Collapsed (N = 82,827), Notice Only (N = 41,401), Notice + Cost (N = 41,426).

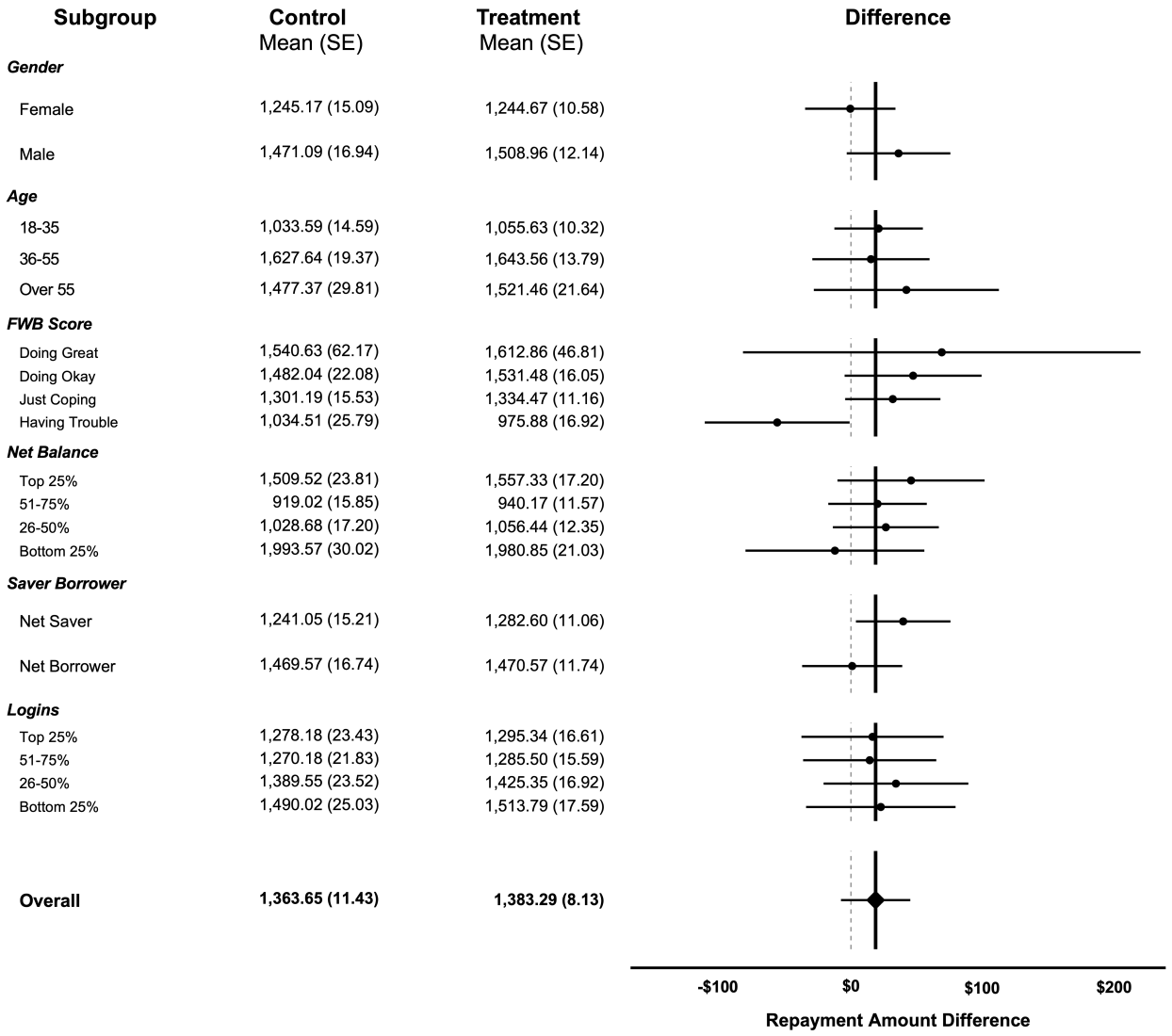


Figure A.2: Subgroup analysis of field experiment treatment effects on credit card repayment amounts. Forest plot displays treatment effects (difference between treatment and control conditions) across participant subgroups defined by demographic characteristics, financial well-being (FWB) scores, account balances, saving behavior, and login frequency. Left columns show mean repayment amounts with standard errors (SE) for control and treatment conditions within each subgroup. Right panel shows treatment effect estimates with 95% confidence intervals represented by horizontal lines. Points indicate point estimates for each subgroup, with the overall treatment effect shown as a diamond at bottom. Dashed vertical line at \$0 indicates no treatment effect. Error bars extending beyond the null line suggest treatment effects distinguishable from zero at the 95% confidence level. Subgroup categories: Gender (Female, Male); Age (18-35, 36-55, Over 55); Financial Well-being scores (Doing Great, Doing Okay, Just Coping, Having Trouble); Net account balance quartiles (Top 25%, 51-75%, 26-50%, Bottom 25%); Saving behavior (Net Saver, Net Borrower); and Login frequency quartiles. Overall sample: Control (N = 41,424), Treatment (Collapsed, N = 82,827).

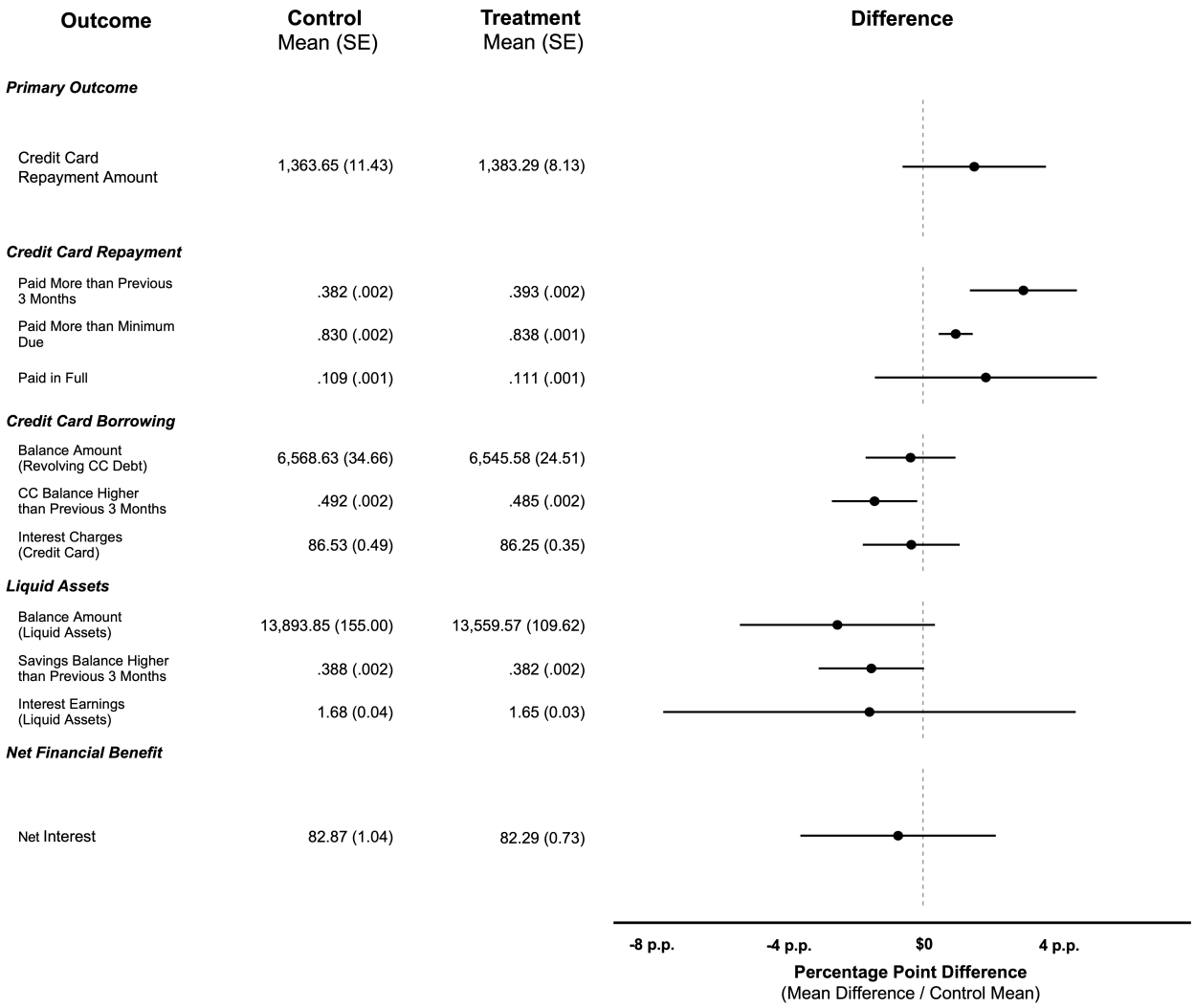


Figure A.3: Treatment effects across multiple financial outcomes in field experiment. Forest plot displays intervention effects on primary and secondary financial outcomes, with treatment versus control differences shown as percentage point changes (treatment effect divided by control mean). Left columns present mean values with standard errors (SE) for control and treatment conditions. Right panel shows standardized treatment effects with 95% confidence intervals represented by horizontal lines. Points indicate point estimates, with dashed vertical line at 0 indicating no treatment effect. Primary outcome measures credit card repayment amount in dollars. Secondary outcomes include credit card repayment behaviors (proportions paying more than previous 3 months, more than minimum due, and in full), credit card borrowing measures (revolving debt balance, proportion with higher balances than previous 3 months, and interest charges), liquid asset outcomes (account balances, proportion with higher savings than previous 3 months, and interest earnings), and net financial benefit (net interest paid/earned). Error bars extending beyond the null line suggest treatment effects distinguishable from zero at the 95% confidence level. Sample sizes: Control (N = 41,424), Treatment (Collapsed, N = 82,827).

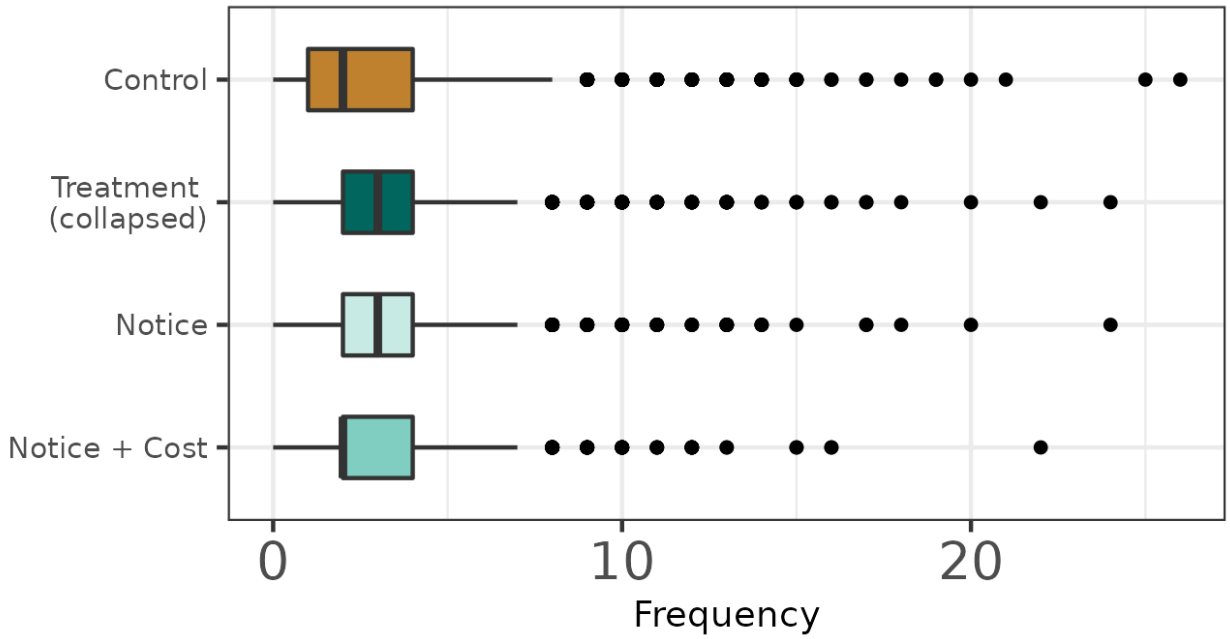


Figure A.4: Distribution of credit card repayment frequency during field experiment month in matched control sample. Box plots display the distribution of number of credit card repayments made by participants during the intervention month, with boxes showing median and interquartile ranges, whiskers extending to 1.5 times the IQR, and individual points representing outliers. Analysis is restricted to matched control sample (N = 4,324) created by pairing control group customers with notification group customers who engaged with the intervention message using propensity score matching. The matching procedure resulted in 2,162 customers per condition. Treatment conditions include notification-only (Notice) and notification plus cost information (Notice + Cost), with Treatment (Collapsed) representing the combined notification conditions. This secondary outcome analysis was conducted after finding no significant difference in total repayment amounts between conditions, focusing instead on payment behavior patterns. Statistical comparison using Wilcoxon Mann-Whitney test revealed significantly higher payment frequency in treated versus matched control participants (treated median = 3, control median = 2, $Z = 6.34$, $p < .001$).

Table A.1: Eligibility Criteria for Recruiting Customers

Inclusion Criteria

- Be a bank customer
- Be over the age of 18
- Be active on the bank's smartphone app ("digitally active")
- Be a primary credit card holder with no secondary card holders
- Have a retail or personal liability business credit card with the bank
- Not have a credit card with honeymoon rates or a staff benefits card
- Not have a legacy card
- Not be in arrears or hardship
- Have not set up automatic payments to repay their full balance
- Have at least one transaction account
- Be co-holding $500 or more (criteria dropped for Non-Co-Holders)
  - Have had a minimum total savings balance of $500 on their past 3 statement due dates
  - Have had a minimum *revolving* balance of $500 over their past 3 statement periods

Exclusion Criteria

- Cards "blocked carrying balance" (as of June 2018) and "legacy" cards
  - These customers were unlikely to be exposed to proposed treatment
- Have a credit card with honeymoon rates or a staff benefits card
  - These cards have lower interest rates, sometimes only for a short period of time
- Customers with Autopay set up to pay off Closing Balance
  - These customers were unlikely to see "pay credit card" screen and were already repaying in full
- Customers with "Hardship" flag
  - Per the bank's policy, these customers were typically excluded from marketing campaigns
- Secondary credit card holders
  - This is meant to focus the analysis on primary card holders and avoid having two customers in the sample from the same account

Table A.2: Pre-Registered Minimum Detectable Effects calculated at 80% Power

	Control Group:		*MDE*
	Mean	SD	
Primary Measure			
Credit Card Repayment Amount	$1,361	$2,312	$39
Credit Card Repayment Domain			
Indicator (1 = Repaid More than pre-intervention average)	.38	.49	.008
Indicator (1 = Repaid more than contracted minimum)	.83	.34	.006
Indicator (1 = Repaid the full amount)	.11	.29	.005
Borrowing Domain			
Revolving Balance	$6,568	$7,016	$118
Indicator (1 = CC balance higher than pre-intervention average)	.49	.50	.008
Saving Domain			
Credit Card Interest Charges	$87	$100	$1.70
Savings Balance	$13,968	$33,146	$556
Indicator (1 = Savings balance higher than pre-intervention average)	.39	.49	.008
Interest Earned on Savings	$1.70	$8.47	$0.14
Net Financial Benefit Domain			
Net Interest (Savings Interest Earned minus Credit Card Interest Paid)	$83	$183	$3.10

Note: Two-tailed comparison of means with Control group sample size of 41,776 and treatment group sample size of 83,552. We assume control and treatment groups have the same standard deviation and use an $\alpha = .05$ and power of $1 - \beta = .80$. Pre-intervention averages are calculated over 3 months preceding the intervention.

Table A.3: Balance Table: Summary statistics for control and treatment groups at baseline

	Condition Assigned:			*P-Value*
	Control	Notice	Notice + Cost	
Sample Size	41,776	41,776	41,776	
Age	40.83	40.83	40.83	1.00
	(12.23)	(12.27)	(12.23)	
Gender (Proportion Male)	.52	.52	.52	1.00
Tenure (Years with Bank)	19.47	19.47	19.47	1.00
	(11.17)	(11.18)	(11.15)	
Log of Mean Repayment	6.38	6.38	6.38	1.00
	(1.26)	(1.27)	(1.26)	
Log of Std Dev Repayment	5.32	5.32	5.32	1.00
	(2.53)	(2.52)	(2.51)	
Log of Mean Closing Balance	8.39	8.39	8.39	1.00
	(.91)	(.91)	(.91)	
Log of Std Dev Closing Balance	5.66	5.66	5.66	1.00
	(1.39)	(1.39)	(1.39)	
Log of Mean Savings	8.43	8.43	8.43	1.00
	(1.25)	(1.24)	(1.26)	
Log of Std Dev Savings	7.19	7.19	7.19	1.00
	(1.20)	(1.20)	(1.20)	
Personal Loan (Proportion)	.17	.17	.17	1.00
Home Loan (Proportion)	.26	.26	.26	1.00
"Awards" Credit Card (Proportion)	.26	.26	.26	1.00
"Low-Fee" Credit Card (Proportion)	.21	.21	.21	1.00
"Low-Rate" Credit Card (Proportion)	.52	.52	.52	1.00
"Other" Credit Card (Proportion)	.01	.01	.01	1.00

Note: Standard deviation reported in parentheses.

Table A.4: Effects of Notification on Credit Card Repayment Amount

	(1)	(2)	(3)
Constant	1,363.65***	35.05	−1,066.30
	(11.47)	(2,301.68)	(1,987.49)
Notification Groups	19.63	20.00	16.78
	(14.05)	(13.84)	(11.95)
Customer Demographics		X	X
(Female, Age Band, Customer Tenure, State†)			
Financial Behaviors			X
(App Login Count†, Financial Wellbeing Score†, Mean Repayment‡, Mean Closing Balance‡, Mean Daily Savings Balance‡, Net Balance Quintile†, Arrears†)			
Observations	124,251	124,249	124,249

Note: Customer-level samples. Robust SE in parenthesis. Notification Groups combines Notice and Notice + Cost groups. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$; †January 2020; ‡Averaged between Nov 2019 - Jan 2020

Table A.5: Alternative Sample Specifications to Estimate the Average Treatment Effect

	Amount of Credit Card Balance Repaid		
	(1)	(2)	(3)
	Full Sample	*Login Sample*	*Matched Sample*
Constant	1,363.65***	1,577.99***	1,258.49***
	(11.47)	(19.60)	(51.25)
Notification Groups (Trt 1 & Trt 2)	19.63	2.11	41.78
	(14.05)	(24.00)	(72.48)
Observations	124,251	118,708	4,324

Note: Robust SE in parenthesis. Col 1 is primary specification for the Full Sample, also presented in Table A.4; Col 2 restricts the sample to customers who logged into the app during the month of the experiment; Col 3 consists of only customers in the matched control sample (see "Matched-Pair Analysis" above); $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

Table A.6: t-Tests comparing Co-Holder and Non Co-Holder responses in descriptive survey

	Co-Holder (N = 107)		Non Co-Holder (N = 283)		P-Value	
	Mean	(SD)	Mean	(SD)	Unadj.	Adj.
Payment Preferences (1 = CC; 4 = Cash)						
Small, Everyday	2.90	(1.10)	1.97	(1.13)	$< .001^{***}$	$< .001^{***}$
Large, Anticipated	2.58	(1.06)	2.10	(1.15)	$< .001^{***}$	$< .001^{***}$
Large, Surprise	1.96	(.95)	1.91	(1.02)	.64	.64
Mental Accounting[a]						
Use CC for certain purchases and cash/savings for others	.71	(1.15)	.41	(1.27)	$.027^{*}$	.053
Don't need cash due to CC access	−1.19	(1.00)	−.97	(1.15)	.066	.066
Self-Control[a]						
Impulsivity (buy when can't afford)	−.60	(1.29)	−1.19	(1.10)	$< .001^{***}$	$< .001^{***}$
Propensity to Plan (spend now)	−.60	(1.09)	−1.00	(1.17)	$.002^{**}$	$.002^{**}$
Time Preference (delay gratification)	.72	(1.00)	1.08	(.88)	$.001^{**}$	$.002^{**}$
Risk Preference (willing to take risks)	−.45	(1.14)	−.18	(1.25)	$.048^{*}$	$.048^{*}$
Thresholds ($)						
Max Debt (feel uneasy)	4,101	(3,082)	2,252	(3,211)	$< .001^{***}$	$< .001^{***}$
Credit Available	6,587	(7,058)	6,674	(9,251)	.92	.92
Min Savings (comfort)	4,281	(7,360)	6,949	(13,500)	$.013^{*}$	$.018^{*}$
General Savings	4,245	(11,372)	10,092	(18,348)	$< .001^{***}$	$< .001^{***}$
Psychological Ownership[†] (3-item avg)	−.38	(1.00)	−.69	(1.00)	$.007^{**}$	$.007^{**}$
Debt Aversion[‡] (5-item avg)	−.21	(.57)	.07	(.64)	$< .001^{***}$	$< .001^{***}$
Sense of Irresponsibility[§] (normalized)						
Revolving >$500 debt	−.43	(.98)	.16	(.96)	$< .001^{***}$	$< .001^{***}$
Keeping <$500 liquid	−.08	(1.02)	.03	(.99)	.34	.34
Liquidity[b] (normalized)						
Spare Money	−.52	(.87)	.20	(.98)	$< .001^{***}$	$< .001^{***}$
Able to come up with:						
$500	−.14	(.99)	.05	(1.00)	.08	.08
$1,000	−.29	(1.11)	.11	(.94)	$.001^{**}$	$.002^{**}$
$1,500	−.32	(1.09)	.12	(.94)	$< .001^{***}$	$< .001^{***}$
$2,000	−.34	(1.06)	.13	(.95)	$< .001^{***}$	$< .001^{***}$
$2,500	−.35	(1.04)	.13	(.95)	$< .001^{***}$	$< .001^{***}$
Intertemporal Choice[#] (prop. larger/later)						
Low Liquidity ($900 assets)	.48		.31		$.003^{**}$	$.006^{**}$
High Liquidity ($2,900 assets)	.13		.07		.12	.12
Money Mgmt (prop. debit better for...)						
Tracking spending	.79		.63		$.002^{**}$	$.006^{**}$
Managing spending	.81		.67		$.003^{**}$	$.006^{**}$
Staying within budget	.86		.75		$.012^{*}$	$.016^{*}$
Disciplined spending	.86		.76		$.019^{*}$	$.019^{*}$
Aware of Co-Holding (CH only, prop.)						
Yes / No / It's Complicated	.76 / .07 / .18		—		—	—
Additional[a]						
Avoid CC surcharges	.09	(1.34)	.59	(1.29)	$.001^{**}$	$.004^{**}$
Worry credit limit cut	−.62	(1.38)	−1.07	(1.16)	$.003^{**}$	$.005^{**}$
Security from liquid assets	1.37	(.83)	1.33	(.90)	.61	.82
Need cash for situations	1.01	(1.06)	1.04	(1.04)	.83	.83

Note: FDR-adjusted p-values using Benjamini-Hochberg method (Benjamini and Hochberg, 1995). CC = Credit Card; CH = Co-Holder. [a]−2 = Strongly Disagree; 2 = Strongly Agree. [b]Higher = more liquidity/ability to access funds. [†]Higher = greater ownership of borrowed money. [‡]Higher = greater debt aversion. [§]Higher = perceive behavior as more irresponsible. [#]Proportion choosing delayed larger payment over immediate smaller payment. Normalized measures are z-scored. $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$.

APPENDIX B

SUPPLEMENT TO ESSAY 3

B.1 Supplement to Studies 1 and 2

B.1.1 Eligibility Criteria

Table B.1: Eligibility Criteria for Recruiting Customers

Inclusion Criteria
• Be a bank customer
• Be over the age of 18
• Be active on the bank's smartphone app ("digitally active")
• Be a primary credit card holder with no secondary card holders
• Have a retail or personal liability business credit card with the bank
• Not have a credit card with honeymoon rates or a staff benefits card
• Not have a legacy card
• Not be in arrears or hardship
• Have not set up automatic payments to repay their full balance
• Have at least one transaction account
• Be co-holding $500 or more (criteria dropped for Non-Co-Holders)
– Have had a minimum total savings balance of $500 on their past 3 statement due dates
– Have had a minimum *revolving* balance of $500 over their past 3 statement periods
Exclusion Criteria
• Cards "blocked carrying balance" (as of June 2018) and "legacy" cards
– These customers were unlikely to be exposed to proposed treatment
• Have a credit card with honeymoon rates or a staff benefits card
– These cards have lower interest rates, sometimes only for a short period of time
• Customers with Autopay set up to pay off Closing Balance
– These customers were unlikely to see "pay credit card" screen and were already repaying in full
• Customers with "Hardship" flag
– Per the bank's policy, these customers were typically excluded from marketing campaigns
• Secondary credit card holders – This is meant to focus the analysis on primary card holders and avoid having two customers in the sample from the same account

B.1.2 Comparing Survey Sample to Representative Sample of Banking Customers

B.1.3 Calculating Co-Holding Amounts

Co-holding involves having revolving, high-interest debt while at the same time having liquid assets available. The amount of co-holding can, therefore, be computed as the overlapping value between these two balances, i.e., the minimum of the two (see Table 2, "Definitions" in Gathergood and Weber, 2014). To illustrate, consider a person who has more in assets than in revolving debt (e.g., assets: $1,000; debt: $700), a *Net Saver*. For this person, the amount they owe ($700) is the amount they are co-holding, because they have at least that much cash that could be used to pay off debt. On the other hand, if a person has more debt than they do assets (e.g., assets: $800; debt: $1,200), a *Net Borrower*, then the amount they are co-holding is the total amount they have in cash because it is the most they could possibly use to repay their debt.

We use this approach to calculate co-holding per customer, per month among the Field Survey sample (n = 2,307) for which we had the most detailed records. We relied specifically on the interest-bearing balance, recorded by the bank, at each credit card statement's due date. This approach excludes any debt that accumulated within a statement period but was paid off before any interest was charged. We matched this debt amount with the sum of the customers' liquid assets (i.e., transaction and savings account) balances on the same day the statement was due. For customers with multiple credit cards, we summed the interest bearing balances in the same month and paired it with the average of the daily balance between the first credit card due date and the last credit card due date that month. We compared this approach for identifying co-holding to others, such as selecting only the balance of the credit card with earlier due date or only the balance of the card with the latest due date and matching the respective daily asset balance. The approach we used is correlated with the

Table B.2: Summary Statistics from January 2020

	Representative Sample			*Survey Sample*		
	Total	CH	NCH	Total	CH	NCH
$N =$	100,000	7,438	92,562	2,307	979	1,328
Marital Status						
Married	0.20	0.28	0.20	0.25	0.25	0.26
Single	0.22	0.26	0.22	0.34	0.34	0.35
Other	0.17	0.13	0.18	0.17	0.18	0.16
N/A	0.40	0.33	0.41	0.23	0.23	0.23
Gender						
Male	0.50	0.53	0.50	0.56	0.51	0.59
Age						
Mean (SD)	44 (18)	45 (14)	44 (18)	49 (14)	48 (13)	50 (14)
Annual Income						
Median	$47,000	$59,000	$45,000	$58,000	$59,000	$59,000
(IQR)	($29,000)	($36,000)	($27,000)	($33,000)	($33,000)	($34,000)
Credit Card						
Proportion	0.32	0.998	0.27	0.96	0.98	0.96
Mdn Balance	$1,570	$3,878	$933	$1,839	$4,400	$737
Mdn Credit Limit	$8,000	$8,000	$8,000	$10,000	$10,000	$9,650
Mdn Utilization	24%	79%	12%	35%	55%	12%
Mean Rate (p.a.)	14.8%	12.2%	14.8%	15.2%	12.3%	15.8%
(SD)	(7.5)	(5.6)	(7.1)	(6.5)	(5.2)	(5.8)
Self-Set Limit On	0.21	0.20	0.21	0.38	0.48	0.28
# of Cards	1	1	1	1	1	1
Savings Account						
Proportion	0.61	0.66	0.60	0.75	0.75	0.75
Mdn Balance	$220	$101	$246	$362	$198	$752
Mean Rate (p.a.)	0.75%	0.75%	0.75%	0.75%	0.75%	0.75%
# of Accounts	1	1	1	1	1	1
Transaction Account						
Proportion	0.78	0.97	0.76	0.989	0.995	0.985
Mdn Balance	$500	$642	$481	$824	$648	$996
# of Accounts	1	1	1	1	1	1

Note: Reported balances are only for those who possess that product. "Representative" customers (Cols 2-4) are a random set of customers primarily banking with this bank. "Survey" customers (Cols 5-7) were selected in Oct 2021 based on additional criteria (see Main Text). Representative, Co-Holders (Col 3), have $500 in revolving debt and $500 in liquid assets in Jan 2020. Survey, Co-Holders (Col 6) are "Co-Holders" in the Field Survey sample. "Self-Set Limit On" is proportion of customers who opted into a feature that allows them cap their credit access below the contractual credit limit.

others we tried, rs $> .97$, $p < .001$, noting that for customers with a single credit card, the results will always be the same. The median customer possessed only one credit card; see Table B.2.

B.1.4 Supplemental Table and Figure

Table B.3: Estimating the Effects of Debit (vs. Credit) Spending by Category on Propensity to Co-Hold

	Survey Month Only					
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.429***	0.428***	0.412***	0.159	0.077	0.451
	(0.010)	(0.010)	(0.020)	(0.231)	(0.233)	(0.486)
Prop Spent on Debit: Small, Everyday (Std)	0.154***			0.094***		
	(0.010)			(0.014)		
Prop Spent on Debit: Large, Anticipated (Std)		0.154***			0.097***	
		(0.010)			(0.015)	
Prop Spent on Debit: Large, Surprise (Std)			0.150***			0.101**
			(0.020)			(0.038)
Total Spend (Winsorized; Std)				0.007	0.006	0.040
				(0.013)	(0.015)	(0.030)
Average Daily Utilization (Std)				0.235***	0.269***	0.127
				(0.032)	(0.037)	(0.110)
Max Daily Utilitization (Std)				−0.068*	−0.097**	0.055
				(0.033)	(0.037)	(0.108)
Customer Controls	No	No	No	Yes	Yes	Yes
Observations	2,221	2,057	539	2,148	1,990	527
Adjusted R^2	0.096	0.096	0.091	0.227	0.224	0.520

Note: Customer Controls include Age Band, Gender, Postcode, State, Tenure with the Bank, and Marital Status as of January 2020. Continuous values have been standardized (Std), $\mathcal{N}(0, 1)$.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

B.2 Supplement to Study 3

B.2.1 Methods

Participants. 360 participants were recruited to take part in the study using the Prolific platform. 57 participants were screened out prior to treatment assignment based on our pre-registered criteria (46 reported no credit card, 1 reported no liquid assets, 4 reported no credit and no liquid assets, 6 did not consent to the study). 303 participants (158 Male, 144 Female, 1 Self-Identify; age: M = 41.5, SD = 13.4; 64.7% White, 11.9% Black, 8.9% Latin American, 5.3% Multi-Racial, 9.2% all others) completed the study. Participants were paid US$1.50 for completing the survey which took approximately 10 minutes for the median participant.

B.2.2 Materials

Advice Column Manipulation

Pro-Credit Card

> *Dear Reader,*
>
> *The key to financial well-being is understanding how to think about your money resources.*
>
> *Financial experts recommend viewing your credit card as the wise choice for everyday expenses.*
>
> *When it comes to small, routine purchases, using a credit card is a smart strategy. Not only does it offer convenience, but many credit cards also provide reward points for your purchases and enhanced fraud protection.*
>
> *Debit cards typically lack these advantages and don't help you build a credit history. Additionally, using debit cards can make it harder to manage your overall finances across accounts and may expose you to increased liability risks in certain fraud situations.*

For most consumers, credit cards offer significant benefits for daily transactions. By using this payment method thoughtfully, you can enhance your financial flexibility while maintaining control over your spending.

Remember: Smart financial management means choosing the payment method that maximizes your benefits while supporting your overall financial goals.

Control

Dear Reader,

The key to financial well-being is understanding how to think about your money resources.

Financial experts recommend diversifying your financial knowledge across multiple areas.

When it comes to managing your finances, learning about different aspects like savings, investments, and budgeting creates a balanced foundation. Not only does this approach provide flexibility, but it also helps you adapt to changing economic conditions.

Setting clear financial goals typically provides direction for your decisions and helps prioritize your actions. Additionally, reviewing your progress regularly can help identify areas for improvement and celebrate successes along your financial journey.

For most consumers, taking a holistic approach offers significant benefits for long-term financial health. By understanding how different financial components work together, you can make informed decisions that align with your personal circumstances.

Remember: Smart financial management means developing comprehensive knowledge while supporting your overall financial goals.

Debt Repayment Scenario

Debt repayment decisions were assessed by having each participant read and respond to the following scenario:

Imagine that it's the end of the month, and you have a credit card balance of $1,050 [$1,400, $1,680, or $2,100].

You have already paid the minimum required amount, and the $1,050 [$1,400, $1,680, or $2,100] is what's left to repay. Any amount unpaid will start to incur interest charges and you need to decide whether or not you want to pay off anymore of your debt this month.

You currently have $1,500 [$2,000, $2,400, or $3,000] available in your checking account. But you also know that, on average, you spend between $900 and $1,200 [$1,200 and $1,600, $1,440 and $1,920, or $1,800 and $2,400] each month across your debit and credit cards.

With all this in mind, how much of the money you have saved up (if any) would you transfer to pay off your credit card debt?

The set of values were randomized between participants. The debt was always .7 of the asset amount and the range of average expenses was always .6 to .8 times the asset.

Participants indicated how much of their available funds they would use to pay off their remaining credit card debt (any value between $0 and the assigned debt amount).

(Ir)Responsibility Measures

Participants completed four sets of responsibility perception questions designed to assess perceived irresponsibility of: (1) having *less* than $500 in liquid assets, (2) having *more* than $500 in credit card debt, (3) *using* debit cards for everyday purchases, and (4) *using* credit cards for everyday purchases.

Each domain included three items measuring whether the behavior would make participants feel (i) irresponsible, (ii) like a bad person, or (iii) subject to negative judgment from others (see Sussman and O'Brien, 2016). For instance, the measures assessing the perceived irresponsibility of making everyday purchases on credit asked participants how much they

agreed with each of the following:

1. "I would feel irresponsible paying for everyday items using my credit card."
2. "Making small, everyday purchases on my credit card would leave me feeling like a bad person."
3. "Others would judge me negatively if they learned I had paid for everyday goods using my credit card."

Participants stated how much they (disagreed) with each of the three statements; the responses ranged from "Very Irresponsible" (-2) to "Very Responsible" (2). These responses were then averaged by set such that higher values indicated greater sense of irresponsibility. The order of domains was randomized for each participant and each domain appeared on a separate page.

Cash Flow Management Strategy Descriptions

> *Please read the following descriptons* [sic] *before answering the two questions below:*
>
> *Some people prefer to use their debit card or cash for everyday expenses. They generally try to only spend money they already have in their accounts, and reserve their credit cards for emergencies or large unexpected expenses.*
>
> *Other people prefer to use their credit cards for most expenses. They track their spending and prioritize paying off their credit card at the end of the month with the money they have or the income they receive.*
>
> **Question 1.** How well does the first description fit you? That is, do you primarily use debit / cash for everyday expenses?
>
> **Question 2.** How well does the second description fit you? That is, do you primarily use credit for everyday expenses?

B.2.3 Supplemental Figures

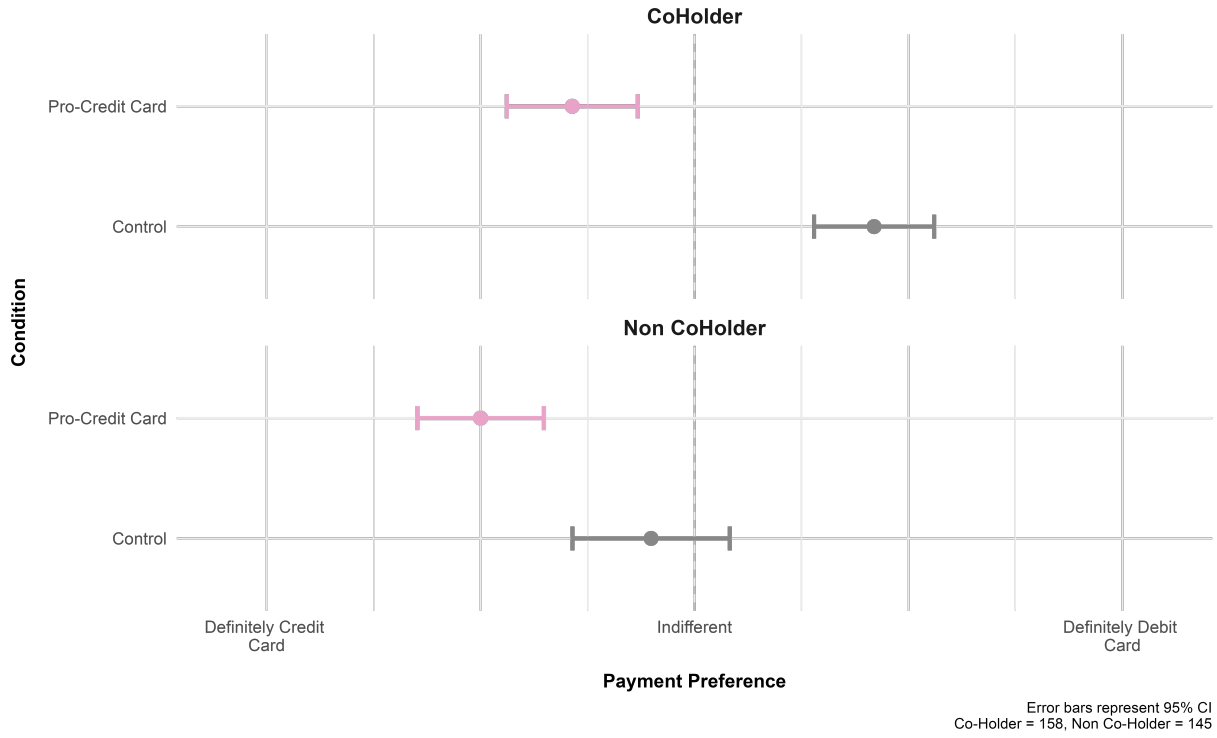


Figure B.1: The payment method preference effect is more pronounced for co-Holders. Online Experiment (N = 721). Error bars represent 1.96 * SE.

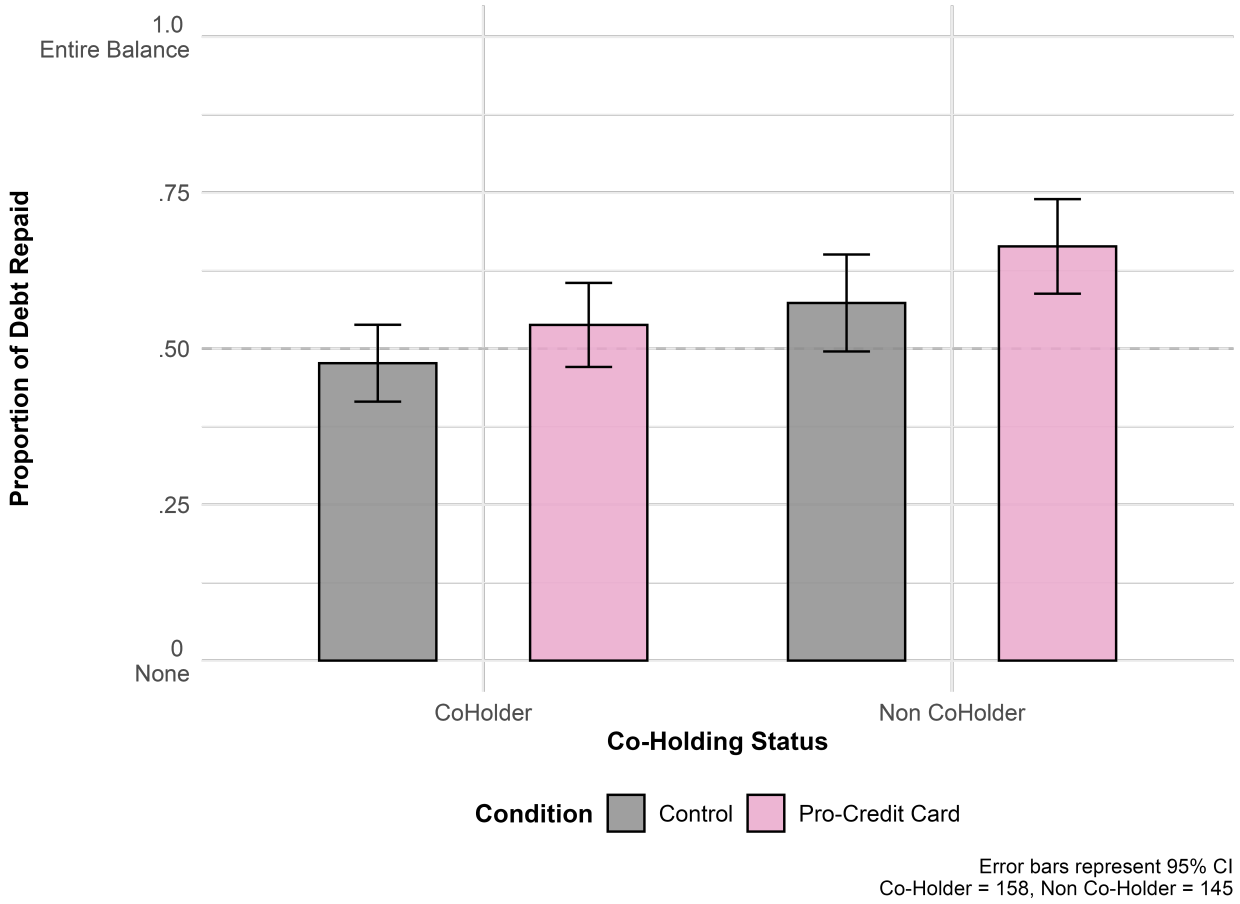


Figure B.2: Co-Holders and Non Co-Holders both repay more when assigned to Pro-Credit Card condition

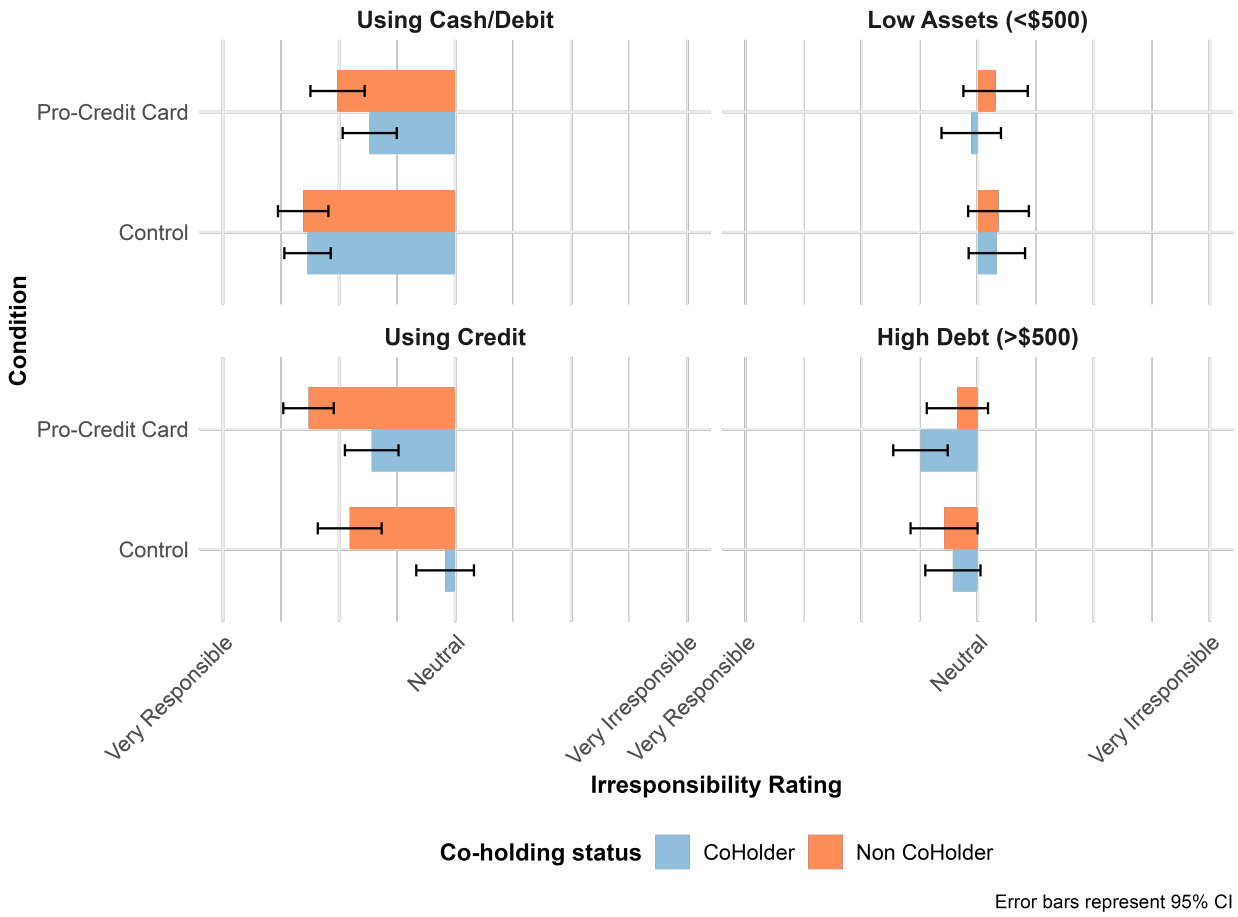


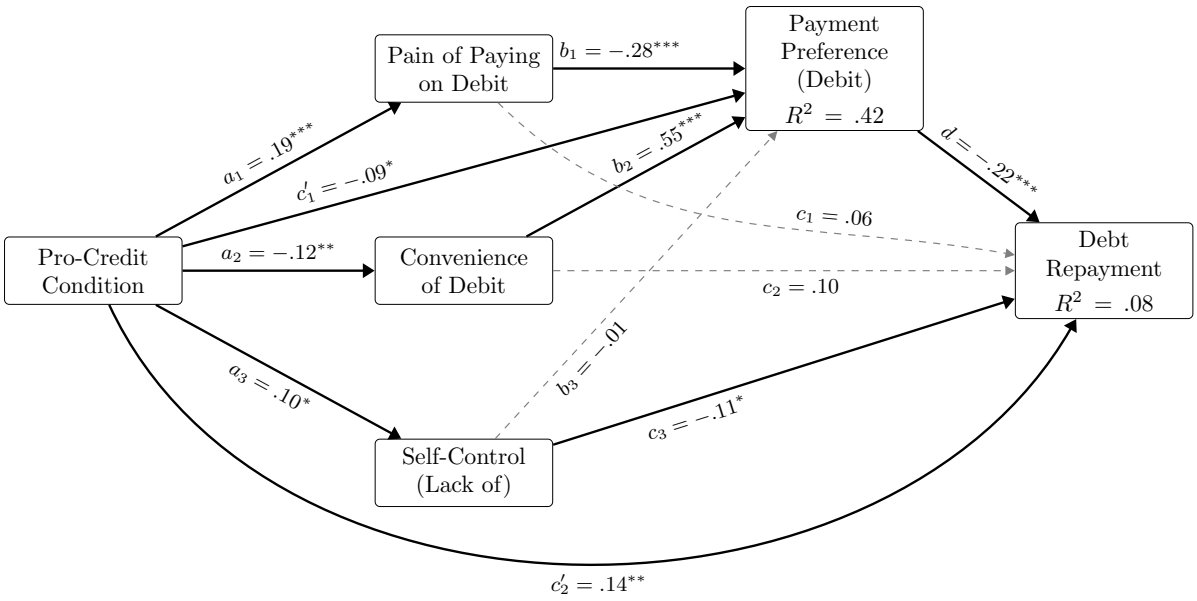
Figure B.3: Condition Assignment Shifts Perceptions of Responsibility for Spending but Not Balances

B.3 Supplement to Study 4

B.3.1 Methods

Participants. 562 participants were recruited to take part in the study using the Prolific platform. 2 participants were removed for appearing as duplicates. 75 of the remaining participants were screened out prior to treatment assignment based on our pre-registered criteria (68 reported no credit card, 1 reported no liquid assets, 6 did not consent to the study). 486 participants (248 Male, 235 Female, 3 Self-Identify; age: M = 40.3, SD = 13.1; 61.7% White, 13.6% Black, 7% Latin American, 5.8% Multi-Racial, 11.9% all others) completed the study. Participants were paid US$1.63 for completing the survey approximately 9 minutes for the median participant.

B.3.2 Supplemental Figures



Serial Mediation Effects:

Pain pathway: $a_1 \times b_1 \times d = .008^{*}$ (7.9%)

Convenience pathway: $a_2 \times b_2 \times d = .010^{*}$ (9.9%)

Combined serial effects: 16.8% of total effect

Legend:

Significant paths

Non-significant paths

$^{***}p < .001,\ ^{**}p < .01,\ ^{*}p < .05$

Figure B.4: Serial Mediation Model for Study 4. Standardized path coefficients from structural equation modeling (N = 486; 5,000 bootstrap resamples). The experimental manipulation affects debt repayment through two serial pathways mediated by payment preference: via pain of paying and via convenience perceptions. Solid lines represent statistically significant paths; dashed lines represent non-significant paths.

B.4 Supplement to Study 5

B.4.1 Methods

Participants. 1242 participants were recruited to take part in the study using the Prolific platform. 196 participants were screened out prior to treatment assignment based on our pre-registered criteria (126 reported no credit card, 13 reported no liquid assets, 60 timed out on the screening questions, 7 did not consent to the study). 4 were removed for appearing as duplicates. 1,046 participants (528 Male, 505 Female, 13 Self-Identify; age: M = 40.4, SD = 13.6; 63.9% White, 13.9% Black, 8.9% Latin American, 13.4% all others) completed the study.

B.4.2 Additional Analyses

Effect of Liquidity Stimuli Sampling on Debt Repayment. While we had no hypothesis regarding the specific values of the stimulus (within a liquidity level), a two-way ANOVA suggests the proportion of debt repaid varied based on the stimuli assigned (Main Effect of Stimuli: $F(2, 1040) = 3.68$, $p = .026$; Main Effect of Liquidity Condition: $F(1, 1040) = 433.91$, $p < .001$; Interaction: $F(2, 1040) = 2.21$, $p = .111$). In particular, we found a significant difference within the high liquidity condition between the lowest level (\$1250 Debt, Prop Repaid = .865 and another level (\$4,250 Debt, Prop Repaid = .771; $p_{Adj} = .044$). We found no other differences between levels within a liquidity condition.

B.5 Supplemental Study: Online Experiment Testing How Payment Preference Affects Decision to Co-Hold

In this study, we examined whether manipulating one's preference to use cash for everyday expenses could lead them to hold onto more of their liquid assets despite having an outstanding credit card balance to repay.

The study was approved by the Institutional Review Board at a Midwestern university in the US and pre-registered on AsPredicted.org (aspredicted.org/BTB_VW4).

B.5.1 Methods

Participants

Consistent with our pre-registration, we recruited 923 participants (449 women, 453 men, 17 self-identified, 4 NAs; age: M = 37.4, SD = 14.0; 67.2% white, 9.5% Black, 7.7% Latin American, 15.6% all others). Participants were paid US$1.00 for completing the survey, which took approximately 6 minutes.

Experiment Design

Participants first consented to the study before being told to read a brief advice column on "tips for improving financial well-being." They were encouraged to read carefully, as there would be follow-up questions to test their understanding of the key concepts.

We randomly assigned participants to a Pro-Credit Card or Pro-Debit Card condition. Both groups read a personal advice column crafted with the assistance of ChatGPT with tips such as "1. Create a budget," "2. Spend smart using a [credit card] [debit card]," "3. Insure against emergencies," and "4. Invest in your future." The second tip varied by condition, promoting using a credit or debit card. Each tip included a few sentences of supporting details. For instance, those in the Pro-Credit Card condition saw:

2. Spend smart using a credit card: When it comes to small, everyday expenses, using a credit card can be a smart choice. Not only does it offer the convenience of not having to carry cash, but many credit cards also offer reward points for your purchases and fraud protection. Debit cards, on the other hand, don't earn points or provide you with the same level of protections. Not to mention the high overdraft fees which adds up in the long-run.

Whereas those in the Pro-Debit Card Condition saw:

2. Spend smart using a debit card: When it comes to small, everyday expenses, using a debit card is a smart choice. Debit cards draw from your checking account, so you're only spending money that you already have. This can help you avoid the trap of overspending and prevent you from getting into debt. Credit cards, on the other hand, can be slow to process payments making it harder to track expenses. Not to mention the high-interest debt which adds up in the long-run.

Participants confirmed their understanding by answering three comprehension questions including: (1) which of five items was NOT a tip; (2) a multiple-choice fill-in-the-blank question: "To prepare against an emergency, it's important to have…", where "insurance" was the correct response. (3) a True or False question, specific to each condition, stating either "Credit cards tend to offer awards points." or "Debit cards make it easier to track expenses." Participants who missed one or more comprehension questions were given a second chance to read the article and respond to the comprehension questions they missed. Those who erred twice were excluded from the analysis (see pre-registration).

We asked participants to imagine having a $1,400 credit card balance and $2,000 in their checking account. To avoid ambiguity with respect to their obligation, we told them "You have already paid the minimum required amount, and the $1,400 is what's left to repay. Any unpaid amount will start to incur interest charges, and it's up to you whether or not you

want to pay it all off this month." We informed them that, on average, they spent between $1,200 and $1,600 each month across both cards. Then we asked, "With all this in mind, how much of the money you have saved up (if any) would you transfer to pay off your credit card debt?" The total amount could be any number between $0 and $1,400, the amount of outstanding debt.

In the pages that followed, we assessed participants' pain of paying for different expenses with credit or debit cards and their general payment method preference. Questions were randomized within each block, and blocks were randomized for each participant.

To assess the pain of paying, participants imagined paying for "everyday purchases" and "large, planned expenses" using credit or debit cards and rated the relative pain on a scale ranging from "Much more painful using my credit card" (-2) to "Much more painful using my debit card" (2).

As a manipulation check, participants were asked how they preferred to pay for expenses in three categories: small, everyday; large, anticipated; and large, surprise. These were the same as the "general" expenses used in the Field Survey. The responses ranged from "Definitely Credit Card" (-2) to "Definitely Cash / Savings" (2), with an "Indifferent" (0) midpoint option instead of the "Alternative Payment" option used in the Field Survey.

Finally, participants answered demographic questions, including their gender, race or ethnicity with which they most identified, age, highest education level completed, and annual pre-tax income. They were also asked about the number of active checking accounts, savings accounts, and credit cards they had, as these were part of our pre-registered exclusion criteria. Participants who reported having at least one credit card were also asked to describe their typical credit card repayment behavior, such as whether they paid off their balance in full each month or only a portion of it.

B.5.2 Results

Of the 923 participants, we excluded 202 participants based on our pre-registered criteria (10 failed the comprehension check twice; 16 had no checking account; 181 had no credit card).

We were primarily interested in how one's preference for payment method would affect the amount of debt they chose to repay and, conversely, the amount of cash they decided to hold. To confirm that our manipulation worked as intended, we compared average responses to the question asking participants how they would pay for an everyday purchase. As intended, those in the Pro-Credit Card condition preferred to pay with their credit card (M = −.50, SD = 1.44), and those in the Pro-Debit Card condition preferred to pay with their debit card (M = .18, SD = 1.59), $t(703.4) = 6.02$, Cohen's $d = -.45$, 95% CI [−.60, −.30], $p < .001$. This is shown in Figure B.5.

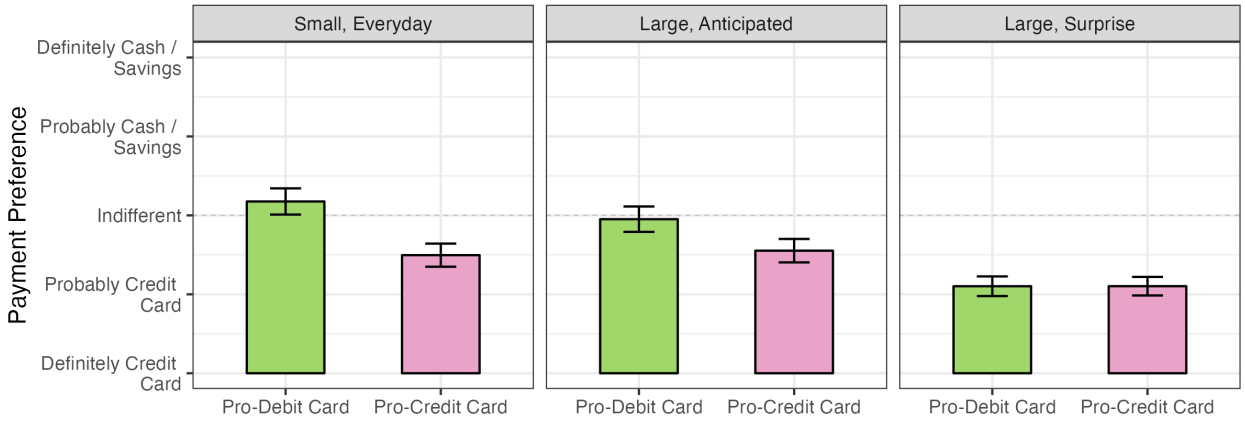


Figure B.5: Manipulation Check: Payment Method Preference by Category. Online Experiment (N = 721). Error bars represent 1.96 * SE.

Our primary outcome was the proportion of outstanding debt repaid, which we calculated by dividing participants' numeric responses by the amount of outstanding debt (i.e., $1,400). As we hypothesized, participants in the Pro-Debit Card (58.6%, SD = 32.9%) condition paid less of their balance than those in the Pro-Credit Card condition (64.7%, SD = 30.9%), $t(710.5) = -2.56$, Cohen's $d = -.19$, 95% CI [−.34, −.04], $p = .011$ (see Figure B.6).

Next, we analyzed responses on the pain of paying question where higher values represented more pain paying with a debit card (less pain paying with credit card) and 0 was the midpoint. Those in the Pro-Credit Card condition anticipated more pain paying using their debit card (less pain using their credit card; M = .26, SD = 1.15) than those in the Pro-Debit Card condition, who anticipated less pain using their debit card (more pain using their credit card; M = −.23, SD = 1.20), $t(713.1) = 5.62$, $d = .42$, 95% CI [.27, .57], $p < .001$.

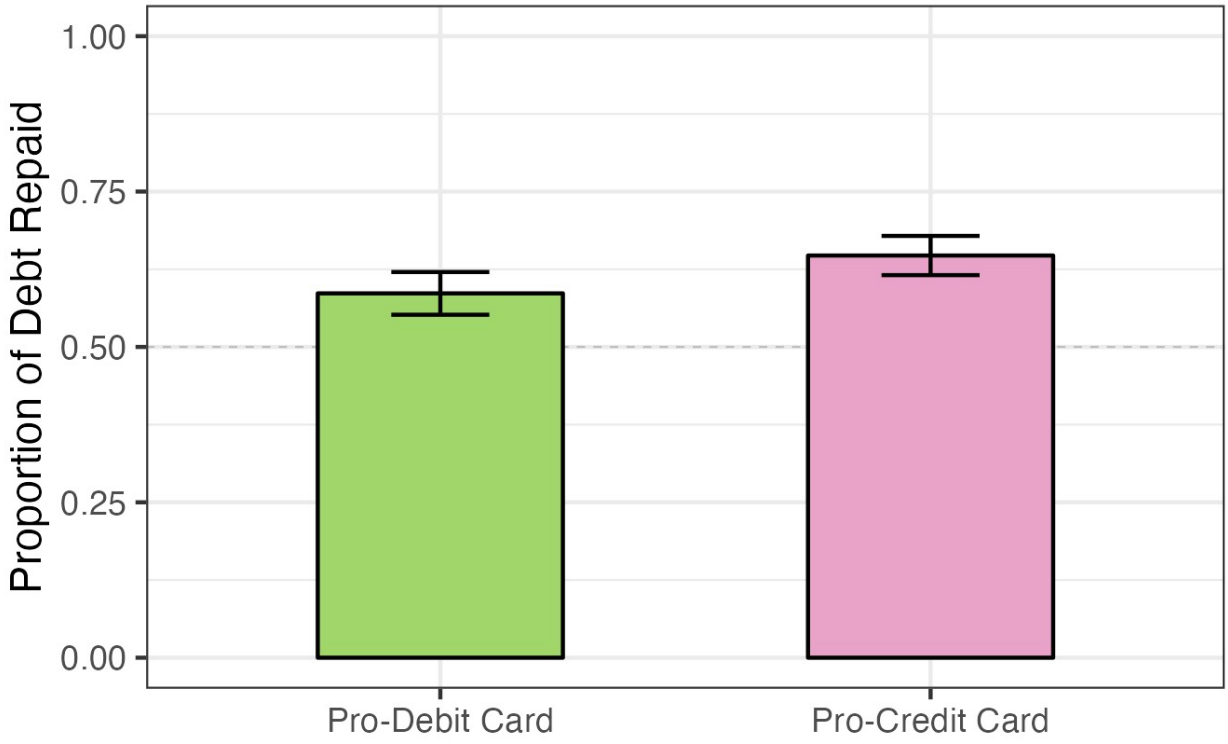


Figure B.6: Primary Outcome: Proportion of debt repaid. Online Experiment (N = 721). Error bars represent 1.96 * SE.

B.5.3 Discussion

This study aimed to establish a causal link between payment method preferences and one's decision to co-hold. By exposing participants to an advice column promoting either debit or credit cards, we influenced the proportion of debt they chose to repay, suggesting a connection between preferences and choice.

One limitation of this study is the hypothetical nature of the choice (but see Camerer and Hogarth, 1999; Gneezy and Rustichini, 2000; Horn and Freund, 2022; Johnson et al., 2013; Read, 2005). Nevertheless, by presenting a hypothetical scenario where participants were asked to imagine having a certain amount of credit card debt, we were able to observe their decision-making process in a tightly controlled experimental design.

Through this approach, we obtained initial causal evidence that suggests one's preference over payment methods could indeed influence their decision to co-hold.

B.6 Supplemental Study: Relabeling Transactions

Methods

This survey was designed to resemble the one described in Quispe-Torreblanca et al. (2019), where online participants annotate several transaction labels.

50 participants were recruited through Prolific to participate in the survey conducted in November 2022.

Participants consented to participate and were provided instructions for the task. They were told that the task would involve "label[ing] a series of spending categories" They saw an example — "Luggage and Leather Goods Stores" — along with the response options: (i) "A large expense that you WERE NOT expecting."; (ii) ""A large expense that you WERE planning for."; (iii) "An everyday purchase."; (iv) "None of the above"; and (v) "I don't know what this is."

As part of the instructions, participants were explicitly told: *Some of the items will be very difficult to label because you will not have enough information or the category may be unclear or you may see it fitting more than one category. Please do your best to answer these questions and select the label that best fits the category provided, even if you feel you don't know enough information. If you truly have no idea, there will be an option for "I don't know what this is", but we ask that you try not to use this.*

Participants then labeled 60 items randomly drawn from a set of 286.

Results

Each item was rated by 8.74 participants on average (SD = 2.73, Med = 8, Min = 2, Max = 15). For each item, we counted the number of ratings per category and selected the modal response as the label for that item

An item like "Computer Repair" had a modal label of "Large, Surprise" while "Detective

Agencies" had a modal label of "Large, Anticipated". The expense "Osteopaths" had a modal rating of "I don't know" and "Bands / Orchestras" was labeled "None of the Above".

"Taxicabs / Limousines", "Barber and Beauty Shops", and "Department Stores" were all seen as "Small, Everyday."

Of the 286 items, 110 were labeled as "Small, Everyday", 124 were labeled as "Large, Anticipated", 25 as "Large, Surprise", 19 as "None of the Above" and 8 received "I don't know".

The resulting data is available on this project's ResearchBox (#1182).[1]

1. researchbox.org/1182&PEER_REVIEW_passcode=NVMAVY

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