The Effect of Stock Ownership on Individual Spending and Loyalty

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Abstract

We show that when individuals own stock from a certain company, they increase their spending in that company's stores. We use data from a FinTech app that opens brokerage accounts for users and rewards them with stock when they shop at pre-selected stores. For identification, we use the staggered distribution of brokerage accounts over time and quasi-randomly distributed stock grants. We also show that loyalty is the dominant psychological mechanism behind our findings, that weekly spending in specific stores is strongly correlated with retail stock holdings of that company, and that stock rewards increase overall investment activity.

Keywords: stock rewards and ownership, spending at owned companies' stores, FinTech

JEL codes: G5, D90, G41, D14

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1 Introduction

According to the canonical economic model, investment decisions should affect consumption only through their effect on wealth, and consumption decisions should not influence investment choices. However, it is well documented that individuals invest in stocks from companies they are familiar with (Huberman, 2001; Keloharju et al., 2012) or loyal to (Cohen, 2009). In this paper, we show that behavioral biases in investing are not restricted to trading but also affect consumption, a direct component of individual utility and welfare. Exploiting several quasi-experimental features of our setting, we show that stock ownership increases spending in the company's stores by 40% to 100%. We then use survey evidence to show that the effect of stock ownership on spending is driven by loyalty.

We analyze the relationship between stock ownership and spending using de-identified transaction-level data from a FinTech company called Bumped. The company provides brokerage accounts for their users. In turn, users link all of their checking and credit card accounts and select their favorite stores in 34 retail categories. If and when they spend at one of their selected stores (online or offline), they receive a 0.5% to 2% fraction of their spending in the company's stock in their brokerage accounts.

When looking at the behavior of app users, standard selection concerns are present: i.e., there may be unobserved reasons that motivate certain individuals to get an app at a certain point in time. For example, individuals could time their sign-up to an app that rewards specific types of transactions when they expect to make a lot of those transactions. To alleviate such concerns, we exploit the fact that individuals in the sample were first required to sign up for a waitlist before getting their brokerage accounts. When individuals signed up for the waitlist, they only provided their email addresses. The company's operations team then released batches of users to onboard on a first-come, first-served basis and the number of new users depended on varying business objectives and constraints.

Users spend a considerable amount of time on the waitlist, an average of 4.5 months. Since we restrict our analysis to users who sign up immediately after being allowed to do so, it is implausible that users hold off on certain types of spending in anticipation of receiving an account. Users have no information when they will receive the account, and the distribution of accounts is not determined by user characteristics, since only their email addresses are known to the company at the time of being waitlisted.[1]

[1]To add credibility to our identification strategy, we show that there is no spending response when individuals choose to waitlist. Additionally, we look at the differential responses of users that were waitlisted for relatively short or long periods of time and do not find large differences in our results. Finally, we show in a randomization check that

We show that customers increase their spending at their selected stores in response to being allocated a brokerage account. Weekly eligible spending, i.e., spending at selected stores that gets rewarded with stocks, jumps up by 40% and stays persistently high for 3 to 6 months. As eligible spending averages $56 per week, this corresponds to a $23 increase in spending per week. We do not find an offsetting impact on ineligible spending, i.e., spending at stores that were not selected and does not get rewarded, and we thus document a small increase in total spending.

We also exploit the quasi-random nature of a promotional program through which a subset of users were awarded $5 or $10 stock grants from the following companies: Red Robin, McDonald's, Exxon Mobile, Chevron, and Yum! brands (e.g., Taco Bell). These stock grants were distributed upon account opening for a number of months, but were not advertised when users chose to waitlist or were allowed to sign up for accounts. In response to receiving a stock grant, we find a spending response of 100% in the stores for which individuals received stocks. For these users, we also find a more persistent response of eligible spending to account opening.

Furthermore, we have quasi-random variation in the fraction of eligible spending that was ultimately rewarded. This is because not all eligible spending was rewarded; rather, approximately 70% was rewarded due to company operations, policy changes, and constraints. When we split users into terciles based on which fraction of their eligible spending got rewarded, we do not find significant differences. Similarly, when we split users into terciles depending on whether their eligible spending is predominantly in low- versus highly-rewarded companies, we do not find significant differences. These findings tell us that the price effect, i.e., the fact that spending at certain brands got cheaper as they are rewarded, is unlikely to fully explain the changes in behavior we observe.

In addition to documenting a causal impact of stock ownership on spending within the framework of the Fintech app, we also analyze the relationship between spending and stock holdings in regular brokerage accounts. We find that daily and weekly spending in a certain store for our user population is strongly correlated with holdings of that company's stock among Robinhood brokerage clients.[2] A 1% increase in weekly holdings of a certain company (relative to holdings of all other companies) increases spending at that company's stores (relative to all other spending) by 0.18% (0.12% controlling for company and week-by-year fixed effects). We argue that this result helps us extrapolate our findings to actual spending and stock ownership in brokerage accounts. We chose Robinhood brokerage account data because Robinhood is the most common brokerage account of our users and, generally, Bumped users are likely a similar population to Robinhood

the time that individuals spend on the waitlist is not explained by individual characteristics.

[2] The holdings data of Robinhood brokerage clients was obtained from robintrack.net.

clients.

Finally, we study the link between owning more stocks and having a broader engagement with the stock market. We find evidence for the fact that receiving stock stimulates individual investments by documenting that outgoing brokerage transfers to other brokers are increased after individuals start receiving stock rewards. This is consistent with survey results showing increases in the self-reported likelihood of investing in stocks outside of Bumped in response to stock ownership through the platform.

What explains customers' spending responses after receiving stock rewards and grants? We argue that the mere monetary value or price effect of rewards is unlikely to fully explain the changes in behavior we observe, as the transaction and hassle costs required to increase spending between 40% to 100% at specific stores likely outweigh the 0.5% to 2% price effect of the rewards. Additionally, as mentioned, variation in the rewarded amounts does not affect users. Consistent with this presumption, we also find that the magnitude of stock rewards is substantially larger than the effect documented in the literature for cash rewards. Vana et al. (2018) calculate that, when an additional \$1 in cashback payment is offered, spending increases by \$3.51, entailing an effectiveness of 351%. In comparison, stock rewards have an effectiveness of 2,053%: we find a \$23 increase in weekly eligible spending when the average amount offered in stock rewards is \$1.12 per week (stock rewards are about 2% of weekly eligible spending, which in turn averages \$56 during the observation period).

We thus argue that there are additional factors, beyond a price effect, that change consumers' behaviors. In particular, we argue that stock ownership triggers feelings of loyalty. Following Cohen (2009), we define loyalty in a broad sense as an emotional tie. Feelings of loyalty then cause a preference for the products of a specific company, for motives beyond the consumption experience. A reward program, based on stock, cash, or any other payoff, can trigger feelings of loyalty, if the reward is perceived as a gift that causes feelings of reciprocity and affect. In that sense, affect and gift exchange are potential mechanisms behind the feelings of loyalty experienced by platform users. Additionally, three other well-documented psychological mechanisms can explain why stock rewards trigger loyalty: illusion of control, reductions in cognitive dissonance, and increases in familiarity.

We complement our empirical analysis with the results from a survey investigating users' motivations and attitudes toward stock ownership and loyalty. We find that 68% of users report feeling more loyal to the companies that they own stock from, and 40% report having a more positive attachment to these companies. Similarly, between 16% and 43% of users report shopping less from competitive brands and being likely to pay more or go out of their way to shop in stores for which they own stock. In addition, as mentioned, survey respondents report being more likely to invest outside of Bumped as a result of owning stock through Bumped. Both the loyalty responses and self-reported increases in the probability of investing outside of Bumped are positively correlated with self-reported measures of feeling excited about stock rewards, suggesting that the non pecuniary benefits of stock ownership are correlated with firm loyalty. These results thus confirm our empirical findings and suggest that loyalty explains the large effects of stock rewards on individual spending.

Our results have implications for the economy and for asset prices. At a high frequency and a very granular level (individual company), we document a strong relationship between spending in certain company products and holdings of the companies' stocks. This relation is stronger than the correlations found in aggregate data, which are too low to be explained with the canonical asset pricing model. Our results suggest that stock price fluctuations affect spending and thus utility in a more direct way than previously thought. Furthermore, previous work has found that brand loyalty and more generally, customer capital, is an intangible asset that leads to lower cash flow volatility and increases firm value (Dou et al., 2019; Larkin, 2013; Dou and Ji, 2020). Our results present stock ownership as a novel trigger of brand loyalty, thus expanding the traditional view that customer capital results from characteristics intrinsic to the market structure (Gourio and Rudanko, 2014)) or consumption experiences (Bronnenberg et al., 2012). Overall, we document that well-known behavioral biases are not restricted to the domain of investing and trading but extend to consumption, a direct component of utility and welfare.

Literature Review

Our study is related to prior literature suggesting that purchase behaviors and beliefs about a company have an impact on investment choices and vice versa. Previous literature shows that investors tend to buy stocks from companies they know (Huberman, 2001; Schoenbachler et al., 2004; Frieder and Subrahmanyam, 2005; MacGregor et al., 2000), portfolio choice is affected by loyalty (Cohen, 2009), and advertising products to investors increases demand for the corresponding stocks (Lou, 2014). We focus on the opposite direction of causality, and study how ownership of a specific stock affects consumption. A few studies have looked at this question before using survey data Aspara et al. (2009), Aspara and Tikkanen (2010), and Aspara and Tikkanen (2011), or from a theoretical perspective, Altinkemer and Ozcelik (2009). Only two previous studies have looked at non-survey data to estimate the effect of stock ownership on consumer demand, but only for a select group of companies.

The first study is Keloharju et al. (2012) which uses individual brokerage account data from

Finland to show that the clients of a given broker invest in the stock of that particular broker and the owners of a given car invest in the respective car company. In addition, and closer to our paper, they report evidence of causality in the other direction, although with a smaller sample size (6,814 individuals which is more than 100 times smaller than their main analysis) and only for the brokerage industry. They show that receiving stock from the same broker through inheritances or gifts has a positive and significant effect on the probability of opening a brokerage account. They complement the causal analysis showing that owning shares from a particular car company is correlated with the probability of buying a car from that company.

The second study, Bernard et al. (2018), looks at the effect of stock ownership of four companies on purchasing decisions in an experimental setting. In this study, 280 graduate students are randomly assigned to receive stock from Starbucks, Microsoft, Procter & Gamble, or 3M and, after several months, are asked to answer a survey in which they report their purchasing history at Starbucks (ideally based on their card monthly statements), and their views about the company. The study shows that, among coffee-drinkers, receiving Starbucks stock leads them to purchase more of Starbucks products. In contrast to those two studies, this paper looks at a naturally occurring setting that elicited participation of a broad cross-section of the US population and a large panel of automatically collected financial transactions. We use quasi-random variation in the distribution of stock from approximately 100 companies in 34 retail categories that are familiar to most customers (e.g., Walmart, Target, McDonalds, Starbucks, Gap, and Macy's among many others).

We document that our causal effect of stock ownership on individual spending is brought about by an increase in loyalty toward a specific brand or company. Our results thus contribute to the growing literature studying the effect of customer capital or brand loyalty on firm fundamentals and asset prices. Larkin (2013) studies the relation between brand perception and cash flow stability. She shows that firms with higher brand loyalty have lower cash flow volatility. Dou et al. (2019) find that firms whose brand loyalty depends more on talent are riskier and have higher expected returns.

Since our analysis is based on a rewards program where rewards could be perceived as gifts, our results also relate to the literature on reciprocity (Falk, 2007), which documents that non-monetary incentives or gifts can have larger impacts than monetary incentives of comparable value. In a controlled field experiment, Kube et al. (2012) recruited workers to catalog books from a library on a temporary basis. They find that incentivizing workers with in-kind gifts (thermos bottles) triggered substantial reciprocity in the form of increased productivity, whereas an equivalent wage increase (20% of the hourly wage) did not lead to increases in productivity. However, gift exchange findings measured in the field were sometimes inconclusive and contradictory (Kessler, 2013). In our setting, we test the role of company stock as a currency for reciprocity.

Our findings are consistent with the supersizing effect of reciprocity on incentives, as they are quantitatively difficult to explain via the monetary value of the rewards. As we discussed, while we are not able to directly compare stock and cash rewards, our measured effects are larger than those of cash-back rewards as documented in the literature (Vana et al., 2018). Stock ownership as a gift is likely to be particularly powerful if individuals are subject to cognitive dissonance and illusion of control, two psychological mechanisms for which evidence exists. Additionally, individuals may become more familiar with the company when they consume more of the company's products.

Our paper is also related to the growing literature on consumer spending using data from new online financial platforms, often called FinTech apps (see Goldstein et al., 2019, for a literature survey), such as Gelman et al. (2015), Baker (2018), Kuchler and Pagel (2019), Olafsson and Pagel (2018), Medina (2020), and Koustas (2018). In the domain of stock market investments, our paper is specifically related to research papers using bank account spending and income data linked with securities trades and holdings data such as Meyer and Pagel (2018) and Loos et al. (2018). In contrast to looking at spending responses to income shocks, nudges, or capital gains, we examine spending responses to rewards in the form of company stock. To that end, our paper is related to new technologies in advising consumers, rewarding consumer behavior, or targeting marketing efforts, e.g., D'Acunto et al. (2019), Vallee and Zeng (2019), Aridor et al. (2020), and Chen et al. (2019).

We organize the remainder of this article in the following way: Section 2 describes the FinTech app setting and our empirical design. Section 3 presents our empirical spending results. Section 4 contains robustness checks, and Section 5 shows survey evidence. Section 6 discusses in detail the psychological mechanisms that are consistent with our findings, and Section 7 concludes our study.

2 Setting, data, and empirical strategy

Subsection 2.1 describes the FinTech app, Subsection 2.2 describes the data used, and Subsection 2.3 discusses the empirical strategy.

2.1 FinTech app setting

Bumped is a loyalty platform that rewards its users with fractional stock from the (online or offline) stores where they make purchases.[3] To receive a user account, individuals first have to sign up for a waitlist on the company's website. At the time of signing up for the waitlist, interested users provide their email addresses and names. No additional information is provided at that time. In turn, on a first-come, first-served basis, users are invited to open a brokerage account. If users failed to open an account when approved, two reminder emails were sent. Once users sign up for an account, they can link all of their checking and credit card accounts. In turn, customers can select their favorite companies in a number of retail spending categories.[4]

All featured companies are divided into 34 different retail categories, and users can select one company from each category. If users then spend at their selected companies' stores, they receive fractional shares of the companies as a reward. Customers can switch their selected companies every 30 days, but only up to three times per year. The functionalities of the brokerage account are limited. Users are not allowed to deposit their own money or purchase additional stocks, but they can sell their (individual or entire) positions at any time, in which case the cash proceeds are transferred to a linked bank account.

Figures A1 and A2 show several screenshots of the FinTech app. Figure A1 shows screenshots of the company selection, switching companies, and linked card screens. In the linked card screens, one can see which transactions were rewarded by stocks. All eligible and ineligible transactions can be seen in the transactions screen in Figure A2. Additionally, this figure shows two screenshots of the portfolio containing the stock rewards the user received and their current value as well as their daily changes. As part of a promotional program, some users received stock grants upon signing up. Figure A3 shows the push notification a user receives upon getting a stock grant.

[3] We are aware of two more companies that reward consumer spending with equity. The first one is called stash.com. This platform offers a membership service and provides their users with a new debit card. Users are rewarded with stock from the companies corresponding to the brands and stores they buy from using the debit card provided by stash.com. The second one is called upromise.com. Members of this platform accrue credits on eligible purchases that are directed to a 529 account for college savings.

[4] We have to note that the Bumped business model majorly changed in the Fall of 2020 (the end of our sample period is March 2020). Instead of distributing stock rewards when users shop at certain stores, users are now signing up to receive certain stock-back promotions as part of the platform, e.g., they sign up for receiving 2% in stock-back after spending at Macy's. In turn, they receive stock rewards from their favorite four companies or a broad-based stock market ETF (VTI).

2.2 Data

Baseline demographics

We received an anonymized subsample of the user base. As of March 2020, our data subsample includes 11,424 users. The dataset includes de-identified information on financial transactions and demographic characteristics, including each user's age, gender, and 5-digit ZIP code.[5] Figure 3 shows the number of users that we observe in each US ZIP code. It is seen that there is considerable geographic variation across the country. To ensure that our empirical results are not driven by transactions being observed after but not before sign-up, we perform checks to exclude linked cards that might be observed imperfectly. We exclude all linked cards with less than 2 transactions in the four two-week periods either before or after the opening account week, before and after the waitlisted weeks, or before and after the grant weeks. These 8-week windows correspond to our estimation period. Additionally, we exclude all months in which there were less than 5 days with spending. The 5-day threshold is commonly used in other research papers using transaction-level data to ensure completeness of records (e.g., see Kuchler and Pagel, 2019; Olafsson and Pagel, 2018; Ganong and Noel, 2019). The first step reduces our sample of linked cards by 6,759 cards, taking it from 26,813 cards to 20,054 cards. The second step reduces our sample of spending days by another 15% from 7,829,699 to 6,771,353 observations. After these adjustments, we have a total of 9,005 users. Summary statistics of the full sample are presented in Table A1. Summary statistics for the adjusted sample are reported in Table 1. We can see that 68% of users are male, the average age is 36 years old and the median age is 34 years. Our user population is, as often the case for Fintech app data, more likely to live in an urban area, be male, and be younger than the average American.

Dates and timeline of users

Bumped was launched in 2017, and we received users' de-identified and aggregated transactions from 2016 to 2020. We observe the dates on which users signed up for the waitlist, when they get off the waitlist and were invited to open their brokerage accounts, and when they effectively opened their brokerage accounts. While the majority of users create their accounts right when they are taken off the waitlist, some users wait a few days before doing so. To avoid selection issues in the timing of account opening after getting off the waitlist, we restrict the analysis to those users who opened their accounts within one week after they were invited to do so. Figure 1 shows the timeline of when the users in our subsample were waitlisted, invited to open their accounts, and

[5] No other personal information of users was shared for this project.

received their accounts. Table 1 shows that the users we observed had to wait an average of 4.5 months between being waitlisted and opening an account, with a standard deviation of 3.3 months.

Spending

We observe de-identified daily data on each user's spending transactions from all linked checking, savings, and credit card accounts. For all linked cards, we not only observe current transactions but also receive a 2-year history of transactions before the card was linked. We then have a flag which transactions were selected and thus eligible for rewards and whether they were actually rewarded. For each transaction after sign-up, therefore, we know whether the transaction was selected and rewarded by stocks and, if so, by how much. Note that, because of internal business operations constraints, not all selected transactions were ultimately rewarded. Finally, we have information on which companies are selected by each user and when they switched their favorite companies.

Users' average monthly total spending is $1,496, and the average total rewards are $37, as shown in Table 1. The average weekly spending is $350, while the average weekly reward to users is $0.40. Note that we only received spending transactions that were classified as belonging to a certain brand or company. In our final dataset, we have 551 different brands or companies at which our users spend, 99 of which could be selected. We do not observe other transactions such as rent payments or income receipts. However, we also received information on brokerage account transfers and ATM withdrawals for our users. 2,156 users have other brokerage accounts, primarily with Robinhood, Etrade, Ameritrade, and Schwab.

Stock grants

Starting in March 2018, some users were granted stocks of certain brands upon signing up for their accounts. Initially, users received a one-time grant of fractional shares from one chain restaurant, Red Robin. Later, users also received stock grants from other companies: Yum! brands (e.g., Taco Bell), McDonald's, Exxon Mobile, and Chevron. The grant was displayed in-app with a description and a "Thank you for choosing the company" message, and a push notification was sent to the user. The amounts and timings were decided by the marketing team. All users who opened an account and selected that brand received the stock grant at the time of the promotional program. Users had no information on the promotion at the time they signed up for the waitlist or were invited to open their accounts.

Figure 1 shows the timeline of how many users received a stock grant. A summary of the transactions of users who were part of the promotional program is given in Table A2. 1,371 users were awarded grants during or one week after the week of opening an account. Over the observation period, users who received stock grants spent \$519 per week on average. The average grant amount was \$10. The distribution of grants was quasi-random, as users were not informed in advance of the promotional program and thus could not select into it endogenously.

We perform a covariate balance test between grant recipients and non-recipients before they get off the waitlist. In Table A6, we can see that grant recipients are very comparable to non-recipients in a number of observable characteristics, including age and eligible and ineligible spending. The only statistically significant difference is in terms of the number of transactions per month. Grant recipients perform 328 transactions per month compared to 301 transactions by non-receivers. We argue that, while statistically significant, the difference is not economically significant. Given that the spending data was not observable to the company before account opening, it could not affect whether or not users received a stock grant.

Comparison to other datasets

In Table 2, we compare our sample to the Consumer Expenditure Survey (CEX). Since this survey is performed at the household level, we normalize spending dividing by the average household size of 2.52. Relative to the average head of household in the CEX, our users are younger and more likely to be men. Our users spend \$1,496 per month on average whereas the average American spends \$2,205 during the same time period. We note that our data includes spending only on 551 identified brands (99 of which are the publicly traded companies that individuals can select, the others may be public or private companies). All transfers, e.g., for rent or utilities, are left out. After taking that into account, we argue that the spending levels of our users are broadly similar to those in the CEX.

In turn, we correlate our spending data with the Safegraph-provided card-level spending data from Facteus. Facteus partners with banks to use a synthetic data process to create a synthetic version of their transaction data. The process obfuscates each transaction to protect individual privacy and ensure a zero exact match possibility. Mathematical noise is injected into key data record attributes; however, when the data is analyzed in aggregate, it retains 99.97% of the statistical attributes of the original dataset. Most transactions are debit card transactions primarily from mobile-only banks with no physical branches. Because of this, the spending likely reflects lower-income and younger consumers. Nevertheless, it is likely a broader fraction of the population than our Bumped users.

In Table 3, we show in a simple regression that our users' brand-level spending data is strongly positively correlated with the Safegraph card spending data. The estimated coefficient of regressing our spending data in certain brands (relative to total spending) on the Safegraph card spending data in the same brand (relative to total spending) corresponds to the raw correlation coefficient, as we normalize the spending data by their standard deviations. These correlation coefficients between the Bumped and Safegraph spending data are 0.476 and 0.442 at the daily and weekly levels, respectively. The estimated coefficients are highly significant, and the adjusted R squared is around 20%. Once we include brand and date fixed effects, we also find high correlations between the two measures within a brand and on a given day or within a given week. We take these results as indicative that our users' spending behavior is broadly consistent with the spending behavior of a more representative sample of the population.

2.3 Empirical strategy

To identify the treatment effect of account opening, we exploit the quasi-random assignment of accounts for users that had signed-up to the waitlist. Specifically, we aggregate the data to the user-week level, keeping track of all eligible and ineligible spending. (In)eligible spending, before and after account opening, is defined as spending in companies' stores that users (do not) select upon account opening. In turn, we run the following specification to look at the response in eligible and ineligible spending upon receiving a brokerage account:

$$Spending_{Eligible}^{iw} = \alpha_i + \eta_w + \sum_{\tau=-8,\ldots,8} \beta_{Bumped}^{\tau} \omega_{Bumped}^{iw\tau} + \epsilon^{iw} \tag{1}$$

In Specification 1, $Spending_{Eligible}^{iw}$ denotes eligible spending (i.e., spending with a company that the user elects at sign-up) by user i in week w, α_i is an individual fixed effect, η_w is a week-by-year fixed effect, and $\omega_{Bumped}^{iw\tau}$ is an indicator of whether user i in week w had received his or her account in his or her user-specific τ's week. The coefficients β_{Bumped}^{τ} thus tell us the path of eligible spending after the user received his or her account. The omitted category of this regression are all weeks outside of the window of +/- 8 weeks.[6] We normalize coefficients to represent deviations relative to the last week before account opening. We estimate this equation for all users as well as separately for users who received a stock grant and those who did not.

This specification allow us to identify the treatment effect of opening a Bumped brokerage account on spending. The identifying assumption is that deviations from average spending on any given week is uncorrelated with the time from the week of account opening. We argue this is plausible and likely to be the case because, while users chose when to sign up to the waitlist, they

[6] In the unbalanced panel, we observe spending for up to 104 weeks before account opening and 96 weeks after account opening.

were not aware of when they would be taken off the waitlist, they remained on the waitlist for an average of 4.5 months, and the time on the waitlist is uncorrelated with user characteristics. Since our identifying variation relies on changes in spending around a random event within individuals, and for specific spending categories, we are able to identify the treatment effect of account opening on spending even when individuals endogenously choose which categories they will be rewarded on. The reason being that it is unlikely that individual users would be able to delay their spending on those categories to match the time of account opening.

Additionally, we report results of a variant of this specification in which we include one dummy for the first 8 weeks after account opening and one dummy for all other weeks after account opening as well as individual and week-by-year fixed effects.

We also run the following specification to look at the response in eligible and ineligible spending (overall and at the companies' stores for which users received the stock grants) upon receiving the stock grant:

$$Spending^{iw}_{Eligible} = \alpha_i + \eta_w + \sum_{\tau=-8,\ldots,8} \beta^{\tau}_{Grant}\omega^{iw\tau}_{Grant} + \epsilon^{iw} \tag{2}$$

In Specification 2, $Spending^{iw}_{Eligible}$, α_i, and η_w are defined as in Specification 1. In turn, $\omega^{iw\tau}_{Grant}$ is an indicator of whether user i in week w had received the grant in his or her τ's week. For users that never received a grant, $\omega^{iw\tau}_{Grant}$ is always zero, but their data is included to identify time fixed effects. The coefficients β^{τ}_{Grant} thus tell us the history of eligible spending before and after a user received the stock grant, which coincides with the date of account opening. We consider 8 weeks before and after individuals received the grant and look at all eligible spending as well as spending in the granted companies' stores. Ineligible spending in this specification is defined as spending in companies that are in categories for which the user received a grant but that were not selected at sign up.

Additionally, we report the results of a variant of this specification in which we include one dummy for the 8 weeks after grant receipt and one dummy for all other weeks after account opening as well as individual and week-by-year fixed effects.

In addition to estimating the treatment effect of account opening on spending, we also estimate the treatment effect of receiving a stock grant on spending. In contrast to the spending response to a rewards account, the magnitude of the spending response to receiving a stock grant does not involve an expectation of additional stock in exchange of consumption: the grants were distributed without prior notice, and as a one-time promotion.

To estimate the treatment effect of receiving a stock grant on consumption, we first use the same specification as in Equation 1, but we split the sample into users who received a stock grant and those who did not. Second, we formally compare the differential responses in the spending of these two groups with the following difference-in-difference specification:

$$Spending^{iw}_{Eligible} = \alpha_i + \eta_w + \sum_{\tau=-8,\ldots,8} \beta^{\tau}_{B} \omega^{iw\tau}_{Bumped} + \sum_{\tau=-8,\ldots,8} \beta^{\tau}_{BG} Grant_i \omega^{iw\tau}_{Bumped} + \epsilon^{iw} \tag{3}$$

In Specification 3, $Spending^{iw}_{Eligible}$, α_i, η_w and $\omega^{iw\tau}_{Bumped}$ are defined as in Specification 1. In turn, $Grant_i$ is a binary variable taking the value of one when a user received a grant at the time of account opening. The coefficients β^{τ}_{BG} thus identify the incremental effect of receiving an account and a grant relative to the effect of receiving an account without a grant, β^{τ}_{B}, in each user-specific τ's week. We consider 8 weeks before and after individuals received the grant. We estimate Equation 3 for both overall eligible spending as well as restricted to the specific companies for which stock was granted.

Finally, as a placebo test, we estimate the following specification to look at the response in eligible and ineligible spending upon signing up to be waitlisted for an account:

$$Spending^{iw}_{Eligible} = \alpha_i + \eta_w + \sum_{\tau=-8,\ldots,8} \beta^{\tau}_{Waitlist} \omega^{iw\tau}_{Waitlist} + \epsilon^{iw} \tag{4}$$

In Specification 4, $Spending^{iw}_{Eligible}$, α_i, and η_w are defined as in Specification 1. In turn, $\omega^{iw\tau}_{Waitlist}$ is an indicator of whether user i in week w was waitlisted in his or her τ's week. The coefficients $\beta^{\tau}_{Waitlist}$ thus tell us the history of eligible spending before and after a user signed up for the waitlist. We consider 8 weeks before and after individuals signed up for the waitlist.

In all specifications, standard errors are clustered at the individual level.

3 Results

3.1 The effect of stock ownership on spending

3.1.1 Account opening analysis

As a starting point, Figure 4 plots the raw data means of eligible and ineligible spending 8 weeks before and after account opening. Here, we look at the ratio of spending relative to each individual's mean average over the entire 16-week period. Thus, the axis shows the percentage deviation of spending relative to each individual's average. We can see in this raw-data plot that eligible spending increases by approximately 40% in the week of account opening and stays high. A large spike is visible in eligible spending, while there is no major change in ineligible spending. Note that, while the coefficient estimates are increasing before the week of account opening, we do not observe a statistically significant pre-trend (the figure displays standard errors, not 95% confidence intervals).

Figure 5 shows the β^{τ}_{Bumped} coefficients and standard errors from Specification 1 for both eligible spending as well as ineligible spending as the left-hand side variables. Spending is measured as the individual-level percentage deviation from the sample average eligible spending in a given week. The coefficients thus represent the percentage deviation in eligible spending before and after users received their accounts. We can clearly see a pronounced spike in eligible spending in the week that users receive their accounts. Weekly spending at selected companies' stores jumps up by 40% and stays persistently high for the 8 weeks we look at. In terms of US dollars, eligible spending averages $56 per week, so this corresponds to approximately a $22.4 increase in spending per week. Additionally, we do not see a comparable pattern in ineligible spending. For ineligible spending, we can rule out a decrease larger than 5% in the weeks after account opening from a basis of $295 per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than $15. Additionally, in the regression specifications we will discuss in Subsubsection 3.1.3, we can rule out any decrease and we can show that total spending increases.

3.1.2 Stock grant analysis

Figure 6 shows the β^{τ}_{Bumped} coefficients and standard errors from Specification 1, splitting the sample into grant receivers and non-receivers. In both cases, we can see again a clear increase in eligible spending along the order of 40% following account opening.

We also present the results from estimating Specification 2 in Figure 7 for both eligible spending in general and eligible spending at the companies' stores of which users were granted stock as the left-hand side variables. As before, the coefficients thus represent the percentage deviation in eligible spending before and after users opened their accounts and received their stock grants. We can clearly see an increase in overall eligible spending in the week after users received their grants of about 40%, which equals the account opening effect. Additionally, eligible spending at the brands for which the user received a grant increases even more by about 100%.

Figure 8 shows the β^{τ}_{BG} coefficients and standard errors from Specification 3. We present the results for (in)eligible spending at the brands for which users received stocks as the left-hand side variables. Spending is measured as the individual-level percentage deviation from the 8-week estimation period average eligible spending in a given week. The coefficients thus represent the incremental effect of receiving an unexpected stock grant at the time of account opening as a percentage deviation of weekly spending before and after users received their stock grants. Figure 8 shows a pronounced effect on spending in brands corresponding to the stock that was granted. The incremental effect in spending is in the order of 200% initially; this is followed by a decrease, and then we observe another increase. Again, we do not observe a statistically significant pre-trend in Figure 8 (the figure displays standard errors, not 95% confidence intervals).

3.1.3 Regression analyses

As a complement to the figures, Column 1 of Table 4 shows the average effect of stock ownership on spending for the 8 weeks following account opening. With this alternative estimation, we obtain a 38% increase in eligible spending relative to the sample average of each individual. Column 2 shows a 3.6% decrease in ineligible spending with a standard error of 2.3%; we can thus rule out a decrease of more than 8.2% in ineligible spending with statistical confidence. Consistent with these estimates and the previous analysis suggesting that not all of the additional eligible spending is offset, we find that total spending increases by 4.4%. Columns 5 and 6 show the results for eligible spending in granted companies' stores, and we can document a 93.4% increase in spending with a negligible effect on ineligible spending in the granted categories, i.e., spending in companies that are in categories for which the user received a grant but that were not selected at sign up.

Columns 1 and 2 of Table A3 show a similar analysis, but, in this case, we directly use dollar spending per week as the dependent variable. Receiving stock rewards leads to substantial increases in average spending per week, in this case an increase of $19 per week in eligible spending and an insignificant $8.4 increase in ineligible spending. Total spending increases significantly by 27 USD per week, which is approximately equal to the sum of the point estimates of eligible and ineligible spending. Finally, Table A4 shows similar effects for log spending instead of the absolute amounts or the percentage deviations from each individual's mean. Note that, we log the spending amounts in this regression but keep values between zero and one as such. In this specification, we can rule out any decrease in ineligible spending with statistical confidence and confirm total spending increases significantly.

Table A3 also shows the results of changes in spending upon receiving a stock grant in dollar terms instead of deviations from weekly averages. Columns 5 and 6 show a mean effect of a $1 increase in eligible spending and a reduction of $0.67 in ineligible spending. Note that the average effect is very small in dollar terms because the number of weeks that the average user frequents a specific store, e.g., McDonalds, is very small. In contrast, in Table A4, we find significant increases of spending in the granted company's stores in Columns 5 and 6.

3.1.4 Long-term effects

We look at the effects of eligible and ineligible spending further out than 2 months. When we consider 3 or even 6 months after account sign-up, we find some dissipation but still a significant increase in eligible spending, as can be seen in Figure 10. When we look at this longer estimation window, weekly spending at selected companies' stores jumps up by 40% and stays persistently high for 3 to 6 months. In terms of US dollars, eligible spending averages $56 per week, so this corresponds approximately to a $23 increase in spending per week. Note that these long-term effects are naturally disseminating if users switch their favorite companies in certain retail categories. In these figures, we only take the initial pick of companies as the measure of eligible spending.

3.1.5 Sample splits by retail categories

Next, we study the spending response of account opening by category. We focus on the six most popular spending categories: groceries, burgers, coffee, superstores, ride share, and drugstores. Figure 9 shows increases in eligible spending between 30% and 100% relative to the average weekly spending during the window of analysis. Superstores are the only category that shows a substantial decrease in spending after an initial jump in the two weeks immediately after account opening. The results for ineligible spending are mixed, with some categories like coffee showing substantial offsetting effects and some others (the majority) showing a flat response to account opening on ineligible spending.

3.1.6 Sample splits by quasi-random variation in rewarded transactions and by size of rewards

Due to company operations and constraints, only an average of 70% of eligible spending transactions were actually rewarded.[7] Because whether or not an eligible transaction was actually rewarded is plausibly exogenous to the user, we can exploit variation in the fraction of eligible spending that was rewarded to see if receiving more rewards leads to differential effects on eligible spending compared to receiving less rewards.

[7]Note that, only eligible spending was ever rewarded; ineligible spending was never rewarded. In our previous analysis, when we look at eligible spending, we look at spending in selected categories rather than spending that was actually rewarded in the pre- and post-periods of account opening.

In Figure A4, we look at the sample splits of terciles of individuals being rewarded many versus fewer eligible spending transactions. Here we basically see the same initial spike and persistent response in eligible spending for those users who were rewarded relatively less versus more.

In addition to this quasi-random variation in the number of transactions that were rewarded, we can split the sample into three subsamples based on the reward amount as a fraction of eligible spending. This split into reward percentages is not random because users can choose to spend in low- versus highly-rewarded categories or companies. We show in Figure A5 that the results stay the same whether reward percentages are small, medium, or large.

3.1.7 Sample splits by login activity

We also look at heterogeneities as a function of login activity, which we use as a proxy for attention to financial accounts. Figure A6 presents the results of estimating Equation 1 after splitting the sample into terciles of login counts per user. Across the spectrum of the attention distribution, eligible spending shows an increase to the order of 40% in the weeks following account opening. Users in the high attention category show larger spikes, reaching up to a 60% increase in eligible spending in week 6.

3.2 Spending and stock ownership outside of the rewards platform

We also document that daily and weekly spending in certain brands for our user population is correlated with holdings of that company's stock among Robinhood brokerage clients (the holdings data is obtained from robintrack.net). We chose Robinhood brokerage account data because Robinhood is the most common other broker of our clients. Additionally, Robinhood clients at large are likely a similar population to Bumped users.

Similar to our previous empirical strategy, we look at the daily and weekly deviation of spending in a certain brand relative to the total amount of spending on that day or in that week. We also look at holdings of a certain brand or company relative to all other holdings of all other companies.

In Table 5, we find that a 1% increase in holdings of a certain company is correlated with spending in that company's stores by 0.12%, controlling for company and date fixed effects. Aggregated to the weekly level, this coefficient increases to 0.14%. We thus find a very strong positive correlation between spending and stock ownership in the observational data.

In turn, we run the same analysis using the Safegraph-provided card-level spending data from Facteus that we described as part of the representativeness discussion in Section 2.2.

In Table 6, we can see that the results line up sensibly. The Safegraph card spending data is positively correlated with Robinhood holdings at the daily and weekly levels as well. After including brand and time fixed effects, the correlations are a bit lower for the Safegraph spending relative to the Bumped spending. This likely reflects the fact that the Bumped population is more interested in stocks (similar to Robinhood clients) than the overall population of younger bank customers, as in the Safegraph data.

We argue that this result helps us to extrapolate our findings to actual spending and stock ownership in brokerage accounts. Our results provide us with a causal estimate of the relationship between spending and stockholdings. In turn, we also find that this relationship exists via observational data from spending and holdings in brokerage accounts.

3.3 Impact on brokerage account transfers

To look at brokerage account transfers, we first flag all ACH account transfers that are categorized as financial.[8] We assert that these are transfers to brokerage accounts. For a subset of these transfers, we know the broker. The most common broker is Robinhood. Additionally, users also broker with Ameritrade, E-trade, and Schwab. Table 7 Column (1) shows the log of brokerage amount transfers post 8 weeks of opening a user account, controlling for all future weeks post 8 weeks after account opening, week-by-year fixed effects, and user fixed effects. It can be seen that log brokerage account transfers increase by 2.7% in the 8 weeks of account opening. shows that the likelihood of brokerage account transfers by a user post 8 weeks after opening an account increases by 1.4%. Relative to the baseline propensity to invest, these coefficients represent an increase of approximately 5% in the amount and likelihood of investing.

As a robustness check, we ensure that we see similar results for those transfers that indicate Robinhood as the brokerage account (Column (3)) and compare that to those with non-Robinhood brokerage accounts (Column (4)). Relative to the baseline propensity to invest, the effects are larger for Robinhood transfers, which is likely due to us misclassifying some ACH transfers as brokerage account transfers.

These results indicate that users not only spend more on companies that give them stocks as rewards, but they also increasingly transfer funds to their brokerage accounts. The stock rewards likely engage users with the stock market on a more frequent and regular basis, which increases their propensity to invest.

[8] An ACH transfer is an electronic, bank-to-bank transfer processed by the Automated Clearing House network.

4 Robustness checks

4.1 Substitution from cash spending

There is a concern that users might be withdrawing money after opening an account or may substitute from cash transactions to card transactions. We look at net ATM withdrawals to address these two possibilities. Table 8, Column (1) shows the net withdrawal ATM amount post 8 weeks of opening an account, controlling for all future weeks post 8 weeks of account opening, week-by-year fixed effects, and user fixed effects. Column (2) looks at the percentage deviation of ATM net withdrawal amounts. The insignificant coefficients in both columns suggest that increases in spending in selected companies' stores for the average user are not associated with a decrease in ATM withdrawals.

4.2 Placebo tests

Figure A7 shows the $\beta^{\tau}_{waitlist}$ standard errors for both eligible and ineligible spending as the left-hand side variables. Again, spending is measured as the individual-level percentage deviation from the sample average eligible spending in a given week. The coefficients thus represent the percentage deviation in eligible spending after users signed up to be waitlisted. As expected, there is no clear pattern in eligible or ineligible spending in the week that users chose to sign up and were waitlisted.

Note that this specification can be seen as a placebo check. We would not expect a response in either type of spending when individuals waitlist. At the time of being waitlisted, individuals do not have much information about which companies are granting stock or which categories they can select companies from.

4.3 Randomization checks for time waitlisted and grant receipt

One of our identification strategies is based on the staggered distribution of brokerage accounts to users who were on a waitlist. We argue that users were not able to predict when they would get out of the waitlist and, as such, could not reasonably time their expenses to match the opening of their brokerage accounts. To assess the validity of this strategy, we first note that the time between being waitlisted and receiving an account is long and exhibits substantial variation across individuals (with a mean of 135.06 days and standard deviation of 97.95 days). Furthermore, accounts were awarded on a first-come, first-served basis in lot sizes determined by the company's business objectives and constraints. When individuals waitlist, only their names and email addresses are recorded. No other pieces of information, including demographic and financial characteristics, were recorded. As a result, the time spent on the waitlist can be considered quasi-random (uncorrelated with user characteristics) and hard to predict by individual users.

To confirm this, we perform a randomization test regressing the time on the waitlist on individual-level characteristics. Column 1 of Table A5 shows that neither age, nor gender, nor spending patterns before account opening are significant predictors for the number of days spent on the waitlist. Furthermore, the R squared is very low (0.0023). As a predictability test, we add a number of fixed effects in Columns 2 and 3 to see how the R squared changes. We find that the R squared remains low. The one exception is fixed effects for week-by-year of account opening. Including these fixed effects increases the R squared to 0.27. However, in contrast to the potential impact of user characteristics, it is unlikely that individual users had expectations regarding how long it would take to get off the waitlist. We thus conclude that users would not be able to predict when they would receive their accounts, and it would be difficult for them to time their spending to coincide with the week in which they received their accounts.

Similarly, we also perform a randomization or covariate balance test between grant recipients and non-recipients before they were taken off the waitlist. In Table A6, we can see that grant recipients are very comparable to non-recipients in a number of observable characteristics, including age as well as eligible and ineligible spending.

4.4 Sample splits by time on waitlist

Additionally, we study heterogeneous treatment effects based on the time spent on the waitlist. We do so by splitting the sample according to when individuals were waitlisted. The results for receiving an account for three terciles of the time individuals were waitlisted can be found in Figure A8. There are no discernable differences.

5 Stock ownership and self-reported loyalty: Survey evidence

We analyze the responses to four surveys sent to Bumped users between 2019 and 2020. The surveys were designed and administered by the Bumped team. The number of respondents varies across the surveys, ranging from 455 to 672 respondents per survey. The specific questions in each survey are also different, which is why the number of users responded to each survey question ranges from 1,160 to 2,217. In this section, we discuss questions that help us understand the characteristics of users and their attitudes toward stock ownership and financial markets. For exposition purposes, we modify the original numbering of the questions. Survey responses do not contain identifiers to link them to the transaction data, and take place only after users sign up to the platform. As a result, we cannot apply our identification strategy to analyze the survey data. We nevertheless argue that the responses are informative of the financial sophistication of Bumped users and of the reasons behind increases in spending.

The first question asks users about their use of different types of financial accounts outside of the rewards platform.

Q1. Do you own stock outside of Bumped? If so, where?

A1.1 Employer-sponsored retirement funds (401k, IRA, etc.)

A1.2 Investments through other apps (Robinhood, Stash, etc.)

A1.3 Traditional or managed investment account

A1.4 Something else

The left panel of Figure 11 shows the fractions of users that responded "yes" to each of the 4 options presented. The right panel of Figure 11 shows the distribution of users according to the number of positive answers provided, which is indicative of the number of different financial accounts held. We can see that the vast majority of survey respondents have at least one financial account outside of the rewards platform. The majority have between 2 and 4 accounts, suggesting that their exposure to the stock market is not limited to their stock rewards.

The second question asks users about their attitudes toward the brands for which they received stock rewards.

Q2. Since signing up for Bumped... (select all that apply)

A2.1 I feel more loyal to the brands that I get rewards from

A2.2 I feel a more positive attachment toward the brands I get rewards from

A2.3 I have told my friends about companies I own through Bumped

A2.4 I have shopped less with competitors of companies I own through Bumped

A2.5 I have paid more for something because I am an owner of the company through Bumped

A2.6 I have traveled farther or gone out of my way to shop at companies I own through Bumped

A2.7 None of the above

A2.8 Something else (free text)

Figure 12 shows that, since starting to use the app, more than 65% of survey respondents report feeling more loyal toward the brands they receive rewards from, 40% report feeling a more positive attachment towards those brands, almost 45% of users shop less with competing brands, 16% report paying more because they are owners of the brand, and 46% report going out of their way or traveling longer distances to shop on brands they own. The responses are consistent with our spending results and with the results of Aspara (2009). Overall, these responses suggest that stock ownership leads to increased brand loyalty, which we argue explains the large spending response to stock rewards.

The third question asks users about their likelihood of investing outside of Bumped in the future as a result of owning stock through Bumped. Figure 13 shows that more than 52% of users responded that they are more likely to invest outside of Bumped in the future as a result of owning stock through Bumped. Again, these results corroborate our empirical findings that receiving stock rewards leads to more engagement with the stock market in general.

Q3. Does owning stock through Bumped make you more likely to invest outside of Bumped in the future?

A3.1 No

A3.2 Maybe

A3.3 Yes

The fourth question asks users about their preferences for stock rewards over other types of rewards on a Likert scale.

Q4. In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)?

A4.1 Significantly less excited

A4.2 Less excited

A4.3 About the same

A4.4 More excited

A4.5 Significantly more excited

We recognize that the sample of survey respondents is already selected, since individuals decide to open a Bumped account are by definition excited about stock rewards. Figure 14 that, not surprisingly, survey respondents strongly prefer stock rewards over traditional rewards such as cash back, points, or coupons. However, we note that even among this selected sample, there is variation in the level of excitement about stock rewards. We use this cross-section variation to proxy for variation in non-pecunary benefits of stock ownership .Table 9 correlates the answers to Question 4 with the answers provided in Questions 2 and 3. The correlation of questions about loyalty and engagement with the stock market with “excitement" about stock rewards is informative about the role of the non-pecuniary benefits of stock ownership on loyalty and stock market participation. Variation in excitement about stock rewards can be interpreted as variation in the non-pecuniary benefits of stock ownership. Therefore, a positive and significant relation between excitement about stock rewards and loyalty can be interpreted as a positive relation between the non-pecuniary benefits of stock ownership and loyalty.

To code the responses to Question 4, we include one dummy variable for every level of the Likert scale in the right-hand side of the estimation equation. The omitted category represents users who report less or significantly less excitement for stock rewards over other types of rewards (we pool those two categories, since very few survey respondents selected them). Columns 1 to 5 show that a preference for stock rewards is a strong predictor of loyalty, i.e., self-reported loyalty, positive attachment feelings, paying a higher price, and traveling farther to spend at the brands individuals own.

Similarly, Column 6 shows that a preference for stock rewards over traditional rewards is also a strong predictor of increases in the likelihood of investing outside of Bumped as a result of owning stock through Bumped. Finally, in Column 7, we test whether the correlation in Column 6 is concentrated for users who do not invest already in different financial instruments. We interact the continuous Likert scale measuring preferences for stock rewards with the number of financial instruments reported in Question 1 and regress the interaction terms with the corresponding main effects on the self-reported increases in the likelihood of investing outside of Bumped as a result of owning stock through Bumped. We find that the correlation of preferring stock rewards and investing outside of Bumped is present across the distribution of the number of financial accounts reported in Question 2 (the main effect of preferring stock is positive and significant, while the interaction coefficient is not statistically significant). Users who invest in several or few financial accounts outside of Bumped are equally likely to increase their likelihood of investing outside of Bumped as a result of owning stock through Bumped. Overall, these results suggest that there are non-pecuniary benefits of owning stock and that, the larger these benefits are, the larger the effect is on both spending and stock-investing activity.

In addition to describing users' attitudes toward stock rewards, the surveys administered by the platform management were also used to identify complaints from users. In an open-ended question, users were asked if there was anything else they would like to tell Bumped. Only 3% of respondents reported having issues linking their cards, which further suggests that the set of accounts actually linked provides a reasonable picture of the spending patterns of platform users.

Finally, users were then asked in an open-ended format to explain why they feel more excited about stock rewards compared to traditional rewards. Here, we quote some of the answers that highlight the impact of ownership on loyalty and brand preferences.

> "Drives much more loyalty. Impacts behavior more."

> "I like knowing that I own shares in the companies I shop at, it enhances my loyalty."

> "I feel attached to a company and feel as if it is a mutual benefiting relationship. As I help the business out, they provide something in return that directly correlates in their success."

> "I feel like I am part of the company when I own shares of it. I like to benefit[s] from their success."

6 Psychological mechanisms

As we mentioned in the Introduction, our findings are unlikely to be purely explained by a price effect. First, the transaction and hassle costs of changing which stores are frequented this substantially seem larger than the 0.5% to 2% magnitudes of the stock rewards. Second, we do not find variation in the increase in eligible spending depending on how generously spending is rewarded, as discussed in Subsubsection 3.1.6.

We also discussed how our effects are larger than those documented for cash rewards. Vana et al. (2018) calculate that, when an additional \$1 in cashback payment is offered, spending increases by \$3.51, entailing an effectiveness of 351%.[9] In comparison, stock rewards have an

[9] Vana et al. (2018) separate the effect of cash-back rewards into two components. The first component relates to the effect of one additional dollar of cash-back offers on spending, where individuals spend to receive the reward offer. The second component captures the effect of effectively receiving a cash-award reward on future spending on the same brand. Both components are jointly estimated with a panel of individual-level spending in a random effects model. To identify the second component (which is the focus of their paper), the authors use quasi-random variation in the time of actually receiving the cash-back reward. The calculation of the total effect (inclusive of both components) as discussed above is taken from their appendix.

effectiveness of 2,053%: we find a \$23 increase in weekly eligible spending when the average amount offered in stock rewards is \$1.12 per week (stock rewards are about 2% of weekly eligible spending, which in turn averages \$56 during the observation period).

We argue that, in addition to the monetary rewards, stock ownership has a large effect on spending because users feel loyal towards the companies that they own. As shown in Figure 12, 68% of individuals subscribe to the statement that they "feel more loyal to the brands they get rewards from." We now discuss the different psychological mechanisms that might trigger and enhance feelings of loyalty.

Affect and gift exchange Individuals tend to rely on affective feelings when making decisions (Slovic et al., 2007). A reward in the form of stocks is likely to accentuate the feelings of affect that the individual has toward the company and to positively influence their consumption decisions (Li and Petrick, 2008). The award of shares should be perceived by the customers as a gesture of goodwill. This perception is expected to enforce the affection of the shareholders and, in turn, alter their behaviors to positively impact the company. Stock owners are likely to identify more closely with the firm (Turner and Tajfel, 1986) and with the shareholder community.

Similarly, gift exchange can also enhance the effectiveness of rewards. Gift exchange refers to the phenomenon of more value being placed on the same objects if they are acquired or received as gifts. It typically refers to altruistic behavior where the identity and intentions of the sender matter (see Kube et al., 2012). In our setting, users are involved in a transactional relationship by which they get rewarded in exchange for specific behavior. But if users perceive the stock rewards as a gift that ultimately came from companies that cooperated with Bumped, then gift exchange would be a relevant mechanism behind the effects we see.

The stock rewards and grant promotional program are funded and administered by Bumped. However, that is not obvious to customers, and we argue that they assume the companies are funding their stock rewards and grants. This is consistent with the visible spending response not only in all eligible spending but specifically in spending at those companies for which individuals received grants.

Looking at the survey responses in Figure 12, we find suggestive evidence for individuals feeling affect towards the companies (rather than Bumped) in response to receiving stock rewards. 40% of individuals subscribe to the statement that they "feel a more positive attachment toward the brands they get rewards from."

In our setting, the currency of gift exchange are stocks, which results in an additional sense of ownership of the company. This ownership is accompanied by psychological mechanisms, such as cognitive dissonance, illusion of control, and familiarity. There exists evidence that these mechanisms are relevant for investors, which we will discuss now.

Cognitive dissonance By cognitive dissonance, we refer to the mental discomfort that people derive from simultaneous but conflicting beliefs or behaviors. This discomfort leads to an alteration in either the beliefs or behaviors in order to reduce the dissonance and restore balance (Bénabou and Tirole, 2011; Festinger, 1962; Gilbert et al., 1998; Gilbert and Ebert, 2002). In the context of stock ownership, investors experience cognitive dissonance when they take actions that do not support the invested-in company. To ease the discomfort, shareowners can change their beliefs by, for example, acknowledging that their individualistic choices are not important enough to tip the scales for the firm. Alternatively, investors could change their behaviors in a way that is favorable for the company (e.g., by purchasing the company's products, paying more for them, and avoiding buying substitute products from a competitor).

Looking at the survey responses in Figure 12, we find suggestive evidence for cognitive dissonance. First, the answer to the question "I have told my friends about companies I own through Bumped" measures individual willingness to acknowledge privately and publicly which companies they own. In turn, the answers to the questions "I have shopped less, paid more, or traveled farther ..." all measure willingness to engage in behaviors that benefit the companies that individuals own.

Illusion of control Receiving the shares of a certain company may make individuals believe that their purchasing decisions actions are able to affect the company's stock price. Despite atomistic behaviors having a very small probability of affecting tangible outcomes (Feddersen, 2004), by believing so, investors tend to make decisions that could positively affect the company's value. The reason for this behavior is that individuals tend to overestimate the likelihood of small probability events (Lichtenstein et al., 1978; Fox and Tversky, 1998) and their ability to influence events they demonstrably cannot (Langer, 1975; Chang et al., 2016). While individual spending could not effectively impact stock prices, the illusion of control could also trigger the perception of alignment of incentives. Giving stock rewards could pay a similar role to that of stock compensation in corporations (Hochberg and Lindsey, 2010; Oyer and Schaefer, 2005; Altinkemer and Ozcelik, 2009), if individuals believe that their individual spending actions can influence firm outcomes.

Again, in Figure 12, we find suggestive evidence for illusion of control. The answers to the questions "I have shopped less, paid more, or traveled farther ..." all measure willingness to engage in behaviors that benefit the companies individuals own. After all, when individuals say they paid more because they are owners, they are likely subject to an illusion of control that this benefits the company in a meaningful way. Additionally, some of the quotes provided by platform users in the open-ended questions of the survey (as shown in Section 5) are consistent with them having an illusion of control.

Familiarity Prior research suggests that customer-stockholders are subject to a familiarity bias, i.e., they tend to gain more exposure to the stocks they know. As a result, familiarity-biased investors hold portfolios containing a fewer number of stocks (Cao et al., 2009) and are less well diversified (Heath and Tversky, 1991; Huberman, 2001; Keloharju et al., 2012). It can be argued that investors are more active in collecting information about the invested-in company and that, in turn, they become more familiar with the products they offer. Hartzmark et al. (2019) provide a model and survey evidence for this idea. An increase in familiarity, in turn, can breed increases in spending by providing new information about the benefits of the product, and by leveraging gift exchange, cognitive dissonance, or illusion of control channels (Zajonc, 1980; Moreland and Zajonc, 1982).

7 Conclusion

In this study, we quantify the effects of receiving stocks from certain companies on spending in the companies' stores. We use data from a new FinTech app called Bumped that opens brokerage accounts for their users and rewards them with company stock when they shop at previously selected brands and stores in several retail categories. For identification, we use the staggered distribution of brokerage accounts over time after individuals signed up for a waitlist. To lend credibility to our identification strategy, we show that the average time spent waitlisted equals 4.5 months, perform a randomization test for time waitlisted, and split our sample by the time users spent on the waitlist. Finally, we show that there is no spending response to users waitlisting. Additionally, we utilize the fact that users received stock grants of certain companies at different points in time as part of a promotional program.

We show that customers increase their spending at the selected companies' stores after receiving stock rewards in their brokerage accounts. Weekly spending at selected companies' stores jumps up by 40% and stays persistently high for 3 to 6 months. In terms of US dollars, eligible spending averages \$54 per week, so this corresponds to approximately a \$23 increase in spending per week. In different specifications, we can rule out decreases in ineligible spending with statistical confidence and also show that overall spending increases.

When users are granted a certain company's stock, we find a weekly spending response of

100% at the companies' stores for which individuals received stock grants. For these users, we also find a more persistent eligible spending response to account opening, as the grant was received in that same week. Finally, for internal company reasons, not all eligible spending was ultimately rewarded. We thus use variation in the amount of spending that got rewarded to show that users respond more persistently if they get rewarded on a more consistent basis.

We argue that our findings cannot be fully explained by a pure price effect, i.e., we would not expect individuals to change their spending behavior in such a material way in response to rewards ranging from 0.5% to 2% and we do not find that user behavior varies with the rewarded amounts. Consistent with this presumption, we estimate considerably larger effects of stock rewards than those documented for cash rewards. Using survey evidence and data on transfers to brokerage accounts, we argue that loyalty is the dominant psychological mechanism explaining the spending responses. Feelings of loyalty could be triggered by gift exchange and affect, familiarity, illusion of control, and reductions in cognitive dissonance.

When interpreted along with the existing literature that documents the effect of brand loyalty on investments (Cohen, 2009; Aspara, 2009), our results suggest that stock ownership leads to additional spending in the company's stores because investors feel loyal towards the company. Such loyalty could result in more stable cash flows and increases in firm value (Larkin, 2013; Dou et al., 2019). Additionally, our results suggest that stock ownership, stock prices, and behavioral biases in investing affect spending in a direct way rather than just affecting trading in brokerage accounts. This results in a direct relationship between stock ownership and consumption, a direct component of utility. The relationship between utility and stock prices may in turn help explain the equity premium puzzle.

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Figures and tables

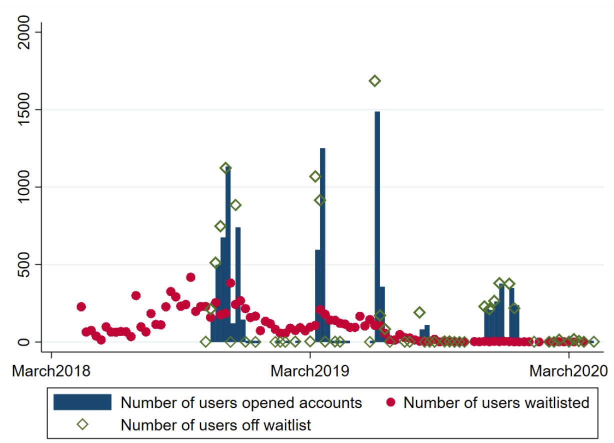


Figure 1: Number of users in our data subsample who were waitlisted and received an account over the timeline of our sample

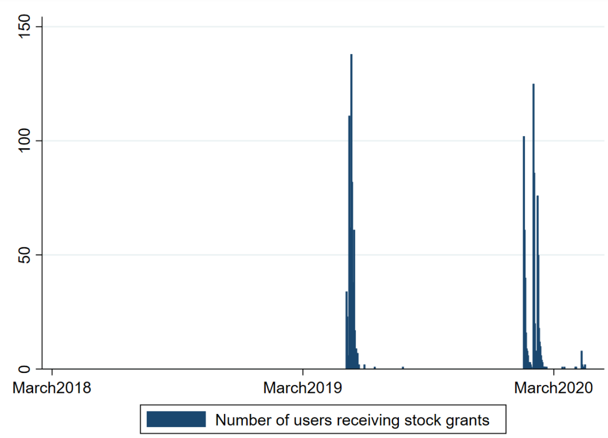


Figure 2: Number of users in our data subsample who received stock grants over the timeline of our sample

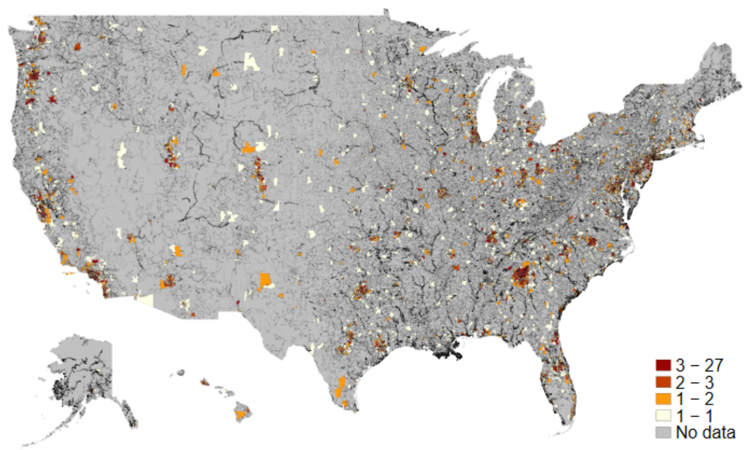


Figure 3: Users by 5-digit zip code in the US

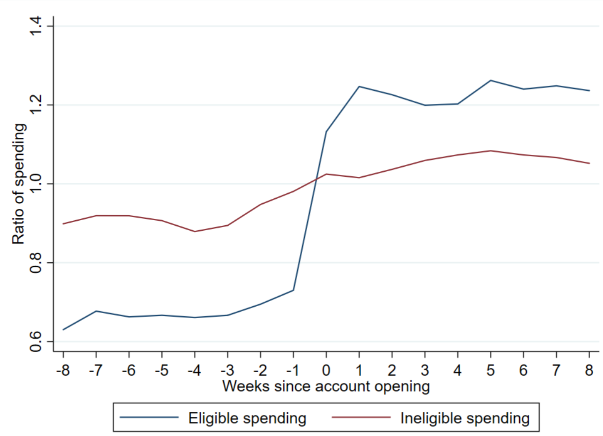
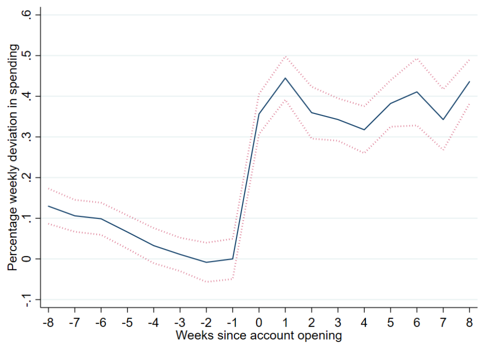
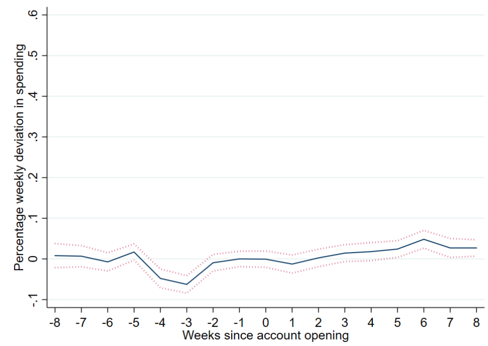


Figure 4: Ratio of eligible and ineligible spending by week relative to average individual-level eligible and ineligible spending over the 16-week window before and after week zero reflecting the week of account opening

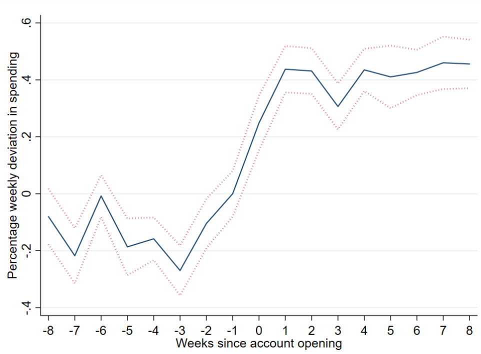


Eligible spending

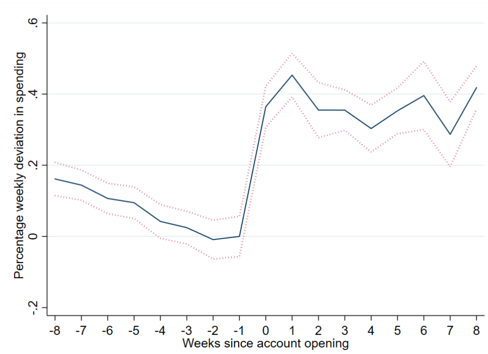


Ineligible spending

Figure 5: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for both eligible and ineligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.

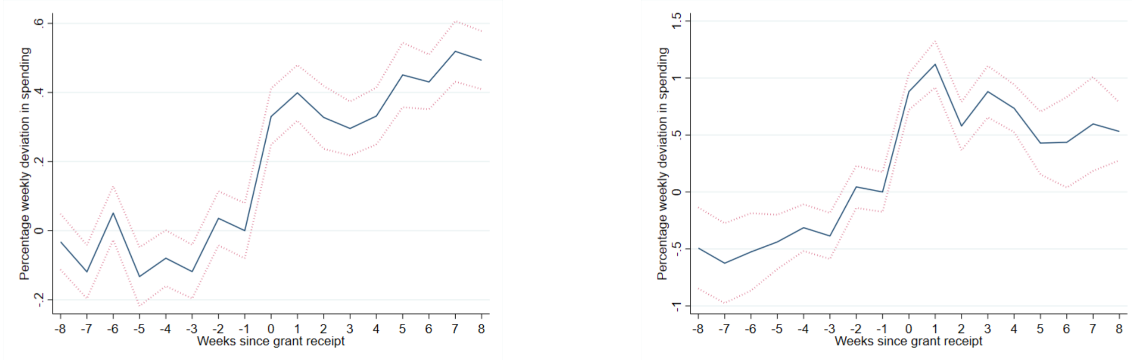


Eligible spending for grant recipients



Eligible spending for grant non-recipients

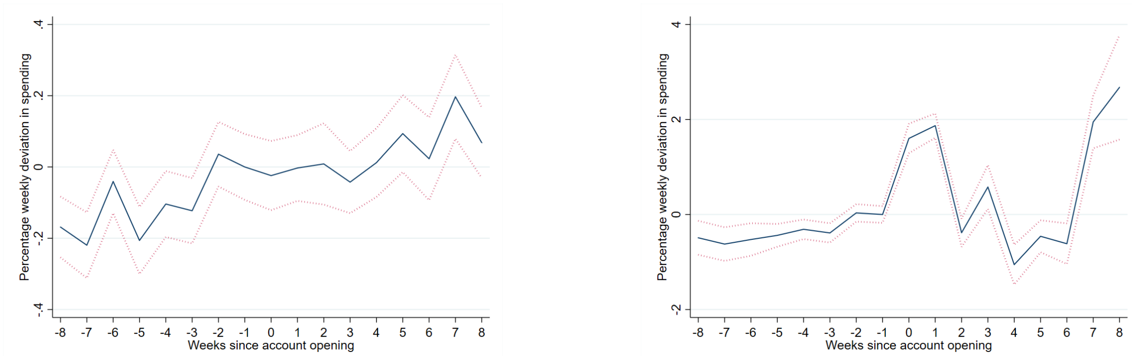
Figure 6: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean) separately for individuals who received the grant and those who did not. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



Eligible spending

Eligible spending at companies of which stock was granted

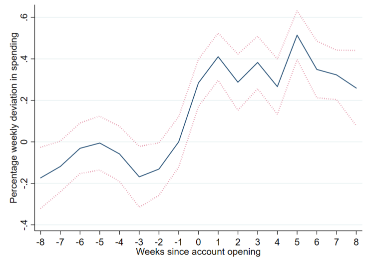
Figure 7: This figure shows the coefficient estimates β^{τ}_{Grant} in Specification 2 for both eligible overall spending and eligible spending at the companies' stores of which users received stock grants (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals received the stock grant. Standard errors are shown as the dotted lines and clustered at the individual level.



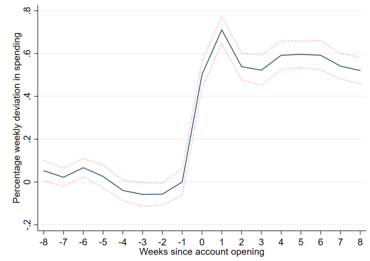
Incremental effect of grant receivers on eligible spending

Incremental effect of grant receivers on eligible spending in granted companies' stores

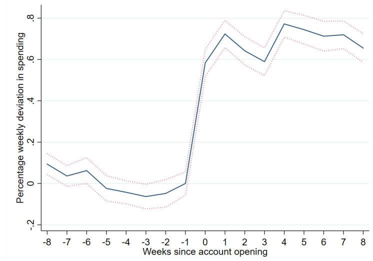
Figure 8: This figure shows the coefficient estimates β^{τ}_{BG} in Specification 3, i.e., the incremental effect of grant receivers on all eligible and grant company spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals received the account and stock grant. Standard errors are shown as the dotted lines and clustered at the individual level.



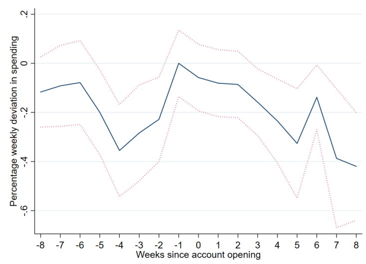
Grocery – Eligible spending



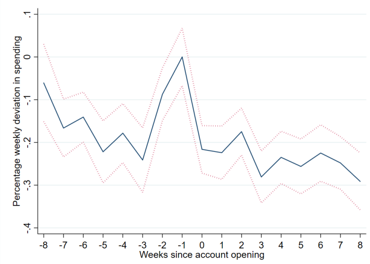
Burgers – Eligible spending



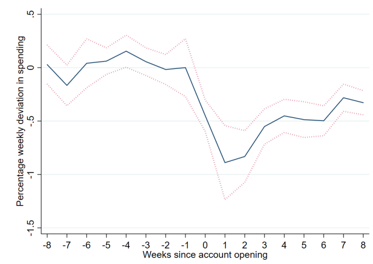
Coffee – Eligible spending



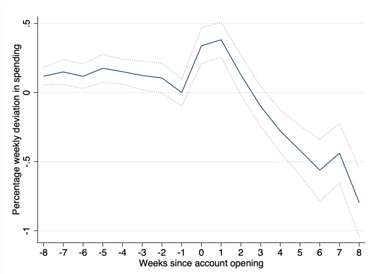
Grocery – Ineligible spending



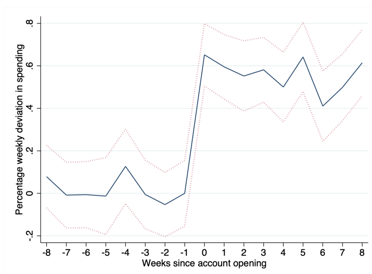
Burgers – Ineligible spending



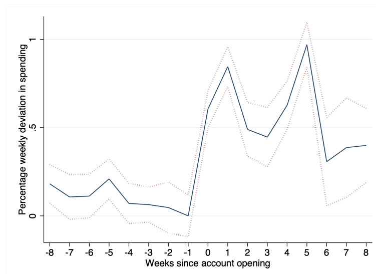
Coffee – Ineligible spending



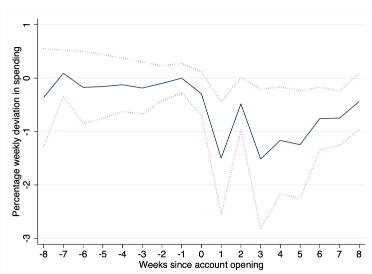
Superstores – Eligible spending



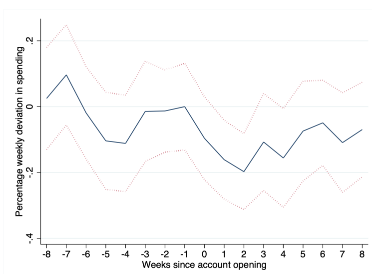
Ride Share – Eligible spending



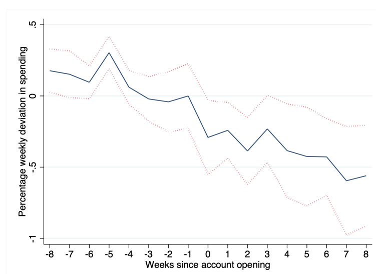
Drug Stores – Eligible spending



Superstores – Ineligible spending

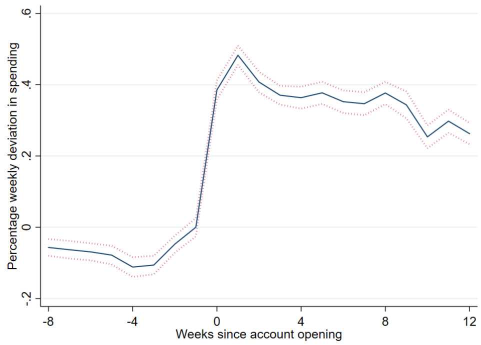


Ride Share – Ineligible spending

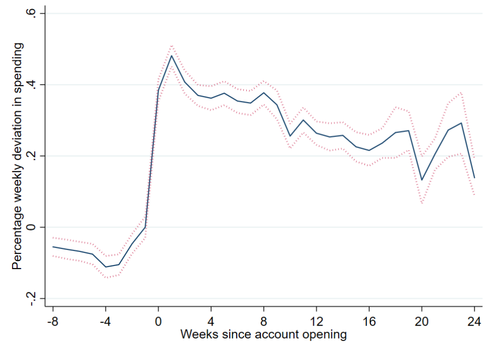


Drug Stores – Ineligible spending

Figure 9: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for the six most popular rewards categories, which are grocery, burgers, coffee, superstores, ride share, and drug stores. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.

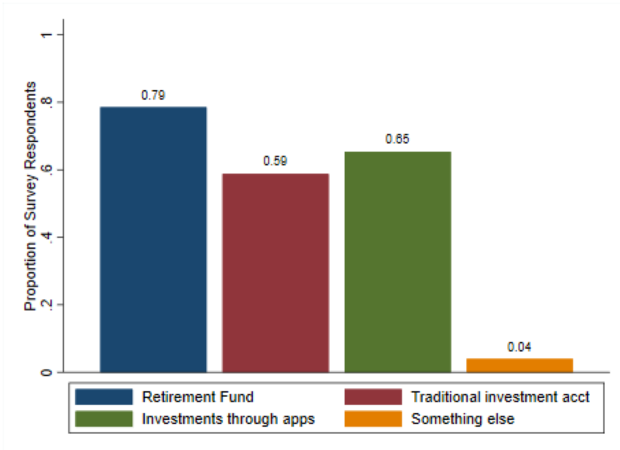


Eligible spending 3 months after account opening



Eligible spending 6 months after account opening

Figure 10: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 3 and 6 months after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



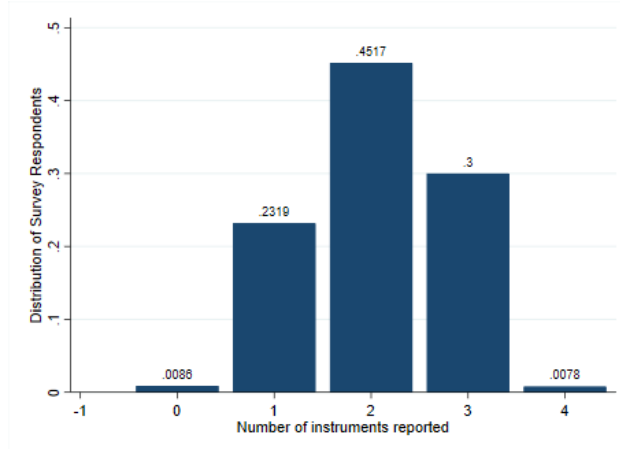


Figure 11: This figure shows the responses to a survey of 1,160 users who were asked about their investing experience, "Do you own stock outside of Bumped? If so, where? 1. Employer-sponsored retirement funds (401k, IRA etc), 2. Traditional or managed investment account, 3. Investments through other apps (Robinhood, Stash etc), 4. Something else." Since users were allowed to select more than one category, the right panel shows the distribution of number of different categories (or accounts) selected.

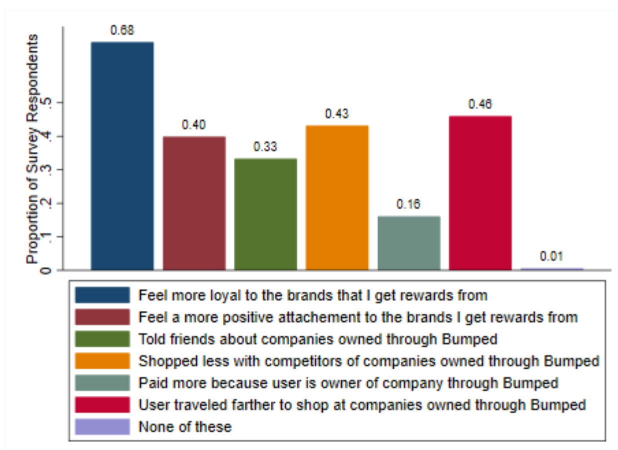


Figure 12: This figure shows the responses of 1127 users who were asked to select all that applies for the following question: "Since signing up for Bumped... 1. I feel more loyal to the brands that I get rewards from, 2. I feel a more positive attachment to the brands I get rewards from, 3. I have told my friends about companies I own through Bumped, 4. I have shopped less with competitors of companies owned through Bumped, 5. I have paid more for something because of owning a company through Bumped, 6. I have traveled farther or gone our of my way to shop at companies owned through Bumped, 7. None of the above 8. Something else."

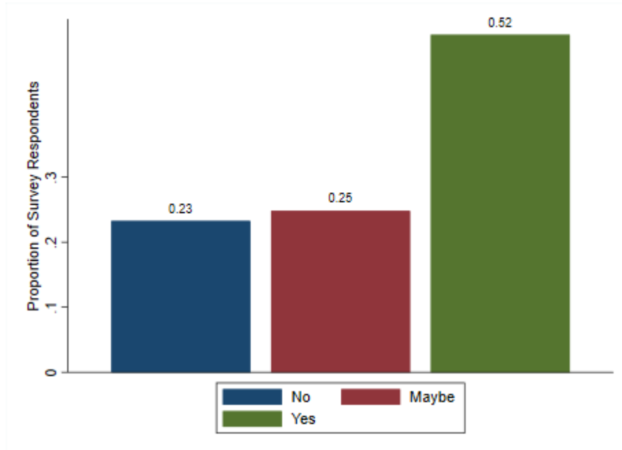


Figure 13: This figure shows responses of 1,160 users who were asked the following question: "Does owning stock through Bumped make you more likely to invest outside of Bumped in the future? 1. No, 2. Maybe, 3. Yes."

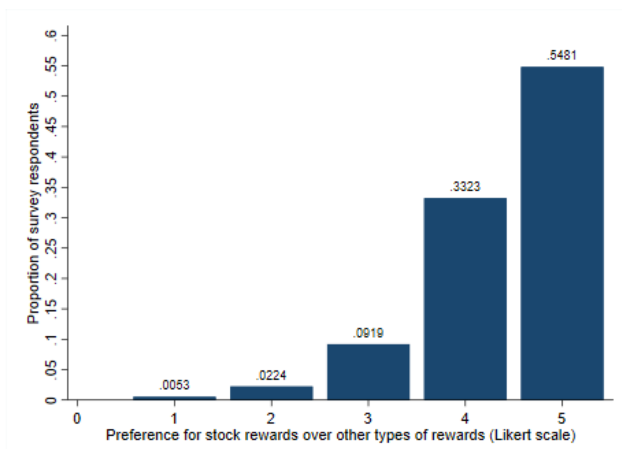


Figure 14: This figure shows responses of 2,287 users who were asked: "In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)? 1. Significantly less excited than traditional rewards, 2. Less excited than traditional rewards, 3. About the same as traditional rewards, 4. More than traditional rewards, 5. Significantly more than traditional rewards."

Table 1: Summary statistics of Bumped users who open their account the same week in which they got off the waitlist or a week after post adjustments to data

	Mean	Std dev	25th percentile	50th percentile	75th percentile
Age	36	9.4	29	34	41
Male	.68	.47	0	1	1
Days from waitlist to open	135	98	70	115	162
Monthly user logins	4.6	9.7	1.4	2.2	3.8
Weekly user logins	2.1	2.9	1	1.3	1.8
Number of transactions	751	749	287	581	1,013
Number of cards linked	1.9	1.2	1	2	2
Total monthly spending	1,496	3,455	648	1,074	1,741
Total weekly spending	350	805	153	252	409
Monthly eligible spending	237	910	55	138	285
Weekly eligible spending	56	211	13	32	67
Monthly ineligible spending	1,258	3,293	530	880	1,440
Weekly ineligible spending	295	767	124	206	339
Grant weekly elgible spending	1	7.2	0	0	0
Grant weekly inelgible spending	22	348	0	0	0
Monthly eligible spending - grocery	49	130	0	0	30
Monthly ineligible spending - grocery	64	151	1.4	16	71
Monthly eligible spending - superstores	31	102	0	0	9.4
Monthly ineligible spending - superstores	78	236	4.5	23	80
Monthly eligible spending - ride sharing	14	46	0	0	8.9
Monthly ineligible spending - ride sharing	20	48	0	2.8	18
Total rewards	37	61	6.8	19	47
Monthly rewards	1.7	2.3	.44	1	2.2
Weekly rewards	.4	.53	.1	.24	.51
Total rewarded/eligible	.69	.26	.5	.74	.92
Monthly rewarded/eligible	.61	.28	.39	.58	.89
Weekly rewarded/eligible	.65	.27	.43	.67	.91
Monthly user brokerage transfers	2,001	10,264	200	545	1,561
Weekly user brokerage transfers	1,424	9,697	134	340	974
Monthly ATM withdrawals	-465	4,027	-296	64	227
Weekly ATM withdrawals	-233	1,731	-163	44	132
Observations	9005				

Notes: 9,005 Bumped users in the final dataset pass the following tests: All linked cards have more than 36 weeks of at least 2 transactions per week and 5 transactions per month around the waitlist, account open, and grant dates. The week of account opening equals the week when the user was off waitlisted or a week after off waitlist. The week of grant receipt equals the week of account opening or a week after. If selections are made before account opening, the opening date of the account is shifted to the date of selection by the user. Total number of transactions and spending (in USD) are calculated per user and include amounts before and after account opening. Spending only includes transactions that were classified as belonging to a certain brand (551 different brands are in the final dataset (in 34 retail catagories) of which 99 can be selected to be rewarded). Spending does not include ATM withdrawals. Brokerage transfers include all ACH transfers that are classified as finance or investments, belonging to an identifiable broker, or belonging to an investment services app. Rewards are in USD.

Table 2: Comparison of summary statistics with the Consumer Expenditure Survey (CEX)

Variable	Consumer Expenditure Survey 2018	Bumped users
Age	51.1	36
Men	0.47	0.68
Monthly spending	2,205	1,496
Monthly grocery Spending	148	114
Monthly restaurant spending	114.4	32
Monthly transportation spending	27	34
Monthly drug spending	16	23.7

Notes: The Consumer Expenditure Survey 2018 is conducted at the household level. Figures in Column (1) are obtained by dividing those numbers by the average household size of 2.52 for comparison with individual-level Bumped data in Column (2).

Table 3: Estimation results of brand spending by Bumped users on Safegraph card spending of that brand

	Daily spending in brands relative to total spending Bumped users			Weekly spending in brands relative to total spending Bumped users		
Daily spending in brand relative to total spending Safegraph data	0.476^{***} (0.007)	0.243^{***} (0.016)	0.240^{***} (0.016)			
Weekly spending in brand relative to total spending Safegraph data				0.442^{***} (0.015)	0.705^{***} (0.040)	0.705^{***} (0.041)
Brand fixed effects		✓	✓		✓	✓
Date or week-by-year fixed effects			✓			✓
Observations	19396	19396	19396	3528	3430	3430
Adj. R squared	0.212	0.886	0.883	0.195	0.938	0.936

Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: In this specification we regress the total daily (weekly) spending in all publicly traded brands of all Bumped users on spending in those brands from the Safegraph card spending data. Date (week-by-year) fixed effects refer to any day (week) of the sample period and brand fixed effects for any publicly traded brand. The time period and selection of brands/tickers is constrained by the Bumped data, however, not all tickers could be matched to the brand spending information in the Safegraph data and we only kept unambiguous matches of the top 200 spending brands in the Safegraph data. The relative Bumped and Safegraph spending data are normalized by their respective standard deviations.

Table 4: Estimation results of spending ratios post account opening or receiving the grant

	All spending			Spending on grant brands	
	Eligible	Ineligible	Total	Eligible	Ineligible
Post 8 weeks	0.384***	-0.036	0.044**	0.666***	-0.043
	(0.068)	(0.023)	(0.020)	(0.142)	(0.103)
Post more than 8 weeks	0.695***	-0.059	0.035	0.816***	0.095
	(0.147)	(0.056)	(0.044)	(0.261)	(0.398)
Constant	1.071***	1.157***	1.076***	0.819***	1.207***
	(0.088)	(0.033)	(0.027)	(0.142)	(0.205)
User fixed effects	✓	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓	✓
Observations	229204	235683	235810	14329	30163
Adj. R squared	0.167	0.121	0.091	0.037	0.059

Standard errors are clustered at the user level. *** p<0.01, ** p<0.05, * p<0.1
Notes: In this specification we regress the ratio of eligible and ineligible spending overall and specifically in granted companies' stores on a post 8 weeks dummy, which takes a value of 1 for transactions during or within 8 weeks of post account opening or receiving the grant, and on a post more than 8 weeks dummy, which takes a value of 1 for transactions more than 8 weeks post account opening or receiving a grant and 0 otherwise. User fixed effects and week-by-year fixed effects are included.

Table 5: Estimation results of Bumped users brand spending on Robinhood clients weekly holdings of that brand

	Daily spending in brands relative to total spending Bumped users			Weekly spending in brands relative to total spending Bumped users		
Daily number of holdings in brand relative to total holdings Robinhood clients	0.176*** (0.007)	0.133*** (0.009)	0.119*** (0.010)			
Weekly number of holdings in brand relative to total holdings Robinhood clients				0.213*** (0.018)	0.152*** (0.015)	0.136*** (0.016)
Brand fixed effects		✓	✓		✓	✓
Date or week-by-year fixed effects			✓			✓
Observations	26958	26958	26958	4155	4155	4155
Adj. R squared	0.022	0.891	0.889	0.032	0.951	0.950

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: In this specification we regress the total daily (weekly) spending in all publicly traded brands of all Bumped users on the daily (weekly) holdings of that company by Robinhood brokerage clients data obtained from robintrack.net. Date (week-by-year) fixed effects refer to any day (week) of the sample period and brand fixed effects for any publicly traded brand. The sample time period is May 2018 to March 2020.

Table 6: Estimation results of Safegraph brand spending on Robinhood clients weekly holdings of that brand

	Daily spending in brands relative to total spending Safegraph card spending			Weekly spending in brands relative to total spending Safegraph card spending		
Daily number of holdings in brand relative to total holdings Robinhood clients	0.270*** (0.014)	0.052*** (0.008)	0.043*** (0.009)			
Weekly number of holdings in brand relative to total holdings Robinhood clients				0.160*** (0.029)	0.074*** (0.008)	0.074*** (0.009)
Brand fixed effects		✓	✓		✓	✓
Date or week-by-year fixed effects			✓			✓
Observations	19396	19396	19396	3528	3430	3430
Adj. R squared	0.019	0.975	0.974	0.008	0.990	0.990

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Notes: In this specification we regress the total daily (weekly) spending in all publicly traded brands of all Safegraph card spending data on the daily (weekly) holdings of that company by Robinhood brokerage clients data obtained from robintrack.net. Date (week-by-year) fixed effects refer to any day (week) of the sample period and brand fixed effects for any publicly traded brand. The time period and selection of brands/tickers is the same as in Table 5, however, not all tickers could be matched to the brand spending information in the Safegraph data and we only kept unambiguous matches of the top 200 spending brands in the Safegraph data.

Table 7: Estimation results of transfers to brokerage accounts post account opening

	Brokerage transfers	Likelihood of transfer		
	Log transfer amount	Any account	Robinhood account	Non robinhood account
Post 8 weeks	0.017**	0.012***	0.002***	0.010***
	(0.009)	(0.002)	(0.001)	(0.002)
Post more than 8 weeks	0.018	0.028***	0.002***	0.025***
	(0.012)	(0.003)	(0.001)	(0.003)
Constant	0.251***	0.249***	0.003***	0.246***
	(0.005)	(0.001)	(0.000)	(0.001)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	958207	958207	958207	958207
Adj. R squared	0.425	0.566	0.155	0.562

Standard errors clustered at the user level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: In Column (1), we regress the log of brokerage account transfers on a post 8 weeks after account opening dummy, which takes a value of 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes a value of 1 for transactions more than 8 weeks post account opening and zero otherwise. Column (1) has the log USD amount in transfers to brokerage accounts, and Columns (2) to (4) have the likelihood to transfer to a brokerage account as the outcome variables. Brokerage transfers include all ACH transfers that are classified as finance or investments, belonging to an identifiable broker, or belonging to an investment services app. Robinhood transfers are transactions that are classified as belonging to Robinhood from the transaction description. User fixed effects and week-by-year fixed effects are included.

Table 8: Estimation results of ATM withdrawals post account opening

	ATM withdrawals	
	Net withdrawal amount	Percentage deviation
Post 8 weeks	16.828	-3.086
	(13.625)	(6.864)
Post more than 8 weeks	23.419	-22.271
	(19.149)	(15.355)
Constant	-81.902***	12.995*
	(8.451)	(6.838)
User fixed effects	✓	✓
Week-by-year fixed effects	✓	✓
Observations	958207	418108
Adj. R squared	0.124	0.024

Standard errors clustered at the user level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: In Column (1), we regress net ATM withdrawal amounts on a post 8 weeks after account opening dummy, which takes a value of 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes a value of 1 for transactions more than 8 weeks post account opening and zero otherwise. Column (1) has the USD amounts in net ATM withdrawals and Column (2) has the percentage deviation in net ATM withdrawals (relative to the individual-level average) as the outcome variables. User fixed effects and week-by-year fixed effects are included.

Table 9: Correlation between self-reported preference for stock rewards, loyalty, and increases in the likelihood of investing outside of Bumped

	More Loyal (Q2)	Positive Attachement (Q2)	Shop Less with Competitors (Q2)	Paid More Because of Ownership (Q2)	Travel Further to Shop at Companies Owned (Q2)	More Likely to Invest Outside of Bumped (Q3)	More Likely to Invest Outside of Bumped (Q3)
Excited about stock rewards (Same)	0.204**	0.028	0.083	-0.009	0.139*	0.105	
	(0.092)	(0.087)	(0.082)	(0.053)	(0.080)	(0.093)	
Excited about stock rewards (More)	0.232***	0.122	0.207***	0.029	0.211***	0.213**	
	(0.083)	(0.079)	(0.074)	(0.049)	(0.071)	(0.084)	
Excited about stock rewards (Significantly more)	0.480***	0.152*	0.254***	0.131***	0.348***	0.388***	
	(0.080)	(0.077)	(0.072)	(0.049)	(0.069)	(0.082)	
Excited about stock rewards (Likert)							0.096**
							(0.046)
Number of financial instruments							-0.030
							(0.096)
Excited about stock rewards (Likert) x Number of financial instruments							0.015
							(0.021)
Constant	0.333***	0.278***	0.222***	0.083*	0.194***	0.222***	0.022
	(0.079)	(0.075)	(0.069)	(0.046)	(0.066)	(0.080)	(0.203)
Mean of dep. var	0.68	0.40	0.43	0.16	0.46	0.52	0.52
Observations	1115	1115	1115	1115	1115	1160	1160
Adj. R squared	0.090	0.005	0.014	0.021	0.031	0.046	0.046

For Columns (1) to (6), the explanatory variables consists of a set of mutually exclusive dummy variables for each value of the Likert scale of question 4: "In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)?" The omitted category are the two lowest levels of the Likert scale (pooled). In Columns (1) to (5) the dependent variable is binary, and takes a value of 1 when a user reports feeling more loyal or more positive attachment to the brands that they get rewards from, shopping less with competitors since receiving stock rewards, paying more because of ownership through Bumped or travelling further to shop at companies owned through Bumped. In Columns (6) and (7) the dependent variable is binary, and takes a value of 1 when a user reports being more likely to invest outside of Bumped as a result of owning stock through Bumped. For Column (7), the set of explanatory variables consist of the continuous Likert scale of question 4, the number of financial accounts held by each user according to question 1, and their interaction. Robust standard errors in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Internet Appendix

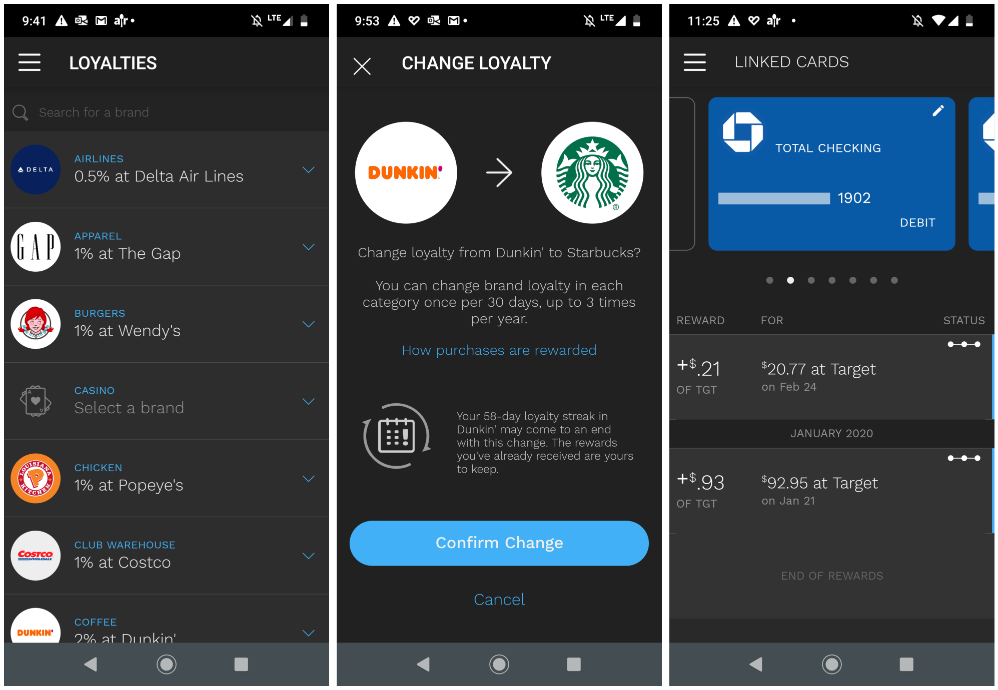


Figure A1: The Bumped app: screenshots of company selection (99 companies in 34 retail characteristics), switching companies, and linked bank accounts and credit card screens

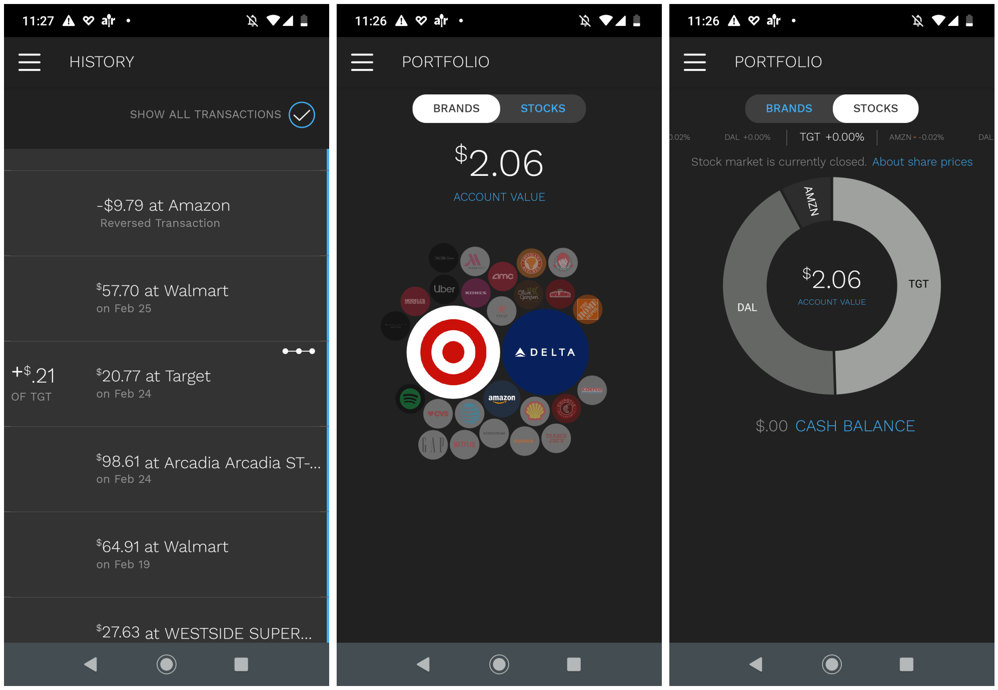


Figure A2: The Bumped app: screenshots of transactions from all linked cards (that may be eligible) and overview of the user's portfolio

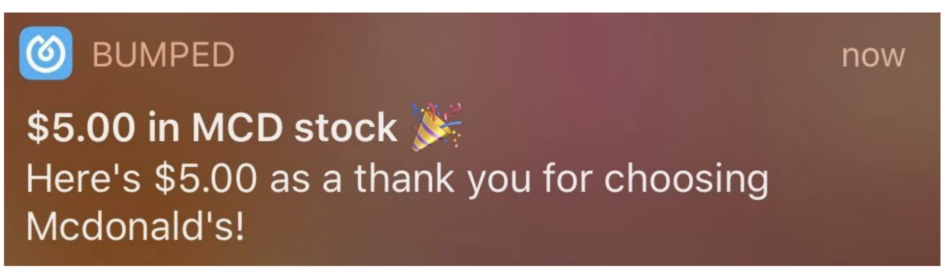
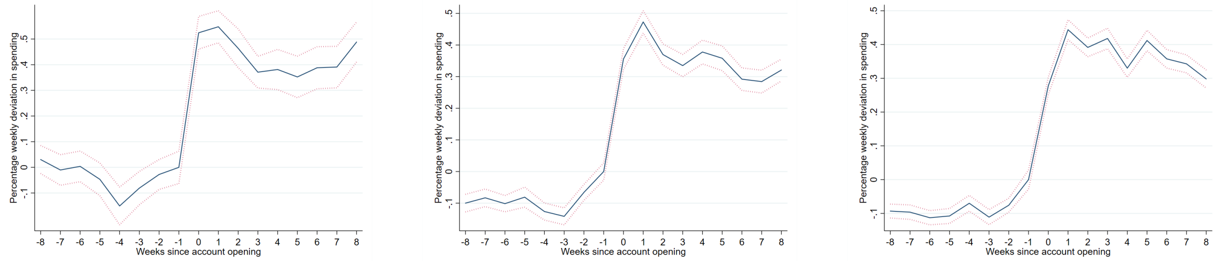
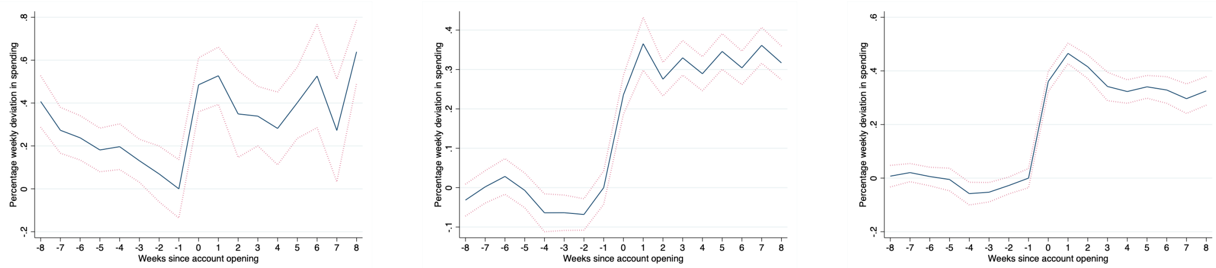


Figure A3: Stock grant notification received by users, push notification



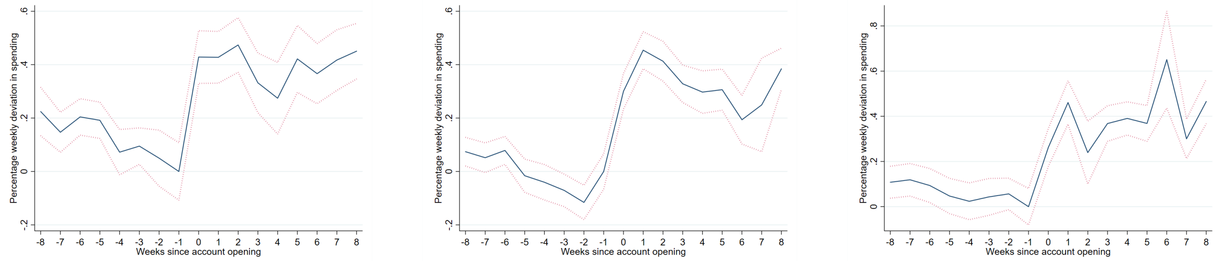
Small fraction rewarded | Medium fraction rewarded | Large fraction rewarded

Figure A4: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean) and for three terciles of actually rewarded spending transactions as a fraction of all eligible spending transactions. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



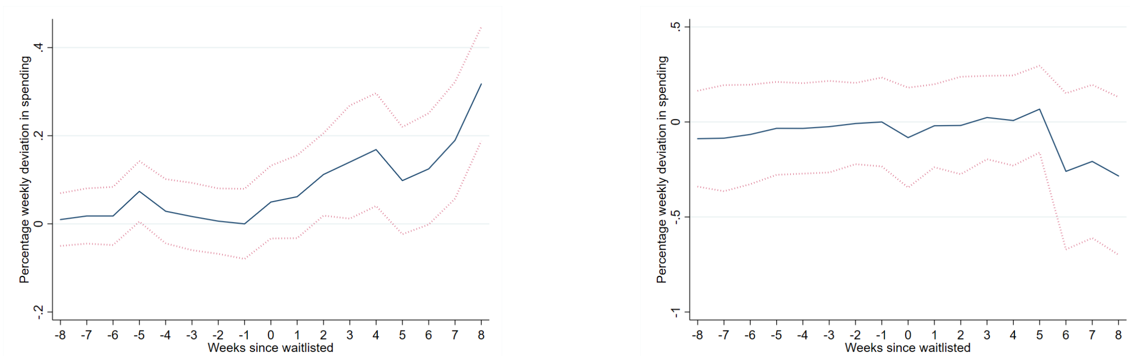
Small Rewards | Medium Rewards | Large Rewards

Figure A5: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean) and for three terciles of reward amount as a percentage of eligible spending. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



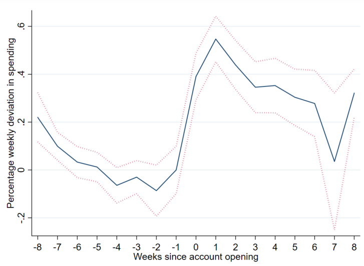
Low attention users Medium attention users High attention users

Figure A6: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for three terciles of user attention, defined by the login counts per user in the 8 weeks after account opening. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.

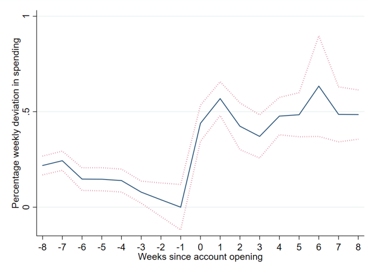


Eligible spending Ineligible spending

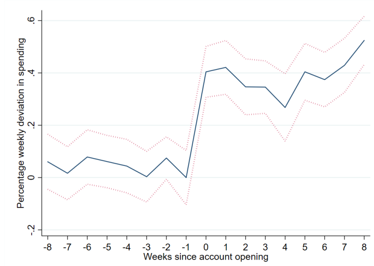
Figure A7: This figure shows the coefficient estimates $\beta^{\tau}_{waitlist}$ in Specification 4 for both eligible and ineligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals signed up for the waitlist. Standard errors are shown as the dotted lines and clustered at the individual level.



Waitlisted shortly



Medium waitlisted



Waitlisted for long

Figure A8: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean) and for three terciles of time spent being waitlisted. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.

Table A1: Summary statistics of Bumped.com users who open their account on the same week in which they came out of the waitlist, or a week after

	Mean	Std dev	25th percentile	50th percentile	75th percentile
Age	36	9.5	29	34	41
Male	.68	.47	0	1	1
Days from waitlist to open	137	99	71	116	164
Monthly user logins	4.6	9.6	1.4	2.2	3.8
Weekly user logins	2	2.9	1	1.3	1.8
Number of transactions	730	748	263	561	996
Number of cards linked	2.4	1.9	1	2	3
Monthly spending	1,795	8,733	702	1,194	1,952
Weekly spending	494	2,342	203	333	525
Weekly eligible spending	71	268	17	42	86
Weekly ineligible spending	423	2,321	165	273	439
Total rewards	37	65	6.3	18	46
Monthly rewards	2	2.9	.47	1.2	2.5
Weekly rewards	.53	.72	.13	.32	.67
Weekly rewarded/eligible	.67	.26	.47	.71	.9
Observations	9378				

Notes: This table includes users using Bumped.com who have account open week same as the offwaitlist week or a week after, which are 9,378. The total number of transactions, and spending (in USD), calulated per user include amounts before and after opening the app. Rewards are in USD.

Table A2: Summary statistics of Bumped users who open their account the same week in which they got off the waitlist or a week after and who received a grant

	Mean	Std dev	25th percentile	50th percentile	75th percentile
Age	37	9.5	30	35	42
Male	.69	.46	0	1	1
Days from waitlist to open	197	88	125	164	268
Monthly user logins	4.9	9.2	1.7	2.7	4.5
Weekly user logins	2.2	2.7	1	1.4	2
Number of transactions	595	603	177	432	847
Number of cards linked	2.2	1.7	1	2	3
Monthly spending	1,801	5,480	716	1,215	1,947
Weekly spending	519	1,860	210	342	523
Weekly eligible spending	70	165	18	42	84
Weekly ineligible spending	449	1,846	169	287	442
Total rewards	25	66	4	11	29
Monthly rewards	1.8	2.9	.39	1.1	2.2
Weekly rewards	.48	.76	.11	.29	.58
Weekly rewarded/eligible	.63	.3	.38	.7	.9
Total grant amount	10	4.2	10	10	10
Observations	1371				

Notes: Out of the 9,378 users enrolled in Bumped for whom the account opening week is the same as the off-waitlist week or a week after, 1,371 users were also part of the grant promotion program who received the grant in the week of account opening. The total number of transactions and spending (in USD) are calulated per user and include amounts before and after account opening. Spending only includes transactions that were classified as belonging to a certain brand (551 different brands are in the final dataset (in 34 retail catagories) of which 99 can be selected to be rewarded). Rewards and grants are in USD.

Table A3: Estimation results of Bumped users on spending amounts post account opening or receiving the grant

	All spending			Spending on grant brands	
	Eligible	Ineligible	Total	Eligible	Ineligible
Post 8 weeks	19.092***	8.450	27.542*	0.177	-9.433
	(1.618)	(14.172)	(14.333)	(0.225)	(11.505)
Post more than 8 weeks	22.467***	-2.779	19.688	0.783***	-2.433
	(3.012)	(17.009)	(17.449)	(0.302)	(6.059)
Constant	52.278***	330.700***	382.978***	0.822***	26.702***
	(1.907)	(12.089)	(12.344)	(0.201)	(6.588)
User fixed effects	✓	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓	✓
Observations	236639	236639	236639	236639	236639
Adj. R squared	0.783	0.272	0.304	0.128	0.171

Standard errors clustered at the user level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: In this specification we regress eligible and ineligible spending overall and specifically in grant brands on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of receiving grant, and on a post more than 8 weeks dummy, which takes value 1 for transactions more than 8 weeks post account opening or receiving grant and 0 otherwise. User fixed effects and week fixed effects are included.

Table A4: Estimation results of Bumped users on log spending amounts post account opening or receiving the grant

	All spending			Spending on grant brands	
	Eligible	Ineligible	Total	Eligible	Ineligible
Post 8 weeks	0.711***	0.157***	0.270***	0.035***	0.041***
	(0.017)	(0.016)	(0.014)	(0.004)	(0.007)
Post more than 8 weeks	0.669***	0.083***	0.208***	0.045***	0.051***
	(0.025)	(0.024)	(0.021)	(0.006)	(0.011)
Constant	2.067***	4.671***	4.958***	0.034***	0.383***
	(0.017)	(0.016)	(0.014)	(0.004)	(0.007)
User fixed effects	✓	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓	✓
Observations	236639	236639	236639	236639	236639
Adj. R squared	0.407	0.371	0.384	0.402	0.771

Standard errors clustered at the user level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: In this specification we regress log eligible and ineligible spending overall and specifically in grant brands on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of receiving grant, and on a post more than 8 weeks dummy, which takes value 1 for transactions more than 8 weeks post account opening or receiving grant and 0 otherwise. Note that when spending is zero, it is replaced with log(1+0). User fixed effects and week fixed effects are included.

Table A5: Estimation results of time on waitlist on user characteristics

	Days on waitlist	Days on waitlist	Days on waitlist
Age	-0.106	-0.114	-0.068
	(0.120)	(0.119)	(0.108)
Female	-2.084	-2.608	0.225
	(2.893)	(2.879)	(2.478)
Weekly spending	0.004	-0.000	-0.034
	(0.032)	(0.032)	(0.030)
Weekly ineligible spending	0.014	0.014	0.054*
	(0.033)	(0.032)	(0.030)
Weekly eligible spending	-0.070	-0.036	-0.029
	(0.045)	(0.045)	(0.041)
Mean of Dep. Var.	135.06	135.06	135.06
Deciles of transaction-history-lenght fixed effects		✓	✓
Week-by-year of account opening fixed effects			✓
Observations	8477.000	8477.000	8470.000
Adj. R squared	0.002	0.038	0.263

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: In this specification we regress the number of days a user was waitlisted on user characteristics. All variables are measured before account opening. Users only indicate their email address and names upon being waitlisted so none of the characteristics are observable to the company at the time of being waitlisted.

Table A6: Randomization check between grant and non-grant receivers before getting off the waitlist

Variable	Non-Grant Receivers	Grant Receivers	Difference
Age (Years)	35.88	36.73	0.85
			(0.280)
Number of transactions per user	328.70	301.28	-27.41
			(8.259)
Monthly spending	1,095.36	1,076.83	-18.52
			(78.864)
Weekly spending	285.52	304.83	19.30
			(20.217)
Eligible monthly spending	141.23	132.93	-8.30
			(23.685)
Eligible weekly spending	36.05	37.91	1.85
			(5.521)
Ineligible monthly spending	954.12	943.90	-10.21
			(74.191)
Ineligible weekly spending	249.47	266.92	17.45
			(19.244)

Notes: We test for covariate balance using a difference in means t-test by estimating equation, $y = \alpha + \beta \cdot \text{GR} + \epsilon$, where y takes on different variables as shown in each row of the table, and GR takes on a value of 1 if a user is a grant receiver and takes on 0 if the user did not receive a grant. There are 1,295 users who received a grant and 7,710 users who did not receive a grant. Column 1 and 2 present the average values of each dependent variable for non-grant and grant recipients respectively before getting off the waitlist. Column 3 shows the coefficient of the grant indicator, i.e., β and standard errors in parenthesis.