

The Effects of Stock Ownership on Individual Spending and Loyalty

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Abstract

We show that when individuals receive stocks from a specific 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 stocks 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 stock rewards increase overall investment activity and that spending in specific stores is strongly correlated with retail stock holdings of that company. A survey reveals that loyalty is the main factor linking stock ownership and retail-store preferences.

Keywords: stock rewards, stock ownership, individual spending, loyalty, FinTech

JEL codes: G5, D90, G41, D14

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1 Introduction

According to standard economic models, retail investors’ consumption decisions should not influence their investment choices. However, it is well documented that individuals invest in stocks of companies they are familiar with (Huberman, 2001; Keloharju et al., 2012) or feel loyal to (Cohen, 2009). Standard economic models also predict that investment decisions should only affect individual consumption because of wealth effects, and that particular stocks (or other securities) in a portfolio only matter as an input to the portfolio’s risk-return profile. In this paper we show that, contrary to the traditional view, ownership of specific stocks increases consumption from companies owned due to increases in loyalty towards them. We thus document that a behavioral bias in investing—in particular, a preference for buying certain stocks rather than holding the market portfolio—doesn’t just affect trading. It also affects consumption, which is a direct component of individual utility and welfare.

We analyze the relationship between stock ownership and spending using anonymous transaction-level data from a FinTech company called Bumped. After users sign up for the app, the company opens brokerage accounts for them. If and when the users spend money at one of the stores they have selected, 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, meaning 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. However, in our setting, the company required users to go on a waitlist when they first sign up for the service, which minimizes this possibility. When individuals sign up for the waitlist, they only provide their email addresses. The company’s operations team then released batches of users to onboard on a first-come, first-served basis. 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. As a result, we argue, it is implausible that users hold off spending in certain stores in anticipation of receiving an account. Users don’t know 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 Additionally, we restrict our analysis to users who sign up immediately after coming off the waitlist, to make sure that the time of account opening does not coincide with previously planned purchases. By the time their accounts go live, users have selected their favorite stores in 34 retail categories and have linked all of their checking and credit card accounts to the app.

We show that customers increase their spending at their selected stores in response to being allocated a brokerage account. Weekly eligible spending, which we define as spending at selected stores that gets rewarded with stock, jumps by 40% and stays high for 3 to 6 months. Given that eligible spending averages \$55 per week, the 40% corresponds to a \$22 weekly rise in spending. We do not find a fully offsetting impact on ineligible spending—that is, on spending that does not get rewarded.

While timing of the allocation of accounts is quasi-random, if individuals would routinely pick companies in the week of account opening that they plan to spend a lot in during that particular week, our estimates would be biased. There are several reasons why we don't think any such bias is present. First, most people tend to do the highest amount of spending, in each of their categories, at the same companies week after week. This is true 68% of the time, or 94% of the time conditional on positive spending in that category. Second, while users have the opportunity to change their selected companies every four weeks, only 7.1% of Bumped users ever change their selections. Furthermore, our definition of eligible spending is based on brand selections at the time of account opening, and not on subsequent changes. Any spending done with companies adopted as “favorites” outside of the account opening period is treated as ineligible spending for the purpose of estimating the effects of account opening. Finally, if individuals do not switch their spending across companies, then our individual fixed effects would capture any time-invariant preference for certain companies.

Nevertheless, to further alleviate this concern, we used a second identification strat-

¹We show that the time spent on the waitlist is uncorrelated with individual-level observable characteristics in a randomization check. Furthermore, we show that there is no spending response when individuals choose to enter the waitlist. Finally, 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.

egy, exploiting the quasi-random nature of promotional programs in which a subset of users were awarded \$5 or \$10 stock grants from Red Robin, McDonald’s, Exxon Mobile, Chevron, and Yum! brands (including Taco Bell). Over a few arbitrary promotional periods, the stock grants were distributed upon account opening to all users who selected one of these companies. The stock grants were not advertised when users were waitlisted nor when users were invited to open an account. Users who were invited to open the account outside of the promotional windows did not receive the stock grants, even when they selected the same companies. Users that received stock grants are comparable to users that did not receive stock grants on a number of observable characteristics, as confirmed in an orthogonality test. In response to receiving a stock grant, we document a spending response of more than 60% in the stores for which individuals received stocks. The incremental effect of receiving the stock grant on spending is thus 20%. Furthermore, this effect is not driven by the communications accompanying the distribution of the stock grants, as revealed by a placebo test isolating the effect of communications about specific brands.

In addition to documenting a causal impact of stock ownership on spending, 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 given company in Robinhood accounts (relative to holdings of all other companies) increases spending at that company’s stores (relative to all other spending) by 0.18% (or 0.12% controlling for company and week-by-year fixed effects). This result allows us to 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 the users in our study, and also because Bumped users are likely a similar population to Robinhood clients. We also show that our users’ spending in specific stores is correlated with parent-company stock returns.

Finally, we study the link between owning more stocks and having a broader engagement with the stock market. We find evidence—in the form of increasing brokerage transfers from Bumped to other brokers after individuals start receiving stock rewards—that

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

receiving stock stimulates individual investments. This is consistent with a user survey documenting 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? Given that our analysis of the effect of stock ownership on spending is embedded in a reward platform, we pay particular attention to the role of price effects—the implicit price discount associated with any reward—as a potential explanation of our results. However, we argue that price effects are unlikely to be the main explanation behind the spending response for three reasons.

First, we don't think that the mere monetary value of Bumped's rewards can fully explain the changes in behavior that we see. In particular, we don't believe that the 0.5% to 2% price effect of the rewards is enough to get people to increase their spending by 40% to 60% at specific stores given the transaction and hassle costs that such buying would entail. The literature surrounding cash rewards supports our skepticism: [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 6,875%: we find a \$22 increase in weekly eligible spending when the average amount offered in stock rewards is \$0.32 per week.

Second, we exploit plausibly exogenous variation in the fraction of eligible spending that was rewarded. Company operations, policy changes, and constraints at Bumped meant that only about 70% of eligible spending ended up getting rewarded. When we split users into terciles based on the fraction of their eligible spending rewarded, we do not find significant differences. Finally, we examine variation of the percentage of spending that is rewarded at the moment of purchase. Purchases in some companies' stores are rewarded by 0.5%; purchases in others offer rewards of up to 2%. When we split users into terciles depending on whether their eligible spending is predominantly in low- versus high-rewarded stores, we do not find significant differences. We thus verify that variation in the price effect, exogenously assigned or endogenously chosen by users, does not affect the magnitude of their spending responses.

We thus argue that there are additional factors, beyond price effects, 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. 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 creates the need to reciprocate, or through an advertisement effect. Either through reciprocity or through traditional advertisement effects, we find that stock ownership is particularly effective at creating loyalty towards the company owned. In addition, loyalty manifests as increases in spending in the corresponding brand (Chaudhuri and Holbrook, 2001). We discuss four mechanisms that may explain why stock ownership leads to increased loyalty and spending: reductions in cognitive dissonance, illusion of control, familiarity, and over-optimistic beliefs about the prospects of the company that an individual owns.

On top of our empirical analysis, we also asked Bumped’s users (through a survey conducted by Bumped) about users’ motivations and attitudes toward stock ownership and spending. Sixty-eight percent of users report feeling more loyal to the companies that they own stock in, and 40% report having a more positive attachment to the companies they own. Similarly, between 16% and 43% of users report shopping less at competitive companies, and being likely to pay more or go out of their way to shop in stores in 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 the platform. 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 nonpecuniary benefits of stock ownership are correlated with company loyalty. The survey 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 pricing models. At a high frequency and a very granular (individual company) level, we document a strong relationship between spending in certain company products, holdings of the companies’ stocks, and share prices. This relation is stronger than the correlations found in aggregate data, which are too low to be explained with the canonical asset pricing model (Mehra and Prescott, 1985). Our results show that stock ownership directly affects spending and thus utility. 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, expanding on the traditional view that loyalty arises from the memory of past positive experiences (Bronnenberg et al., 2012, 2019) or switching costs (Gourio and Rudanko, 2014).

Literature Review

Our study is related to prior literature linking individual preferences to 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, namely Aspara et al. (2009), Aspara and Tikkanen (2010), and Aspara and Tikkanen (2011), or from a theoretical perspective as in 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 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 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 stocks 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 (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 these 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. The fact that consumer branding and brand preferences matter has been extensively studied in marketing as well as economics ([Bronnenberg et al., 2019](#)). By documenting that stock ownership triggers spending and loyalty, we identify a new determinant of brand loyalty. With this new finding, our results 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. We show that stock ownership itself affects loyalty and thus, stock ownership itself can influence the stability of firm cash flows and its value.

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. Stock ownership as a gift is

likely to be particularly powerful at influencing spending if individuals are subject to cognitive dissonance, illusion of control, familiarity, and over-optimism, four psychological mechanisms for which evidence exists.

Many other papers look at the effects of stock prices on individual behaviour other than spending, including health outcomes and domestic violence (Engelberg and Parsons, 2016; Schwandt, 2018; Lin and Pursiainen, 2020). In terms of spending, our paper is also related to the growing literature using data from new online financial platforms (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 stock ownership and rewards. 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 Over our sample period from 2016 to 2018, individuals had to first sign up for a waitlist on the Bumped website to receive a user account. At the time of signing up for the waitlist, interested users provided their email addresses and names. No additional information was provided at that time. In turn, on a first-come, first-served basis, users were 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 could link all of their checking and credit card accounts. In turn, customers could select their favorite companies in a number of retail spending categories.⁴

The platform featured 99 companies that were divided into 34 different retail categories. In each category, users could select one out of two to five companies. If users then spent at their selected companies' stores (online or offline), they received fractional shares of the companies as a reward. Customers could switch their selected companies every 30 days, but only up to three times per year. The functionalities of the brokerage account were limited. Users were not allowed to deposit their own money or purchase additional stocks, but they could sell their (individual or entire) positions at any time, in which case the cash proceeds were 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 el-

³Bumped introduced stocks as a new category of rewards. Their core mission is to create an ownership economy and getting people access to the stock market sooner and more simply. They view stock rewards as a collective win for brands and consumers. We are aware of two more companies that reward consumer spending with stocks. 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 their new debit card. 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.

⁴Note 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).

igible and ineligible transactions could 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, their current prices, as well as their daily price changes.

As part of a promotional program, some users received stock grants upon signing up for the account. Figure A3 shows the push notification a user receives upon getting a stock grant.

2.2 Data

Baseline demographics and data cleaning

We received an anonymized random sample of the user base. In March 2020, our overall data subsample includes 12,628 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 1 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.

Users can link all of their checking, savings, and credit card accounts. For all linked cards, we not only observe current transactions but also up to a 2-year history of transactions before the card was linked. 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 five two-week periods either before or after the opening account week. 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](#)). Finally, we exclude all users that do not sign up immediately after being asked to do so to address the concern that individuals endogenously time their sign-up with certain spending patterns. 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 and third steps reduce 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 5,409 users.

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

As we will discuss in greater detail in Section 5, the platform management administered surveys and elicited complaints from users. In an open-ended question, users were asked if there was anything they would like to say. 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 our users.

Summary statistics of the full sample are presented in Table A1. Summary statistics for the cleaned sample are reported in Table 1. We can see that 69% of users are male, the average age is 36 years and the median is 35 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

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, as mentioned, we restrict the analysis to those users who opened their accounts within one week after they were invited to do so. Figure 2 shows the timeline of when the users in our subsample were waitlisted, invited to open their accounts, and 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 months. Less than 100 users were asked to sign up for a user account in the same week that they signed up for the waitlist.

Spending

We observe de-identified daily data on each user's spending transactions from all linked checking, savings, and credit card accounts. We then have a flag which transactions were selected and thus eligible for rewards, whether they were actually rewarded and, if so, by

how much. Note that, because of internal business operations constraints, not all eligible 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 \$1,443, and the average total rewards are \$37, as shown in Table 1. The average weekly spending is \$335, while the average weekly reward to users is \$0.32. 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 are the publicly traded companies that individuals could select, the others may be public or private companies. We do not observe other transactions such as rent payments or income receipts. However, we also received information on ACH account transfers and ATM withdrawals.⁶ For the ACH transfers, we know when they belonged to an identifiable broker. 2,156 of our users have other brokerage accounts, primarily with Robinhood, Etrade, Ameritrade, and Schwab. We keep all ACH transfers that are classified as finance or investments and/or belong to an identifiable broker or investment services app.

Stock grants

Starting in March 2018, a quasi-random selection of 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 on a push notification. The dollar amount of each grant, as well as the duration and timing of the promotional program were determined by the marketing team.

Figure 2 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. 1,500 users were awarded grants. Over the observation period, users who received stock

⁶An ACH transfer is an electronic, bank-to-bank transfer processed by the Automated Clearing House network. ACH and ATM transactions are identified by their transaction description. For ACH transfers and ATM withdrawals we report only outgoing ones and they are expressed as positive numbers.

grants spent \$501 per week on average. The average grant amount was \$9.4.

We argue that the distribution of these grants was quasi-random due to the following two reasons. First, all users who opened an account during a few arbitrary time periods over which the promotional program ran and selected one of the participating brands received the stock grant. The company could not have targeted the grants to users with specific characteristics since at the time of account opening and grant distribution, the company did not have access to any spending or demographic characteristics of the users: only emails and names were collected when users signed up for the waitlist. Second, users had no information about the grants at the time they signed up for the waitlist or at the time they were invited to open their accounts and select their favorite companies.

Subsection 4.2 provides an orthogonality test confirming that users who received stock grants are no different than users who did not receive stock grants. In Table A8 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. 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. As mentioned, 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 broader spending datasets and reweighted estimation strategy

Our sample of users is not representative of the US population as a whole. As discussed, we look at a sample of users that is younger, more often male, more urban, more tech-savvy, and likely more interested in stock markets than the average American. To address concerns about external validity, we now compare our users' spending behaviors to other, more representative datasets.

In Table 2, we compare summary statistics of our users to those of the Consumer Expenditure Survey (CEX) 2018 wave. 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 \$1,443 per month (plus \$95 in ATM withdrawals) on average whereas the average American spends \$2,205. As mentioned our spending data includes spending only in 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 and creates a synthetic version of their transaction data.⁷ 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. To analyze the two spending datasets, we run the following regression:

$$RelSpending_{ct}^B = \alpha_c + \eta_t + \beta_{SH} RelSpending_{ct}^{SG} + \epsilon^{ct} \quad (1)$$

The variable $RelSpending_{it}^B$ is the deviation of spending in a certain company relative to the total amount of spending on that day or in that week t of all of our Bumped users. α_c and η_t are company and day or week fixed effects. $RelSpending_{it}^{SG}$ is the relative amount of SafeGraph spending in a certain company relative to the sum of spending in all the other companies on that day or in that week.

In Table 3, we show 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. The 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,

⁷The process obfuscates each transaction to protect individual privacy by injecting noise 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.

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.

Beyond comparing our users' spending patterns with broader datasets, we also provide re-weighted estimates in Subsection 3.1.3. The weights are chosen to match the Current Population Survey (CPS) statistics for the following dimensions: age, sex, state, and income bins.

2.3 Empirical strategy

Treatment effect of account opening

To identify the treatment effect of account opening, we exploit the quasi-random allocation of accounts to users that had signed up for 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:

$$Spending_{Eligible}^{iw} = \alpha_i + \eta_w + \sum_{\tau=-8,\dots,9} \beta_{Bumped}^{\tau} \omega_{Bumped}^{iw\tau} + \epsilon^{iw} \quad (2)$$

In Specification (2), $Spending_{Eligible}^{iw}$ denotes eligible spending (i.e., spending with any company that the user selects 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 before and after account opening. The coefficients β_{Bumped}^{τ} thus tell us the path of eligible spending before and after the user received his or her account. The omitted category in the estimation are all weeks previous to week - 8.⁸ All periods after $\tau = 9$ are absorbed into $\tau = 9$.

⁸In the unbalanced panel, we observe spending for up to 185 weeks before account opening and 75 weeks after account opening.

We normalize coefficients to represent deviations relative to the week immediately 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.

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.

This specification allow us to identify the treatment effect of opening a Bumped brokerage account on spending under certain identifying assumptions. The identifying assumption is that deviations from average spending in any given week are uncorrelated with the time from the week of account opening. We argue this is plausible because, while users chose when to sign up to the waitlist, they 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 (see Subsection 4.2). One potential threat to this identification strategy is that individuals elect to receive rewards in brands in which they plan to spend a lot in the particular week of account opening. While this identification is robust to time-invariant preferences for a particular brand, selecting the brand in which individuals get rewards can be problematic if individuals preferences for a specific brand change over time. For example, if they change the brands in which they spend every week (or do not spend in a given brand every week) our results could be driven by selection at the week-by-brand level, where time is measured relative to the week of account opening. To alleviate this concern, we exploit the quasi-random distribution of unconditional stock grants, as we will describe next.

Treatment effect of receiving a stock grant

We 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 without any spending condition.

We run the following specification to look at the response in eligible spending, overall

and at the companies' stores for which users received the stock grants, upon receiving the stock grant:

$$Spending_{Eligible}^{iw} = \alpha_i + \eta_w + \sum_{\tau=-8, \dots, 9} \beta_{Grant}^{\tau} \omega_{Grant}^{iw\tau} + \epsilon^{iw} \quad (3)$$

In Specification (3), $Spending_{Eligible}^{iw}$, α_i , and η_w are defined as in Specification (2). Beyond looking at overall eligible spending, we specifically look at eligible spending in the company's stores that users received a stock grant of. In turn, $\omega_{Grant}^{iw\tau}$ 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, $\omega_{Grant}^{iw\tau}$ is always zero, but their data is included to identify the week-by-year fixed effects. The coefficients β_{Grant}^{τ} thus tell us the history of eligible spending (overall or in the granted companies stores) before and after a user received the stock grant, which coincides with the date of account opening. We focus on the 8 weeks before and after individuals received the grant.

Incremental treatment effect of receiving a stock grant on the account opening response

We then estimate the incremental difference in the spending response to account opening of individuals who did or did not receive a stock grant with the following difference-in-difference specification:

$$Spending_{Eligible}^{iw} = \alpha_i + \eta_w + \sum_{\tau=-8, \dots, 9} \beta_B^{\tau} \omega_{Bumped}^{iw\tau} + \sum_{\tau=-8, \dots, 8} \beta_{BG}^{\tau} Grant_i \omega_{Bumped}^{iw\tau} + \epsilon^{iw} \quad (4)$$

In Specification (4), $Spending_{Eligible}^{iw}$, α_i , η_w and $\omega_{Bumped}^{iw\tau}$ are defined as in Specification (2). 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_{BG}^{τ} thus identify the incremental effect of receiving an account and a grant relative to the effect of receiving an account without a grant, β_B^{τ} , in each user-specific τ 's week. We consider 8 weeks before and after individuals opened their account and received the grant. We estimate Specification (4)

for both overall eligible spending as well as spending in the specific companies' stores of which stock was granted.

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; both dummies are then interacted with a dummy of whether or not a user received a grant.

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 around account opening. Thus, the vertical 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.

Figure 5 shows the β_{Bumped}^τ coefficients and standard errors from Specification (2) 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 individual's sample average weekly eligible spending. That is, we divide the weekly spending of each user by his or her average weekly spending over the whole sample period. 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 8 weeks after account opening. In terms of US dollars, eligible spending averages \$55 per week, so this corresponds to approximately a

\$22 increase in spending per week. We do not see a pre-trend in the estimated coefficients before account opening and the figure reports the F-statistic of a joint test that all pre-account-opening coefficients are zero.

In Figure 5, 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 \$279 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 show that total spending increases and we can rule out any decrease in log ineligible spending.

3.1.2 Stock grant analysis

Figure 6 shows the β_{Bumped}^{τ} coefficients and standard errors from Specification (2), splitting the sample into grant receivers and non-receivers. In both cases, we can see again a clear increase in eligible spending in the order of 40% following account opening.

We also present the results from estimating Specification (3) 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 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 by about 60%.

Figure 8 shows the β_{BG}^{τ} coefficients and standard errors from Specification (4). We present the results for overall eligible spending and spending at the brands for which users were granted stocks as the left-hand side variables. 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 opened their accounts and 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 20%. We do not find an incrementally larger effect on overall eligible spending in

Figure 8. Therefore, the stock grants increase spending in the granted companies' stores but not overall eligible spending.

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 39% increase in eligible spending relative to each individual's sample average. Column 2 shows a 7% decrease in ineligible spending with a standard error of 0.9%; we can thus rule out a decrease of more than 9% in ineligible spending with statistical confidence. In this specification, total spending increases only marginally. Column 5 show the results for eligible spending in granted companies' stores. We see the baseline effect of account opening, a 34% increase in spending in the granted categories, and then the interaction effect for individuals that received a grant, which shows another 11% incremental effect of the stock grant receipt.

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 \$16 per week in eligible spending and an insignificant \$1.2 decrease in ineligible spending. Total spending increases significantly by \$15 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 percentage deviations from each individual's mean or the absolute amounts. Note that, we take the log of spending amounts in this regression but keep values between zero and one. In this specification, we document a 61% increase in overall eligible spending, can rule out any decrease in ineligible spending with statistical confidence, and also 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. Column 5 shows a mean effect of a \$2 increase in all eligible spending and an incremental effect of \$3 for grant recipients. 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. Using the

specification in logs, in Table A4, we find a significant percentage incremental increase of spending in the granted company’s stores in Column 5 of 17%.

Because our sample is not representative of the US as a whole, we may not be able to extrapolate our main estimates to the whole population. Beyond showing that our users’ spending patterns are consistent with those of broader populations in Subsection 2.2, as another reassuring exercise, we provide the estimation results after re-weighting our sample on several dimensions using CPS weights: age, sex, state, and income bins.⁹ The reweighted estimates look very similar to the main estimates as can be seen in Table A5.

3.1.4 Long-term effects

We now 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 9. 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. 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. That said, as we mentioned, only 7.1% of user-categories in our sample ever change their selections.

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 10 shows significant increases in eligible spending relative to the average weekly eligible spending in that category. The effect is more pronounced for burgers, coffee shops and drug stores. The results for ineligible spending are mixed, with

⁹We proxy annual income by summing up all food spending and multiplying it by the inverse of the share of food spending in disposable personal income in 2020 in the US (8.6 percent as provided by the Economic Research Service of the US Department of Agriculture). We observe the age, gender, state, and income of almost all of our users.

some categories like coffee showing substantial offsetting effects and some others (the majority) showing a flat or noisy response to account opening on ineligible spending.

3.1.6 Sample splits by size of rewards

Due to company operations and constraints, only an average of 70% of eligible spending transactions were actually rewarded.¹⁰ Because whether an eligible transaction was actually rewarded is plausibly exogenous to the user, we exploit variation in the fraction of eligible spending rewarded to see if receiving more rewards leads to differential effects on eligible spending compared to receiving less rewards.

In Figure A4, we look at the sample splits of terciles of individuals being rewarded many versus fewer eligible spending transactions. We 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 the reward percentages a user receives are small, medium, or large.

3.1.7 Other sample splits: login activity and gender

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 Specification (2) after splitting the sample into terciles of login counts per user. Across the spectrum of 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, but they are not statistically

¹⁰Note that, only eligible spending was ever rewarded; ineligible spending was never rewarded. In our previous analysis, when we define 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.

significantly different from the increases in spending of users that log in less to the app. Further, we look at sample splits by gender. Male users represent close to 70% of the sample. Figure A7 shows the results on eligible spending for both male and female users. The results are similar.

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.¹¹ 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 then run the following regression:

$$RelSpending_{ct} = \alpha_c + \eta_t + \beta_{SH} Rel Holding_{ct} + \epsilon^{it} \quad (5)$$

The variable $RelSpending_{it}$ is the deviation of spending in a certain company relative to the total amount of spending on that day or in that week t of all of our Bumped users. α_c and η_t are company and day or week-of-sample fixed effects. $Rel Holding_{it}$ are the relative number of Robinhood users that hold a certain company relative to the sum of Robinhood users holding all the other companies on that day or in that week.

In Table 5, we show the coefficient β_{SH} telling us that a 1% increase in holdings of a certain company is correlated with spending in that company’s stores by 0.18%, controlling for company and day-of-the-sample fixed effects. Aggregated to the weekly level, this coefficient increases to 0.21%. We thus find a very strong positive correlation between spending and stock ownership in the observational data.

In Table 6 we also show that there exists a correlation, at the daily and weekly levels, between spending in certain companies’ stores of our users and the stock returns of that company. This significant correlation, however, does not survive company and day or week-of-sample fixed effects.

¹¹Information on the number of Robinhood clients that hold a certain stock on a certain day is obtained from robintrack.net. We merge this dataset with ours at the daily-stock level.

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 A7, 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 day or week 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 a causal estimate of the relationship between spending and stockholdings. In turn, we also find that this relationship in observational data from spending and holdings in brokerage accounts.

3.3 Impact on brokerage account transfers

Next, we look at brokerage account transfers. First, we flag all ACH account transfers that are categorized as financial. 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 amount of brokerage transfers normalized by users own averages 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. Column (2) looks at the absolute transfer amounts, Column (3) at the log amounts, and Column (4) at the likelihood of making a transfer. It can be seen that brokerage account transfers are significantly increased in the 8 weeks of account opening. The effects are small but the baseline is small too, e.g., relative to the baseline amount invested and propensity to invest, the coefficients on the amounts and likelihood to make a transfer represents an increase of approximately 20%.

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. As we will discuss below, we also confirm this result in survey data.

4 Robustness checks

4.1 Substitution from cash spending

There is a concern that users may substitute from cash transactions to card transactions after account opening. We look at net ATM withdrawals to address this possibility. Table 8, Column (1) shows the net withdrawal ATM amount relative to users' own averages 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 absolute ATM net withdrawal amounts and Column (3) the log amounts. We estimate very small positive coefficients. On the one hand, this is reassuring as it tells us that users did not decrease their ATM withdrawals after account opening (and substitute to card spending). On the other hand, this may be concerning as individuals may have started to use one particular card, hence the increase in ATM withdrawals, after account opening, substituting away from other cards. To address this concern, we note that the estimated coefficients are extremely small, less than \$1.5 in absolute amounts, 3% in relative amounts, and 0.6% in the likelihood, so we argue that substitution from some cards to others is not a major concern.

4.2 Orthogonality tests for time on waitlist and grant receipt

Our first identification strategy is based on the staggered distribution of brokerage accounts to users who were on the waitlist. We argue that users were not able to predict when they would get off 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 128 days and standard deviation of 90 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 but no other information. As a result, the time spent on the waitlist is uncorrelated with user characteristics and hard to predict by individual users.

To confirm this, we perform an orthogonality test regressing the time on the waitlist on individual-level characteristics. Column 1 of Table A6 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. 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 an orthogonality test between grant recipients and non-recipients before they were taken off the waitlist. In Table A8, 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.

4.3 Sample splits by time on waitlist

Additionally, we can split the sample based on the time spent on the waitlist. The results for receiving an account for three terciles of the time individuals were waitlisted can be found in Figure A9. There are no discernable differences.

4.4 Placebo test for getting on the waitlist

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:

$$Spending_{Eligible}^{iw} = \alpha_i + \eta_w + \sum_{\tau=-8,\dots,9} \beta_{Waitlist}^{\tau} \omega_{Waitlist}^{iw\tau} + \epsilon^{iw} \quad (6)$$

In Specification (6), $Spending_{Eligible}^{iw}$, α_i , and η_w are defined as in Specification (2). In turn, $\omega_{Waitlist}^{iwt}$ is an indicator of whether user i in week w was waitlisted in his or her τ 's week. The coefficients $\beta_{Waitlist}^{\tau}$ thus tell us the history of eligible spending before and after a user signed up for the waitlist. We focus on spending 8 weeks before and after individuals signed up for the waitlist.

Figure A8 shows the $\beta_{Waitlist}^{\tau}$ 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 each individual's 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.

4.5 Placebo test for receiving push notifications about specific brands

Our preferred identification strategy is based on the quasi-random distribution of stock grants among platform users. As part of the distribution of the grants, individuals received a push notification informing about the grant they received (See Figure A3). This push notification included the specific name of the company to which the stock belongs and as such it can be interpreted as an additional salience shock for spending at the company's stores. To rule out that the effects we found are driven by the pure advertising effect of the push notification, we exploit the quasi-random distribution of push notifications about Uber and Lyft that took place when the ride sharing category was introduced to the platform on August 13, 2019. When the ride sharing category was introduced, all existing users (as of August 13, 2019) received a push notification (and an email) informing them about the introduction of the category. This push notification and email included references to Uber and Lyft, and as such, can be thought of as a salience shock for those brands. None of the users who opened their accounts after August 13, 2019 received this

salience shock. We thus compare the spending response to account opening on Uber and Lyft for individuals who opened their account after August 13, 2019 and for individuals who opened their account in the weeks immediately before the ride sharing category was introduced.

Specifically, consistent with our strategy to identify the incremental effect of stock grants, we estimate a version of Specification (4) to estimate the incremental effect of the push notifications replacing the dummies for receiving a stock grant, with a dummy for receiving a push notification within 8 weeks after account opening, i.e., a dummy for opening the account in the 8 weeks before August 13, 2019. We note that, for individuals who opened their account more than 8 weeks before the introduction of the ride sharing category, looking at the spending response during the 8 weeks following account opening would be incorrect since at that time the platform did not have the infrastructure to reward those transactions. We thus focus only on individuals who opened their account between 1 and 8 weeks before the introduction of the ride sharing category (treatment group), or after the introduction of the ride sharing category (control group). Table A9 shows the regression results separately for different windows of weeks since the introduction of the ride sharing category.

As expected, we can see a positive and significant effect of account opening on the ride sharing category during the first 8 weeks of account opening, and in the long term (coefficients between 0.055 and 0.074). However, the incremental effect of the push notification is not statistically significant neither in the short term (8 weeks after account opening), nor in the long term (more than 8 weeks after account opening). The coefficient for the incremental effect of push notifications during the 8 weeks following account opening ranges between 0.005 and 0.018. The coefficients for the incremental effect in the long term (more than 8 weeks after account opening) range between -0.021 and 0.029. While the statistical power of this test is limited, the magnitude of the coefficients in this placebo test is much lower than the magnitude of the coefficients for the incremental effect of stock grants. Furthermore, the pattern of a strong short term effect, followed by a much stronger effect in the long term is not present in the results for the incremental effect of push notifications. We thus conclude that the treatment effect of receiving stock grants, described in Section 3, is unlikely to be driven by the brand-specific communications accompanying

the distribution of the stock grants.

5 Stock ownership and self-reported loyalty: Survey evidence

In this section, 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. We can use these surveys to 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 the increases in spending in response to receiving stock rewards.

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 likelihoods 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 shows 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-pecuniary 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 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.

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

Clearly, after users received a Bumped account, shopping at the selected companies is rewarded and thus technically cheaper by 0.5% to 2%. However, as we mentioned in the Introduction, we argue that our findings are unlikely to be purely explained by a price effect for three reasons. First, if people change by 40 to 60% which stores they go to, their incurred transaction and hassle costs should be 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, using both exogenous and endogenous variation in the price effect.

Third, our effects are an order of magnitude 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%.¹² In comparison,

¹²[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.

stock rewards have an effectiveness of 6,875%: we find a \$22 increase in weekly eligible spending when the average amount offered in stock rewards is \$0.32 per week.

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." Loyalty in turn manifests through increases in spending in the corresponding brands (Chaudhuri and Holbrook, 2001). We now discuss the different psychological mechanisms that might trigger and enhance feelings of loyalty and increases in spending.

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 shareholder affect 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 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. 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 actually 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 that we find in Figure 8.

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 can be accompanied by psychological mechanisms, such as cognitive dissonance, illusion of control, familiarity, and over-optimism that enhance and trigger loyalty and a preference for spending in the corresponding brands. For all of these mechanisms, there exists evidence.

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 behaviors in a way that is favorable to the company (e.g., by purchasing the company's products, paying more for them, and avoiding buying substitute products from a competitor). Alternatively, they could change their beliefs by, acknowledging that their individualistic choices are not important enough to tip the scales for the firm.

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 individual purchasing decisions 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 may 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 at a specific store could not effectively impact stock prices, the illusion of control could also trigger the perception that buying products of the company owned has a material impact on its stock price, or more generally its financial performance.

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 Similarly, ownership of a specific stock can also affect how individuals learn about the quality of the products offered by the corresponding company, thus providing new information about the benefits of the product and leading to increases in demand, in a way akin to the traditional familiarity bias in investing (Cao et al., 2009; Heath and Tversky, 1991; Huberman, 2001; Keloharju et al., 2012).

Login activity into the app can increase individuals exposure towards the brands they own and lead to a different type of familiarity effect, by which individuals consume more of the brands with which they are familiar with. We do not find significant heterogeneities based on login activity. However, we interpret this results with caution since login activity may be correlated with other unobservable characteristics.

Over-optimistic beliefs about firm prospects Hartzmark et al. (2019) documents that ownership of a good affects learning and beliefs about its quality. Ownership of a specific stock can lead individuals to have over-optimistic beliefs about the prospect of the company after receiving positive signals. This learning distortion would lead individuals to

mis-perceive the value of the company as more than the net present value of its future cash flows, in a manner similar to overconfidence (which has been documented among retail investors, see e.g., [Barber and Odean, 2001](#)). If this is the case, then stock ownership itself would create a feedback effect that would render stock as a particularly effective reward, which is consistent with our results about the effectiveness of stock rewards and stock grants.

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 the company's stores. For identification, we use the quasi-random distribution of brokerage accounts and stock grants upon account opening.

When users are granted a certain company's stock, we find a weekly spending response of 60% at the companies' stores for which individuals received stock grants. For these users, we thus find a 20% incremental response relative to the user responses to account opening without receiving a stock grant.

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, 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, reductions in cognitive dissonance, and over-optimistic beliefs; five psychological biases in investing for which evidence exists.

Our results suggest that behavioral biases in investing — in particular, the preference to buy specific stocks rather than holding the market portfolio, don't just affect trading. They also affect consumption and customer capital. The former is a direct component of individual utility and welfare. The later leads more stable cash flows and increases in firm

value (Larkin, 2013; Dou et al., 2019).

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Figures and tables

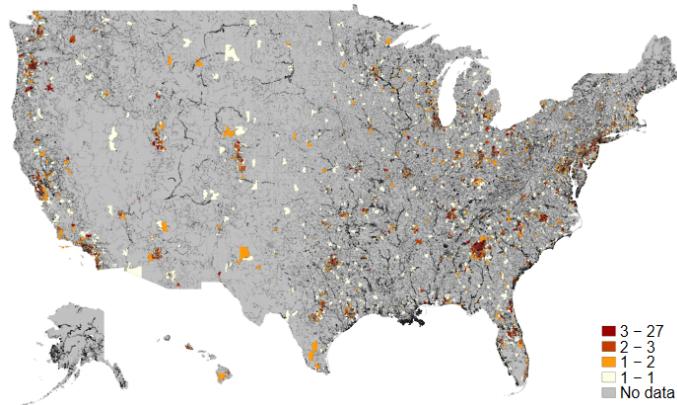


Figure 1: Users by 5-digit zip code in the US.

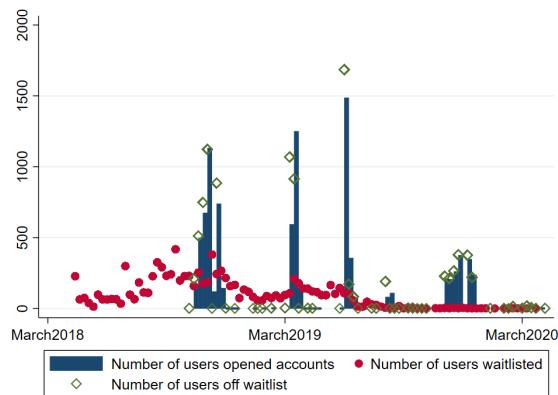


Figure 2: Number of users in our data subsample who were waitlisted, invited to open an account (off waitlist), and actually opened an account over the weeks-by-year of our sample.

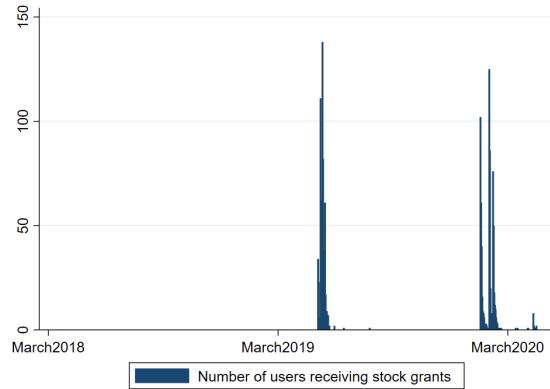


Figure 3: Number of users in our data subsample who received stock grants over the weeks-by-year of our sample.

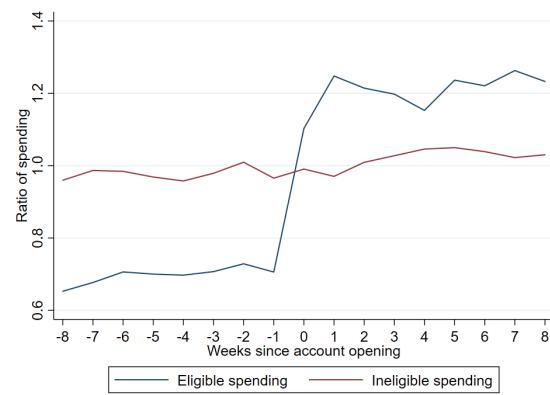


Figure 4: Ratio of weekly eligible and ineligible spending relative to average individual-level eligible and ineligible spending over the 16-week window surrounding week zero reflecting the week of account opening.

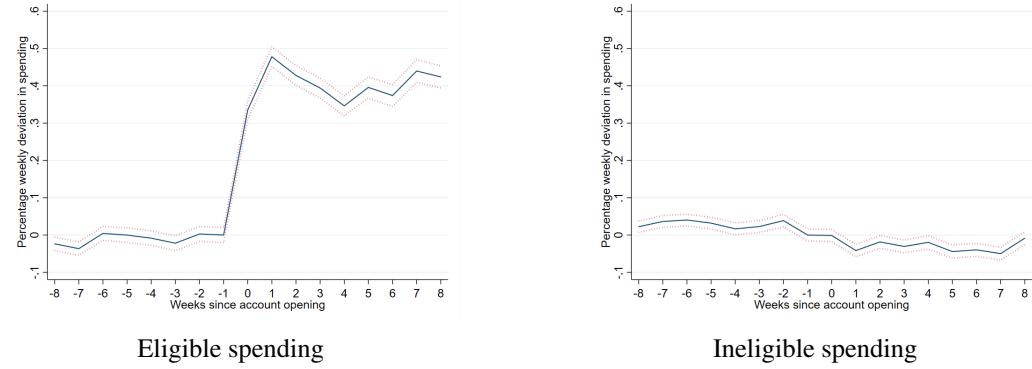


Figure 5: This figure shows the coefficient estimates β_{Bumped}^τ in Specification (2) for both eligible and ineligible spending (the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level. The F-statistic for the joint test that all pre-account-opening coefficients are zero for the left-hand-side panel is 0.898 with a p-value of 0.507, while the F-statistic for the right-hand-side panel is 0.925 with a p-value of 0.485.

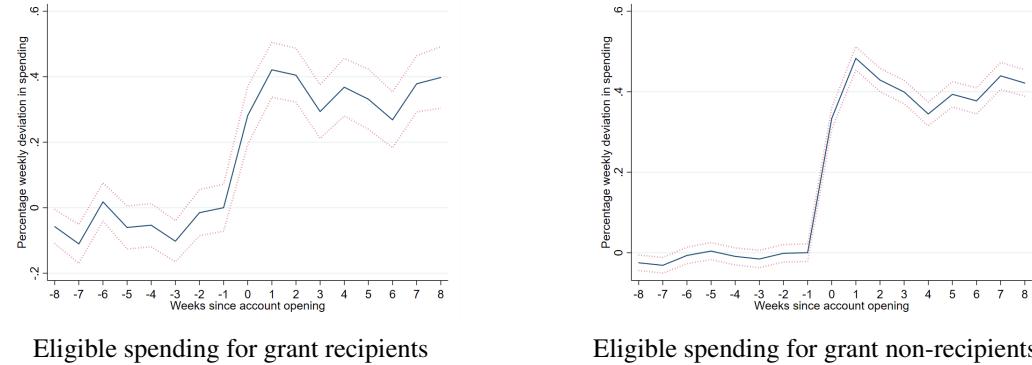


Figure 6: This figure shows the coefficient estimates β_{Bumped}^τ in Specification (2) for eligible spending (the percentage deviation from the individual-level mean) separately for individuals who received the grant and those who did not. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level. The F-statistic for the joint test that all pre-account-opening coefficients are zero for the left-hand-side panel is 0.867 with a p-value of 0.532, while the F-statistic for the right-hand-side panel is 0.540 with a p-value of 0.805.

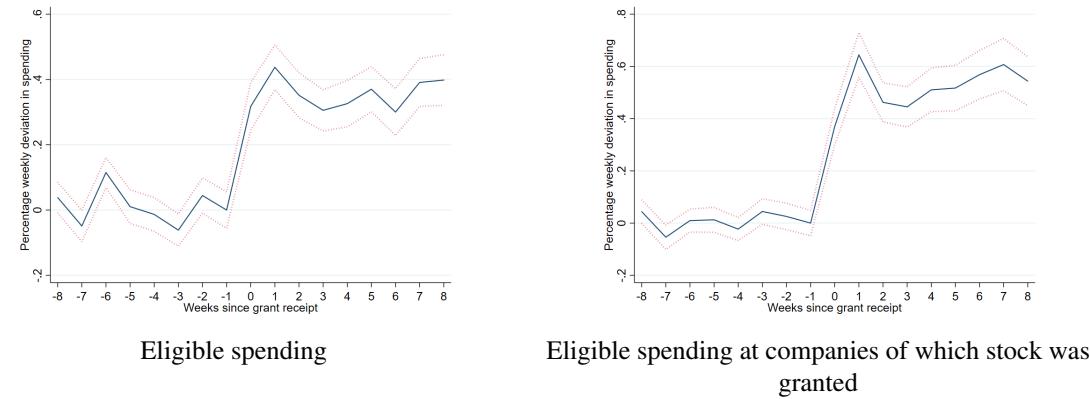


Figure 7: This figure shows the coefficient estimates β_{Grant}^{τ} in Specification (3) for both eligible overall spending and eligible spending at the companies' stores of which users received stock grants (the percentage deviation from the individual-level mean). We control for individual and week-by-year 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. The F-statistic for the joint test that all pre-account-opening coefficients are zero for the left-hand-side panel is 1.634 with a p-value of 0.121, while the F-statistic for the right-hand-side panel is 0.743 with a p-value of 0.635.

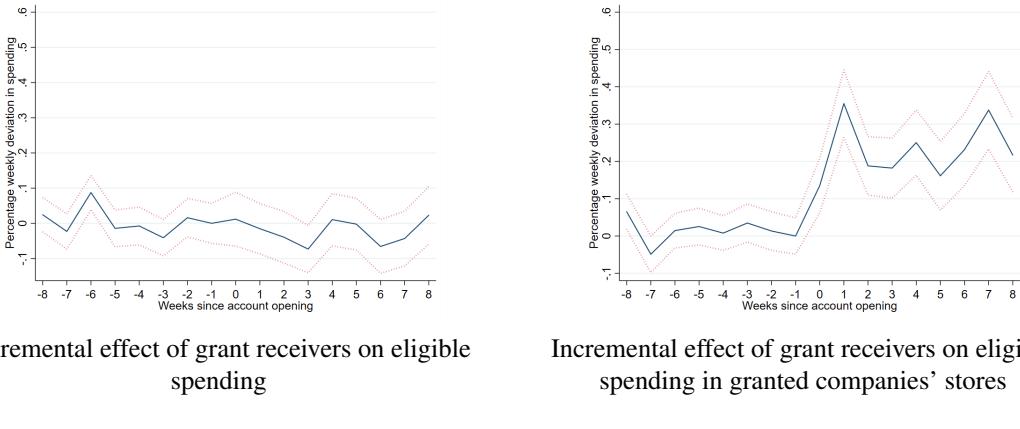
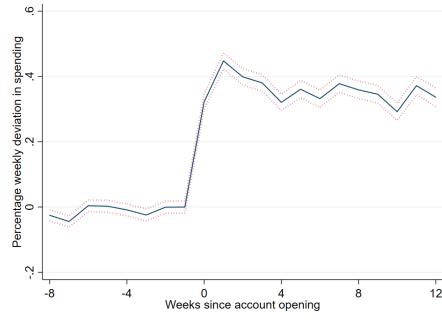
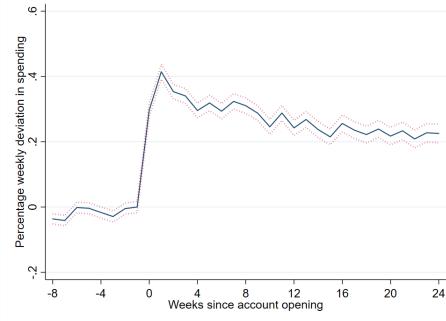


Figure 8: This figure shows the coefficient estimates β_{BG}^τ in Specification (4), i.e., the incremental effect of grant receivers on all eligible and grant company spending (the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 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. The F-statistic for the joint test that all pre-account-opening coefficients are zero for the left-hand-side panel is 0.660 with a p-value of 0.706, while the F-statistic for the right-hand-side panel is 0.616 with a p-value of 0.744.



Eligible spending 3 months after account opening



Eligible spending 6 months after account opening

Figure 9: This figure shows the coefficient estimates β_{Bumped}^τ in Specification (2) for eligible spending (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. The p-value of the F-statistic for the joint hypothesis that all pre-account-opening coefficients are zero is 0.314.

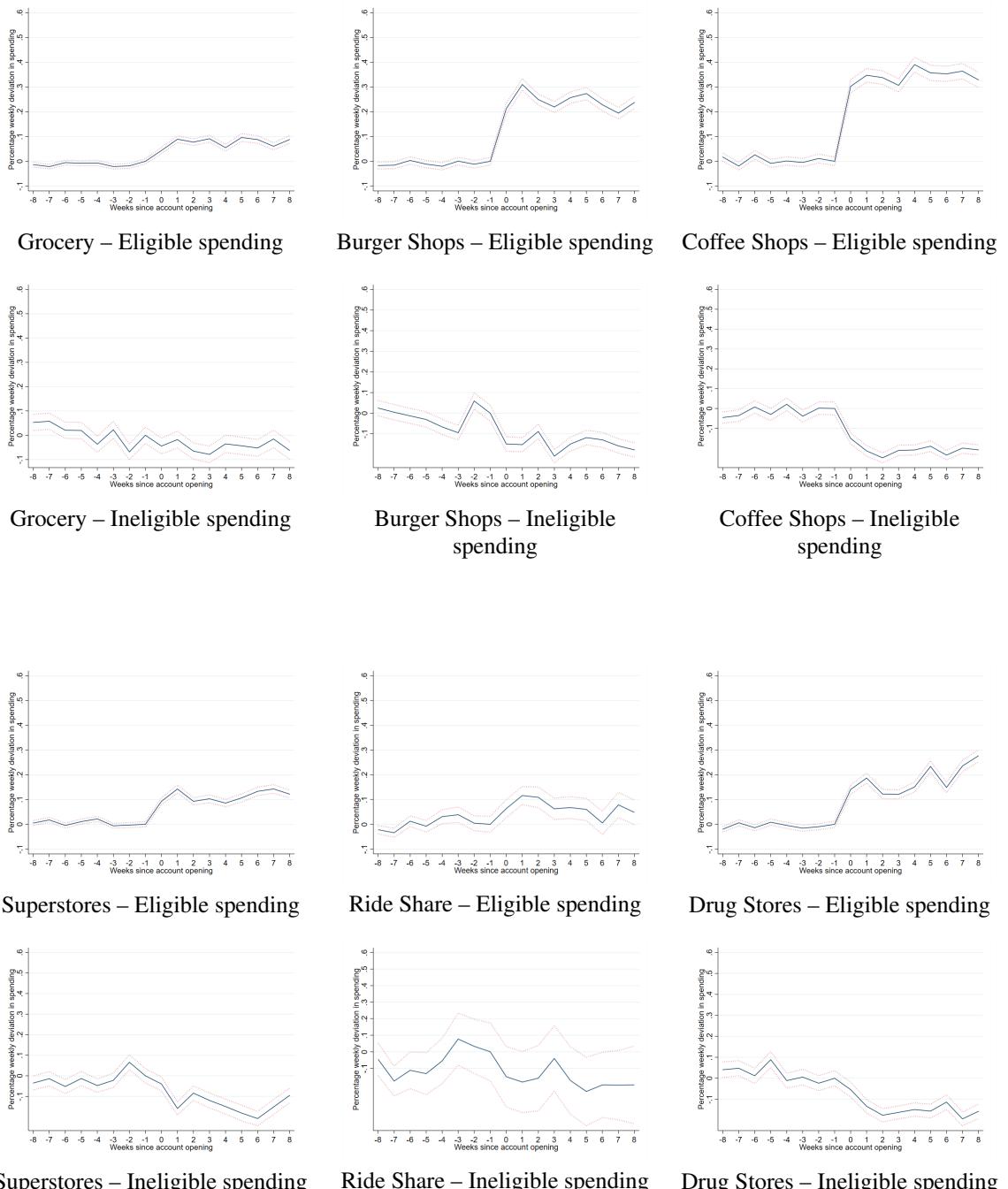


Figure 10: This figure shows the coefficient estimates β_{Bumped}^τ in Specification (2) for the six most popular rewards categories, which are grocery, burgers, coffee, superstores, ride share, and drug stores. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level. For ride share we consider only accounts opened after the category was introduced (see Subsection 4.5). The F-statistic for the joint test that all pre-account-opening coefficients are zero are: F=0.925 (Grocery – Eligible), F=2.186 (Grocery – Ineligible), F=0.470 (Burger Shops – Eligible), F=2.038 (Burger Shops – Ineligible), F=1.110 (Coffee Shops – Eligible), F=0.791 (Coffee Shops – Ineligible), F=1.230 (Superstores – Eligible), F=1.209 (Superstores – Ineligible), F=1.664 (Ride Share – Eligible), F=0.983 (Ride Share – Ineligible), F=0.886 (Drug Stores – Eligible), F=1.056 (Drug Stores – Ineligible).

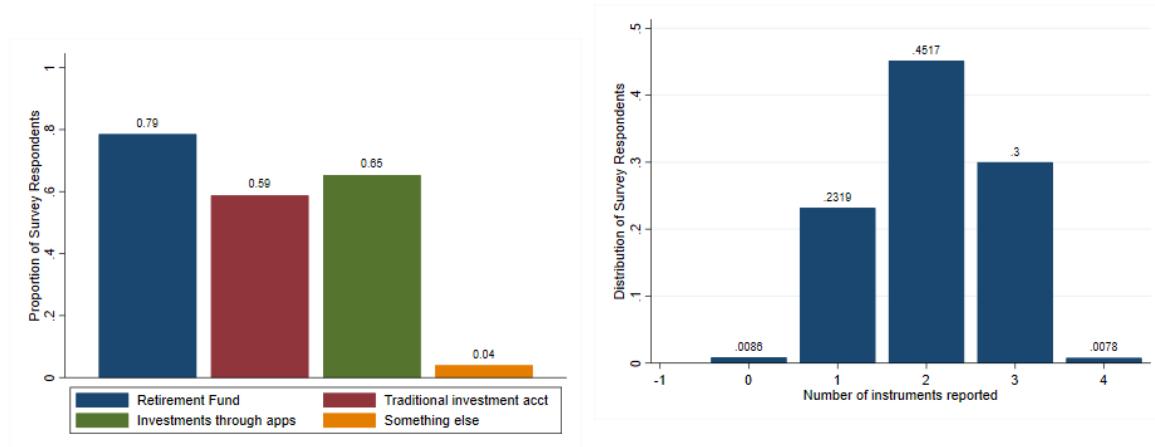


Figure 11: This figure shows the responses to a survey of 1,160 users who were asked about their investing experience. The survey question is: "Do you own stock outside of Bumped? If so, where? 1. Employer-sponsored retirement funds (401k, IRA etc), 2. Traditional or managed investment account, 3. Investments 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.

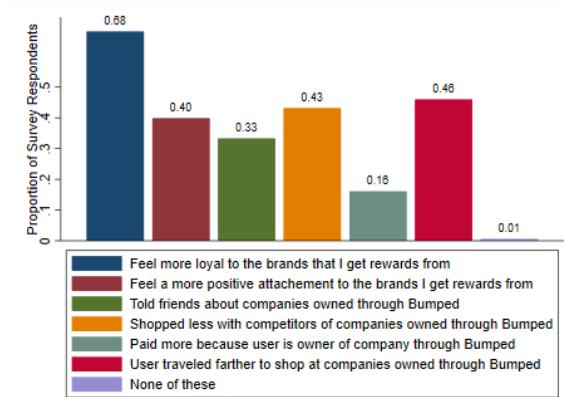


Figure 12: This figure shows the responses of 1127 users who were asked to select all that applies for the following question: "Since signing up for Bumped... 1. I feel more loyal to the brands that I get rewards from, 2. I feel a more positive attachment to the brands I get rewards from, 3. I have told 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." Users were allowed to select any number of answers.

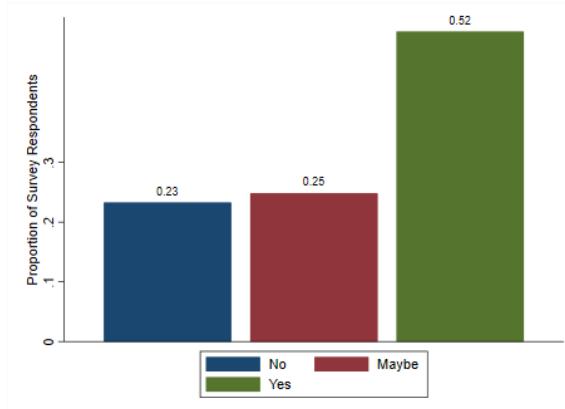


Figure 13: This figure shows responses of 1,160 users who were asked the following question: "Does owning stock through Bumped make you more likely to invest outside of Bumped in the future? 1. No, 2. Maybe, 3. Yes." Users were allowed to select one answer.

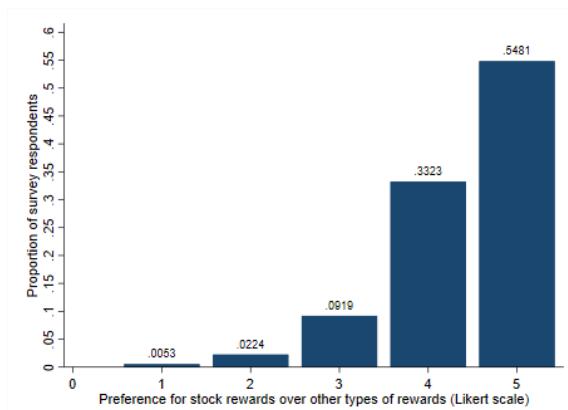


Figure 14: This figure shows responses of 2,287 users who were asked: "In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)? 1. Significantly less excited than traditional rewards, 2. Less excited than traditional rewards,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

Table 1: Summary statistics of users, final sample post data cleaning

	Mean	Std dev	25th percentile	50th percentile	75th percentile
Age	36	9.2	30	35	41
Male	.69	.46	0	1	1
Days from waitlist to open	128	90	70	108	154
Monthly user logins	4.6	10	1.4	2.2	3.7
Weekly user logins	2	3	1	1.3	1.7
Number of transactions	1,066	850	584	912	1,342
Number of cards linked	2.5	2	1	2	3
Total monthly spending	1,443	3,448	632	1,022	1,695
Total weekly spending	335	802	147	238	393
Monthly eligible spending	238	1,142	50	134	275
Weekly eligible spending	55	263	12	31	64
Monthly ineligible spending	1,205	3,219	505	839	1,402
Weekly ineligible spending	279	750	117	195	325
Grant weekly eligible spending	4.8	17	0	.96	4.7
Grant weekly ineligible spending	16	195	0	0	0
Monthly eligible spending - grocery	48	129	0	0	28
Monthly ineligible spending - grocery	65	125	2.4	19	75
Monthly eligible spending - superstores	36	103	0	0	20
Monthly ineligible spending - superstores	65	145	3.4	17	64
Monthly eligible spending - ride sharing	9.7	34	0	0	5
Monthly ineligible spending - ride sharing	22	51	0	3.9	21
Total rewards	37	57	7	19	47
Monthly rewards	1.4	2	.31	.77	1.7
Weekly rewards	.32	.46	.073	.18	.41
Total rewarded/eligible	.56	.25	.37	.56	.77
Monthly rewarded/eligible	.48	.24	.31	.43	.65
Weekly rewarded/eligible	.52	.24	.34	.48	.71
Monthly ACH transfers	299	3,168	0	0	26
Weekly ACH transfers	199	3,005	0	0	20
Monthly ATM withdrawals	95	281	0	3.9	88
Weekly ATM withdrawals	22	65	0	.89	20
Observations	5409				

Notes: 5,409 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 company (551 different companies are in the final dataset (in 34 retail categories) of which 99 can be selected to be rewarded). Spending does not include ATM withdrawals. ATM withdrawals are expressed as positive numbers. ACH transfers include all outgoing ACH transfers that are classified as finance or investments, belonging to an identifiable broker, or belonging to an investment services app. Number of cards linked is calculated before the adjustment for card-week pairs with at least 2 transactions around 10 weeks of the account opening week, to capture all cards linked for users in the final sample.

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

Variable	Consumer Expenditure Survey 2018	Bumped users
Age	51.1	36
Men	0.47	0.69
Monthly spending	2,205	1,443
Monthly grocery spending	148	113
Monthly restaurant spending	114.4	32
Monthly transportation spending	27	31.7
Monthly drug spending	16	23.7

Notes: The survey statistics stem from the 2018 wave of the CEX. The CEX is conducted by the Bureau of Labor Statistics and provides data on expenditures, income, and demographic characteristics of consumers in the United States. It 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). Bumped users' monthly spending only includes transactions that were classified as belonging to a certain company (551 different companies are in the final dataset (divided into 34 retail categories) of which 99 could be selected to be rewarded). Spending does not include ATM withdrawals (\$95 per month on average).

Table 3: Estimation results of spending by Bumped users on Safegraph card spending in the same companies' stores

	Daily spending in companies' stores relative to total spending Bumped users		Weekly spending in companies' stores relative to total spending Bumped users
Daily spending in companies' stores relative to total spending Safegraph data	0.476*** (0.007)	0.243*** (0.016)	0.240*** (0.016)
Weekly spending in companies' stores relative to total spending Safegraph data			0.442*** (0.015) 0.705*** (0.040) 0.705*** (0.041)
Company fixed effects	✓	✓	✓
Date or week-by-year fixed effects		✓	✓
Observations	19396	19396	19396
R squared	0.212	0.887	0.887
	3528	3430	3430
	0.195	0.939	0.939

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

Notes: In Specification (1), we regress the daily (weekly) spending in all publicly traded companies of all Bumped users on spending in those companies' stores from the Safegraph card spending data relative to total spending in that day or week. The relative Bumped and Safegraph spending data are normalized by their respective standard deviations. Date (week-by-year) fixed effects refer to any day (week) of the sample period and company fixed effects for any publicly traded companies. The time period and selection of companies (tickers) is constrained by the Bumped data, however, not all tickers could be matched to the spending in the companies' stores in the Safegraph data and we only kept unambiguous matches of the top 200 companies (in terms of the amount spent in their stores) in the Safegraph data. The sample time period is May 2018 to March 2020.

Table 4: Estimation results of Bumped users' spending ratios on a post account opening interacted with a dummy if users received a grant

	All spending			Spending in granted companies' stores
	Eligible	Ineligible	Total	Eligible
Post 8 weeks	0.391*** (0.013)	-0.072*** (0.009)	0.007 (0.008)	
Post more than 8 weeks	0.581*** (0.021)	-0.006 (0.011)	0.053*** (0.009)	
Post 8 weeks times grant recipient				0.341*** (0.012)
Post more than 8 weeks times grant recipient				0.108*** (0.033)
Post more than 8 weeks times grant recipient				0.323*** (0.017)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	611651	612774	612800	608292
R squared	0.152	0.110	0.118	0.149

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

Notes: In these variants of Specifications (2) and (4), we regress eligible and ineligible spending overall and specifically in granted companies' stores (the percentage deviation from the individual-level mean) on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of account opening or receiving a 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. For spending in granted companies' stores, we interacted the post dummies with a dummy for whether a user received a grant. User fixed effects and week fixed effects are included.

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

	Daily spending in companies' stores relative to total spending Bumped users	Daily spending in companies' stores relative to total spending Bumped users	Weekly spending in companies' stores relative to total spending Bumped users
Daily number of holdings in company relative to total holdings Robinhood clients	0.176*** (0.007)	0.133*** (0.009)	0.119*** (0.010)
Weekly number of holdings in company relative to total holdings Robinhood clients			0.213*** (0.018) 0.152*** (0.015) 0.136*** (0.016)
Company fixed effects	✓	✓	✓
Date or week-by-year fixed effects		✓	✓
Observations	26958	26958	26958
R squared	0.022	0.892	0.032
	4155	4155	4155
	0.952	0.952	0.952

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

Notes: In Specification (5), we regress the total daily (weekly) spending in all publicly traded companies 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 company fixed effects for any publicly traded company. The sample time period is May 2018 to March 2020.

Table 6: Estimation results of Bumped users spending on the returns of that company

	Daily spending in companies' stores relative to total spending Bumped users	Weekly spending in companies' stores relative to total spending Bumped users
Daily return of company less the market return	0.044*** (0.016)	0.001 (0.005)
Weekly return of company less the market return		0.040*** (0.014)
Company fixed effects	✓	✓
Date or week-by-year fixed effects		✓
Observations	26907	26907
R squared	0.000	0.891
	26907	4155
	0.891	0.002
	4155	0.951
	0.951	0.951

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

Notes: In this variant of Specification (5), we aggregate the spending data to the day or week-by-company levels and regress the total daily (weekly) spending in all publicly traded companies of all Bumped users on the daily (weekly) returns of that company obtained from the CRSP US Stock and Index Database. We normalize the daily (weekly) returns by the market return. Date (week-by-year) fixed effects refer to any day (week) of the sample period and company fixed effects for any publicly traded company. The sample time period is May 2018 to March 2020.

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

	Brokerage transfers			
	Transfer ratio	Transfer amount	Log transfer amount	Likelihood of transfer
Post 8 weeks	0.013*** (0.002)	1.019*** (0.340)	0.023*** (0.008)	0.004*** (0.001)
Post more than 8 weeks	-0.001 (0.003)	0.393 (0.457)	0.008 (0.010)	0.002 (0.002)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	610367	612800	612800	612800
R squared	0.233	0.277	0.303	0.314

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

Notes: In Columns (1) to (3), we regress the ratio, amount, and 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. Columns (4) uses the likelihood to transfer to a brokerage account as the outcome variable. 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. User fixed effects and week-by-year fixed effects are included.

Table 8: Estimation results of ATM withdrawals post account opening

	ATM withdrawals			
	ATM withdrawal ratio	ATM withdrawal amount	Log ATM withdrawal amount	Likelihood ATM withdrawal
Post 8 weeks	0.006 (0.012)	1.479*** (0.538)	0.032*** (0.011)	0.006*** (0.002)
Post more than 8 weeks	0.012 (0.015)	-0.346 (0.595)	-0.006 (0.012)	-0.001 (0.002)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	608749	612800	612800	612800
R squared	0.135	0.269	0.308	0.297

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) uses the percentage deviation in net ATM withdrawals (relative to the individual-level average) as the outcome variable, Column (2) uses the USD amounts in net ATM withdrawals, and Column (3) uses the log of USD amounts in net ATM withdrawals. 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 Attachment (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** (0.092)	0.028 (0.087)	0.083 (0.082)	-0.009 (0.053)	0.139* (0.080)	0.105 (0.093)	
Excited about stock rewards (More)	0.232*** (0.083)	0.122 (0.079)	0.207*** (0.074)	0.029 (0.049)	0.211*** (0.071)	0.213** (0.084)	
Excited about stock rewards (Significantly more)	0.480*** (0.080)	0.152* (0.077)	0.254*** (0.072)	0.131*** (0.049)	0.348*** (0.069)	0.388*** (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*** (0.079)	0.278*** (0.075)	0.222*** (0.069)	0.083* (0.046)	0.194*** (0.066)	0.222*** (0.080)	0.022 (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

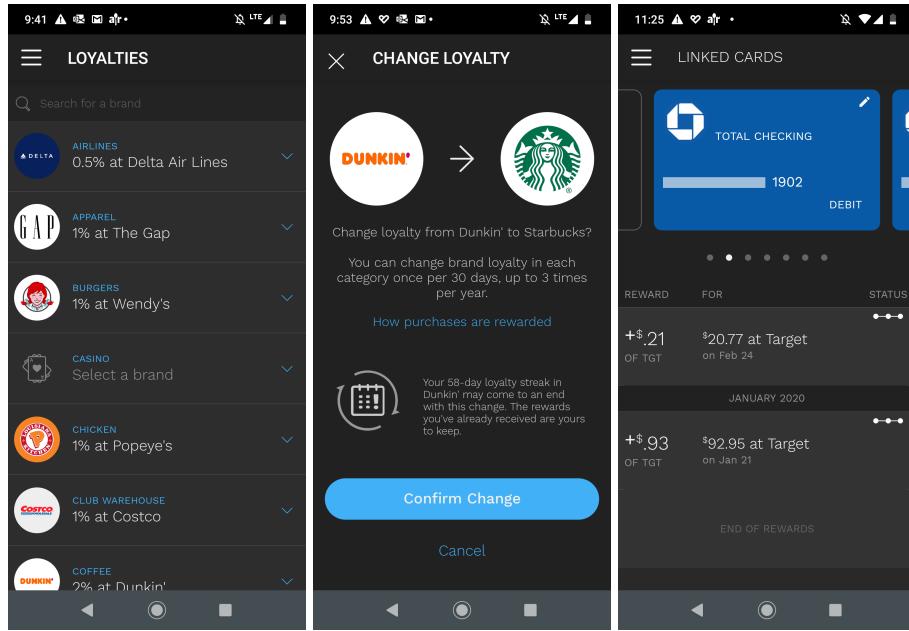


Figure A1: The Bumped app: screenshots of company selection (99 companies in 34 retail characteristics), switching companies, and linked bank accounts and credit card screens.

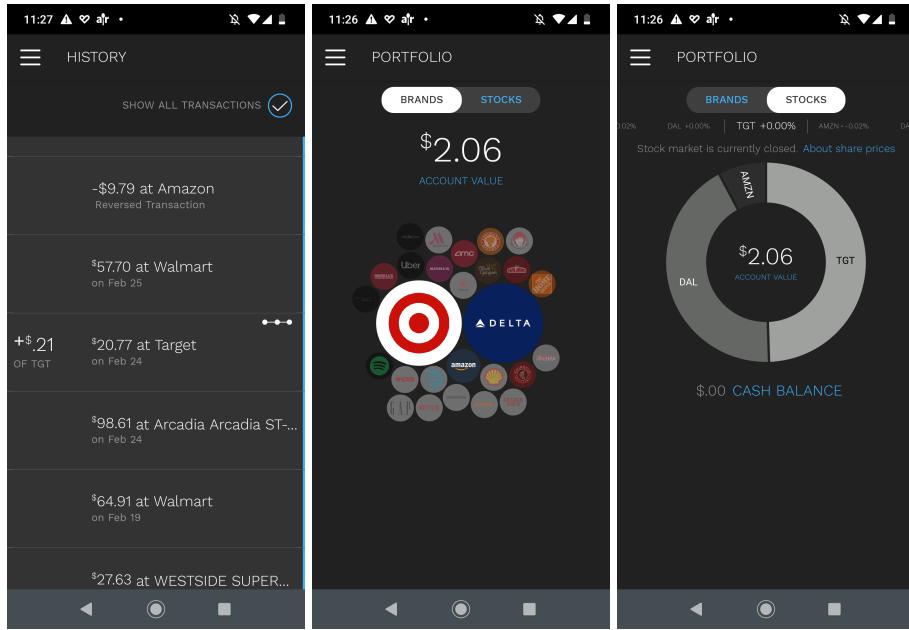


Figure A2: The Bumped app: screenshots of transactions from all linked cards (that may be eligible) and overview of the user's portfolio.

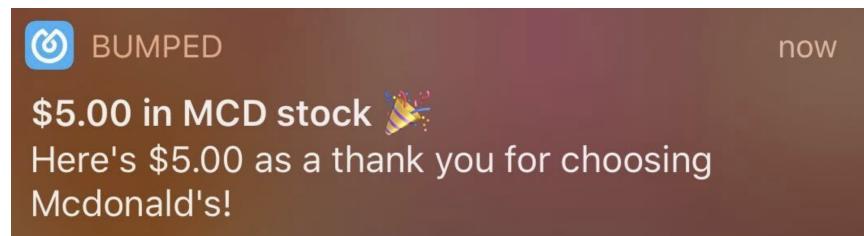


Figure A3: Stock grant notification received by users, push notification.

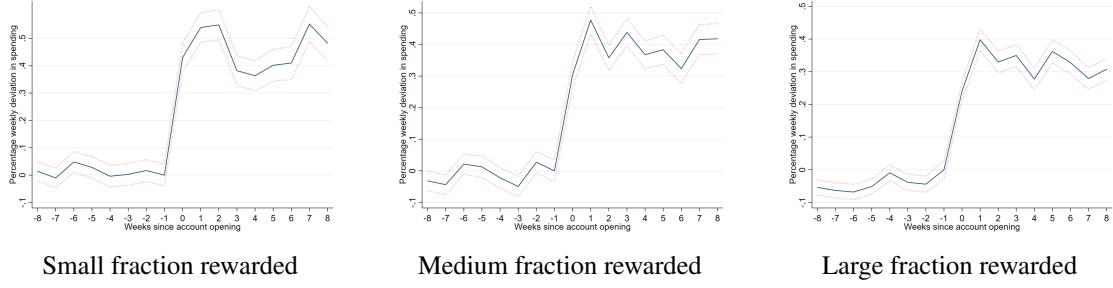


Figure A4: This figure shows the coefficient estimates β_{Bumped}^T in Specification (2) for eligible spending (the percentage deviation from the individual-level mean) and for three terciles of actually rewarded spending transactions as a fraction of all eligible spending transactions. We control for 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. The F-statistics for the joint test that all pre-account-opening coefficients are zero are for the three panels are: F=0.442 (p-value=0.876) for small fraction rewarded, F=1.118 (p-value=0.349) for medium fraction rewarded, and F=1.292 (p-value=0.250) for large fraction rewarded.



Figure A5: This figure shows the coefficient estimates β_{Bumped}^T in Specification (2) for eligible spending (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. The F-statistic for the joint test that all pre-account-opening coefficients are zero are for the three panels are: F=0.546 (p-value=0.800) for small rewards, F=0.852 (p-value=0.544) for medium rewards, and F=0.376 (p-value=0.917) for large rewards.

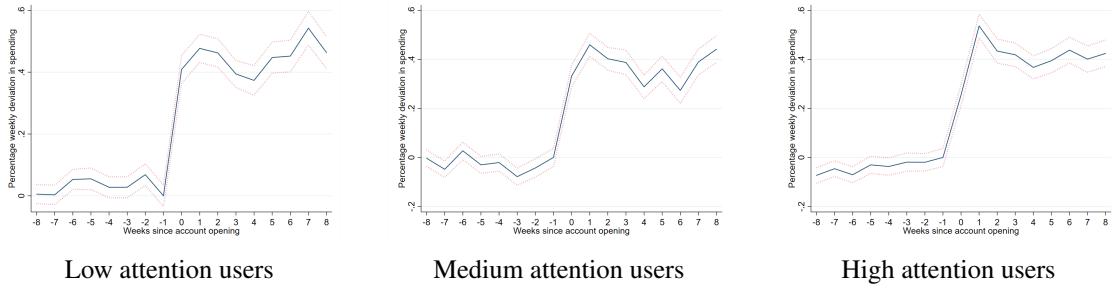


Figure A6: This figure shows the coefficient estimates β_{Bumped}^τ in Specification (2) for three terciles of user attention, defined by the login counts per user in the 8 weeks after account opening. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level. The F-statistics for the joint test that all pre-account-opening coefficients are zero are for the three panels are: F=1.005 (p-value=0.426) for low attention users, F=1.384 (p-value=0.208) for medium attention users, and F=0.877 (p-value=0.524) for high attention users.

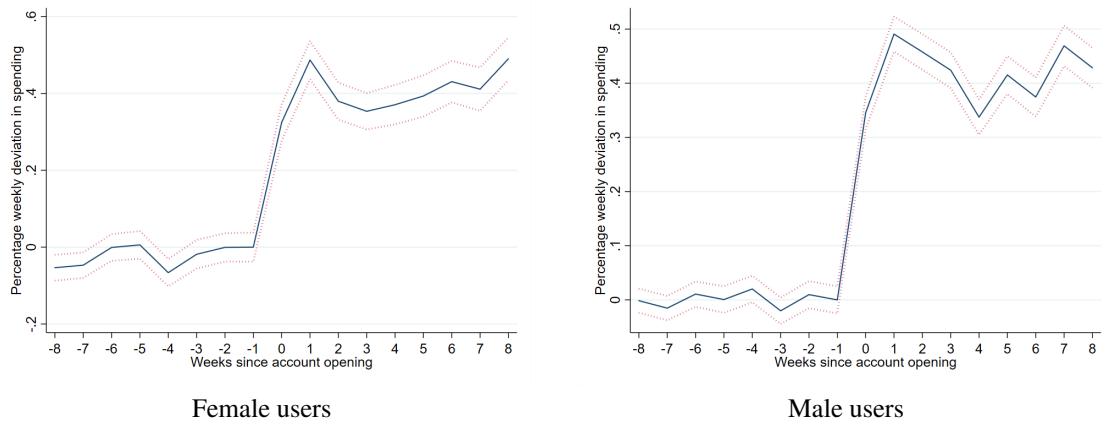


Figure A7: This figure shows the coefficient estimates β_{Bumped}^τ in Specification (2) for eligible spending (the percentage deviation from the individual-level mean) based on whether the user is male or female. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level. The F-statistic for the joint test that all pre-account-opening coefficients are zero for female users is 0.950 and the p-value is 0.466, while the F-statistic and p-value for male users is 0.453 and 0.869 respectively.

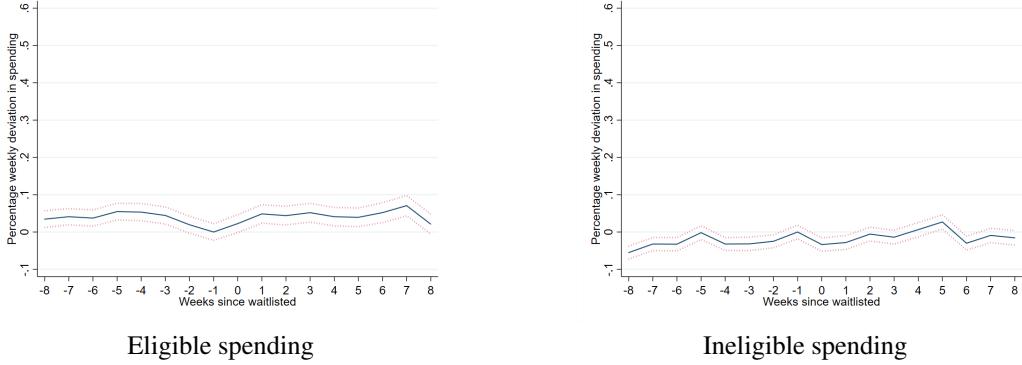


Figure A8: This figure shows the coefficient estimates $\beta_{waitlist}^{\tau}$ in Specification (6) for both eligible and ineligible spending (the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals signed up for the waitlist. Standard errors are shown as the dotted lines and clustered at the individual level. The F-statistic (p-value) for the joint test that all pre-account-opening coefficients are zero for the left-hand-side panel is 1.225 (0.285), while the F-statistic (p-value) for the right-hand-side panel is 1.143 (0.333).

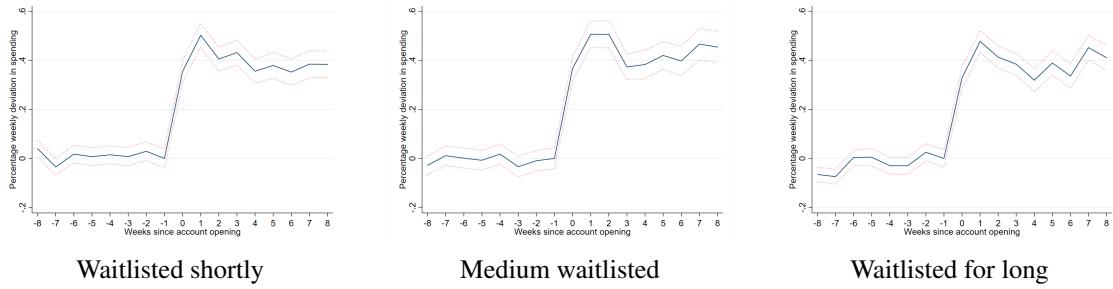


Figure A9: This figure shows the coefficient estimates β_{Bumped}^{τ} in Specification (2) for eligible spending (the percentage deviation from the individual-level mean) and for three terciles of time spent being waitlisted. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level. The F-statistics for the joint test that all pre-account-opening coefficients are zero for the three panels are: F=0.715 (p-value= 0.660) for short waitlist, F=0.436 (p-value=0.880) for medium waitlist, and F=1.680 (p-value=0.109) for long waitlist.

Table A1: Summary statistics of users, initial sample pre data cleaning

	Mean	Std dev	25th percentile	50th percentile	75th percentile
Age	36	9.6	29	34	41
Male	.68	.47	0	1	1
Days from waitlist to open	146	106	73	119	180
Monthly user logins	4.5	9.3	1.4	2.1	3.7
Weekly user logins	2	2.8	1	1.3	1.8
Number of transactions	732	746	261	561	999
Number of cards linked	2.4	1.9	1	2	3
Total monthly spending	1,575	7,528	534	1,051	1,809
Total weekly spending	434	2,018	159	294	488
Monthly eligible spending	244	940	37	127	301
Weekly eligible spending	64	229	11	35	80
Monthly ineligible spending	1,331	7,438	421	852	1,487
Weekly ineligible spending	370	1,997	125	240	403
Grant weekly eligible spending	7.6	29	0	1.2	6.9
Grant weekly ineligible spending	24	500	0	0	0
Monthly eligible spending - grocery	48	134	0	0	22
Monthly ineligible spending - grocery	62	182	0	11	63
Monthly eligible spending - superstores	37	208	0	0	9.3
Monthly ineligible spending - superstores	73	219	.78	16	71
Monthly eligible spending - ride sharing	14	51	0	0	7.6
Monthly ineligible spending - ride sharing	21	57	0	1.3	17
Total rewards	38	66	6.3	19	47
Monthly rewards	1.8	2.8	.28	.99	2.3
Weekly rewards	.48	.7	.083	.27	.62
Total rewarded/eligible	.68	.25	.5	.73	.91
Monthly rewarded/eligible	.64	.27	.43	.66	.89
Weekly rewarded/eligible	.67	.26	.47	.71	.9
Monthly ACH transfers	179	1,066	0	0	0
Weekly ACH transfers	144	902	0	0	0
Monthly ATM withdrawals	57	256	0	0	0
Weekly ATM withdrawals	57	256	0	0	0
Observations	12628				

Notes: This table includes all Bumped users, which are 12,628. The total number of transactions, and spending (in USD), calculated per user include amounts before and after opening the app. Rewards are in USD. Spending only includes transactions that were classified as belonging to a certain company (551 different companies are in the final dataset (in 34 retail categories) of which 99 can be selected to be rewarded). Spending does not include ATM withdrawals. ATM withdrawals are expressed as positive numbers. ACH transfers include all outgoing ACH transfers that are classified as finance or investments, belonging to an identifiable broker, or belonging to an investment services app.

Table A2: Summary statistics of users who received a stock grant, initial sample pre data cleaning

	Mean	Std dev	25th percentile	50th percentile	75th percentile
Age	37	9.3	30	35	42
Male	.69	.46	0	1	1
Days from waitlist to open	200	89	127	179	271
Monthly user logins	4.8	9	1.7	2.6	4.4
Weekly user logins	2.1	2.6	1	1.4	2
Number of transactions	619	609	193	456	879
Number of cards linked	2.2	1.7	1	2	3
Total monthly spending	1,738	4,646	700	1,185	1,884
Total weekly spending	501	1,572	202	334	512
Monthly eligible spending	254	618	58	147	306
Weekly eligible spending	68	160	17	41	81
Monthly ineligible spending	1,485	4,569	567	990	1,550
Weekly ineligible spending	433	1,556	164	277	429
Grant weekly eligible spending	24	67	1.8	9.5	26
Grant weekly ineligible spending	202	1,438	54	99	173
Monthly eligible spending - grocery	71	167	0	.75	66
Monthly ineligible spending - grocery	59	299	0	9	50
Monthly eligible spending - superstores	0	0	0	0	0
Monthly ineligible spending - superstores	133	295	14	59	158
Monthly eligible spending - ride sharing	13	38	0	0	5.9
Monthly ineligible spending - ride sharing	13	40	0	.35	10
Total rewards	25	67	4.2	11	29
Monthly rewards	1.7	2.8	.37	1	2.1
Weekly rewards	.46	.74	.1	.28	.57
Total rewarded/eligible	.64	.29	.4	.72	.9
Monthly rewarded/eligible	.61	.3	.34	.66	.9
Weekly rewarded/eligible	.63	.3	.37	.69	.9
Monthly ACH transfers	162	1,020	0	0	0
Weekly ACH transfers	152	1,015	0	0	0
Monthly ATM withdrawals	31	146	0	0	0
Weekly ATM withdrawals	31	146	0	0	0
Total grant amount	9.7	3.9	5	10	10
Observations	1500				

Notes: Out of the 12,628 users enrolled in Bumped, 1,500 users were also part of the grant promotion program. The 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 categories) of which 99 can be selected to be rewarded). Rewards and grants are in USD.

Table A3: Estimation results of Bumped users' spending amounts on a post account opening interacted with a dummy if users received a grant

	All spending			Spending in granted companies' stores
	Eligible	Ineligible	Total	Eligible
Post 8 weeks	16.120*** (0.601)	-1.216 (2.633)	15.136*** (2.907)	
Post more than 8 weeks	15.855*** (0.764)	-17.003*** (3.353)	-0.104 (3.710)	
Post 8 weeks				1.988*** (0.109)
Post 8 weeks times grant recipient				1.936*** (0.353)
Post more than 8 weeks				1.393*** (0.122)
Post more than 8 weeks times grant recipient				2.650*** (0.394)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	612800	612800	612800	612800
R squared	0.381	0.340	0.372	0.351

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

Notes: In these variants of Specifications (2) and (4), we regress eligible and ineligible spending overall and specifically in granted companies' stores on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of account opening or receiving a 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. For spending in granted companies' stores, we interacted the post dummies with a dummy for whether a user received a grant. User fixed effects and week fixed effects are included.

Table A4: Estimation results of Bumped users' log spending amounts on a post account opening interacted with a dummy if users received a grant

	All spending			Spending in granted companies' stores
	Eligible	Ineligible	Total	Eligible
Post 8 weeks	0.612*** (0.015)	0.041*** (0.012)	0.153*** (0.011)	
Post more than 8 weeks	0.470*** (0.020)	-0.088*** (0.016)	0.028** (0.014)	
Post 8 weeks				0.227*** (0.010)
Post 8 weeks times grant recipient				0.157*** (0.029)
Post more than 8 weeks				0.165*** (0.011)
Post more than 8 weeks times grant recipient				0.218*** (0.034)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	612800	612800	612800	612800
R squared	0.403	0.315	0.333	0.380

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

Notes: In these variants of Specifications (2) and (4), we regress log eligible and ineligible spending overall and specifically in granted companies' stores on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of account opening or receiving a 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. For spending in granted companies' stores, we interacted the post dummies with a dummy for whether a user received a grant. Note that we keep spending amounts between 0 and 1 as absolute numbers. User fixed effects and week fixed effects are included.

Table A5: Reweighted estimation results of Bumped users' ratio of spending, spending amounts, and log spending amounts on a post account opening dummy

	All spending		
	Eligible spending ratio	Eligible spending amount	Eligible spending log
Post 8 weeks	0.443*** (0.024)	12.002*** (0.702)	0.604*** (0.022)
Post more than 8 weeks	0.569*** (0.040)	9.330*** (0.894)	0.403*** (0.031)
User fixed effects	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓
Observations	503001	504007	504007
R squared	0.152	0.306	0.352

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

Notes: In these variants of Specification (2), we perform a weighted regression of the ratio relative to own mean, amounts, and log of eligible spending on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes value 1 for transactions more than 8 weeks post account opening and 0 otherwise. Note that we keep spending amounts between 0 and 1 as absolute numbers. User fixed effects and week fixed effects are included. All estimates are weighted at a user level by age, sex, income, and state of residence to match CPS aggregate figures for 2019.

Table A6: Estimation results of time on waitlist on user characteristics

	Days on waitlist	Days on waitlist	Days on waitlist
Age	0.218 (0.142)	0.129 (0.143)	0.026 (0.127)
Female	0.966 (3.399)	0.474 (3.378)	2.573 (2.876)
Weekly spending	-0.042 (0.029)	-0.054* (0.030)	-0.058* (0.031)
Weekly ineligible spending	0.057* (0.030)	0.062** (0.031)	0.069** (0.032)
Weekly eligible spending	-0.030 (0.042)	0.003 (0.042)	-0.002 (0.041)
Mean of Dep. Var.	127.51	127.51	127.51
Deciles of transaction-history-length fixed effects		✓	✓
Week-by-year of account opening fixed effects			✓
Observations	5409	5409	5405
Adj. R squared	0.002	0.022	0.298

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 A7: Estimation results of Safegraph spending on Robinhood clients weekly holdings of that company

	Daily spending in companies' stores relative to total spending	Daily spending in companies' stores relative to total spending	Weekly spending in companies' stores relative to total spending	Weekly spending in companies' stores relative to total spending
Daily number of holdings in company relative to total holdings Robinhood clients	0.270*** (0.014)	0.052*** (0.008)	0.043*** (0.009)	
Weekly number of holdings in company relative to total holdings Robinhood clients			0.160*** (0.029)	0.074*** (0.008)
Company fixed effects	✓	✓	✓	✓
Date or week-by-year fixed effects		✓		✓
Observations	19396	19396	19396	3528
R squared	0.019	0.975	0.975	0.009
			3430	3430
			0.990	0.990

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

Notes: In this variant of Specification (5), we aggregate the spending data to the day or week-by-company levels and regress the total daily (weekly) spending in all publicly traded companies of all Safegraph card spending data on the daily (weekly) holdings of that company by Robinhood brokerage clients (the data is obtained from robintrack.net). Date (week-by-year) fixed effects refer to any day (week) of the sample period and company fixed effects for any publicly traded companies. The time period and selection of companies (tickers) is the same as in Table 5, however, not all tickers could be matched to the company's spending information in the Safegraph data and we only kept unambiguous matches of the top 200 companies (in terms of the amount spent in their stores) in the Safegraph data. The sample time period is May 2018 to March 2020.

Table A8: Orthogonality test 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, $Y_i = \alpha + \beta \cdot Grant_i + \epsilon_i$, where Y_i takes on different variables as shown in each row of the table, and $Grant_i$ 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. Columns (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.

Table A9: Estimation results of Bumped users' spending ratios on a post account opening dummy interacted with a dummy for users who were sent push notifications about the ride sharing category

	Spending in ride sharing category, percentage deviation							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 8 weeks	0.074*** (0.021)	0.078*** (0.020)	0.078*** (0.020)	0.080*** (0.020)	0.080*** (0.020)	0.080*** (0.020)	0.080*** (0.020)	0.078*** (0.020)
Post 8 weeks times push notification	0.010 (0.047)	0.018 (0.043)	0.018 (0.043)	0.007 (0.043)	0.007 (0.043)	0.007 (0.043)	0.005 (0.042)	0.006 (0.041)
Post more than 8 weeks	0.055* (0.030)	0.066** (0.029)	0.066** (0.029)	0.068** (0.029)	0.068** (0.029)	0.068** (0.029)	0.067** (0.029)	0.063** (0.028)
Post more than 8 weeks times push notification	0.029 (0.064)	-0.020 (0.043)	-0.020 (0.043)	-0.014 (0.045)	-0.014 (0.045)	-0.014 (0.045)	-0.015 (0.044)	-0.021 (0.041)
User fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Weeks since introduction of the ride sharing category	- 1 or more	- 2 or more	- 3 or more	- 4 or more	- 5 or more	- 6 or more	- 7 or more	- 8 or more
Observations	71332	74687	74687	74956	74956	74956	75072	75748
R squared	0.253	0.254	0.254	0.257	0.257	0.257	0.258	0.258

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

Notes: This table presents the results of estimating Specification (4), in which we regress spending in the ride sharing category (the percentage deviation from the individual-level mean) on a post 8 weeks dummy, which takes value 1 for transactions during or within 8 weeks of account opening, or a post more than 8 weeks dummy, which takes value 1 for transactions more than 8 weeks post account opening and 0 otherwise, and their interaction with a dummy that takes the value of one when a given user was sent a push notification announcing the introduction of the ride sharing category within 8 weeks after his or her account opening week. The ride sharing category was introduced on August 13, 2019. All existing users as of that date received push notifications introducing the ride sharing category. Users who opened their account after this date did not receive those communications. In Columns (1) to (8), we consider users who opened their account 1 to 8 weeks before the introduction of the ride sharing category, respectively, and thus received the communications within 1 to 8 weeks after account opening. User fixed effects and week fixed effects are included.