The Effect of Stock Ownership on Individual Spending and Loyalty

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Abstract

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

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

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

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

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

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

[1]To add credibility to our identification strategy, we show that there is no spending response in either eligible or ineligible spending when individuals choose to waitlist. Additionally, we look at the differential responses of users

We show that customers increase their spending at their selected stores in response to being allocated a brokerage account. Weekly eligible spending at selected stores jumps up by 40% and stays persistently high for 3 to 6 months. As eligible spending averages $56 per week, this corresponds to a $23 increase in spending per week. For ineligible spending, we can rule out a decrease larger than 1% in the weeks after account opening from a basis of $295 per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending is smaller than $3.

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

Finally, we have quasi-random variation in the fraction of eligible spending that was ultimately rewarded. This is because not all eligible spending was rewarded; rather, approximately 70% was rewarded due to company operations, policy changes, and constraints. When we split users into terciles based on which fraction of their eligible spending got rewarded, we find a more persistent response in eligible spending for users who had more transactions rewarded.

Beyond documenting a causal impact of stock ownership on spending, we also document 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] In fact, a 1% increase in weekly holdings of a certain company (relative to holdings of all other companies) increases spending at that company's stores (relative to all other spending) by 0.18% (0.12% controlling for company and week-by-year fixed effects). We argue that this result helps us extrapolate our findings to actual spending and stock ownership in brokerage accounts. We chose Robinhood brokerage account data because Robinhood is the most common brokerage account of our users. Additionally, Robinhood clients are likely a similar population to Bumped users.

Finally, we study the link between owning more stocks and having a broader engagement with the stock market. We find evidence for the fact that receiving stock stimulates individual investments by documenting that outgoing brokerage transfers to other brokers are increased after

that were waitlisted for relatively short or long periods of time and do not find large differences in our results. Finally, we show that when individuals are off-waitlisted is not well explained by individual characteristics in a randomization check.

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

individuals start receiving stock rewards. This is consistent with survey results showing increases in the self-reported likelihood of investing in stocks outside of Bumped in response to stock ownership through the platform.

What explains customers' spending responses after receiving stock rewards and grants? We argue that the mere monetary value or price effect of rewards is unlikely to fully explain the changes in behavior we observe, as the transaction and hassle costs required to increase spending between 40% to 100% at specific stores likely outweigh the 0.5% to 2% price effect of the rewards. Instead, we argue that stock ownership carries non-pecuniary benefits that trigger loyalty, i.e., a preference for a specific company's products that goes beyond the consumption experience. In general, any reward program, based on stock, cash, or any other payoff, can increase loyalty, if the reward is perceived as a gift that triggers reciprocity and affect. In that sense, affect and gift exchange are potential mechanisms behind our results. However, the magnitude of our results is substantially larger than the effect documented in the literature for cash rewards. Vana et al. (2018) calculate that, when an additional $1 in cashback payment is offered, spending increases by $3.51, entailing an effectiveness of 351%. In comparison, stock rewards have an effectiveness of 2,053%: we find a $23 increase in weekly eligible spending when the average amount offered in stock rewards is $1.12 per week (stock rewards are about 2% of weekly eligible spending, which in turn averages $56 during the observation period). We thus argue that there are additional factors other than affect and gift exchange that further enhance the feelings of loyalty experienced by stock owners. We discuss a number of psychological mechanisms that can explain why stock rewards trigger loyalty, such as familiarity, the illusion of control, and reductions in cognitive dissonance.

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

Our results have implications for the economy and for asset prices. The canonical consumption-based asset pricing model is inconsistent with the empirical fact that aggregate consumption and

stock prices are not very highly correlated. In contrast, at a high frequency and a very granular level (individual company), we document a relationship between spending in certain company products and holdings of the companies' stocks that is stronger than the correlations found in more aggregate data. This suggests that stock price fluctuations affect spending and thus utility in a more direct way than previously thought. In addition, previous work has found that brand loyalty is an intangible asset that leads to lower cash flow volatility and can influence firm value (Dou et al., 2019; Larkin, 2013). We document that stock ownership itself triggers loyalty and spending, which could thus influence company value. Finally, we document that well-known behavioral biases are not restricted to the domain of investing and trading but extend to consumption, a direct component of loyalty and welfare.

Literature Review

Our study is related to prior literature suggesting that purchase behaviors and beliefs about a company have an impact on investment choices and vice versa. Previous literature shows that investors tend to buy what 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). Closest to our work, Keloharju et al. (2012) uuse individual brokerage account data to show that the clients of a given broker invest in the stock of that particular broker and the owners of a given car invest in the respective car company. The authors also document a causal link between receiving stock from the same broker or auto company through inheritances and the demand for their products and services. However, that link is weaker. Additionally, Aspara et al. (2009), Aspara and Tikkanen (2010), and Aspara and Tikkanen (2011) use survey data to provide an association between stock ownership and future purchase intentions, even in the event of price increases, which they interpret as loyalty. From a theoretical perspective, Altinkemer and Ozcelik (2009) explore the role of alignment of incentives between customers and firms when loyalty rewards are provided in the form of equity instead of cash.

To the best of our knowledge, only one existing study, Bernard et al. (2018), looks at the random distribution of stocks from various companies to individuals in an experimental setting. In this study, graduate students are randomly assigned some companies' stocks in the lab without any prior knowledge of whether the students are already customers of the invested-in firms. The study shows that customer-stockowners purchase more of the firms' products and argues that the subjects changed their preferences to favor the invested-in firms in line with Fama and French (2007). This research thus sheds additional light on the behavioral links between stock ownership

and firm performance via increased sales. In contrast, this paper looks at a naturally occurring setting that elicited participation of a broad cross-section of the US population. We then utilize quasi-random variation in the distribution of stock from approximately 100 companies in 34 retail categories that were selected and are familiar to most customers (e.g., Walmart, Target, Starbucks, Gap, and Macy's).

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. In that sense, our results contribute to the growing literature studying the effect of customer capital or brand loyalty on 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 argue that stock ownership itself triggers brand loyalty through a variety of behavioral mechanisms and increases spending on brands corresponding to those stocks.

Since our analysis is based on a rewards program where rewards could be perceived as gifts, our results also relate to the literature on reciprocity (Falk, 2007), which documents that non-monetary incentives or gifts can have larger impacts than monetary incentives of comparable value. In a controlled field experiment, Kube et al. (2012) recruited workers to catalog books from a library on a temporary basis. They find that incentivizing workers with in-kind gifts (thermos bottles) triggered substantial reciprocity in the form of increased productivity, whereas an equivalent wage increase (20% of the hourly wage) did not lead to increases in productivity. However, gift exchange findings measured in the field were sometimes inconclusive and contradictory (Kessler, 2013). In our setting, we test the role of company stock as a currency for reciprocity. Our findings are consistent with the supersizing effect of reciprocity on incentives, as they are quantitatively difficult to explain via the monetary value of the rewards. While we are not able to directly compare stock and cash rewards, our measured effects are larger than those of cash-back rewards as documented in the literature (Vana et al., 2018).[3]

Our paper is also related to the growing literature on consumer spending using data from new online financial platforms, often called FinTech apps (see Goldstein et al., 2019, for a literature survey), such as Gelman et al. (2015), Baker (2018), Kuchler and Pagel (2019), Olafsson and

[3] 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.

Pagel (2018), Medina (2020), and Koustas (2018). In the domain of stock market investments, our paper is specifically related to research papers using bank account spending and income data linked with securities trades and holdings data such as Meyer and Pagel (2018) and Loos et al. (2018). In contrast to looking at spending responses to income shocks, nudges, or capital gains, we examine spending responses to rewards in the form of company stock. To that end, our paper is related to new technologies in advising consumers, rewarding consumer behavior, or targeting marketing efforts. For instance, see 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.[4] To receive a user account, individuals first have to sign up for a waitlist on the company's website. At the time of signing up for the waitlist, interested users provide their email addresses and names. No additional information is provided at that time. In turn, on a first-come, first-served basis, users are invited to open a brokerage account. If users failed to open an account when approved, two reminder emails were sent. Once users signed up for an account, they can link all of their checking and credit card accounts. In turn, customers can select their favorite companies in a number of retail spending categories.

All featured companies are divided into 34 different retail categories, and users can select one company from each category. If users then spend at their selected companies' stores, they receive

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

fractional shares of the companies as a reward. Customers can switch their selected companies every 30 days, but only up to three times per year. The functionalities of the brokerage account are limited. Users are not allowed to deposit their own money or purchase additional stocks, but they can sell their (individual or entire) positions at any time, in which case the cash proceeds are transferred to a linked bank account.

Figures 1 and 2 show several screenshots of the FinTech app. Figure 1 shows screenshots of the company selection, switching companies, and linked card screens. In the linked card screens, one can see which transactions were rewarded by stocks. All eligible and ineligible transactions can be seen in the transactions screen in Figure 2. Additionally, this figure shows two screenshots of the portfolio containing the stock rewards the user received and their current value as well as their daily changes. As part of a promotional program, some users received stock grants upon signing up. Figure 3 shows the push notification a user receives upon getting a stock grant.[5]

2.2 Data

Baseline demographics

We received an anonymized subsample of the user base. As of March 2020, our data subsample includes 11,424 users. The dataset includes de-identified information on financial transactions and demographic characteristics, including each user's age, gender, and 5-digit ZIP code.[6] Figure 6 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. The demographic data is summarized in Table 1. 67% of users are male, 17% are female, and 16% do not report their gender. 871 users are less than 25 years old, 9,431 are between 25 and 49, and 1,122 are greater than 50 years of age. Our user population is, as often the case for Fintech app data, more likely to live in an urban area, be male, and be younger than the average American.

Dates and timeline of users

Bumped was launched in 2017, and we received users' de-identified and aggregated transactions from 2016 to 2020. We observe the dates on which users signed up for the waitlist, when they

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

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

get off the waitlist and were invited to open their brokerage accounts, and when they effectively opened their brokerage accounts. While the majority of users create their accounts right when they are taken off the waitlist, some users wait a few days before doing so. To avoid selection issues in the timing of account opening after getting off the waitlist, we restrict the analysis to those users who opened their accounts within one week after they were invited to do so. Figure 4 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 3.3 months.

Spending

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

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

To ensure that our empirical results are not driven by transactions being observed after but not before sign-up, we perform checks to exclude linked cards that might be observed imperfectly. We exclude all linked cards with less than 2 transactions in the four two-week periods either before or after the opening account week, before and after the waitlisted weeks, or before and after the grant weeks. These 8-week windows correspond to our estimation period. Additionally, we exclude all months in which there were less than 5 days with spending. The 5-day threshold is commonly used in other research papers using transaction-level data to ensure completeness of records (e.g., see Kuchler and Pagel, 2019; Olafsson and Pagel, 2018; Ganong and Noel, 2019). The first step

reduces our sample of linked cards by 6,759 cards, taking it from 26,813 cards to 20,054 cards. The second step reduces our sample of spending days by another 15% from 7,829,699 to 6,771,353 observations. Summary statistics for the adjusted sample are reported in Table 3. After these adjustments, we have a total of 9,005 users.

Stock grants

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

Figure 4 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 2. 1,371 users were awarded grants during or one week after the week of opening an account. Over the observation period, users who received stock grants spent $519 per week on average. The average grant amount was $10. The distribution of grants was quasi-random, as users were not informed in advance of the promotional program and thus could not select into it endogenously.

We perform a covariate balance test between grant recipients and non-recipients before they get off the waitlist. In Table 13, we can see that grant recipients are very comparable to non-recipients in a number of observable characteristics, including age and eligible and ineligible spending. The only statistically significant difference is in terms of the number of transactions per month. Grant recipients perform 328 transactions per month compared to 301 transactions by non-receivers. We argue that, while statistically significant, the difference is not economically significant. Given that the spending data was not observable to the company before account opening, it could not affect whether or not users received a stock grant.

Comparison to other datasets

In Table 4, we compare our sample to the Consumer Expenditure Survey (CEX). Since this survey is performed at the household level, we normalize spending dividing by the average household size of 2.52. Relative to the average head of household in the CEX, our users are younger and more

likely to be men. Our users spend of $1,496 per month on average whereas the average American spends $2,205 during the same time period. We note that our data includes spending only on 551 identified brands. All transfers, e.g., for rent or utilities, are left out. After taking that into account, we argue that the spending levels of our users are broadly similar to those in the CEX.

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

In Table 5, we show in a simple regression that our users' brand-level spending data is strongly positively correlated with the Safegraph card spending data. Excluding brand and date fixed effects, the daily estimated coefficient of our spending data in certain brands relative to total spending with the Safegraph card spending data in the same brand relative to total spending corresponds to the raw correlation coefficients, as we normalize the spending data by their standard deviations. The raw data correlation coefficients between the Bumped and Safegraph spending data are 0.476 and 0.442 at the daily and weekly levels, respectively. The estimated coefficient is highly significant, and the adjusted R squared is around 20%. We take these results as indicative that our users' spending behavior is broadly consistent with the spending behavior of a more representative sample of the population.

2.3 Empirical strategy

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:

$$Spending^{iw}_{Eligible} = \alpha_i + \eta_w + \sum_{\tau=-8,\ldots,8} \beta^{\tau}_{Bumped} \omega^{iw\tau}_{Bumped} + \epsilon^{iw} \tag{1}$$

In Specification 1, $Spending^{iw}_{Eligible}$ denotes eligible spending (i.e., spending with a company that the user elects at sign-up) by user i in week w, α_i is an individual fixed effect, η_w is a week-

by-year fixed effect, and $\omega^{iw\tau}_{Bumped}$ is an indicator of whether user i in week w had received his or her account in his or her user-specific τ's week. The coefficients β^{τ}_{Bumped} thus tell us the path of eligible spending after the user received his or her account. We consider 8 weeks before and after receiving the account. 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 8 weeks after account opening and one dummy for all other weeks as well as individual and week-by-year fixed effects.

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

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

In Specification 2, $Spending^{iw}_{Eligible}$, α_i, and η_w are defined as in Specification 1. In turn, $\omega^{iw\tau}_{Grant}$ is an indicator of whether user i in week w had received the grant in his or her τ's week. For users that never received a grant, $\omega^{iw\tau}_{Grant}$ is always zero. The coefficients β^{τ}_{Grant} thus tell us the history of eligible spending before and after a user received the stock grant, which coincides with the date of account opening. We consider 8 weeks before and after individuals received the grant and look at all eligible spending as well as spending in the granted companies' stores. Additionally, we report the results of a variant of this specification in which we include one dummy for the 8 weeks after grant receipt and one dummy for all other weeks after account opening as well as individual and week-by-year fixed effects.

We also perform a differential analysis to study the effect of stock grants on spending upon account opening. First, we estimate the same specification as in Equation 1, but we split the sample into users who received a stock grant and those who did not. Second, we formally compare the differential responses in the spending of these two groups with the following difference-in-difference specification:

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

In Specification 3, $Spending^{iw}_{Eligible}$, α_i, η_w and $\omega^{iw\tau}_{Bumped}$ are defined as in Specification 1. In turn, $Grant_i$ is a binary variable taking the value of 1 when a user received a grant at the time of account opening. The coefficients β^{τ}_{BG} thus identify the incremental effect of receiving an account

and a grant relative to the effect of receiving an account without a grant, β_B^τ, in each user-specific τ's week. We consider 8 weeks before and after individuals received the grant. We estimate Equation 3 for both overall eligible spending as well as restricted to the specific companies for which stock was granted.

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

$$Spending_{Eligible}^{iw} = \alpha_i + \eta_w + \sum_{\tau=-8,\ldots,8} \beta_{Waitlist}^{\tau} \omega_{Waitlist}^{iw\tau} + \epsilon^{iw} \tag{4}$$

In Specification 4, $Spending_{Eligible}^{iw}$, α_i, and η_w are defined as in Specification 1. In turn, $\omega_{Waitlist}^{iw\tau}$ 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 consider 8 weeks before and after individuals signed up for the waitlist.

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

3 Results

3.1 Main effects on spending

As a starting point, Figure 7 plots the raw data means of eligible and ineligible spending 8 weeks before and after account opening. Here, we look at the ratio of spending relative to each individual's mean average over the entire 16-week period. Thus, the axis shows the percentage deviation of spending relative to each individual's average. We can see in this raw-data plot that eligible spending increases by approximately 40% in the week of account opening and stays high. A large spike is visible in eligible spending, while there is no major change in ineligible spending.

Figure 8 shows the β_{Bumped}^{τ} coefficients and standard errors from Specification 1 for both eligible spending as well as ineligible spending as the left-hand side variables. Spending is measured as the individual-level percentage deviation from the sample average eligible spending in a given week. The coefficients thus represent the percentage deviation in eligible spending before and after users received their accounts. We can clearly see a pronounced spike in eligible spending in the week that users receive their accounts. Weekly spending at selected companies' stores jumps up by 40% and stays persistently high for the 8 weeks we look at. In terms of US dollars, eligible spending averages $56 per week, so this corresponds to approximately a $22.4 increase in spending per week. Additionally, we do not see a comparable pattern in ineligible spending. For ineligible

spending, we can rule out a decrease larger than 5% in the weeks after account opening from a basis of $295 per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than $15.

Figure 9 shows the β^{τ}_{Bumped} coefficients and standard errors from Specification 1, splitting the sample into grant receivers and non-receivers. In both cases, we can see again a clear increase in eligible spending along the order of 40% following account opening.

We also present the results from estimating Specification 2 in Figure 10 for both eligible spending in general and eligible spending at the companies' stores of which users were granted stock as the left-hand side variables. As before, the coefficients thus represent the percentage deviation in eligible spending before and after users opened their accounts and received their stock grants. We can clearly see an increase in overall eligible spending in the week after users received their grants of about 40%, which equals the account opening effect. Additionally, eligible spending at the brands for which the user received a grant increases even more by about 100%.

As a complement to the figures, Column 1 of Table 6 shows the average effect for the 8 weeks following account opening. With this alternative estimation, we obtain a 38% increase in eligible spending relative to the sample average of each individual. Column 2 shows a 3.6% decrease in ineligible spending with a standard error of 2.3%; we can thus rule out a decrease of more than 1% in ineligible spending with statistical confidence. Columns 3 and 4 show the results for eligible spending in granted companies' stores, and we can document a 93.4% increase in spending with a negligible effect on ineligible spending, i.e., spending in categories for which the user received a grant but did not select at sign up.

Columns 1 and 2 of Table 7 show a similar analysis, but, in this case, we directly use dollar spending per week as the dependent variable. Receiving stock rewards leads to substantial increases in average spending per week, in this case an increase of $19 per week in eligible spending and an insignificant $8.4 increase in ineligible spending. We can thus rule out any decrease in ineligible spending with statistical confidence. Finally, Table 8 shows similar effects for log spending instead of the absolute amounts or the percentage deviations from each individual's mean.

Table 6 summarizes the graphical results of changes in spending upon receiving a stock grant. In columns 3 and 4, we can see that, during the 8 weeks following grant disbursement, there is a 100% increase in spending on brands that match the stock grant and a non-significant decrease in ineligible spending. These coefficients represent deviations from weekly average spending during the period of analysis. Table 7 shows similar results, estimating the effects on spending directly in dollar terms instead of deviations from weekly averages. Columns 3 and 4 show a mean effect of a $1 increase in eligible spending and a reduction of $0.67 in ineligible spending. Note that

the average effect is very small in dollar terms because the number of weeks that the average user frequents a specific store, e.g., McDonalds, is very small.

Figure 11 shows the β^{τ}_{BG} coefficients and standard errors from Specification 3. We present the results for (in)eligible spending at the brands for which users received stocks as the left-hand side variables. Spending is measured as the individual-level percentage deviation from the 8-week estimation period average eligible spending in a given week. The coefficients thus represent the incremental effect of receiving an unexpected stock grant at the time of account opening as a percentage deviation of weekly spending before and after users received their stock grants. Figure 11 shows a pronounced effect on spending in brands corresponding to the stock that was granted. The incremental effect in spending is in the order of 200% initially; this is followed by a decrease, and then we observe another increase.

3.1.1 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 12 shows increases in eligible spending between 30% and 100% relative to the average weekly spending during the window of analysis. Superstores are the only category that shows a substantial decrease in spending after an initial jump in the two weeks immediately after account opening. The results for ineligible spending are mixed, with some categories like coffee showing substantial offsetting effects and some others (the majority) showing a flat response to account opening on ineligible spending.

3.1.2 Sample splits by rewards ratios

Due to company operations and constraints, only an average of 70% of eligible spending was rewarded. Only eligible spending was ever rewarded; ineligible spending was never rewarded. When we look at eligible spending, we look at spending in selected categories rather than spending that was actually rewarded in the pre- and post-periods of account opening. To the extent that whether a transaction is rewarded is exogenous to the user, we can exploit variation in the fraction of eligible spending that was rewarded to see if receiving higher rewards leads to differential effects on spending compared to receiving lower levels of reward.

Do users behave differently in response to the account opening if a lot instead of little of their eligible spending is rewarded? We look at the sample splits of terciles of individuals being rewarded by a lot versus a little relative to their eligible spending in Figure 13. While we see the

same initial spike, we see a more persistent response in eligible spending for those users who were rewarded at relatively higher levels.

3.1.3 Sample splits by login activity

We also look at heterogeneities as a function of login activity, which we use as a proxy for attention to financial accounts. Figure 14 presents the results of estimating Equation 1 after splitting the sample into terciles of login counts per user. Across the spectrum of the attention distribution, eligible spending shows an increase to the order of 40% in the weeks following account opening. Users in the high attention category show larger spikes, reaching up to a 60% increase in eligible spending in week 6.

3.1.4 Long-term effects

We look at the effects of eligible and ineligible spending further out than 2 months. When we consider 3 or even 6 months after account sign-up, we find some dissipation but still a significant increase in eligible spending, as can be seen in Figure 15. When we look at this longer estimation window, weekly spending at selected companies' stores jumps up by 40% and stays persistently high for 3 to 6 months. In terms of US dollars, eligible spending averages $56 per week, so this corresponds approximately to a $23 increase in spending per week. Note that these long-term effects are naturally disseminating if users switch their favorite companies in certain retail categories. In these figures, we only take the initial pick of companies as the measure of eligible spending.

3.2 Spending and stock ownership outside of the rewards platform

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

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

In Table 9, we find that a 1% increase in holdings of a certain company is correlated with spending in that company's stores by 0.12%, controlling for company and date fixed effects. Ag-

gregated to the weekly level, this coefficient increases to 0.14%. We thus find a very strong positive correlation between spending and stock ownership in the observational data.

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

In Table 10, we can see that the results line up sensibly. The Safegraph card spending data is positively correlated with Robinhood holdings at the daily and weekly levels as well. After including brand and time fixed effects, the correlations are a bit lower for the Safegraph spending relative to the Bumped spending. This likely reflects the fact that the Bumped population is more interested in stocks (similar to Robinhood clients) than the overall population of younger bank customers, as in the Safegraph data.

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

3.3 Impact on brokerage account transfers

To look at brokerage account transfers, we first flag all ACH account transfers that are categorized as financial.[7] 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 11 Column (1) shows the log of brokerage amount transfers post 8 weeks of opening a user account, controlling for all future weeks post 8 weeks after account opening, week-by-year fixed effects, and user fixed effects. It can be seen that log brokerage account transfers increase by 2.7% in the 8 weeks of account opening. shows that the likelihood of brokerage account transfers by a user post 8 weeks after opening an account increases by 1.4%. Relative to the baseline propensity to invest, these coefficients represent an increase of approximately 5% in the amount and likelihood of investing.

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

These results indicate that users not only spend more on companies that give them stocks as rewards, but they also increasingly transfer funds to their brokerage accounts. The stock rewards

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

likely engage users with the stock market on a more frequent and regular basis, which increases their propensity to invest.

4 Robustness checks

4.1 Placebo tests

Figure 16 shows the $\beta^{\tau}_{waitlist}$ standard errors for both eligible and ineligible spending as the left-hand side variables. Again, spending is measured as the individual-level percentage deviation from the sample average eligible spending in a given week. The coefficients thus represent the percentage deviation in eligible spending after users signed up to be waitlisted. As expected, there is no clear pattern in eligible or ineligible spending in the week that users chose to sign up and were waitlisted.

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

4.2 Orthogonality tests for time waitlisted and grant receipt

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

To confirm this, we perform a randomization test regressing the time on the waitlist on individual-level characteristics. Column 1 of Table 12 shows that neither age, nor gender, nor spending patterns before account opening are significant predictors for the number of days spent on the waitlist. Furthermore, the R squared is very low (0.0023). As a predictability test, we add a number of fixed

effects in Columns 2 and 3 to see how the R squared changes. We find that the R squared remains low. The one exception is fixed effects for week-by-year of account opening. Including these fixed effects increases the R squared to 0.27. However, in contrast to the potential impact of user characteristics, it is unlikely that individual users had expectations regarding how long it would take to get off the waitlist. We thus conclude that users would not be able to predict when they would receive their accounts, and it would be difficult for them to time their spending to coincide with the week in which they received their accounts.

Similarly, we also perform a covariate balance test between grant recipients and nonrecipients before they were taken off the waitlist. In Table 13, 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 study heterogeneous treatment effects based on the time spent on the waitlist. We do so by splitting the sample according to when individuals were waitlisted. The results for receiving an account for three terciles of the time individuals were waitlisted can be found in Figure 17. There are no discernable differences.

4.4 Substitution from cash spending

There is a concern that users might be withdrawing money after opening an account or may substitute from cash transactions to card transactions. We look at net ATM withdrawals to address these two possibilities. Table 14, Column (1) shows the net withdrawal ATM amount post 8 weeks of opening an account, controlling for all future weeks post 8 weeks of account opening, week-by-year fixed effects, and user fixed effects. Column (2) looks at the percentage deviation of ATM net withdrawal amounts. The insignificant coefficients in both columns suggest that increases in spending in selected companies' stores for the average user are not associated with a decrease in ATM withdrawals.

5 Stock ownership and self-reported loyalty: Survey evidence

We analyze the responses to four surveys sent to Bumped users between 2019 and 2020. The surveys were designed and administered by the Bumped team. The number of respondents varies across the surveys, ranging from 455 to 672 respondents per survey. The specific questions in each

survey are also different, which is why the number of users responded to each survey question ranges from 1,160 to 2,217. In this section, we discuss questions that help us understand the characteristics of users and their attitudes toward stock ownership and financial markets. For exposition purposes, we modify the original numbering of the questions.

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 18 shows the fractions of users that responded "yes" to each of the 4 options presented. The right panel of Figure 18 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 19 shows that, since starting to use the app, more than 65% of survey respondents report feeling more loyal toward the brands they receive rewards from, 40% report feeling a more positive attachment towards those brands, almost 45% of users shop less with competing brands, 16% report paying more because they are owners of the brand, and 46% report going out of their way or traveling longer distances to shop on brands they own. The responses are consistent with our spending results and with the results of Aspara (2009). Overall, these responses suggest that stock ownership leads to increased brand loyalty, which we argue explains the large spending response to stock rewards.

The third question asks users about their likelihood of investing outside of Bumped in the future as a result of owning stock through Bumped. Figure 20 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

Figure 21 shows that, not surprisingly, survey respondents strongly prefer stock rewards over traditional rewards such as cash back, points, or coupons. 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."

Table 15 correlates the answers to Question 4 with the answers provided in Questions 2 and 3. The correlation of questions about loyalty and engagement with the stock market with “excitement" about stock rewards is informative about the role of the non-pecuniary benefits of stock ownership on loyalty and stock market participation. Variation in excitement about stock rewards can be interpreted as variation in the non-pecuniary benefits of stock ownership. Therefore, a positive and significant relation between excitement about stock rewards and loyalty can be interpreted as a positive relation between the non-pecuniary benefits of stock ownership and loyalty.

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

Similarly, Column 6 shows that a preference for stock rewards over traditional rewards is also a strong predictor of increases in the likelihood of investing outside of Bumped as a result of owning stock through Bumped. Finally, in Column 7, we test whether the correlation in Column 6 is concentrated for users who do not invest already in different financial instruments. We interact the continuous Likert scale measuring preferences for stock rewards with the number of financial

instruments reported in Question 1 and regress the interaction terms with the corresponding main effects on the self-reported increases in the likelihood of investing outside of Bumped as a result of owning stock through Bumped. We find that the correlation of preferring stock rewards and investing outside of Bumped is present across the distribution of the number of financial accounts reported in Question 2 (the main effect of preferring stock is positive and significant, while the interaction coefficient is not statistically significant). Users who invest in several or few financial accounts outside of Bumped are equally likely to increase their likelihood of investing outside of Bumped as a result of owning stock through Bumped. Overall, these results suggest that there are non-pecuniary benefits of owning stock and that, the larger these benefits are, the larger the effect is on both spending and stock-investing activity.

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

6 Psychological mechanisms

Our empirical results show that stock rewards lead to large increases in spending that are not commensurate with the small monetary value of the reward. We argue that, in addition to the monetary rewards, users receive non-pecuniary benefits that trigger loyalty, i.e., a preference for a specific brand that goes beyond the consumption experience. In general, any reward program based on stock, cash, or any other payoff can lead to loyalty if the reward is perceived as a gift that triggers reciprocity and affect. In that sense, affect and gift exchange are potential mechanisms behind the general result that reward programs lead to large 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 the affection of the shareholders and, in turn, alter their behaviors to positively impact the company. More generally, people are inclined to have a positive view of themselves and their associations (Greenwald and Banaji, 1995). Given that stock owners are likely to identify more closely with the firm (Turner and Tajfel, 1986) and

with the shareholder community, the positive views investors have about themselves can result in additional company-specific feelings of affect.

Similarly, gift exchange can also enhance the effectiveness of rewards. Gift exchange refers to the phenomenon of more value being placed on the same objects if they are acquired or received as gifts rather than bought. 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. Furthermore, the company rewarding the stocks can be thought of as a third party. However, if users perceive the stock rewards as an unconditional gift that ultimately came from companies that cooperated with Bumped, then gift exchange would be a relevant mechanism behind the effects we see. The stock rewards and grant promotional program are funded and administered by Bumped. However, that is not obvious to customers, and they may 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.

In addition to the general gift exchange and affect effects, the currency of the rewards and grants may enhance their impact on loyalty and spending. While we are not able to provide conclusive evidence of an incremental effect of stock rewards over cash rewards, our results are substantially larger than the results in the literature documenting the impact of cash-back through online platforms on spending. In the following, we present a number of psychological mechanisms that may be behind the large effect of rewards on spending when the rewards take the form of stock.

Cognitive dissonance By cognitive dissonance, we refer to the mental discomfort deriving from simultaneous and conflicting beliefs or behaviors. This status leads to an alteration in either the beliefs or behaviors in order to reduce the dissonance and restore balance (Bénabou and Tirole, 2011; Festinger, 1962; Gilbert et al., 1998; Gilbert and Ebert, 2002). In the context of stock ownership, investors experience cognitive dissonance when they take actions that do not support the invested-in company. To ease the discomfort, shareowners can change their beliefs by, for example, acknowledging that their individualistic choices are not important enough to tip the scales for the firm. Alternatively, investors could change their behaviors in a way that is favorable for the company (e.g., by avoiding buying substitute products from a competitor). In Figure 19, we find suggestive evidence for the latter.

Illusion of control Receiving the shares of a certain company may make individuals believe that their actions are able to affect the company's stock price. Despite atomistic behaviors having a

very small probability of affecting tangible outcomes (Feddersen, 2004), by believing so, investors tend to make decisions that could positively affect the company's value. The reason for this behavior is that individuals tend to overestimate the likelihood of small probability events (Lichtenstein et al., 1978; Fox and Tversky, 1998) and their ability to influence events they demonstrably cannot (Langer, 1975). This mechanism could explain why we see more spending at the brands for which individuals received stock in response to the grant. While individual spending could not effectively affect stock prices, the illusion of control could also trigger the perception of alignment of incentives. Giving stock rewards could pay a similar role to that of stock compensation in corporations (Hochberg and Lindsey, 2010; Oyer and Schaefer, 2005; Altinkemer and Ozcelik, 2009). Some of the quotes provided by platform users in the open-ended questions of a survey are consistent with them having an illusion of control.

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

7 Conclusion

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

We show that customers increase their spending at the selected companies' stores after receiv-

ing stock rewards in their brokerage accounts. Weekly spending at selected companies' stores jumps up by 40% and stays persistently high for 3 to 6 months. In terms of US dollars, eligible spending averages $54 per week, so this corresponds to approximately a $23 increase in spending per week. We can rule out a decrease larger than 1% in the 8 weeks after account opening from a basis of $292 per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than $3.

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

We argue that our findings cannot be fully explained by a pure price effect, i.e., we would not expect individuals to change their spending behavior in such a material way in response to rewards ranging from 0.5% to 2%. Using survey evidence and data on transfers to brokerage accounts, we argue that loyalty appears to be the dominant psychological mechanism explaining the spending responses. As with any reward program, this loyalty could be triggered by gift exchange and affect if consumers perceive the rewards as a positive gesture from the companies. Furthermore, familiarity, illusion of control, and reductions in cognitive dissonance could explain the enhanced effect of stock rewards relative to estimates in the literature for the effect of cash rewards.

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

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Figures and tables

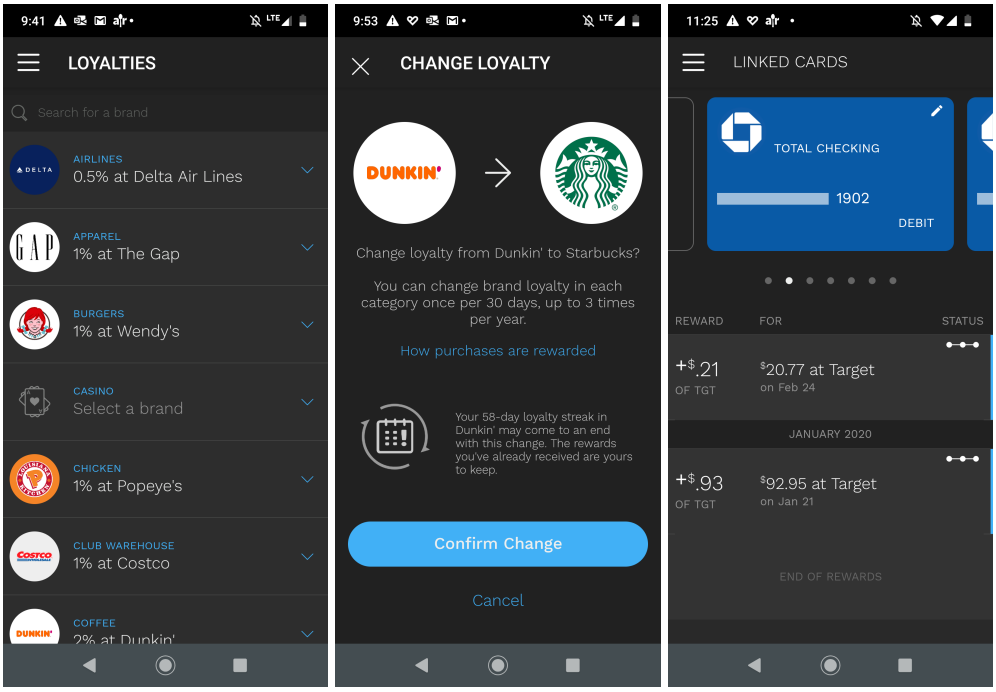
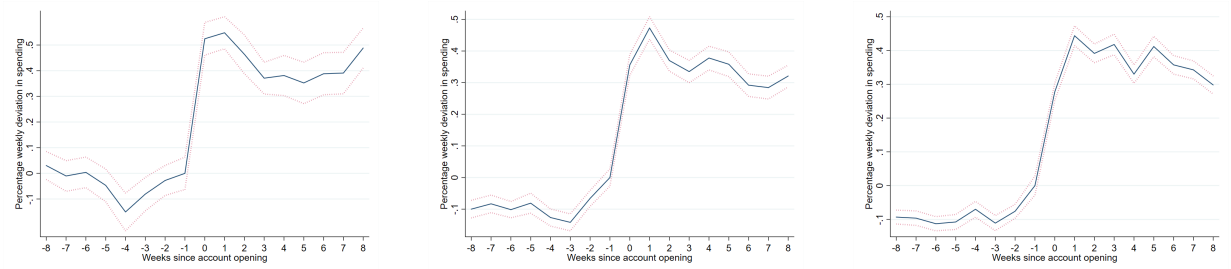


Figure 1: The Bumped app: screenshots of company selection (99 companies in 34 retail characteristics), switching companies, and linked bank accounts and credit card screens

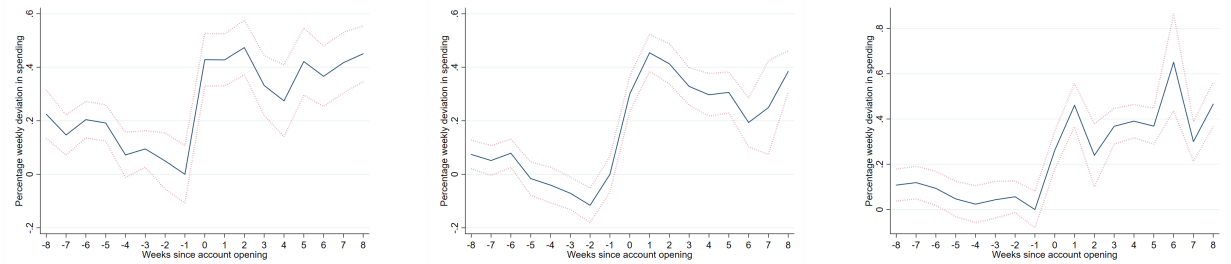
Figure 2: The Bumped app: screenshots of transactions from all linked cards (that may be eligible) and overview of the user's portfolio

Figure 3: Stock grant noti cation received by users, push noti cation



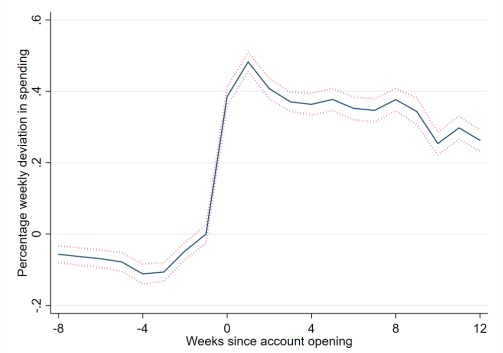
Small fraction rewarded | Medium fraction rewarded | Large fraction rewarded

Figure 13: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for three terciles of actually rewarded spending as a fraction of eligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



Low attention users | Medium attention users | High attention users

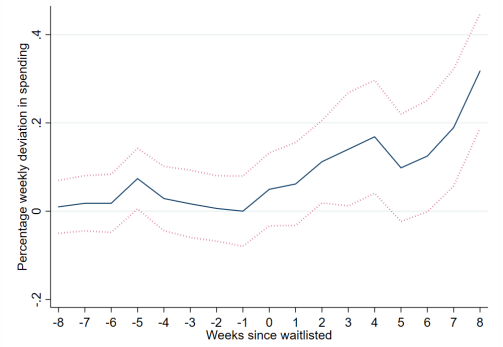
Figure 14: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for three terciles of user attention, defined by the login counts per user in the 8 weeks after account opening. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



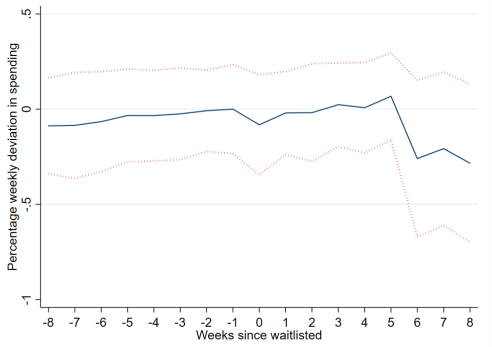
Eligible spending 3 months after account opening

Eligible spending 6 months after account opening

Figure 15: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 3 and 6 months after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.

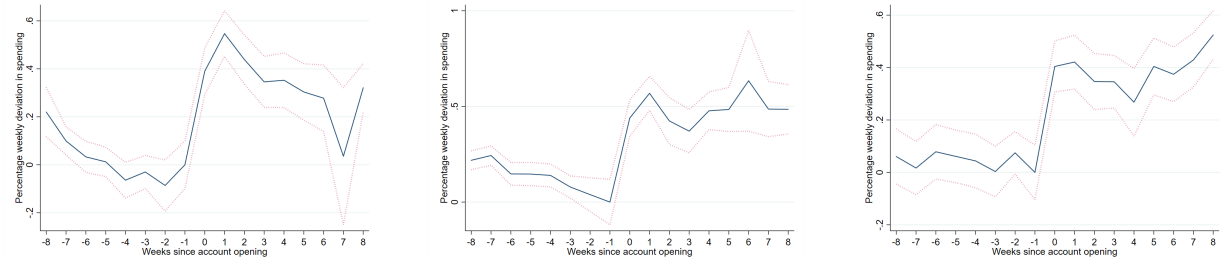


Eligible spending



Ineligible spending

Figure 16: This figure shows the coefficient estimates $\beta^{\tau}_{waitlist}$ in Specification 4 for both eligible and ineligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals signed up for the waitlist. Standard errors are shown as the dotted lines and clustered at the individual level.



Waitlisted shortly | Medium waitlisted | Waitlisted for long

Figure 17: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for eligible spending (defined as the percentage deviation from the individual-level mean) and for three terciles of time spent being waitlisted. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.

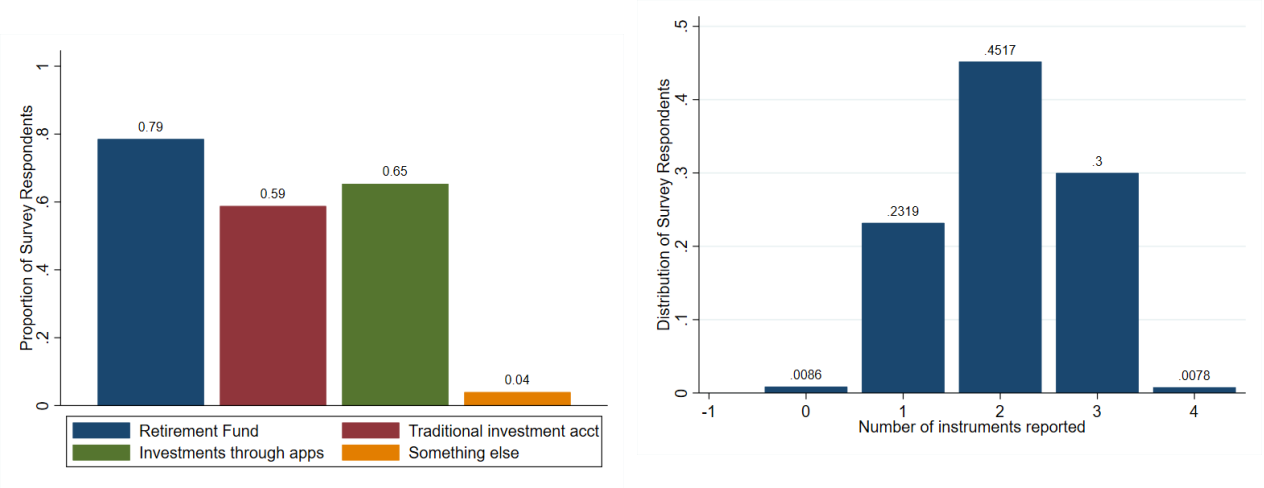


Figure 18: This figure shows the responses to a survey of 1,160 users who were asked about their investing experience, "Do you own stock outside of Bumped? If so, where? 1. Employer-sponsored retirement funds (401k, IRA etc), 2. Traditional or managed investment account, 3. Investments through other apps (Robinhood, Stash etc), 4. Something else." Since users were allowed to select more than one category, the right panel shows the distribution of number of different categories (or accounts) selected.

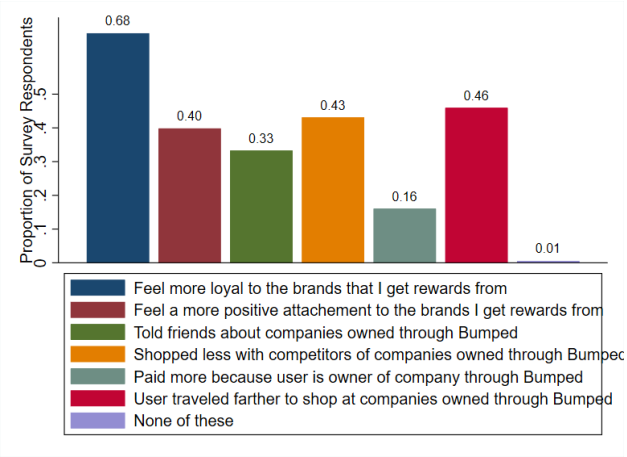


Figure 19: This figure shows the responses of 1127 users who were asked to select all that applies for the following question: "Since signing up for Bumped... 1. I feel more loyal to the brands that I get rewards from, 2. I feel a more positive attachment to the brands I get rewards from, 3. I have told my friends about companies I own through Bumped, 4. I have shopped less with competitors of companies owned through Bumped, 5. I have paid more for something because of owning a company through Bumped, 6. I have traveled farther or gone our of my way to shop at companies owned through Bumped, 7. None of the above 8. Something else."

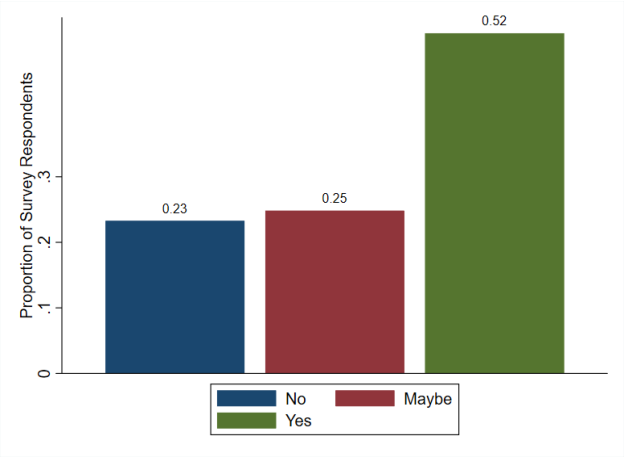


Figure 20: This figure shows responses of 1,160 users who were asked the following question: "Does owning stock through Bumped make you more likely to invest outside of Bumped in the future? 1. No, 2. Maybe, 3. Yes."

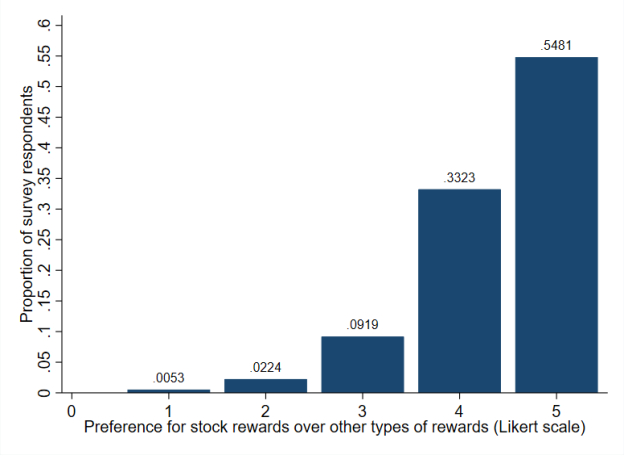


Figure 21: This figure shows responses of 2,287 users who were asked: "In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)? 1. Significantly less excited than traditional rewards, 2. Less excited than traditional rewards, 3. About the same as traditional rewards, 4. More than traditional rewards, 5. Significantly more than traditional rewards."

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

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

Notes: This table includes users who signed up for Bumped and opened their account the same week that they got off-waitlisted or a week after, which are 9,378. The total number of transactions and spending (in USD) are calulated per user and include amounts before and after account opening. Spending only includes transactions that were classified as belonging to a certain brand (551 different brands are in the final dataset (in 34 retail catagories) of which 99 can be selected to be rewarded). Rewards are in USD.

Table 2: Summary statistics of Bumped users who open their account the same week in which they got off the waitlist or a week after and who received a grant

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

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

Table 3: Summary statistics of Bumped users who open their account the same week in which they got off the waitlist or a week after post adjustments to data

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

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

Table 4: Comparison of summary statistics with the Consumer Expenditure Survey (CEX)

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

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

Table 5: Estimation results of brand spending by Bumped users on Safegraph card spending of that brand

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

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

Table 6: Estimation results of spending ratios post account opening or receiving the grant

	All spending		Spending on grant brands	
	Eligible	Ineligible	Eligible	Ineligible
Post 8 weeks	0.384***	-0.036	0.934***	-0.303
	(0.068)	(0.023)	(0.311)	(0.197)
Post more than 8 weeks	0.695***	-0.059	1.432***	0.376***
	(0.147)	(0.056)	(0.516)	(0.135)
Constant	1.071***	1.157***	0.782***	1.275***
	(0.088)	(0.033)	(0.165)	(0.054)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	229204	235683	44417	96775
Adj. R squared	0.167	0.121	0.026	0.084

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

Table 7: Estimation results of Bumped users on spending amounts post account opening or receiving the grant

	All spending		Spending on grant brands	
	Eligible	Ineligible	Eligible	Ineligible
Post 8 weeks	19.092***	8.450	0.822***	-3.410
	(1.618)	(14.172)	(0.157)	(4.238)
Post more than 8 weeks	22.467***	-2.779	0.917***	-0.619
	(3.012)	(17.009)	(0.230)	(2.826)
Constant	52.278***	330.700***	0.323***	19.675***
	(1.907)	(12.089)	(0.105)	(0.867)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	236639	236639	840908	840908
Adj. R squared	0.783	0.272	0.053	0.194

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

Table 8: Estimation results of Bumped users on log spending amounts post account opening or receiving the grant

	All spending		Spending on grant brands	
	Eligible	Ineligible	Eligible	Ineligible
Post 8 weeks	0.711***	0.157***	0.041***	0.029***
	(0.017)	(0.016)	(0.003)	(0.007)
Post more than 8 weeks	0.669***	0.083***	0.021***	-0.008
	(0.025)	(0.024)	(0.004)	(0.008)
Constant	2.067***	4.671***	0.031***	0.362***
	(0.017)	(0.016)	(0.002)	(0.004)
User fixed effects	✓	✓	✓	✓
Week-by-year fixed effects	✓	✓	✓	✓
Observations	236639	236639	840908	840908
Adj. R squared	0.407	0.371	0.356	0.734

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

Table 9: Estimation results of Bumped users brand spending on Robinhood clients weekly holdings of that brand

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

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

Table 10: Estimation results of Safegraph brand spending on Robinhood clients weekly holdings of that brand

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

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Notes: In this specification we regress the total daily (weekly) spending in all publicly traded brands of all Safegraph card spending data on the daily (weekly) holdings of that company by Robinhood brokerage clients data obtained from robintrack.com. Date (week-by-year) fixed effects refer to any day (week) of the sample period and brand fixed effects for any publicly traded brand. The time period and selection of brands/tickers is the same as in Table 9, however, not all tickers could be matched to the brand spending information in the Safegraph data and we only kept unambiguous matches of the top 200 spending brands in the Safegraph data.

Table 11: Estimation results of transfers to brokerage accounts post account opening

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

Standard errors clustered at the user level. *** p<0.01, ** p<0.05, * p<0.1
Notes: In Column (1), we regress the log of brokerage account transfers on a post 8 weeks after account opening dummy, which takes a value of 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes a value of 1 for transactions more than 8 weeks post account opening and 0 otherwise. Column (1) has the log USD amount in transfers to brokerage accounts, and Columns (2) to (4) have the likelihood to transfer to a brokerage account as the outcome variables. Brokerage transfers include all ACH transfers that are classified as finance or investments, belonging to an identifiable broker, or belonging to an investment services app. Robinhood transfers are transactions that are classified as belonging to Robinhood from the transaction description. User fixed effects and week-by-year fixed effects are included.

Table 12: Estimation results of time on waitlist on user characteristics

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

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

Table 13: Covariate balance check between grant and non-grant receivers before getting off the waitlist

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

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

Table 14: Estimation results of ATM withdrawals post account opening

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

Standard errors clustered at the user level. *** p<0.01, ** p<0.05, * p<0.1
Notes: In Column (1), we regress net ATM withdrawal amounts on a post 8 weeks after account opening dummy, which takes a value of 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes a value of 1 for transactions more than 8 weeks post account opening and 0 otherwise. Column (1) has the USD amounts in net ATM withdrawals and Column (2) has the percentage deviation in net ATM withdrawals (relative to the individual-level average) as the outcome variables. User fixed effects and week-by-year fixed effects are included.

Table 15: Correlation between self-reported preference for stock rewards, loyalty and increases in the likelihood of investing outside of Bumped

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

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