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THE EFFECT OF STOCK OWNERSHIP ON INDIVIDUAL SPENDING AND LOYALTY

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ABSTRACT

In this study, we quantify the effects of receiving stocks from certain brands on spending in the brand's stores. We use data from a new FinTech company called Bumped that opens brokerage accounts for its users and rewards them with stocks when they shop at previously elected stores. For identification, we use 1) the staggered distribution of brokerage accounts over time after individuals sign up for a waitlist and 2) randomly distributed stock grants. We find that individuals spend 40% more per week at elected brands and stores after being allocated an account. In response to receiving a stock grant, individuals increase their weekly spending by 100% on the granted brands. Beyond documenting a causal link between stock ownership and individual spending, we show that weekly spending in certain brands of our users is strongly correlated with stock holdings of that brand by Robinhood brokerage clients. Finally, we present survey evidence to argue that loyalty is the dominant psychological mechanism explaining our findings. We thus provide micro evidence for the idea that stock ownership drives brand loyalty, which is an intangible asset that leads to lower firm cash flow volatility.

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1 Introduction

According to the canonical asset pricing model, individuals' consumption should not be affected by their holdings of specific stocks and individuals' investment decisions should not be affected by their consumption of specific brands. However, it is well documented that investors invest in stocks from companies with which they are familiar with (Huberman, 2001; Keloharju et al., 2012), and base their investment decisions on feelings of loyalty (Cohen, 2009; Aspara, 2009). In this paper, we study how individual ownership of specific stocks increases loyalty and affects spending behavior. We thus show that behavioral biases in investing are not restricted to trading but affect consumption, a direct component of individual utility and welfare.

We analyze the relationship between stock ownership and spending using de-identified transaction-level data from a FinTech company called Bumped. The company opens a brokerage account for its users, and rewards them with stocks from the brands and stores they buy from. After the brokerage account is opened, customers link all their checking and credit card accounts, and select their favorite brands in several retail categories. If and when they spend at their selected brands, they receive a fraction of their spending in that brand's stock in their brokerage accounts. For identification, we use the staggered allocation of accounts over time after individuals initially sign up for a waitlist. Additionally, we look at the effects of stock grants distributed to users at different points in time.

We show that customers increase their spending at the selected brands in response to opening their brokerage accounts. Weekly eligible spending at selected brands jumps up by 40% and stays persistently high for 3 to 6 months. As eligible spending averages 56 USD per week, this corresponds to a 23 USD increase in spending per week. For ineligible spending, we can rule out a decrease larger than 5% in the weeks after account opening, from a basis of 295 USD per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than 16 USD.

When looking at the behavior of app users after they signed 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 a unique feature of our setting: individuals in the sample were first required to sign up to a waitlist before getting their brokerage accounts. When individuals signed up for the waitlist, they only provided their email address. The company's operations team then released batches of users to onboard, how many depended on varying business objectives and constraints, on a first-come-first-served basis.

Users spend a considerable amount of time waitlisted, on average, 4.5 months. Because we restrict our analysis to users who sign up immediately after being allowed to do so, we feel it is implausible that users hold off 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, at that time, only their email addresses are known.[1]

We lend additional credibility to our account opening result by exploiting another unique feature in our setting. A subset of users were granted with \$5 or \$10 stock grants from the following companies: Red Robin, Taco Bell, McDonald's, Exxon Mobile, Chevron and Yum! Brands, upon account opening. These stock grants were distributed for a number of months but were not advertised when users chose to waitlist or were allowed to sign up for an account. In response to receiving a stock grant, we find a spending response of 100% in the brands of which individuals received stocks. For these users, we also find a more persistent response of eligible spending to account opening.

Finally, we have some quasi-random variation in the fraction of eligible spending that ulti-

[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 response of users that were waitlisted for relatively short or long periods of time and do not find large differences in our results. Finally, we show that the amount of time individuals are waitlisted is not well explained by individual characteristics.

mately got rewarded. The reason is that due to company operations, policy changes, and constraints, not all eligible spending was rewarded for all users but only approximately 70%. When we split all users into terciles based on which fraction of their eligible spending got rewarded, we find a more persistent response in eligible spending for those users that had more of their transactions rewarded.

In addition to our spending analysis, we are interested in the link between owning more stocks and investing more or having a broader engagement with the stock market. We find evidence for the fact that receiving stocks stimulates individual investments, by documenting that outgoing brokerage transfers to other brokers are increased after individuals start receiving stock rewards.

To explore the external validity of our findings, we also document that daily and weekly spending in certain brands for our user population is strongly correlated with holdings of that company's stock among Robinhood brokerage clients (the holdings data is obtained from robintrack.com). 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 0.12%, controlling for brand and week-by-year fixed effects. We argue that this result helps us to extrapolate our findings to actual spending and stock ownership in brokerage accounts. We chose Robinhood brokerage account data as Robinhood clients are likely a similar population to Bumped users. In fact, the most common brokerage account of our users is provided by Robinhood.

What explains customers' spending responses after receiving the stock rewards and grants? We argue that the mere monetary value or price effect of the rewards is unlikely to fully explain the changes in behavior we observe. It seems likely that customers pay more in transaction costs when they alter their spending behavior by 40% to 100% in response to a 1% to 2% price effect. We argue that, in addition to the monetary benefits of the stock rewards and grants, 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 additional potential mechanisms behind our results. However, the magnitude of our results is substantially larger than the effect of cash back on spending documented in the literature. Vana et al. (2018) calculate that when an additional \$1 in cashback payment is offered, spending increases by \$3.51, or an effectiveness of 351%. In contrast, stock rewards have an effectiveness of 2000%: we find a \$23 increase in weekly eligible spending when the average amount offered in stock rewards are \$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 that further enhance the feelings of loyalty that owners of company stock experience. We discussed a number of psychological mechanisms that can explain by stock rewards trigger loyalty, ranging from familiarity, reductions in cognitive dissonance, and illusion of control.

We also present survey results that inform us about users' motivations and attitudes towards stock ownership and loyalty. Consistent with Cohen (2009) and Aspara (2009), we find that a significant fraction of Bumped users report that they are likely to go out of their way to shop in brands for which they owned stock, shop less from the competition, or pay a higher price. These measures of loyalty are positively correlated with users reporting being "more excited" about stock rewards than other types of traditional rewards. Furthermore, survey respondents report being more likely to invest outside of Bumped as a result of owning stock through Bumped, and this response is also positively correlated with excitement about stock rewards. Overall, survey results suggest that stock ownership creates non-pecunary benefits which we argue, explain the large effects of stock rewards on individual spending.

Our results have important implications for the economy and for asset prices. The high correlation between spending in certain brands and holdings of that stock suggest that stock price fluctuations affect spending and thus utility in a more direct way. Additionally, previous work has found that brand loyalty is an intangible asset that leads to lower cash flow volatility (Dou et al., 2019; Larkin, 2013), which we confirm in our setting.

Literature Review

Our research is related to prior literature suggesting that purchase behaviors and beliefs about a company have an impact on investment choices, investors tend to buy what they know, e.g., Huberman (2001), Keloharju et al. (2012), Schoenbachler et al. (2004), Frieder and Subrahmanyam (2005), and MacGregor et al. (2000). Closest to our paper, Keloharju et al. (2012) use individual brokerage account data to show that clients of a given broker invest in the corresponding broker stock, and owners of a given car make invest in the respective car company. The authors also document a causal link between receiving broker and auto stock but that link is weaker. Similarly, 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) explores the role of alignment of incentives between customers and firms, when loyalty rewards are provided in the form of equity, instead of cash-back. Additionally, the existing literature has documented that portfolio choice is affected by loyalty Cohen (2009), advertising to investors increases sales (Lou, 2014) and firms derive benefits when their stocks are owned by financial intermediaries, i.e., Almazan et al. (2005), Chen et al. (2007), and Aghion et al. (2013).

To the best of our knowledge, one existing study looks at the random distribution of stocks from various companies to individuals in an experimental setting is Bernard et al. (2018). In this study, graduate students are randomly assigned some companies' stocks in an economic experiment, without any prior knowledge of whether the students are already customers of the invested-in firms. This study sheds additional light on the behavioral links between stock ownership and firm performance via increased sales. This study also shows that customer-stockowners purchase more of the firm's products because they changed their preferences that favor the invested-in firm in line with Fama and French (2007). In contrast to this paper, we look at a naturally occurring setting that elicited participation of a broad cross section of the US population and companies that were selected and are familiar to most customers (i.e., Walmart, Target, and Macy's) from a wide range of retail categories.

A causal effect of stock ownership on individual spending can be interpreted as an increase in loyalty towards 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 talents are riskier and have higher expected returns. We argue that stock ownership itself triggers brand loyalty, through a variety of behavioral mechanisms by which ownership of specific stocks causes increases in spending on brands corresponding to those stocks.

Our results also relate to the literature studying reciprocity (Falk, 2007), documenting a larger impact of non-monetary incentives, relative to 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 by the monetary value of the rewards. While we are not able to compare directly the effect of stock as a currency for monetary rewards, we benchmark our results with previous work looking at the effect of cash-back rewards on consumers spending. Using data from an anonymous cash-back website, Vana et al. (2018) find that for every dollar of cash-back offered, spending increases by $3.51.[2]

[2] Vana et al. (2018) decompose the effect of cash-back rewards into two components. The first component relates to the effect of while 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-ward 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-

Our paper is also related to the growing literature on consumer spending using data from new online financial platforms, often called FinTech apps (see Goldstein et al., 2019, for a literature survey), such as Gelman et al. (2015), Baker (2018), Kuchler and Pagel (2019), Olafsson and Pagel (2018), Medina (2020), and Koustas (2018). In the domain of stock market investments, our paper is also related to research papers using bank account data linked with securities trades and holding data such as Meyer and Pagel (2018) and Loos et al. (2018). But in contrast to looking at spending responses to income shocks, financial advice, or stock market investments, we look at 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, 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.

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 brands and stores they buy from.[3] To receive a user account, individuals have to first sign up for a waitlist on the

random variation in the time to actually receive the cash-back reward. The calculation of the total effect (inclusive of both components) cited above is taken from the appendix.

[3] We are aware of two more companies that 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

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-serve basis, users are taken out of the waitlist and are invited to open a brokerage account. If users fail to open an account when approved, two reminder emails are sent. However, we restrict our analysis to users who sign up for their accounts within a week of being off-waitlisted. Once users signed up for an account, they can link all their checking and credit card accounts. In turn, customers can select their favorite brands in a number of spending categories.

All featured brands are divided into 16 different retail categories and users can select one brand from each category. If users then spend at their selected brands, they receive fractional shares of the corresponding company as a reward. Customers can switch their selected brands every 30 days, and only up to three times per year. The functionalities of the brokerage account are limited to the users' stock rewards, for instance, the users are not allowed to deposit their own money or purchase additional stocks. They are however allowed to sell their (entire) position at any time, in which case the cash preceedings are transferred to a linked bank account.

Figures 1 and 2 show several screenshots of the FinTech app. Figure 1 shows the screenshots of brand selection, switching brands, 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 for a certain period of time. Figure 3 shows the notification a user receives on receiving a stock grant.

We received an anonymized subsample of the user base. As of March 2020, our data subsample includes 11,424 users. Figure 4 shows the timeline of when our subsample of users were waitlisted

stock from 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 platfom accrue credits on eligible purchases that are directed to a 529 account for college savings.

for an account, invited to open an account, and received their accounts.

2.2 Data

The dataset we received includes de-identified information on financial transactions and demographic characteristics, i.e., information on each user in our data sample's age, gender, and 5-digit zip code.[4] 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. In terms of other demographics, 67% of user are male, 17% are female, and 16% do not report their gender. 871 users are less than 25 years, 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, thus more likely to be male, younger, and educated than the average American.

Additionally, we use de-identified daily data on each user's spending transactions from all linked checking, savings, and credit card accounts. We observe the date of when users sign up for the waitlist, when they get off the waitlist and invited to open their brokerage account, as well as the date in which they effectively open 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 users that opened their accounts within one week after they were invited to do so.

In turn, we see all linked cards and the corresponding history of transactions before and after each card was linked. We then have a flag of which transactions were selected and thus eligible for rewards and whether they were actually rewarded. For each transaction after sign-up, we thus 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 brands are selected by each user, and when they

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

switched their favorite brands.

Bumped was launched in 2017, and we received a subsample of users' de-identified and aggregated transactions from 2016 to 2020. Thus, we can see transactions by a user both before and after joining the app. This data is summarized in Table 1. Our subsample of 11,424 users were waitlisted initially and then received their brokerage accounts. The users we observe had to wait on average 4.5 months between being waitlisted on the app to opening an account, with a standard deviation of 3.3 months. The users on average perform 730 transactions with an average of 2.4 linked cards. The average monthly total spending is 1,496 USD, and the average total rewards are 37 USD. The average weekly spending is 350 USD, while the average weekly rewards to users are 0.40 USD. Note that, we only received transactions that were classified as belonging to a certain brand. In our final dataset, we have 551 different brands that our users spend at.

We also received information on brokerage account transfers and ATM withdrawals for our users. 2,156 users have other brokerage accounts, which are primarily with Robinhood, Etrade, Ameritrade, and Schwab. The median user tranfers 545 USD per month to his or her brokerage account. We observe ATM transactions for 4,912 users, and see that an average user withdraws 465 USD a month and 233 USD a week.

Additionally, starting March 2018, 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: Taco Bell, McDonald's, Exxon Mobile, and Chevron. The grant was displayed in-app with a description 'thank you for choosing the brand', and a push notification was sent to the user. The amount and timing was 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 has no information of the promotion at the time they signed up for the waitlist.

Figure 4 shows the timeline of how many users received a stock grant. Summary of 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 account open. Over the observation period, users that received stock grants spent 519 USD per week on average. The average grant amount was 10 USD. 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 can perform a covariate balance test between grant recipients and non-recipients, before they get off the waitlist. In Table 6 we can see that grant recipients are very comparable to non-recipients in a number of observable characteristics, including age, as well as eligible and ineligible spending. The only statistically significant difference is in terms of the number of transactions per month. Grant recipients perform 328 transactions per month, compared to 301 transactions by non-receivers. We argue that while statistically significant, the difference is not economically significant, and did not affect whether or not users received a stock grant.

To ensure that our empirical results are not driven by transactions being observed after but not before sign-up, we perform a number of checks to then 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-days threshold is commonly used in other research papers using transaction-level data to ensure completeness of records (see, e.g. 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 from 26,813 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. Post these adjustments, we have a total of 9,005 users.

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. Our users are younger, more likely to be men, and show an average spending of

$1,496 per month, whereas the average American spends $2,205 during the same time period. We note that our data includes spending only on 551 identified brands. After taking that into account we argue that 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 partnered 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 as the original data set. 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 more broad fraction of the population than the Bumped users.

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. In Table 5 we show this 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, correspond to the raw correlation coefficients as we normalize the spending data by their standard deviations. The estimated coefficient are highly significant and the adjusted R squared is around 20%. We take these results as indicative that the 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 all ineligible spending. (In)eligible spending, before and after account opening, is defined as spending in brands 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:

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

In Specification 1, $Spending^{iw}_{Eligible}$ denotes eligible spending (i.e., spending at a brand 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 $w^{iw\tau}_{Bumped}$ is an indicator whether user i in week w had received his or her account in his or her user specific τ 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. Standard errors are clustered at the individual level. 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 those brands of which users received the stock grants) upon receiving the stock grant:

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

In Specification 2, $Spending^{iw}_{Eligible}$, α_i, and η_w are defined as in Specification 1. In turn, $w^{iw\tau}_{Grant}$ is an indicator whether user i in week w had received the grant in his or her τ's week. For users that never received a grant, $w^{iw\tau}_{Grant}$ 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 brands. Standard errors are clustered at the individual level. Additionally, we report 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 an analysis to study the effect of stock grants on spending. First, we estimate the same specification as in Equation 1, but splitting the sample into users who received a stock grant and those who did not. Second, we formally compare the differential response in spending of these two groups, with the following difference-in-difference specification:

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

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

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

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

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

Standard errors are shown as the dotted lines and 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. We here look at the ratio of spending relative to the mean average over the entire 16-week period. Thus, the axis shows the percentage deviation of spending relative to the sample average in the 8 weeks before and after account opening. 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 rise in ineligible spending.

Figure 8 shows the β^{τ}_{Bumped} coefficients and standard errors from Specification 1 for both eligible spending as well as ineligible spending as the left-hand side variables. Spending is measured as the individual-level percentage deviation from the sample average eligible spending in a given week. The coefficients thus represent the percentage deviation in eligible spending before and after users received their accounts. We can clearly see a pronounced spike in eligible spending in the week that users receive their accounts. Weekly spending at selected brands jumps up by 40% and stays persistently high for the 8 weeks we look at. In terms of USD, eligible spending averages 56 USD per week so this corresponds to approximately a 22.4 USD 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 USD per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than 15 USD.

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, in 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, as well as eligible spending at the brands of which users received stocks as the left-hand side variables. As before, the coefficients thus represent the percentage deviation in eligible spending before and after users 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 of which the user received a grant increases even more, by about 100%.

As a complement, Column 1 of Table 8 shows the average effect during 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 an 3.6% decrease in ineligible spending with a standard error of 2.3%, we can thus rule out a decrease of more than 8.2% in ineligible spending with statistical confidence. Columns 3 and 4 show the results for eligible spending in granted brands and we can document a 93% increase in spending with a negligible effect on ineligible spending, i.e., spending in categories of which the user received a grant but didn't select at sign up.

Columns 1 and 2 of Table 9 shows 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 \$19 per week in eligible spending, and an insignficant \$8.4 increase in ineligible spending. We can rule out a decrease in ineligible spending of more than \$20 with statistical confidence. Finally, Table 7 shows similar effects for log spending instead of the absolute amounts.

Table 8 summarizes the graphical results on 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 100% increase in spending on brands that match the stock grant, and a non-significant decrease on ineligible spending. These coefficients represent deviations from weekly average spending during the period of analysis. Table 9 shows similar results, estimating the effects on spending directly in dollar terms, instead of deviations from weekly averages. Columns 3 and 4 show a 4 dollar increase in eligible spending, and a reduction of 67 cents in ineligible spending.

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 of 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, then there is a decrease, and then we observe another increase.

3.1.1 Sample splits by rewards ratios

Due to company operations and constraints, not all eligible spending was rewarded, on average over the entire sample period. About 70% of eligible spending was actually rewarded. Only eligible spending was ever rewarded, ineligible spending was never rewarded. Whether an eligible transaction was rewarded depended on internal business policy shifts over time. In other words, the rules governing whether a transaction was rewarded changed over the life of the company and it is largely not documented how and when. Additionally, some transactions that should have been rewarded may have been missed. When we look at eligible spending, we thus look at spending in elected categories rather than spending that was actually rewarded in the pre- and post-periods of account opening.

We exploit variation in the fraction of eligible spending that was rewarded, to see if receiving higher rewards lead to differential effects on spending, compared to receiving lower levels of reward. Are users behaving differently in response to the account opening if a lot instead of little of their eligible spending was rewarded? We look at the sample splits of terciles of individuals being rewarded a lot versus little relative to their eligible spending in Figure 16. While we see the same initial spike, we see a more persistent response in eligible spending for those users who got rewarded relatively more.

3.1.2 Sample splits by retail categories

Finally, we study the spending response of account opening by category. We focus in the six most popular spending categories: groceries, burgers, coffee, superstores, ride share and drug stores. 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.

3.1.3 Sample splits by attention

We also look at heterogeneities, as a function of login activity, which we use as a proxy for attention to financial accounts. Figure 17 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 in 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 on 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 13. When we look at this longer estimation window, weekly spending at selected brands jumps up by 40% and stays persistently high for 3 to 6 months. In terms of USD, eligible spending averages 56 USD per week so this corresponds to approximately a 23 USD increase in spending per week.

3.2 Spending and stock ownership

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

In turn, we run the same analysis but 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 11, 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. 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 in observational data from spending and holdings in brokerage accounts that this relationship exists. We chose Robinhood brokerage account data as Robinhood clients are likely a similar population as Bumped users.

3.3 Impact on brokerage account transfers

We observe brokerage account transfers for 2,156 of our users. Most of these users have a Robinhood or an Ameritrade account, while some users use E-trade or Schwab. Table 12 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 of 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. Column (2) shows that the likelihood of brokerage account transfers by a user post 8 weeks of opening an account increases by 1.4%. Relative to the baseline propensity to invest, these coefficients represent an increase of approximately 5% in the amounts and likelihood of investing. We see similar results for the case of transfers to a Robinhood brokerage account (Column (3)) or a non Robinhood brokerage account (Column (4)). These results indicate that not only users spend more on brands they get stocks as rewards from, but they also increasingly transfer funds to their brokerage accounts. The stock rewards likely engage user 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 14 shows the $\beta^{\tau}_{waitlist}$ coefficients and standard errors for both eligible spending as well as 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 get 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. The reason is that individuals do not have much information about which companies are granting stock or which categories they can select companies from, at the time of being waitlisted.

4.2 Randomization test for time waitlisted and grant receipt

There is substantial variation in the time between being waitlisted and receiving an account. The average time individuals are waitlisted is longer than the 8-week windows we consider in our estimations (mean of 135.06 days, and standard deviation of 97.95 days). Furthermore, who received an account was decided on a first-come-first-serve basis given the company's business objectives and constraints. At the time individuals are waitlisted, only individuals' email addresses but no other information is known. In turn, they allocate accounts on a simple first-come-first-serve basis. 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 13 shows that neither age, gender, or spending patterns before account opening are significant predictors for the number of days in the waitlist. Furthermore, the R2 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 R2 changes. We find that the R2 remains low. The one exception are fixed effects for week-by-year of account opening. Including them increases the R2 to 0.27. However, in contrast to the potential impact of user characteristics, it is unlikely that individual users had an expectation of specific time trends on the time it would take to get out of 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 actual accounts.

As discussed, we can also perform a covariate balance test between grant recipients and non-recipients, before they get off the waitlist. In Table 6 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 splitting the sample according to the time individuals were waitlisted at. The results for receiving an account for three terciles of the time individuals were waitlisted can be found in Figure 15.

4.4 Substitution from cash spending

There is a concern that users might be withdrawing money post 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. An insignificant result in both columns show that increase in spending on eligible brands by users is not associated with an increase in ATM withdrawals.

5 Stock ownership and self reported loyalty: survey evidence

We analyze the responses of 5 surveys sent to Bumped users between 2019 and 2020. The surveys were designed and administered by the Bumped team. The number of respondents vary across surveys, ranging from 358 to 673 respondents per survey. The specific questions in each survey are also different. We now discuss questions that help us understand the characteristics of users, and their attitudes towards stock ownership and financial markets. For exposition purposes, we modify the original numbering of the questions.

The first question asks users about users' different financial instruments as follows:

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 by each user, 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 Bumped. The majority have between 2 and 4 instruments, suggesting that there exposure to the stock market is not limited to their stock rewards.

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

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

A2.1 I have told my friends about companies I own through Bumped

A2.2 I have shopped less with competitors of companies I own through Bumped

A2.3 I have paid more for something because I am an owner of the company through Bumped

A2.4 I have traveled farther or gone out of my way to shop at companies I own through Bumped

A2.5 I haven't done any of these

Figure 19 shows that, since starting to use the app, almost 50% of users shop less with competing brands, about 20% report to pay more because they are owners of the brand, and almost 50% reported going out of their way or travel longer distances to shop on brands they own. The responses are consistent with our spending results and the results in Aspara (2009). Overall, these responses suggest that stock ownership leads to increased brand loyalty.

The third question asks users about users likelihood of investing outside of Bumped in the future, as a results of owning stock through Bumped. Finally, the fourth question asks users about their preferences for stock rewards over other types of rewards, on a likert scale.

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

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 20 shows that more than 53% of users responded that they are more likely to invest outside of Bumped in the future, as a result of owning stock through Bumped. Figure 21 shows that, not surprisingly, survey respondents strongly prefer stock rewards over traditional rewards such as cash back, points, coupons and similar. Again, these results corroborate our empirical findings that receiving stock rewards leads to more engagement with the stock market in general.

Table 15 correlates the answers to question 4 with the answers provided in Questions 2 and 3. Specifically, 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 significantly less or simply less excitement for stock rewards over other types of rewards (we pool those two categories since, as expected, very few survey respondents selected them). Columns 1 to 3 show that a preference for stock rewards is a strong predictor of loyalty. Similarly, Column 4 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 5 we test whether the correlation in Column 4 is concentrated on 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 on question 1, and regress the interaction terms with the corresponding main effects against 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 preferences for stock rewards, and the likelihood to invest outside of Bumped as a result of owning stock through Bumped, is present across the distribution of the number of financial instruments reported in question 2 (the main effect of preference for stok is positive and significant, while the interaction coefficient is not statistically significant). Users who invest in several or few financial instruments 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 these benefits affect both spending and investment decisions.

6 Psychological mechanisms

Our empirical results show that stock rewards leads to large increases on consumption, 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 consumption.

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 individuals have towards 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 that 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 affect.

Similarly, gift exchange can also enhance the effectiveness of rewards. It refers to the phenomenon that the same objects are valued more if acquired or received as gift, rather than if bought. Gift exchange 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 cooperate with Bumped, then gift exchange would be a relevant mechanism behind the effects we see. The stock grant promotional program, however, was funded and administered by Bumped. Still, we see a spending response not only in all eligible spending but specifically in spending at those brands of 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 consumption. While we are not able to provide conclusive evidence of an incremental effect of stock rewards over cash re-wards, compared to the results in the literature about the impact of cash-back on spending, our results are substantially larger. In the following, we present a number of psychological mechanisms that may be behind the large effect of rewards that are specifically in the form of stock.

Familiarity Prior research suggests that customer-stockholders are subject to a familiarity bias, i.e., they tend to gain more exposure towards the stocks they know. As a result, familiarity-biased investors hold portfolios containing 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 assumed that investors are more active in collecting information about the invested-in company and that, in turn, they become more familiar with it. An increase in familiarity can breed positive behaviors by investors, by leveraging the gift exchange, cognitive dissonance, or effect heuristic channels (Zajonc (1980), Moreland and Zajonc (1982)).

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 to reduce the dissonance and restore balance (Festinger (1962), Gilbert et al. (1998)). In the context of share ownership, investors experience cognitive dissonance when taking 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). Gilbert and Ebert (2002) have shown that individuals are more likely to act not in a fully rational manner, if the decisions are reversible ex-ante.

Upon receiving shares, we expect customers to feel as part of a community and to perceive it as a betrayal if they engage in behavior that can damage the company. In line with Gilbert and Ebert (2002), we assume that customers are able to ex-ante tell which behaviors could cause them discomfort. Consequently, customers are expected to act coherently with the goal of avoiding experiencing cognitive dissonance (e.g. they would not buy products of other companies, but rather drive an increase in the consumption of company products).

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 to produce tangible outcomes (Feddersen (2004)), by believing so investors tend to make decisions that positively affect the company's share price, and consequently their investments' 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)). Given that the stock grant promotional program was funded and administered by Bumped. This mechanism could explain why we see more spending at the brands of which individuals received stock in response to the grant. While individual spending could not effectively affect stock prices, illusion of control could also trigger alignment of incentives, giving stock rewards a similar role to that of stock compensation in corporations (Hochberg and Lindsey, 2010; Oyer and Schaefer, 2005; Altinkemer and Ozcelik, 2009).

7 Conclusion

In this study, we quantify the effects of receiving stocks from certain brands on spending in the brand's stores. We use data from a new FinTech app called Bumped that opens a brokerage account for their users and rewards them with company stock when they shop at previously elected brands and stores in several retail categories. For identification, we use the staggered distribution of brokerage accounts over time after individuals had signed up for a waitlist. To lend credibility to our identification strategy, we show that the average time spent waitlisted equals 4.7 months, we perform a randomization test for time waitlisted, and we 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 brands after receiving stock rewards in their brokerage accounts. Weekly spending at selected brands jumps up by 40% and stays persistently high for 3 to 6 months. In terms of USD, eligible spending averages 54 USD per week so this corresponds to approximately a 23 USD increase in spending per week. We can rule out a decrease larger than 5% in the weeks after account opening, from a basis of 292 USD per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than 16 USD.

When users are granted with a certain company's stock, we find a weekly spending response of 100% at the brands of 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 got 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 1% 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-back 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).

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Figures and tables

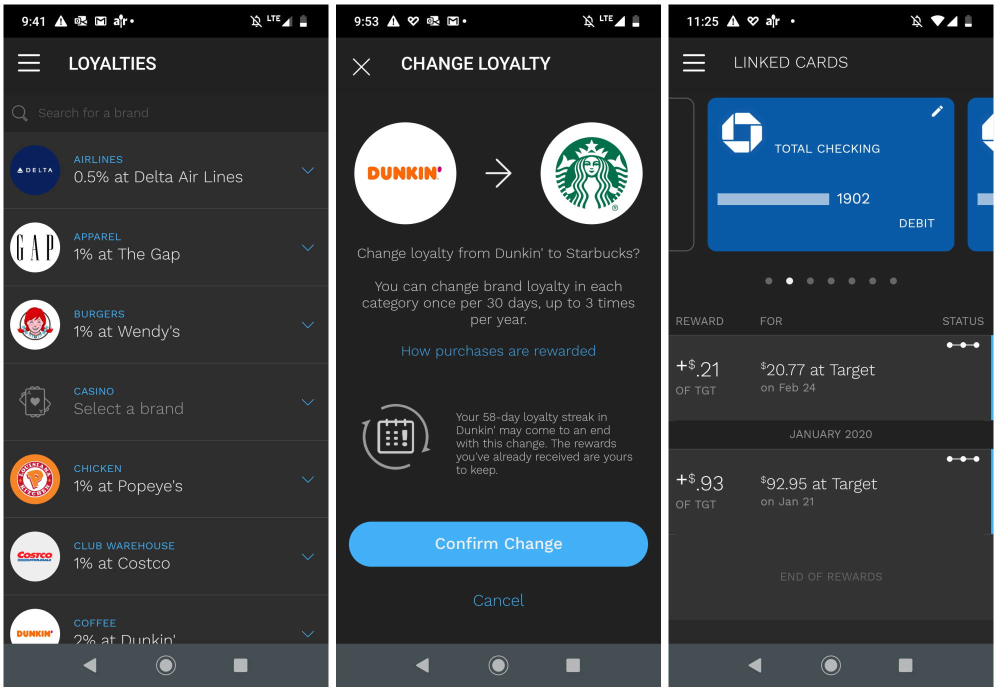


Figure 1: The Bumped app: screenshots of brand selection, switching brands, and linked card screens

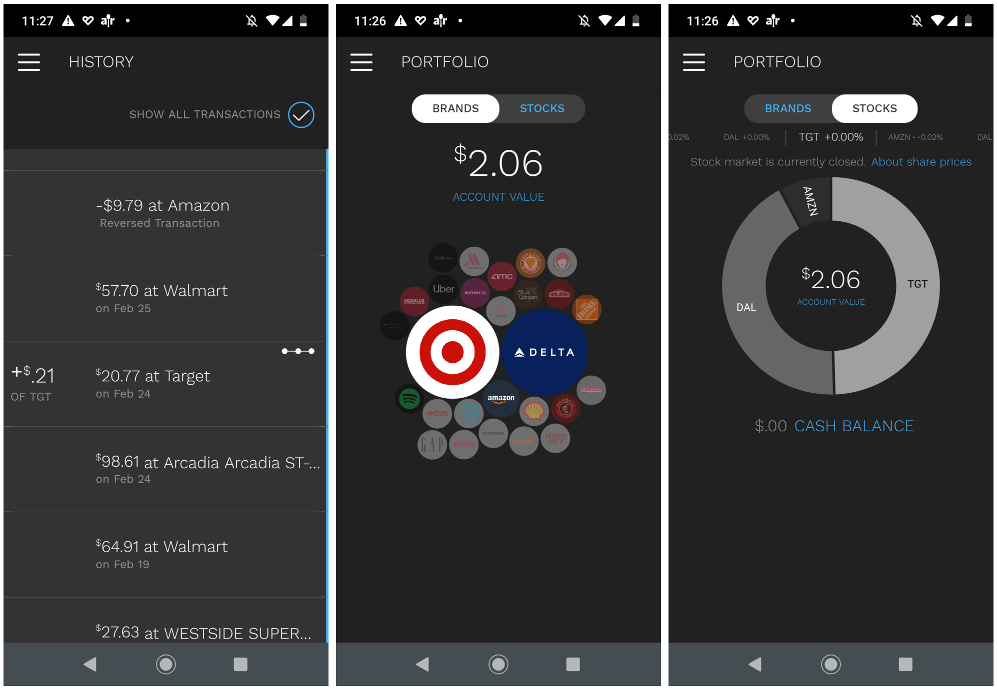


Figure 2: The Bumped app: screenshots of transactions and portfolio screens

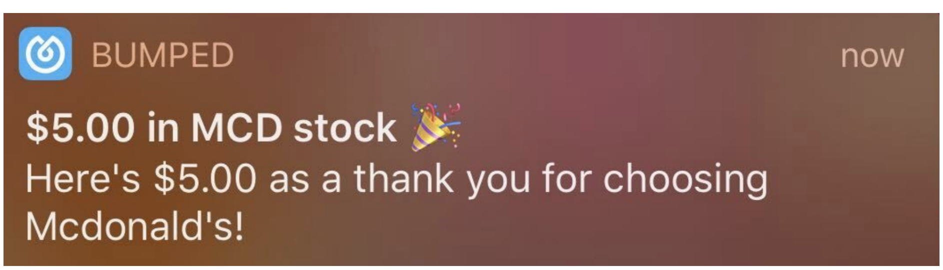


Figure 3: Stock grant notification received by users

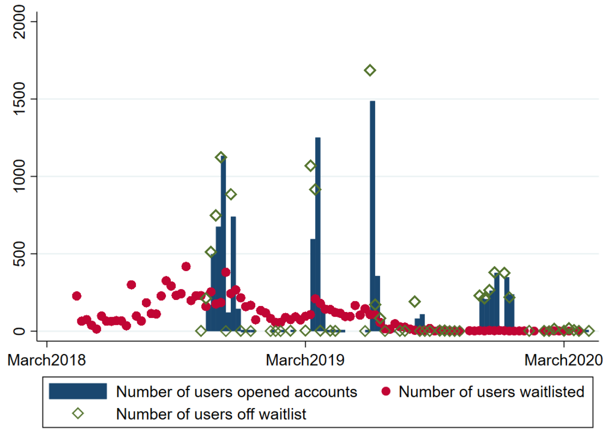


Figure 4: Number of users in our data subsample who were waitlisted and received an account

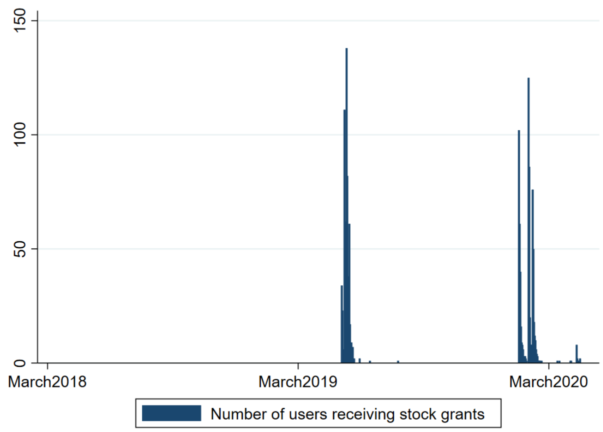


Figure 5: Number of users in our data subsample who received stock grants

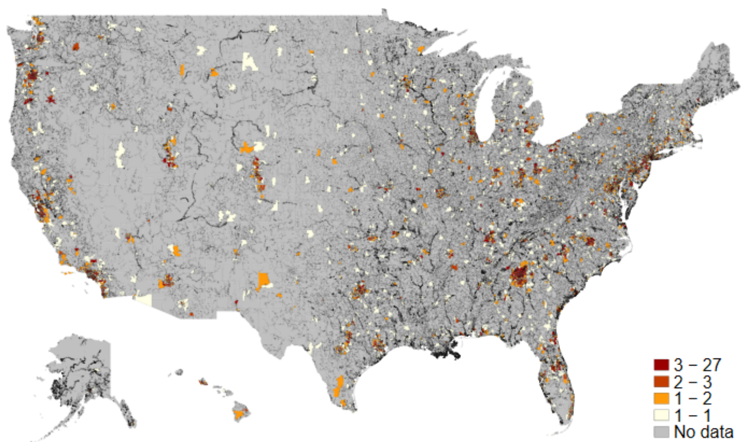


Figure 6: Users by 5-digit zip code in the US

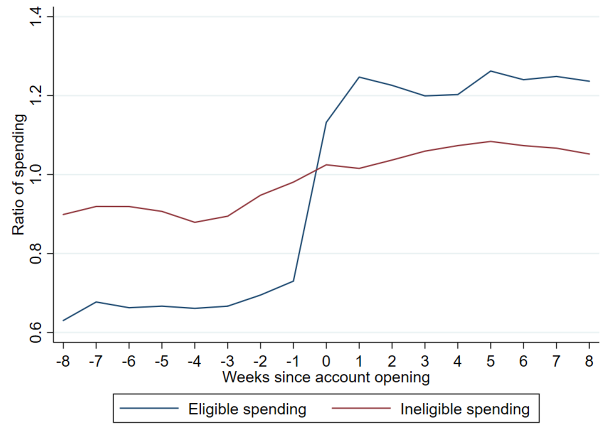
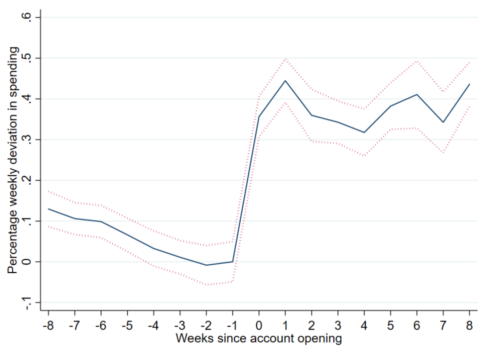
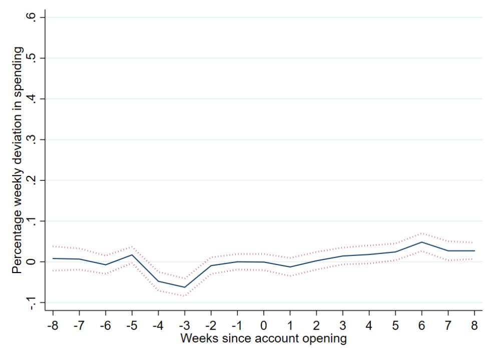


Figure 7: Ratio of eligible and ineligible spending by week relative to account opening

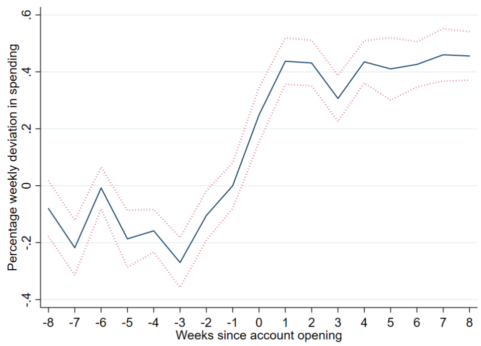


Eligible spending

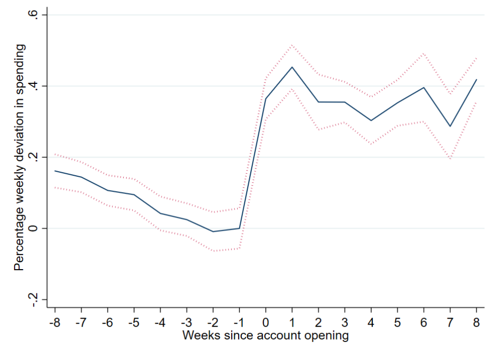


Ineligible spending

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

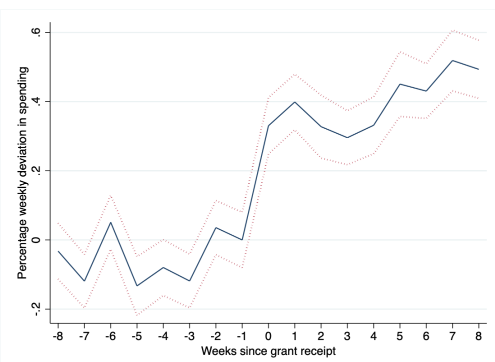


Eligible spending for grant recipients

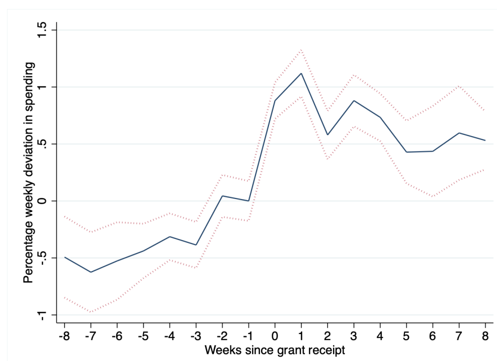


Eligible spending for grant non-recipients

Figure 9: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 separately for individuals who received the grant and those who did not and 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.

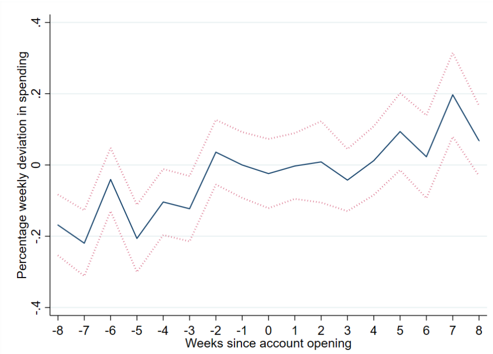


Eligible spending

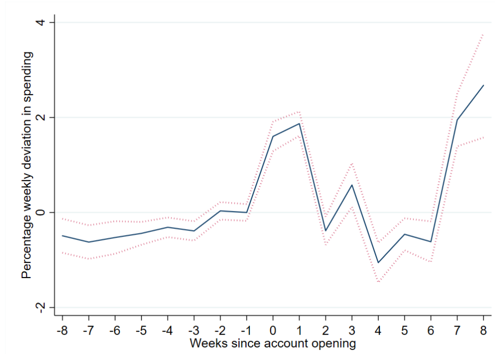


Eligible spending at brands of which stock was granted

Figure 10: This figure shows the coefficient estimates β^{τ}_{Grant} in Specification 2 for both eligible overall spending and eligible spending at the brands of which users received stock grants (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals received the stock grant. Standard errors are shown as the dotted lines and clustered at the individual level.

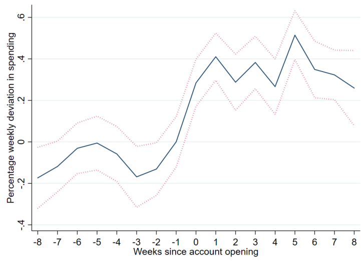


Incremental effect of grant receivers on eligible spending

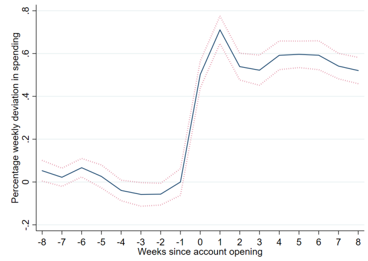


Incremental effect of grant receivers on eligible spending in grant brands

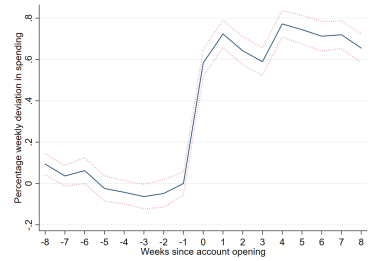
Figure 11: This figure shows the coefficient estimates β^{τ}_{BG} in Specification 3, i.e., the incremental effect of grant receivers on all eligible and grant brand spending (defined as the percentage deviation from the individual-level mean) post account opening. We control for individual and week-by-year fixed effects and consider 8 weeks before and after individuals received the stock grant. Standard errors are shown as the dotted lines and clustered at the individual level.



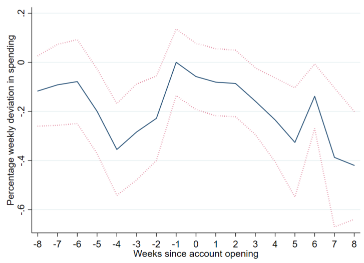
Grocery – Eligible spending



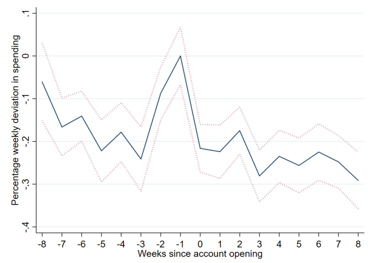
Burgers – Eligible spending



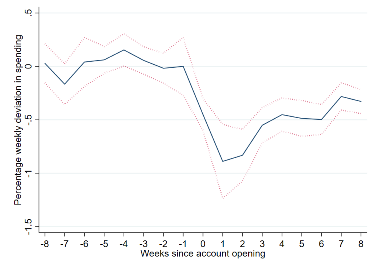
Coffee – Eligible spending



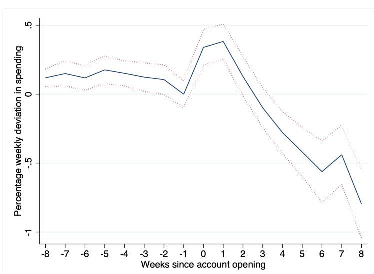
Grocery – Ineligible spending



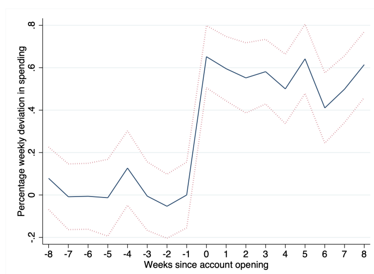
Burgers – Ineligible spending



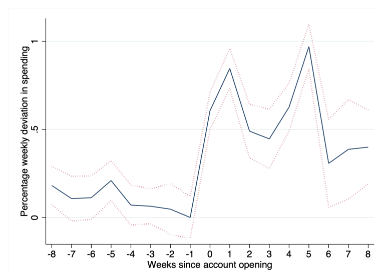
Coffee – Ineligible spending



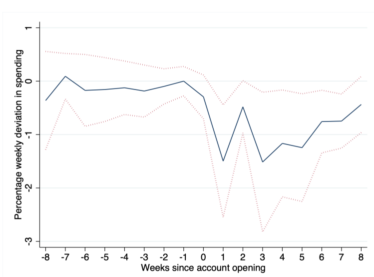
Superstores – Eligible spending



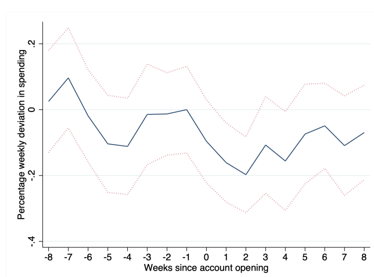
Ride Share – Eligible spending



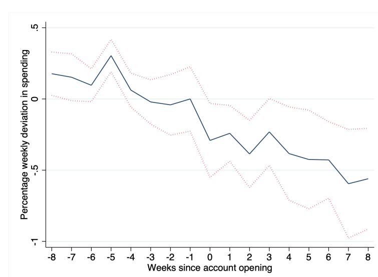
Drug Stores – Eligible spending



Superstores – Ineligible spending

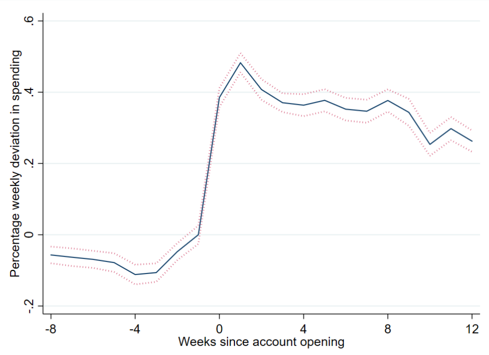


Ride Share – Ineligible spending



Drug Stores – Ineligible spending

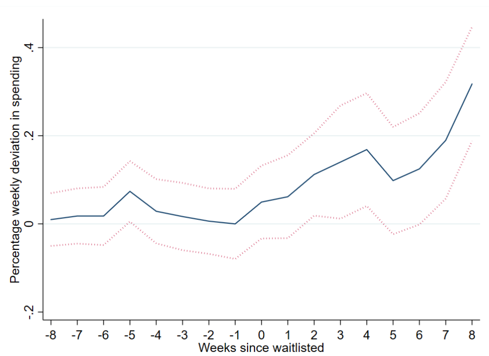
Figure 12: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for the six most popular rewarded categories, which are grocery, burgers, coffee, superstores, ride share, and drug stores. We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving the account. Standard errors are shown as the dotted lines and clustered at the individual level.



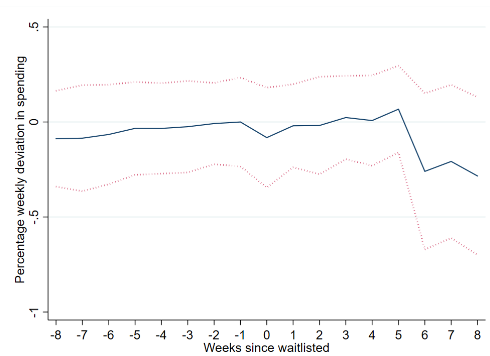
Eligible spending 3 months after account opening

Eligible spending 6 months after account opening

Figure 13: 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.

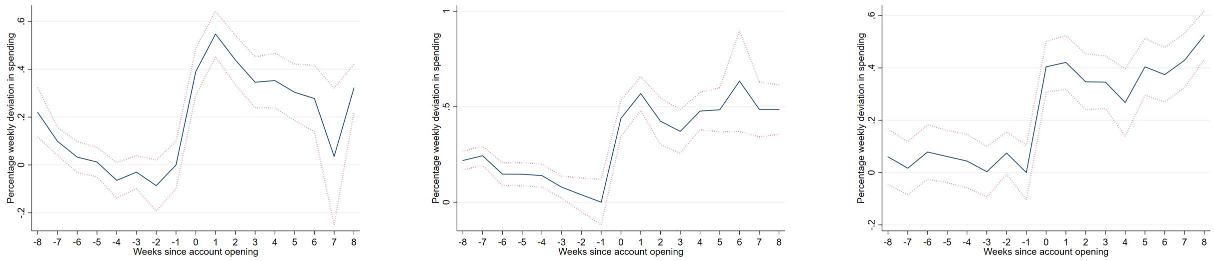


Eligible spending



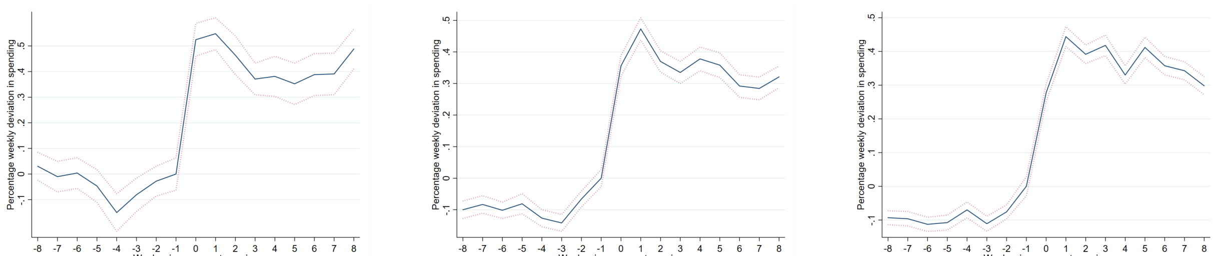
Ineligible spending

Figure 14: 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.



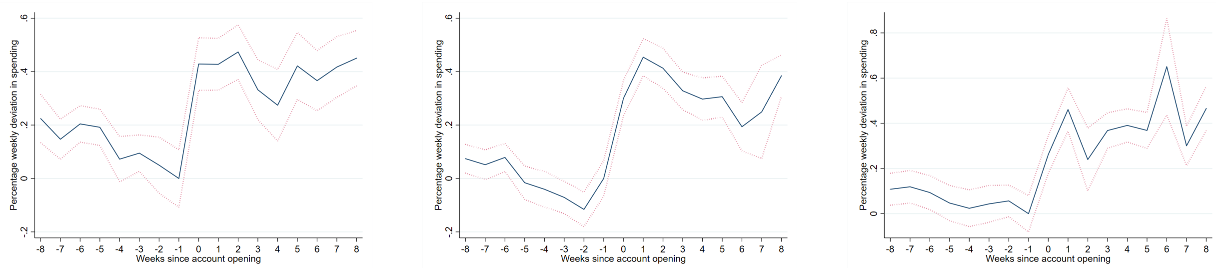
Waitlisted shortly | Medium waitlisted | Waitlisted for long

Figure 15: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for three terciles of time spent being waitlisted and 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.



Small fraction rewarded | Medium fraction rewarded | Large fraction rewarded

Figure 16: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for three terciles of actually rewarded 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 Bumped 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 17: 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. 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.

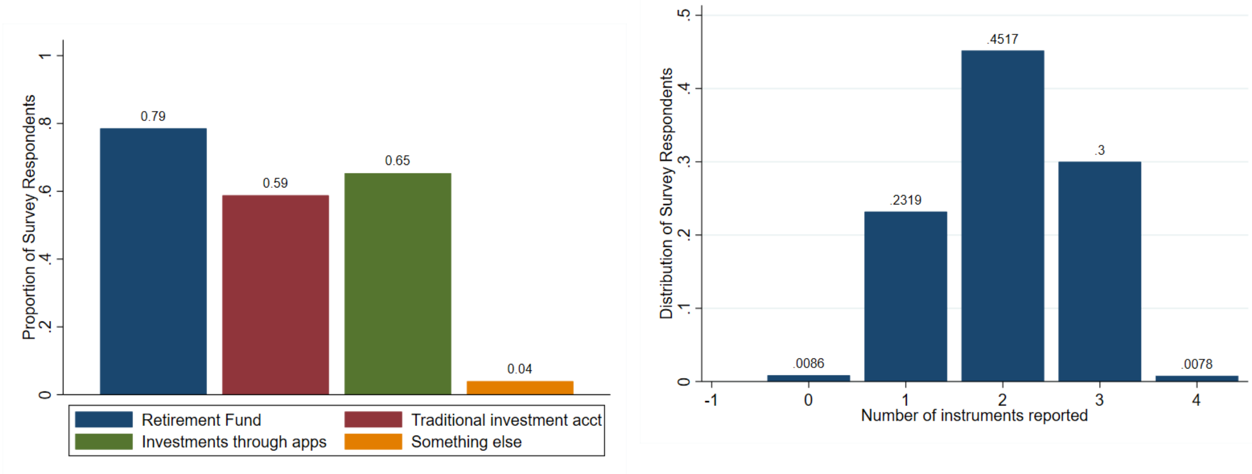


Figure 18: This figure shows the responses to a survey of 1,160 users who were asked about their investing experience, "Do you own stock outside of Bumped? If so, where? 1. Employer-sponsored retirement funds (401k, IRA etc), 2. Traditional or managed investment account, 3. Investments through other 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.

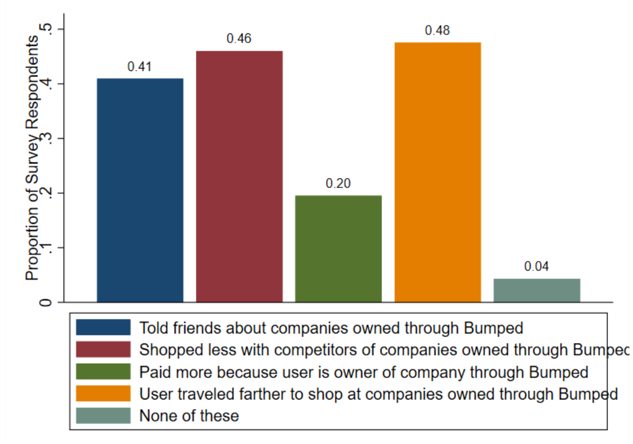


Figure 19: This figure shows responses of 1485 users who were asked to select all that applies for the following question: "Since signing up for Bumped... 1. told my friends about companies I own through Bumped, 2. shopped less with competitors of companies owned through Bumped, 3. paid more for something because of owning a company through Bumped, 4. traveled farther or gone our of my way to shop at companies owned through Bumped, 5. none of these".

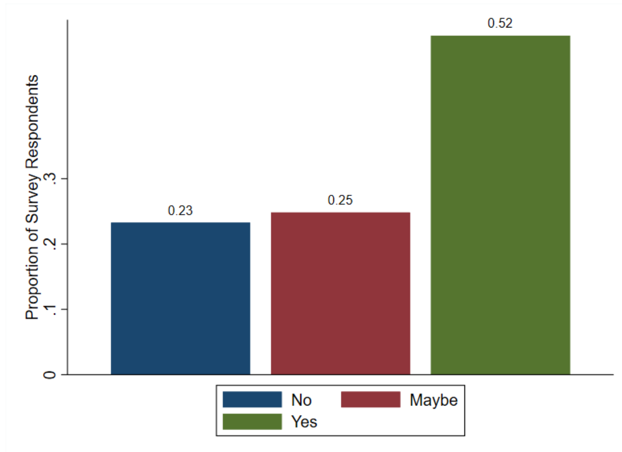


Figure 20: This figure shows responses of 1,160 users who were asked the following question: "Does owning stock through Bumped make you more likely to invest outside of Bumped in the future? 1. No, 2. Maybe, 3. Yes".

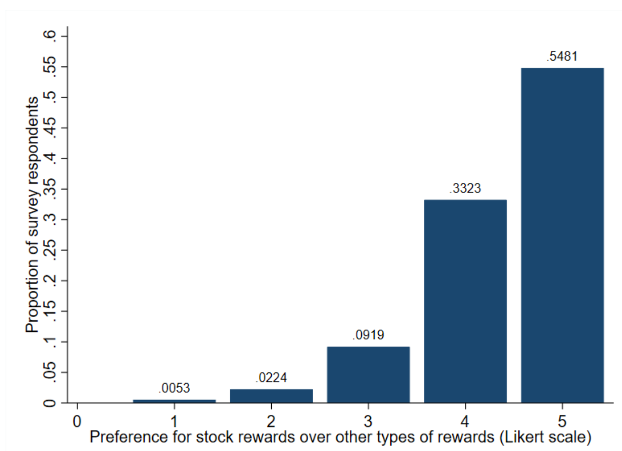


Figure 21: This figure shows responses of 2,287 users who were asked: "In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)? 1. Significantly less excited than traditional rewards, 2. Less excited than traditional rewards,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

Table 1: Summary statistics of Bumped.com users who open their account on the same week in which they came out of the waitlist, or a week after

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

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

Table 2: Summary statistics of Bumped.com users having account open week same as offwaitlist week or a week after who received a grant

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

Notes: Out of the 9,378 users enrolled in Bumped.com for whom account open week is same as offwaitlist 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), calulated per user include amounts before and after opening the app. Rewards and grants are in USD.

Table 3: Summary statistics of Bumped.com users who open their account on the same week in which they came out of the waitlist, or a week after 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: Bumped.com users in the final dataset that pass the following tests are 9,005: 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 open 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 open or a week after account open. If selections are made before account open, opening date of the account is shifted to the date of selection by user. Total number of transactions, and spending (in USD), calculated per user include amounts before and after opening the 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: 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 above, and GR takes on value 1 if a user is a grant receiver and takes on 0 if the user did not receive a grant. Data is at the user-level. 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 7: 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 8: 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 clustered at the user level. *** p<0.01, ** p<0.05, * p<0.1
Notes: In this specification we regress ratio of 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-by-year fixed effects are included.

Table 9: 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 10: Estimation results of Bumped users brand spending on Robinhood clients weekly holdings of that brand

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

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

Notes: In this specification we regress the total daily (weekly) spending in all publicly traded brands of all Bumped users on the daily (weekly) holdings of that company by Robinhood brokerage clients data obtained from robintrack.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 11: Estimation results of Safegraph brand spending on Robinhood clients weekly holdings of that brand

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

Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: In this specification we regress the total daily (weekly) spending in all publicly traded brands of all Safegraph card spending data on the daily (weekly) holdings of that company by Robinhood brokerage clients data obtained from robintrack.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 10, 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 12: 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 log of brokerage account transfers on a post 8 weeks after account opening dummy, which takes value 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes value 1 for transactions more than 8 weeks post account opening and 0 otherwise. 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. User fixed effects and week-by-year fixed effects are included.

Table 13: 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 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 value 1 for transactions during or within 8 weeks of account opening, and on a post more than 8 weeks dummy, which takes value 1 for transactions more than 8 weeks post account opening and 0 otherwise. Column (1) has the USD amounts in net ATM withdrawals and Column (2) has the percentage deviation in net ATM withdrawals 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

	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)	0.083	-0.009	0.139*	0.105	
	(0.082)	(0.053)	(0.080)	(0.093)	
Excited about stock rewards (More)	0.207***	0.029	0.211***	0.213**	
	(0.074)	(0.049)	(0.071)	(0.084)	
Excited about stock rewards (Significantly more)	0.254***	0.131***	0.348***	0.388***	
	(0.072)	(0.049)	(0.069)	(0.082)	
Excited about stock rewards (Likert)					0.096**
					(0.046)
Number of instruments					-0.030
					(0.096)
Excited about stock rewards (Likert) * Number of instruments					0.015
					(0.021)
Constant	0.222***	0.083*	0.194***	0.222***	0.022
	(0.069)	(0.046)	(0.066)	(0.080)	(0.203)
Mean of dep. var	0.46	0.20	0.48	0.52	0.52
Observations	1115	1115	1115	1160	1160
Adj. R squared	0.014	0.021	0.031	0.046	0.046

For columns 1 to 4, 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 1,2 and 3 the dependent variable is binary, and takes the value of 1 when a user reports shopping less with competitors since receiving stock rewards, paying more because of ownership through Bumped or traveling further to shop at companies owned through Bumped. In columns 4 and 5 the dependent variable is binary, and takes the 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 5, the set of explanatory variables consist of the continuous likert scale of question 4, the number of financial instruments held by each user according to question 1, and their interaction. In columns. Robust standard errors in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix

Survey Questions

Q1. We'd love to hear more about your experience in investing. Do you have any of the following? (Select all that apply)

A1.1 Stock (in addition to what's in my Bumped account).

A1.2 Retirement fund, such as a 401k or IRA.

A1.3 Investment fund, such as mutual funds or exchange-traded funds (ETFs).

A1.4 Bonds.

A1.5 Education savings or 529 plan.

A1.6 Something else. content...

Q2. In general, how informed do you feel about investing?

Q3. Since signing up for Bumped... (select all that apply)

A3.1 I have told my friends about companies I own through Bumped

A3.2 I have shopped less with competitors of companies I own through Bumped

A3.3 I have paid more for something because I am an owner of the company through Bumped

A3.4 I have traveled farther or gone out of my way to shop at companies I own through Bumped

A3.5 I haven't done any of these

Q4. As a reward for shopping with a company, would you rather receive 1. Stock in that company or 2. Air miles

Q5. As a reward for shopping with a company, would you rather receive 1. Stock in that company or 2. Coupons

Q6. As a reward for shopping with a company, would you rather receive 1. Stock in that company or 2. Cashback

Q7. As a reward for shopping with a company, would you rather receive 1. Stock in that company or 2. Points

Q8. How do you feel when you get a new stock reward through the Bumped app?

Q9. In general, how excited do you feel about ownership (stock) compared to traditional rewards (points, coupons, cash back, and similar)?

A9.1 About the same

A9.2 Less excited than traditional rewards

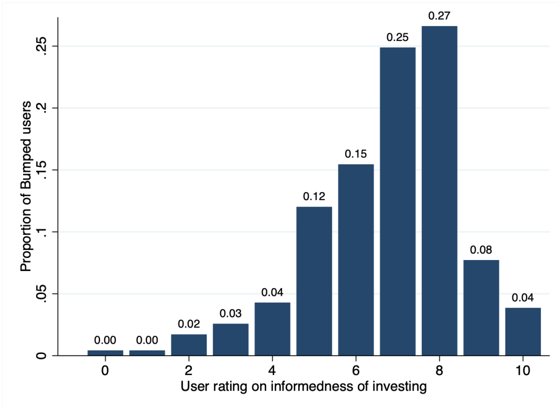
A9.3 More excited than traditional rewards

A9.4 Significantly less excited than traditional rewards

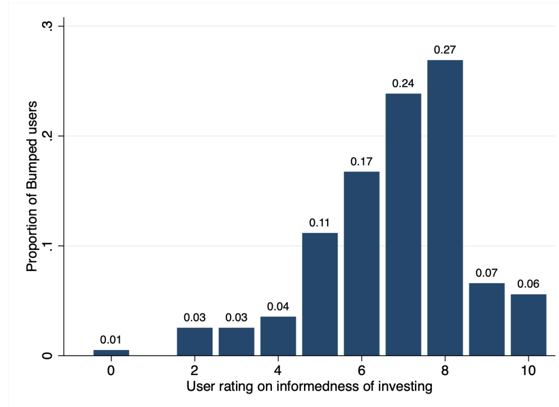
A9.5 Significantly more excited than traditional rewards

A9.6 Why?

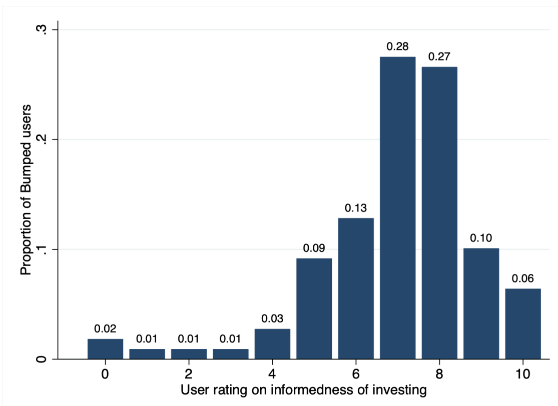
Q10. Anything else you'd like to tell us?



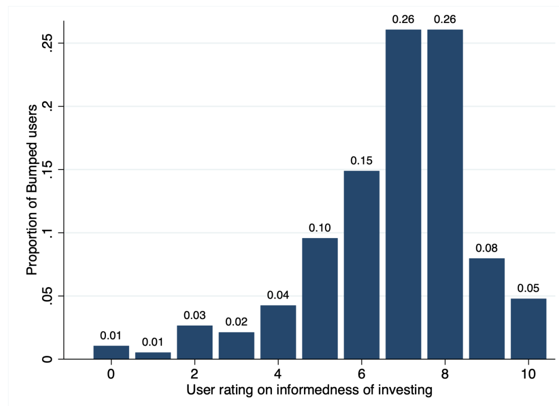
Told friends about companies owned through Bumped



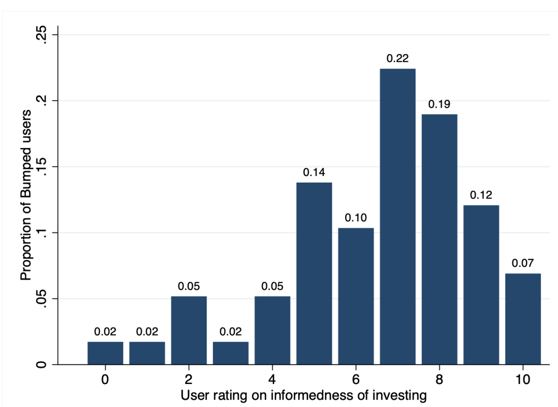
Shopped less with competitors of companies owned through Bumped



Paid more because user is owner of company through Bumped

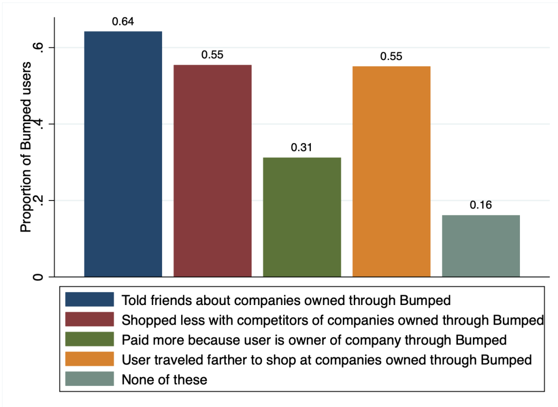


User traveled farther to shop at companies owned through Bumped

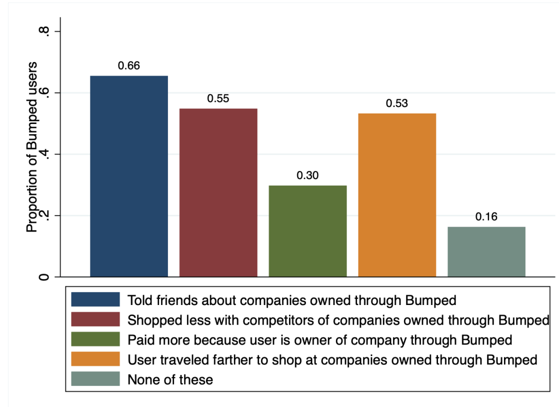


None of these

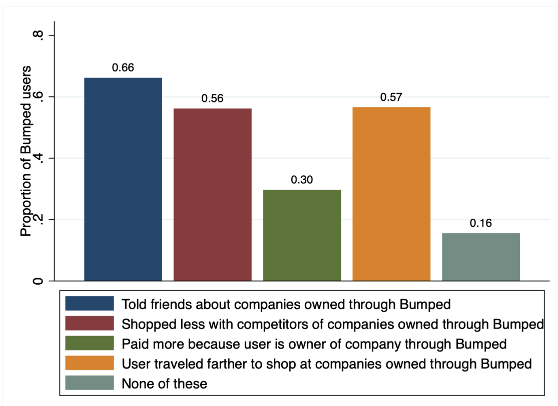
Figure 22: This figure shows users' informedness of investing according to the survey responses of 358 users during the Fall of 2019. Users were asked "In general, how informed do you feel about investing?" and had to rate from 1 to 10. Additionally, they were also asked to select all that applies for the following question: "Since signing up for Bumped... 1. told my friends about companies I own through Bumped, 2. shopped less with competitors of companies owned through Bumped, 3. paid more for something because of owning a company through Bumped, 4. traveled farther or gone our of my way to shop at companies owned through Bumped, 5. none of these".



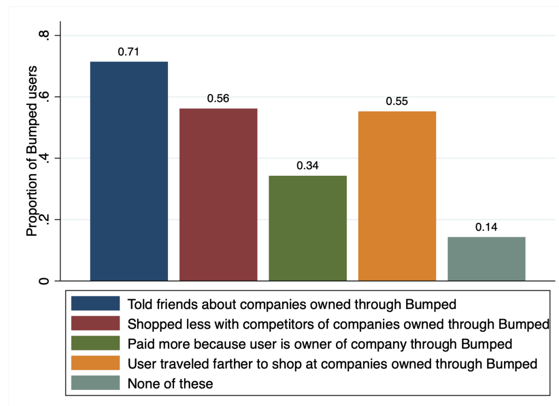
Stock (in addition to Bumped account)



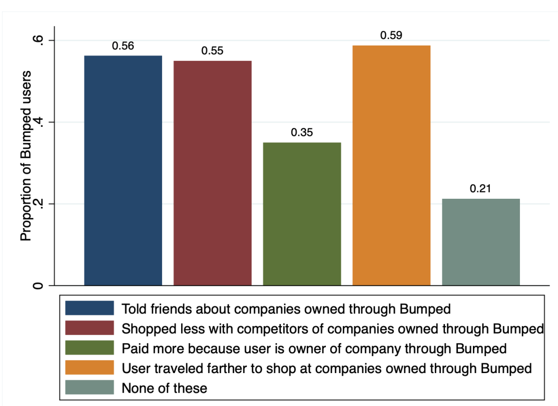
Retirement Fund



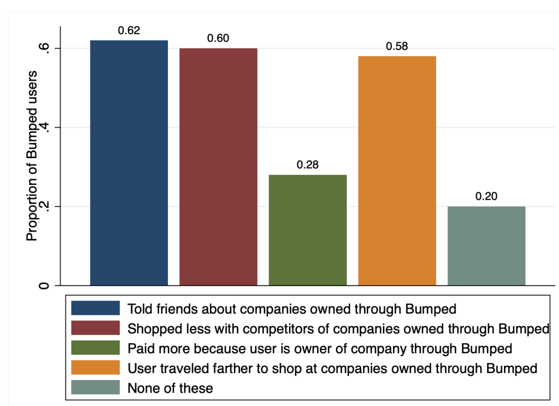
Investment Fund



Bonds



Education Savings



Something Else

Figure 23: This figure shows responses to a survey sent to 358 users during the Fall of 2019. Users were asked about their investing experience, "We'd love to hear more about your experience in investing. Do you have any of the following? 1. stock (in addition to what's in my Bumped account), 2. retirement fund, such as 401k or IRA, 3. investment fund, such as mutual funds or exchange-traded funds (ETFs), 4. bonds, 5. education savings or 529 plan, 6. something else". Additionally, they were also asked to select all that applies for the following question: "Since signing up for Bumped... 1. told my friends about companies I own through Bumped, 2. shopped less with competitors of companies owned through Bumped, 3. paid more for something because of owning a company through Bumped, 4. traveled farther or gone our of my way to shop at companies owned through Bumped, 5. none of these".