Bumped: The Effects of Stock Ownership on Individual Spending

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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 app, Bumped, that opens a brokerage account for its users and rewards them with stock when they shop at previously elected stores in several retail categories. For identification, we use the staggered distribution of Bumped accounts over time after individuals had signed up for a waitlist. We find that individuals spend approximately 40% more per week at elected brands and stores after being allocated a Bumped account. Additionally, Bumped granted some of the users with stocks from several companies as part of a promotional program. In response to the stock grant, individuals increase their weekly spending by 100% at the brands of which they received the stocks. 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.

Keywords: stock rewards, spending at owned brands, FinTech

JEL codes: G5, D90, G41, D14

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1 Introduction

We analyze de-identified transaction-level data from a FinTech company called Bumped. Bumped serves as a loyalty platform that rewards its users with stock from the brands and stores they buy from. Customers sign up for a Bumped brokerage account and link all their checking and credit card accounts. In turn, customers can select their favorite brands in several retail categories. If and when they spend at their selected brands, they receive a fraction of that brand's stock in their Bumped brokerage account. For identification, we use the staggered allocation of Bumped accounts over time after individuals initially sign up for a waitlist. Additionally, we look at the effects of stock grants that Bumped distributed to some of their clients at certain points in time.

We show that customers increase their spending at the selected brands after receiving a Bumped account. 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. 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 FinTech 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 and remain there for an unknown period before getting their Bumped account. When individuals come across the Bumped website, they can only enter their email address to be waitlisted for a Bumped account. They cannot directly or immediately open a Bumped account. The Bumped operations team then releases batches of users to onboard in a first-come-first-served fashion. The number of users to release is based on varying business objectives and constraints. In the data, we see when individuals were waitlisted and when they were allowed to open their accounts. Users spend a considerable amount of time waitlisted, on average, 4.5 months. Additionally, we restrict our analysis to users who sign up immediately after being allowed to do so. Therefore, we feel it is implausible that Bumped 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, Bumped does not know anything about them except their email address.

To add credibility to our identification strategy, we show that there is no spending response in either eligible or ineligible spending at the time of being waitlisted. 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.

We lend additional credibility to our account opening result by exploiting another unique feature in our setting. Bumped granted users with $5 or $10 company stock grants from Red Robin, Taco Bell, McDonald's, Exxon Mobile, and Chevron, upon account opening as part of a promotional program. This promotional program ran for a number of months but was not advertised. 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 to account opening on eligible spending.

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

Beyond documenting a causal link between stock ownership and spending, we also document that daily and weekly spending in certain brands for the Bumped 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 company and week-by-year fixed effects. We argue that this results 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 users of Bumped.

Literature Review:

Our paper is 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 (2016), 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).

Our research is also 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, i.e., MacGregor et al. (2000), Schoenbachler et al. (2004), Frieder and Subrahmanyam (2005), Aspara and Tikkanen (2011), and Keloharju et al. (2012). Additionally, the existing literature has documented that 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). However, little attention has been given to the advantages to firms when individuals hold their stock or to the effects of receiving stocks as a reward on customer behavior.

To the best of our knowledge, the only existing study that looks at the random distribution of stocks to individuals 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. 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. As Bernard et al. (2018), we show that stock ownership can benefit the firm, by modifying individuals' spending behaviors. 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), we predict Bumped users to regard stocks not only as investments but also as a type of consumption good that increases loyalty to the company. We provide a detailed discussion of the psychological mechanisms at play: affect and gift exchange, cognitive dissonance, illusion of control, familiarity, endowment effects and loss aversion, ownership, and alignment of incentives.

We organize the remainder of this article in the following way: Section 2 describes the Fin-Tech app setting and our empirical design. Section 3 presents our empirical results. Section 5 discusses in detail the psychological mechanisms that are consistent with our findings and Section 6 concludes.

2 Setting, data, and empirical strategy

Section 2.1 describes Bumped, the FinTech app. Section 2.2 describes the data used. Section 2.3 discusses the empirical strategy.

2.1 FinTech app setting

To become a user of Bumped, individuals have to first sign up for a waitlist on the company's website. At the time of signing up for the waitlist, interested users provide their email addresses and names. No additional information is provided at that time. In turn, on a first-come-first-serve basis, users are taken out of the waitlist and are invited to open a brokerage account with Bumped. If users fail to open a brokerage account with Bumped when approved, two reminder emails are sent. However, we restrict our analysis to users who sign up for their Bumped account within a week of being off-waitlisted. Once users signed up for a Bumped brokerage 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.

Bumped currently divides their brands into 16 different retail categories and allows users to 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 a Bumped brokerage account are limited to the users' stock rewards, in that 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 Bumped 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

Bumped portfolio containing the stock rewards the user received and their current value, as well as their daily changes.

Additionally, as part of a promotional program, Bumped granted stock to all users 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 Bumped 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 for a Bumped account, when our subsample of users were invited to open a Bumped account, and when our subsample of users received their account since Bumped launched.

2.2 Data

The dataset we received includes de-identified and aggregated 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 (no other personal information was shared). 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 Bumped brokerage account. 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 Bumped account 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 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 Bumped users were waitlisted initially and then received their Bumped 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 cards being linked to Bumped. 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 by Bumped. In our final dataset, we have 551 different brands that our users spend at.

Additionally, starting March 2018, Bumped launched a promotional program. Users were granted stock of certain brands upon signing up for their Bumped account. Initially, the program consisted of a one-time grant of fractional shares from one chain restaurant, Red Robin. In turn, Bumped also granted stock to its users from other companies: Taco Bell, McDonald's, Exxon Mobile, and Chevron. The grant was displayed in-app with a description noting it was a 'thank you' for choosing the brand, and a push notification was sent as well. The amount and timing were decided by the marketing team. The promotion was not restricted in any way, in that all users who had selected that brand received the stock grant at the time of the promotional program. Users did not know 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. We argue that 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 justify this argument performing a covariate balance test between grant recipients and non-recipients, before they get off the waitlist. In Table 5 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 27 fewer transactions per month, compared to 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. Since this survey is performed at the household level, we normalize spending dividing by the average household size of 2.52. Bumped users are younger and more likely to be men. Bumped data shows 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 selected brands tracked by Bumped. After taking that into account we consider the spending levels of Bumped users to be broadly similar to those in the Consumer Expenditure Survey.

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 Bumped account:

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

In Specification 1, $Spending_{Eligible}^{iw}$ 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 $w_{Bumped}^{iw\tau}$ is an indicator whether user i in week w had received his or her Bumped account in his or her user specific τ week. The coefficients β_{Bumped}^{τ} thus tell us the path of eligible spending after the user received his or her Bumped account. We consider 8 weeks before and after receiving the Bumped 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:

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

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:

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

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 being Bumped with a grant, relative to the effect of being Bumped without a grant, β_B^τ, 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 a Bumped account:

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

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 waitlist for the Bumped account. Standard errors are shown as the dotted lines and clustered at the individual level.

3 Results

3.1 Spending Results

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 thus 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 Bumped account. We can clearly see a pronounced spike in eligible spending in the week that users receive their Bumped 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 60 USD per week so this corresponds to approximately a 24 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 310 USD per week. We can thus say with statistical confidence that the offsetting impact on ineligible spending was smaller than 16 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 Bumped stock grant. 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 6 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 that were not the granted brands.

Columns 1 and 2 of Table 7 shows a similar analysis, but in this case, we directly use dollar spending per week as the dependent variable. We confirm that receiving a Bumped account 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 8 shows similar effects for log spending instead of the absolute amounts.

Table 6 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 7 shows similar results, estimating the effects on spending directly in dollar terms, instead of deviations from weekly averages. Columns 3 and 4 show a 4 dollar increase in eligible spending, and a reduction of 67 cents in ineligible spending.

Figure 11 shows the β_{BG}^{τ} 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 Bumped stock grant. 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.

As part of the routine operations of Bumped, 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.

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.

3.2 Attention and Spending Response

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.

3.3 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.

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 for a Bumped account and got 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 for their Bumped accounts.

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.

There is substantial variation in the time between being waitlisted and receiving a Bumped account. The average time individuals are waitlisted is longer than the 8-week windows we consider in our estimations. Furthermore, who received a Bumped account was decided on a first-come-first-serve basis given the company's business objectives and constraints. At the time individuals are waitlisted, Bumped only knows individuals' email addresses but no other information is known. In turn, they allocate accounts on a simple first-come-first-serve basis. This is confirmed when we regress the time on the waitlist on individual-level characteristics in Table 9. In this Table, we see that no observable characteristic seems to explain well who got bumped from the waitlist. Additionally, we can perform a sample split according to the time individuals were waitlisted at. The results for receiving a Bumped account for three terciles of individuals being waitlisted can be found in Figure 15.

4.2 Spending and stock ownership

We also document that daily and weekly spending in certain brands for the user population of Bumped 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. 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.

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 than for 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.

5 Psychological mechanisms

The main hypothesis underlying the distribution of rewards to customers is that the stock rewards alter customers' behaviors in ways that benefit the company. In general, if individuals perceive the rewards as a gift from the company in which they routinely purchase, they may feel the need to reciprocate by buying more from that company (see Kube et al., 2012). Here we are interested on whether the specific type of rewards under consideration, stocks, change customers' behaviors, beyond generic affect and gift exchange effects. In the following, we first discuss affect and gift exchance and then other psychological mechanisms that may be at play for stocks as rewards specifically.

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.

Gift exchange is also a potential mechanism behind our results. 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 this case, Bumped users are involved in a transactional relationship by which they get rewarded in exchange for specific behavior. Furthermore, the companies reward the stock through Bumped which can be thought of as a third party. However, if Bumped 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. That is potentially why we see a spending response in all eligible spending and not only in spending at those brands of which individuals received grants.

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)).

Illusion of control When it comes to stock ownership, 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.

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.

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).

Endowment effects and loss aversion As shown by Kahneman et al. (1990), “measures of willingness to accept greatly exceed measures of willingness to pay". Much experimental evidence has shown that losses and disadvantages have greater impact on preferences than gains and advantages (Tversky and Kahneman (1991)). Moreover, investors who are the most informed take the least risk (Thaler et al. (1997)).

In synergy with the “familiarity" dynamics, loss aversion should lead the reward recipients (who should become better informed than non-shareholder clients) to avoid behaviors which could cause a depreciation of stocks and a loss in their portfolios.

Ownership Shang et al. (2017) examined the influence of perceived ownership (self/other) and perceived chooser (self/other) of stocks on brain activity. By looking at differences in FRN waves [1] between losses and gains which reflect violations of expectancy to stock outcomes, the authors have shown that observations of stock outcomes among four types of chooser-owner relationships obtained diverse feedback-related negativity. By showing that the d-FRN[2] discrepancy was sig-

[1] FRN (feedback-related negativity) waves consist in differential brain responses to stock outcomes.

[2] differentiated feedback-related negativity

nificant for the other-chosen-self-owned outcome evaluations but not for the other-chosen-other-owned outcome evaluations, the results conveys the existence of the ownership effect (Beggan (1992)).

Shang et al. (2017) found that “when the chooser was the other, participants tended to have more positive expectations for self-owned stock outcomes than for other-owned stock outcomes." In other words, when not involved in the share choosing process, people tend to prefer self-profit from stocks to non-self-profit. On the other hand, self-chosen-self-owned stock outcomes was not significantly different, in terms of d-FRN, from other-chosen-self-owned stock outcomes, suggesting that the chooser ship effect was outweighed by the ownership effect. As long as investors possess the shares, it is not relevant whether they choose them or not.

The parallel with the rewards program is: customers do not self-choose the shares, but they own the shares assigned to them. Given that the chooser ship channel loses its importance once the individual owns the stocks, we assume that who receives shares also value them similarly to someone who personally chooses them. Moreover, if the effect of the gift in the form of shares is stronger than the one of the gifts in the form of cash, this would mean that there is a specific effect due to share ownership.

Alignment of incentives The functioning of the rewards program closely resembles the compensation programs which address executives through stocks. As shown by ample empirical evidence, the granting of shares leads to an increase in employee retention rates (Oyer and Schaefer (2005)). Furthermore, it has been found that employee stock ownership plans increase productivity (Hochberg and Lindsey (2010)) and produce net cost benefits (Gong (2017)). With underlying dynamics like the employee stock ownership plans, the loyalty program aligns the incentives of executives and customers who all benefit from a good performance of the firm. Furthermore, upon receiving shares, customers have an opportunity cost of not maintaining their affiliation with the company. Indeed, as employees lose firm’s stocks upon resignation, so clients do if they leave the company. Aspara et al. (2009) have shown that shareholder customers have, on average, 3.3 more active years in the firm than the non-shareholder customers.

6 Conclusion

In this study, we quantify the effects of awarding 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 Bumped 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 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 Bumped granted users with company stock at different points in time, as part of a promotional program.

We show that customers increase their spending at the selected brands after receiving a Bumped account. Weekly spending at selected brands jumps up by 30% to 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 17 to 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 14 USD.

When Bumped granted users with company stock at different points in time, as part of a promotional program, we find a weekly spending response of 100% at those brands of which individuals received stock. 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.

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Figures and tables

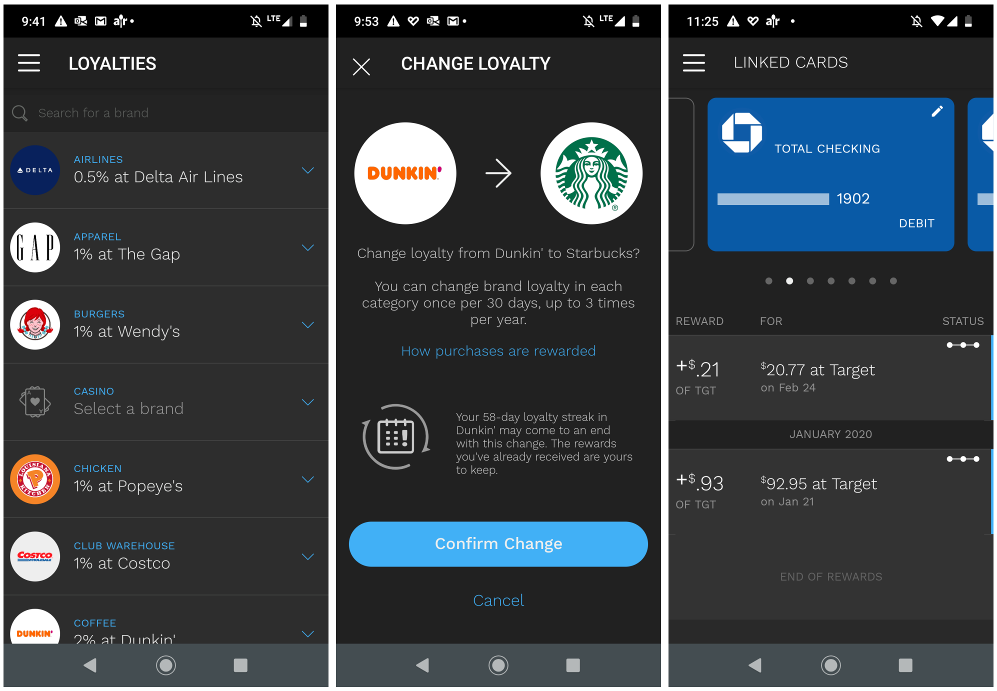


Figure 1: The Bumped app: screenshots of brand selection, switching brands, and linked card screens

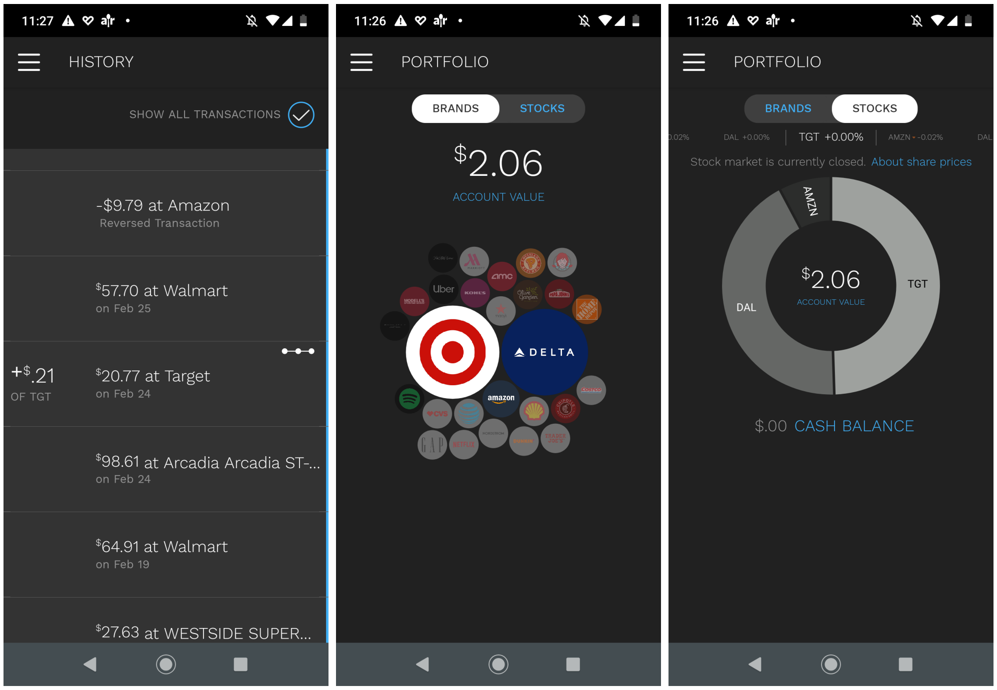


Figure 2: The Bumped app: screenshots of transactions and portfolio screens

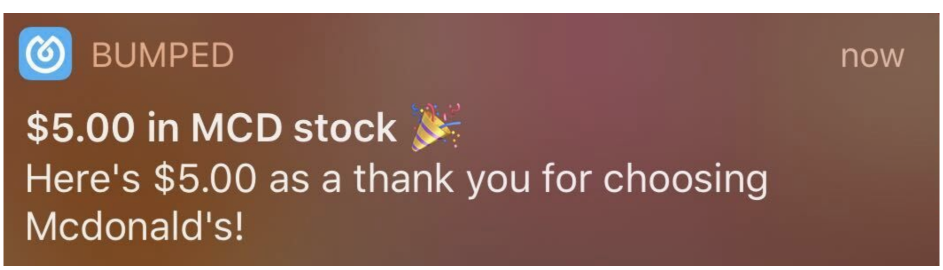


Figure 3: Stock grant notification received by Bumped users

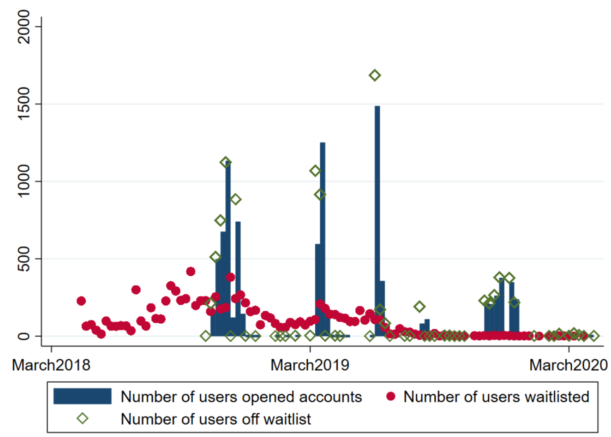


Figure 4: Number of users in our data subsample who were waitlisted and received a Bumped account

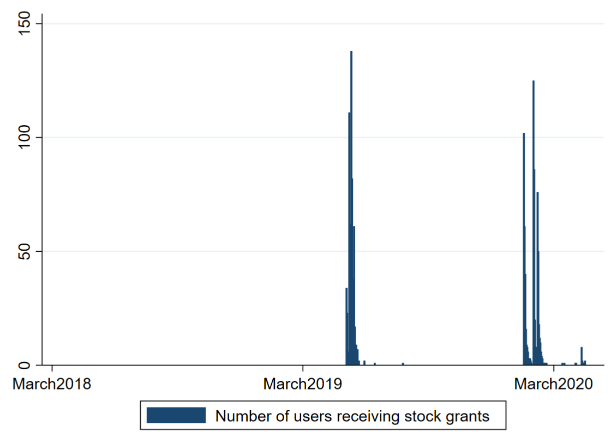


Figure 5: Number of users in our data subsample who received stock grant since Bumped launched

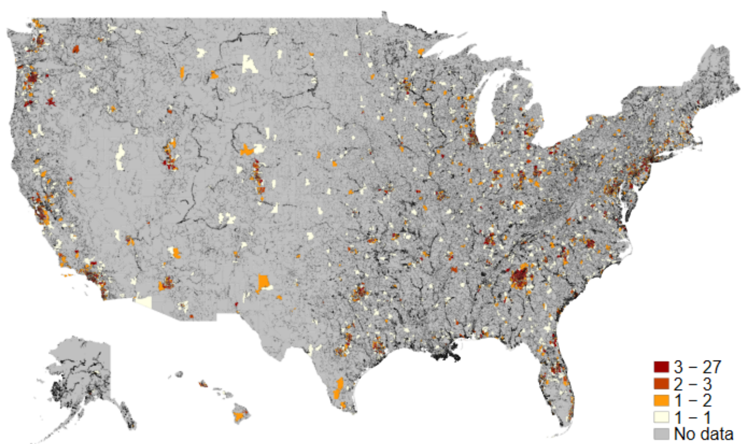


Figure 6: Bumped users by 5-digit zip code in the US

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
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: Covariate Balance Check between Grant and Non-Grant Receivers before getting out of 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.

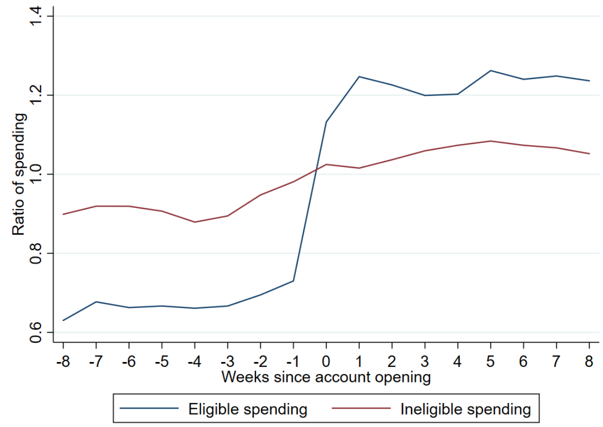
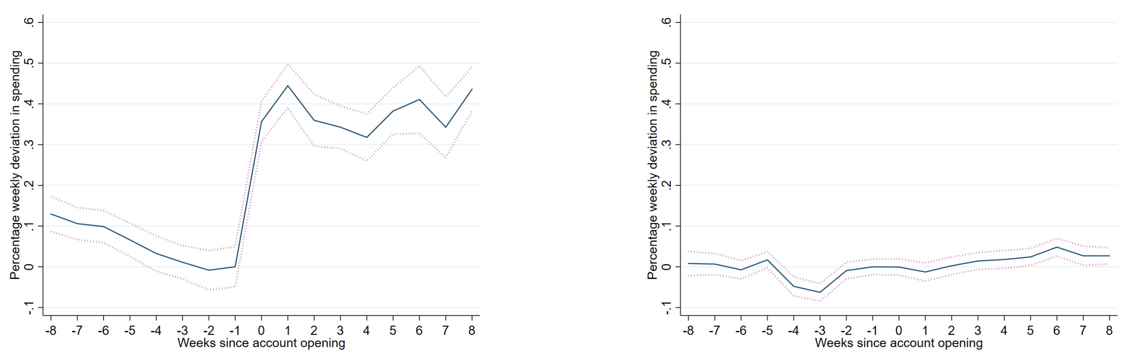


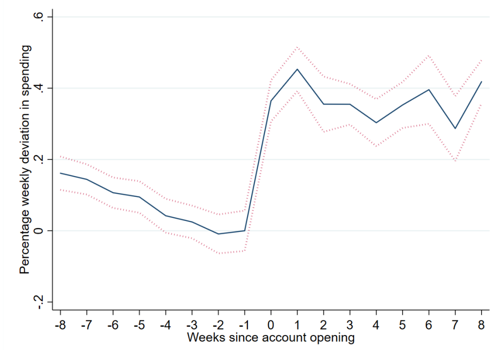
Figure 7: Ratio of eligible and ineligible spending by week relative to account opening



Eligible spending Ineligible spending

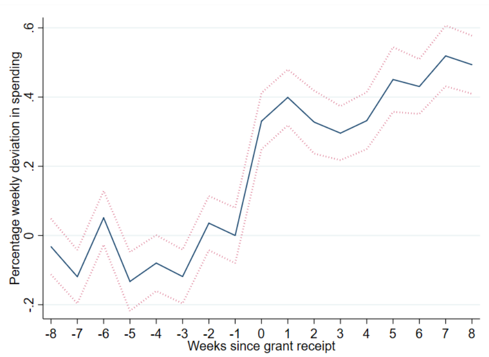
Figure 8: This figure shows the coefficient estimates β^{τ}_{Bumped} in Specification 1 for both eligible and ineligible spending (defined as the percentage deviation from the individual-level mean). We control for individual and week-by-year fixed effects and consider 8 weeks before and after receiving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

Eligible spending for grant recipients

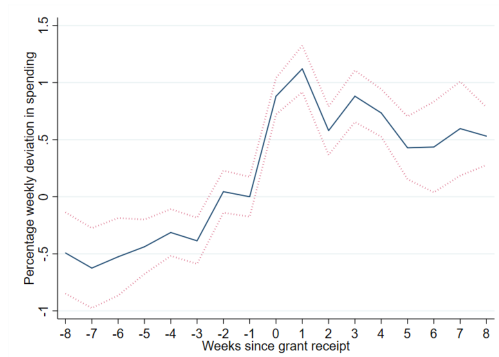


Eligible spending for grant non-recipients

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 Bumped account. Standard errors are shown as the dotted lines and clustered at the individual level.

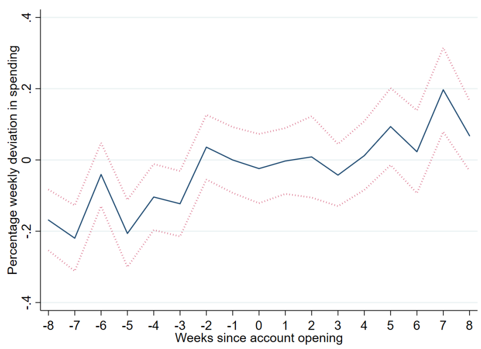


Eligible spending

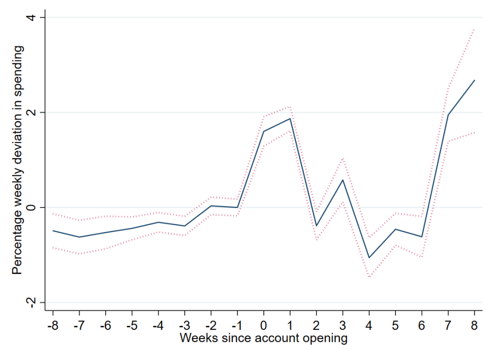


Eligible spending at brands of which stock was granted

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.

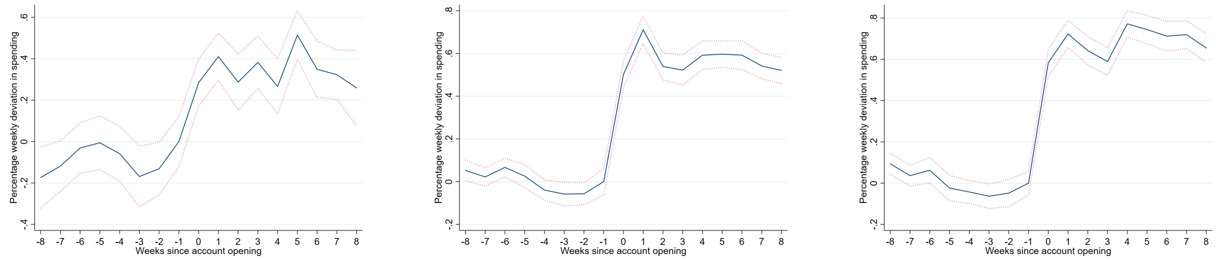


Incremental effect of grant receivers on eligible spending

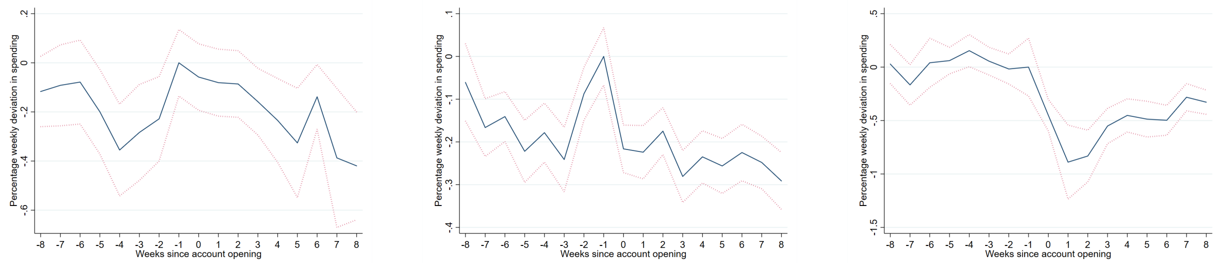


Incremental effect of grant receivers on eligible spending in grant brands

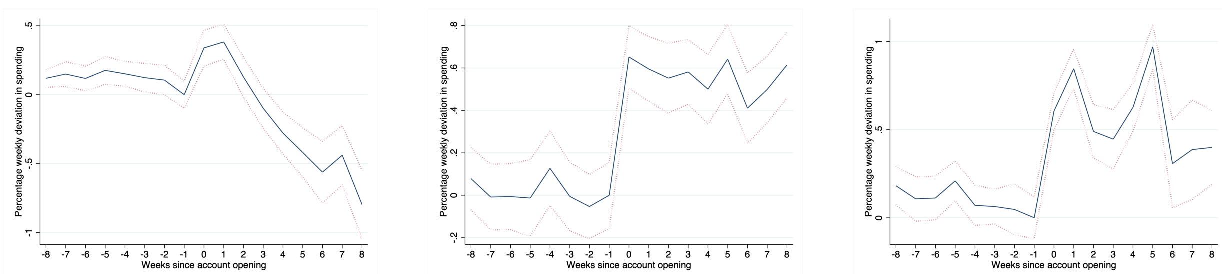
Figure 11: This figure shows the coefficient estimates β^{τ}_{BG} in Specification 3, i.e., the incremental effect of grant receivers on all eligible and grant brand spending (defined as the percentage deviation from the individual-level mean) post account opening. We control for individual and week-by-year 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.



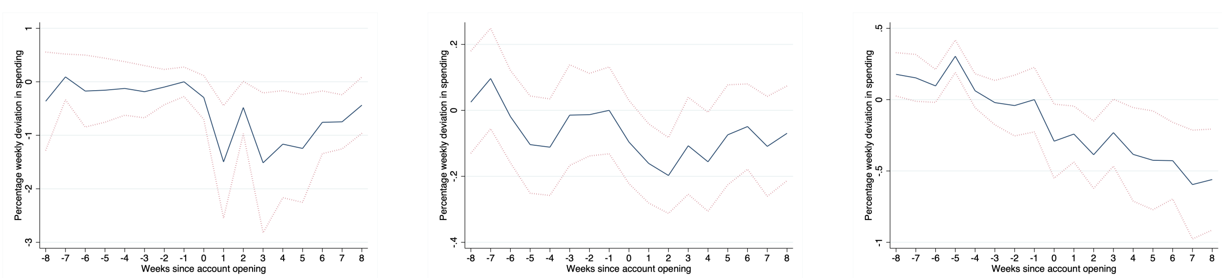
Grocery – Eligible spending | Burgers – Eligible spending | Coffee – Eligible spending



Grocery – Ineligible spending | Burgers – Ineligible spending | Coffee – Ineligible spending

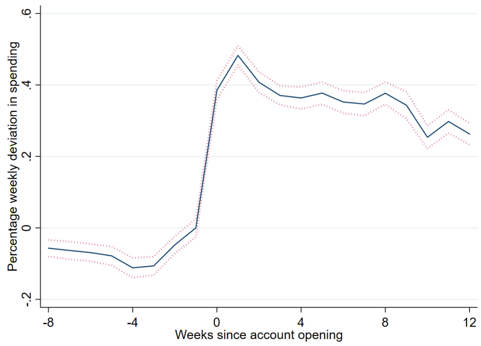


Superstores – Eligible spending | Ride Share – Eligible spending | Drug Stores – Eligible spending

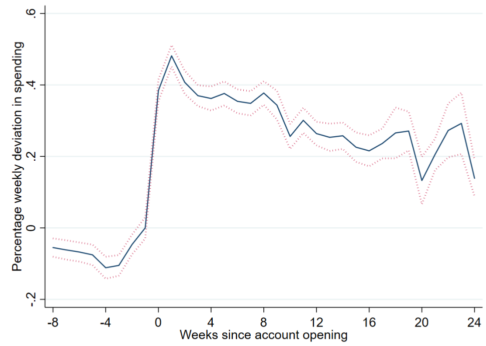


Superstores – Ineligible spending | Ride Share – Ineligible spending | Drug Stores – Ineligible spending

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 Bumped account. Standard errors are shown as the dotted lines and clustered at the individual level.

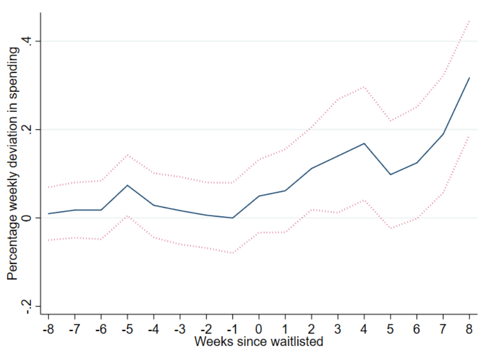


Eligible spending 3 months after account opening

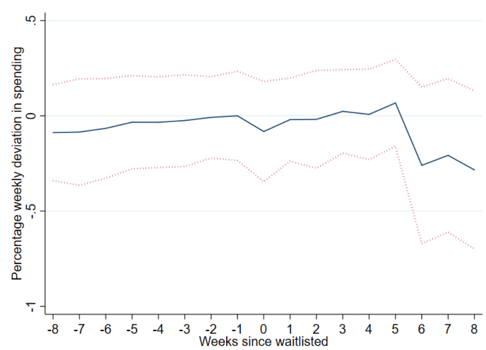


Eligible spending 6 months after account opening

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 Bumped account. Standard errors are shown as the dotted lines and clustered at the individual level.

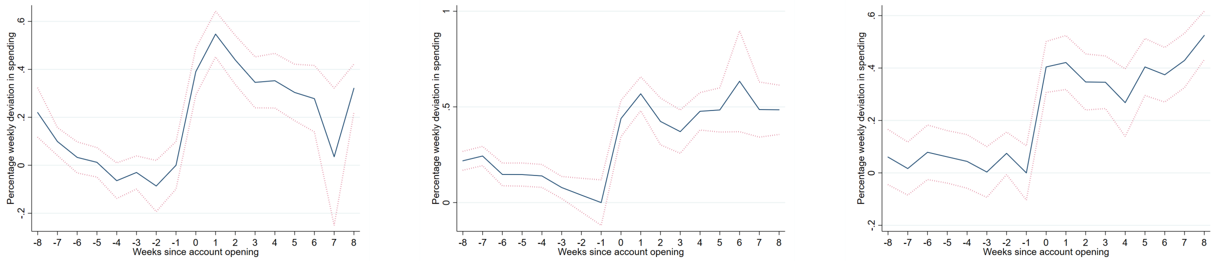


Eligible spending



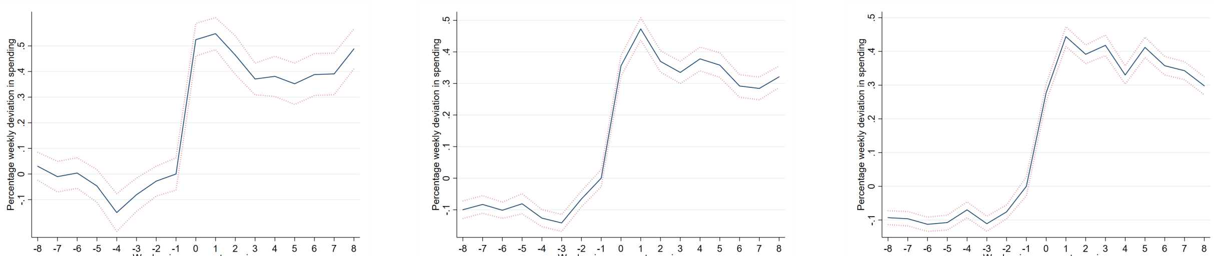
Ineligible spending

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 waitlisted for the Bumped account. 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 Bumped 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 Bumped account. Standard errors are shown as the dotted lines and clustered at the individual level.

Table 6: Estimation results of bumped.com users on 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 fixed effects are included.

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

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

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

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

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

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

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

Table 9: Estimation results of time on waitlist on user characteristics

	Days on waitlist	Days on waitlist	Days on waitlist
Constant	128.843***	172.792***	170.381***
	(0.106)	(0.146)	(0.144)
Age	✓	✓	✓
Gender	✓	✓	✓
Number of transactions		✓	✓
Number of linked cards		✓	✓
Total spent		✓	✓
Total amount rewarded		✓	✓
Month-by-year fixed effects			✓
Observations	8245614	8245614	8245614
Adj. R squared	0.001	0.105	0.113

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. Gender is controlled for in dummies of three categories. All other variables are controlled for in dummies of quintiles. 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 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		✓	✓		✓	✓
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 fixed effects refer to any day 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		✓	✓		✓	✓
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 fixed effects refer to any day 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.