Windfall or Rational Choice? An Experiment on the Determinants of Excess Consumption*

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Abstract

Free samples, coupons, and promotion codes are commonplace in business and are targeted at potential customers. However, many of these free transfers could fall into the hands of existing customers, and traditional consumer theory predicts that a small in-kind transfer of a good already being consumed is unlikely to increase the recipient's consumption of that good. The transfer simply displaces purchases, leading to a marginal propensity to consume of approximately 0%. This prediction was tested with a large field experiment conducted in a substantial operating business. Surprisingly, we find an MPC of 17% for transfers going to those already habitually buying and consuming the good. A behavioral explanation for this excess consumption is that windfalls are treated more loosely than purchases. However, there is empirical evidence for a rival rational explanation that considers the timing of purchases and consumption and uncertainty about future personal demand. The transfer creates an "uncomfortable" level of inventory, and transfer recipients want to spend it down quickly, lest it lose its value.

JEL Codes: A11, B22, C33

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1 Introduction

There are many scenarios where some firm or organization would like to induce more consumption of some good by an individual consumer. One common approach is an in-kind transfer of the good. Although the goal is for the transfer to reach a potential customer, it often can go to an existing customer. And for an existing customer, a transfer is likely to prove ineffective. If the consumer receives $1 worth of the good—and they would have consumed a least a $1 of that good but for the transfer—consumption would only increase due to income effects: the excess consumption is the same as the amount they would have bought if instead given $1 in cash. If pre-transfer expenditure on the good is a small fraction of their total budget, anything but a tiny increase in consumption due to income effects is implausible.[1]

This paper reports the results of a field experiment that tests these consumer theory predictions. The experiment was conducted on an operating platform business. A treated group received an unexpected transfer of a digital good they had been habitually consuming. A control group received no such transfer. The transferred goods were tokens for the usage of its services that would otherwise have to be purchased. Then, in the following weeks, we observed what happened to purchases and consumption of that good. We can summarize the net stimulating effect of the transfer by computing the marginal propensity to consume the transfer. At MPC = 0, there is no increase in net consumption; at MPC = 1, the transfer stimulates additional net consumption equal to the full transfer.

Before discussing the results, it is useful to highlight the advantages of this setting for our research question. It was a real-world setting, with individuals making consequential decisions over long time frames. The platform is the only place where the good can be used. It is the only seller of the good, and the good cannot be transferred to others or resold. And as a digital good, the platform's only cost from the transfers it provided was forgone sales, which are measured precisely. These features provided a highly controlled environment, and thus took away a raft of concerns about alternative explanations or measurement issues. For example, there was no possibility that transfers would lead to unmeasured decreases in the sales of other goods, and that the transferred good would be put to some other use or resold. Absent these assurances, observed increases in "usage" might be illusory (Ashenfelter, Farber and Ransom, 2010; Cunha, De Giorgi and Jayachandran, 2019). Moreover, as a digital good, its storage cost is 0 and never decays. The transfer was large enough to be meaningful but not so large that income effects would likely matter. These features eliminate many rational reasons for finding a non-zero MPC.

Our main result is that the overall MPC is about 60%. However, much of this consumption was by consumers who had already exited the market at the time of transfer. Although the transfer targeted those habitually consuming, this targeting was imperfect. As such, the MPC

[1] If it is a Giffen good, consumption could decrease following a transfer.

should be expected to differ depending on an individual's recent consumption and spending behavior. Some recipients simply ignore the transfer, which has no effect on the MPC calculation.

To decompose the sources of the increase in consumption, it is useful to divide users into three types based on what is observed in the control group after allocation in the experiment. The key groups and their shares of the market are:

- E - (15% in the control), "Exit": users who stopped buying and consuming before the allocation to the experiment
- F - (43% in the control), "Fumes": users who consume but do not buy post-allocation
- G - (42% in the control), "Gangbusters": users who consume and buy post allocation

Some of the increased consumption came from inducing Es to consume coins after the allocation. About 2% of the recipients were Es who consumed in the post period. As expected, the Fs increased their consumption far more than the Es. However, the transfers did not affect purchases by Es or Fs. For neither group was their 0 intended purchases affected. For these consumers, it is unsurprising that transfers stimulated additional consumption. But as we show, their role was insufficient to explain the overall observed MPC. As the good was transferred to Gs, individuals both consuming and purchasing, a reasonable expectation was that MPC would be close to 0. This is far from what we find. Free goods created significant additional consumption, including those who would otherwise buy in the future. The empirical analysis below demonstrates this finding; the theoretical analysis provides a plausible explanation.

For Gs, the simple expectation would be that transfers would not affect consumption (MPC = 0), but would displace purchases on a one-for-one basis. The behavior of Gs cannot be observed directly because some control Gs look like treatment Fs. However, straightforward calculations show that the Gs had an MPC of at least 0.17. To further examine this calculated MPC, we estimated quantile treatment effects, which reveal unambiguous increases in consumption at parts of the consumption distribution that could only be due to Gs.

Why do the Gs not have an MPC = 0? One conceivable explanation is that it is a behavioral phenomenon, with the "free" samples being thought of as a windfall that is consumed less cautiously or because it is put in a mental account and is thus earmarked for consumption of that good(Thaler, 1990; Milkman and Beshears, 2009; Hastings and Shapiro, 2013). Although behavioral explanations are possible, we present a theoretical model that yields a rational explanation that may apply to settings like ours.

The rational explanation shows that an unexpected transfer can increase the consumption rate for a good where there is a hazard of not needing the good. That transfer increases the stock of the good, and as one's stock increases, one has a lower reservation value for spending the good, since it is more likely not to be needed. Empirically, we show that users in our context are subject to the hazard of inventory becoming useless, as in the model. Unlike the straight "saw-tooth" in Baumol (1952)/Tobin (1956), our modeling assumptions lead to a curved saw-tooth, with faster rates of consumption when balances are higher. This curvature can create a non-zero MPC—a transfer that moves the recipient to a place on their consumption path where the consumption *rate* is higher. The model shows that changes in the consumption rate are sufficient statistic to estimate the MPC, based on the empirical change in consumption rates. For our data, the consumption rate increase predicts an MPC of 0.2, close to the 0.17 we estimated.

The main conceptual contribution of this paper is to identify, precisely, the sources of excess consumption from an in-kind transfer. It shows that the MPC depends critically on where users are in their consumption plan. We are the first to show this, to the best of our knowledge.[2] One major practical implication is that free transfers intended to boost consumption are less costly in cannibalized sales than predicted. Additional consumption can be obtained even from those that theory predicts should offer no additional consumption. Even if the firm does not care about stimulating consumption *per se*, so long as it has some markup such that price is greater than marginal cost, it would prefer to give a free good to an MPC = 1 consumer than an MPC = 0 consumer, as $p > c$.

More generally, our findings imply that the efficiency of transfers could be improved by paying attention to the consumption patterns of users. For example, targeting Fs and Es can increase consumption with no loss of sales. For Gs, targeting those at certain points in their consumption path could lead to greater MPC and less loss of purchases. However, it seems almost certain that if transfers became anticipated and conditioned on behaviors, behaviors would change. This seems like a fruitful area for future research. Although not our focus, the model makes some predictions that could explain certain counter-intuitive behaviors by firms engaged in promotional activities.

Although our setting is not directly policy-relevant, how consumers respond to in-kind transfers is policy relevant. For example, there is a large and somewhat unsettled literature on the effects of Food Stamps but concerned with the same basic question that we are Hoynes and Schanzenbach (2009); Hastings and Shapiro (2013). Ironically, perhaps such a small amount of money that mental accounts are not even worth setting up.

The problem we focus on is quite general. There are numerous examples of firms or organizations trying to stimulate consumption. In terms of applications, a government or philanthropy might give away goods—face masks, vaccines, pollution control technologies—not just

[2]While money is quite different from an in-kind good, there is a clear analogy between the platform problem and the government problem of targeting a stimulus, which is also trying to seek out individuals with a high MPC (Gross, Notowidigdo and Wang, 2016; Shapiro and Slemrod, 2003; Souleles, 1999; Olafsson and Pagel, 2019). If the unexpected windfall is cash, an individual dealing with a shock has some nice options—they can save it with little or no loss in lifetime utility (Friedman, 1957).

to help those receiving the good, but also because of the external benefits conferred to others from own consumption. The WIC and Food Stamps are intended to increase net consumption. In-kind transfer of goods may seem only relevant in government social welfare or development contexts, even for-profit firms frequently engage in these activities. Business logic often supports the distribution of "free samples"—in-kind transfers of a good. For an experience good, getting a potential customer to try the good might stimulate future consumption that offsets the upfront cost.

But even with search goods, a firm might give away some of a good to boost consumption if that consumption yielded benefits apart from the immediate price received. For example, a firm might give away a durable good that complements a consumable (e.g., a "razor-blade" company might give away "razors"). The seller of a network good might give away goods to achieve a profitable scale. The operator of a two-sided platform might want to stimulate consumption on one side if that made the platform more attractive to participants on the other side (free emailed credits for Uber, Lyft, Venmo, and so on are commonplace).

The rest of the paper is organized as follows. Section 2 describes the empirical context for our study. Section 3 presents the experiment's design and the sample's construction. Section 4 reports the main experimental results. Section 5 parses the source of excess consumption on an empirical basis. Section 6 presents a simple model that can explain the non-zero MPC results even among habitual consumers. Section 7 concludes.

2 Empirical context

Our study is conducted in a large online labor market (Horton, 2010; Agrawal, Horton, Lacetera and Lyons, 2015; Horton, Kerr and Stanton, 2017). In such markets, employers hire users to perform tasks that can be done remotely, such as computer programming, graphic design, data entry, research, and writing. Each market differs in scope and focus, but platforms commonly provide ancillary services that include maintaining job listings, hosting user profile pages, arbitrating disputes, certifying user skills, and maintaining feedback systems (Filippas, Horton and Zeckhauser, 2020).

2.1 The cost of using the platform

Users can apply directly to jobs by using up an in-platform currency called "coins." Coins are sold through the platform and cost \$0.15 each. The number of coins required to apply to a specific job—the cost of an application—is determined by the platform using a proprietary formula. The formula only considers job-specific attributes, such as the anticipated job duration and earnings. During the experiment, the cost of applying for a job ranged from 1 to 6 coins. Employers may also invite users to apply to jobs; a potential employee pays no coins when applying in response (Filippas, Horton and Sorokin, 2022). New users seldom receive such invites.

Users receive 20 coins upon joining the platform, but have to purchase coins after this initial grant. Users can purchase up to 80 coins ($12) at one time. However, balances are not capped at 80 coins, and immediate repeat purchases are possible. Coins are placed in a non-interest-bearing account, cannot be converted back to cash, and expire one year after the purchase. These prices and rules remained constant during the period of our analyses.

3 Experiment

We conducted a randomized controlled experiment. Users were divided into two groups: treatment and control. Users who had applied to a job in a select number of technical categories were allocated to the experiment upon winning an interview with an employer. Treated users received an unexpected transfer of coins. Control group users did not receive a transfer, the status quo. Users allocated to the treatment received an initial transfer of 10 coins. That amount was about 6 days of "inventory" at the modal coin consumption rate. Recipients were also eligible for an additional 10-coin transfer if they won additional interviews, up to 50 coins in total. Treated users were notified of the transfer and the additional transfer opportunity. Users in the control group stayed in the status quo experience: no transfers, no incentives.

The allocation period began on March 10, 2020 and ended on April 6, 2020. A total of 8,721 users were engaged in the experiment. Of that total, 4,346 (49.83%) were allocated to the treatment group (T), and 4,375 (50.17%) to the control group (C). The experimental groups were well-balanced across several pre-experimental observables. Appendix A reports two-sided t-tests for various user-level attributes, as well the number of users allocated to the control and treatment cells over time. There was no evidence of systematic differences in the distributions of these attributes.

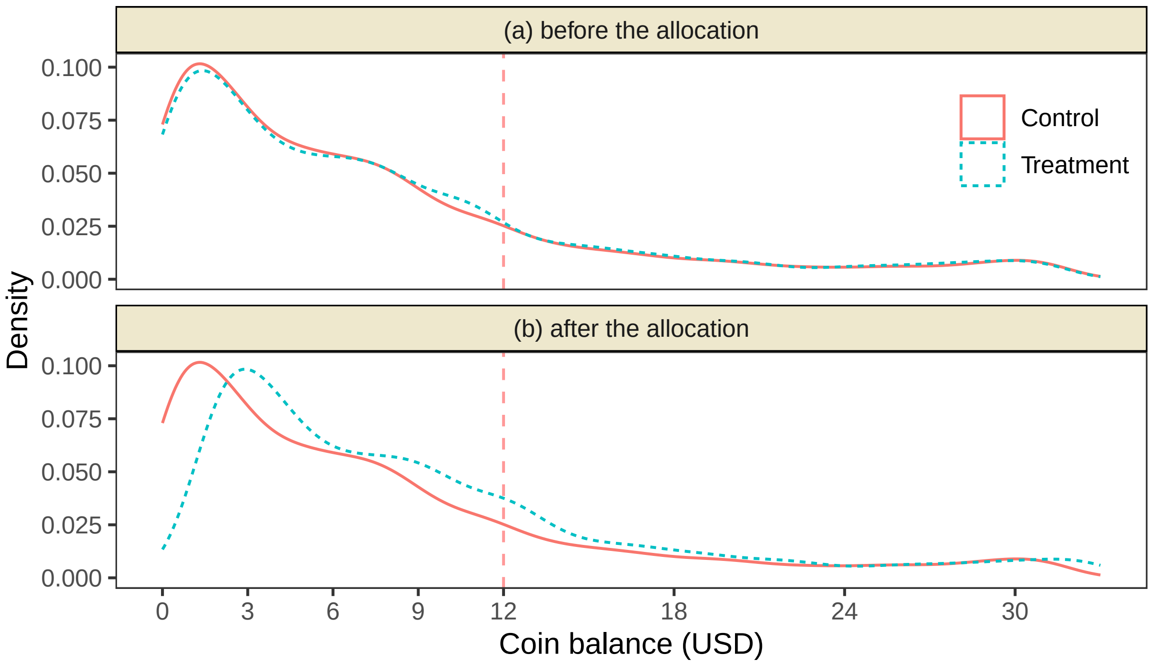
3.1 The treatment was delivered

All users in our data received the "correct" coin transfer, according to their treatment status. Figure 8 plots the kernel density estimate of the distribution of coin balances among users, first right before they were allocated and then right after.

The top image shows that the treatment and control groups were well-balanced on coin balances. The bottom image shows that the distribution of coin is shifted to the right for the treatment group. Visual inspection shows that shift to be about 10 coins, which is the size of the initial transfer. However, the shift is not mechanically exactly 10 coins: users could have purchased more coins, consumed coins, gotten refunds, and so on in the interim.

It is worth noting that only a small fraction of balances are zero or near zero. This is consistent with the treatment targeting users during a job search spell. Such users were unlikely to have fully spent down their balances or exited the platform.

Figure 1: Coin balances before and after allocation for treatment and control groups.



Notes: This figure shows the kernel density estimates of balances of treated and control users group immediately before and immediately after the allocation.

3.2 Sample construction and summary statistics

Our analyses employ both panel and cross-sectional data. To construct the panel, we collected observations on all users, and computed when each outcome was observed in days relative to the user's allocation in the experiment, with millisecond accuracy. We then divided that number by 7 and "floored" the result. As such, an application sent 6 hours after allocation takes place in period 0, an application sent 8 days after allocation takes place in period 1, and the allocation-triggering invitation occurred in period -1. Throughout the paper, we refer to period 0 as the "allocation period."

We keep 26 post-allocation periods and 26 pre-allocation periods for each user. Therefore, we observe each user's outcomes for about 6 months before and 6 months after they were allocated to the experiment. We fill in all missing user/period outcomes with 0. For example, if a user sent applications only in periods -1, 0, and 11, then we fill in periods –26 to -2, 2 to 10, and 12 to 25 with zeroes for the missing outcomes. Hence, the panel is unbalanced, but the missing observations are random and unrelated to treatment status.

Table 1 reports summary statistics for users in the control group, for the period before, and one period after the allocation to the experiment (periods -1 and 1). On the extensive margin, consuming coins is more prevalent than buying coins in all periods—for the same reason that eating is more common than grocery shopping on a given day.

As we would expect, coin consumption is higher in the week before the allocation, given that winning an interview—which triggers one's allocation to the experiment—necessarily follows a job application, and hence one's consumption of coins. Buying and consuming are declining on the extensive and intensive margins across the two periods. This suggests that the experiment "caught" users in a period of strong job search. Hence, the 96% consumption rate in period -1 is unsurprising. Job applications also decline across the two periods because users reduce their search intensity once they get hired. The weekly measure of hiring also shows that, on

Table 1: Summary statistics for control group users before and after the allocation period.

Period	Mean	StD	Median	Min	Max	N
(a) any coins consumed?						
week before allocation	0.96	0.20	1.00	0	1	4,375
week after allocation	0.57	0.50	1.00	0	1	4,375
(b) any coins purchased?						
week before allocation	0.27	0.45	0.00	0	1	4,375
week after allocation	0.21	0.41	0.00	0	1	4,375
(c) coins consumed						
week before allocation	29.77	43.18	18.00	0	986	4,375
week after allocation	19.38	41.37	4.00	0	535	4,375
(d) coins purchased						
week before allocation	19.98	45.01	0.00	0	900	4,375
week after allocation	16.17	40.71	0.00	0	560	4,375
(e) job applications						
week before allocation	7.74	10.19	5.00	0	212	4,375
week after allocation	5.28	10.27	2.00	0	121	4,375
(f) number of hires						
week before allocation	0.34	0.65	0.00	0	6	4,375
week after allocation	0.21	0.60	0.00	0	10	4,375

Notes: This table reports summary statistics for users in the control group using panel data. Outcomes are reported for the week before, during, and after the allocation to the experiment (periods -1, 0, and 1). The reported outcomes are (a) whether any coins were consumed, (b) whether any coins were purchased, (c) the amount of coins consumed, (d) the amount of coins bought, (e) the number of job applications, and (f) the number of hires. See Section 3.2 for more details on the panel's construction.

average, multiple weeks of job search are required to be hired.

4 Experimental results

By-period estimates of the by-period effect of the treatment are reported in Figure 2. In the pre-period, as expected, there is no evidence of an effect on any outcome. In the post-period, as panels (a) through (c) show, the treatment had the intended effects for the platform: Treated users sent more applications, won more interviews, and were hired more frequently. Those effects were concentrated in the first period after the transfer, though there is some evidence that the effects persisted.

Our primary interests are in the effects on consuming and purchasing of the good. As to purchases, the question is how greatly transfers crowd-out purchases. The empirical results on consuming and buying are reported in panels (d) and (e). They reveal a clear increase in coins spending (i.e., consumption) and a clear decrease in purchases.

4.1 Estimating the marginal propensity to consume

We define the MPC as the fraction of the transfer that a user consumes within the experimental period. Because the experimental transfer was small, we assume that the MPC does not depend on the size of the transfer. Let y_t^B and y_t^C be the coins bought and consumed by a user if the treatment group, y_c^B and y_c^C be the coins bought and consumed by a user in the control group, and A be the number of coins transferred. The consuming and buying outcomes for each user relate as follows:

$$y_t^C = y_c^C + A \times \text{MPC}$$
$$y_t^B = y_c^B - A \times (1 - \text{MPC}).$$

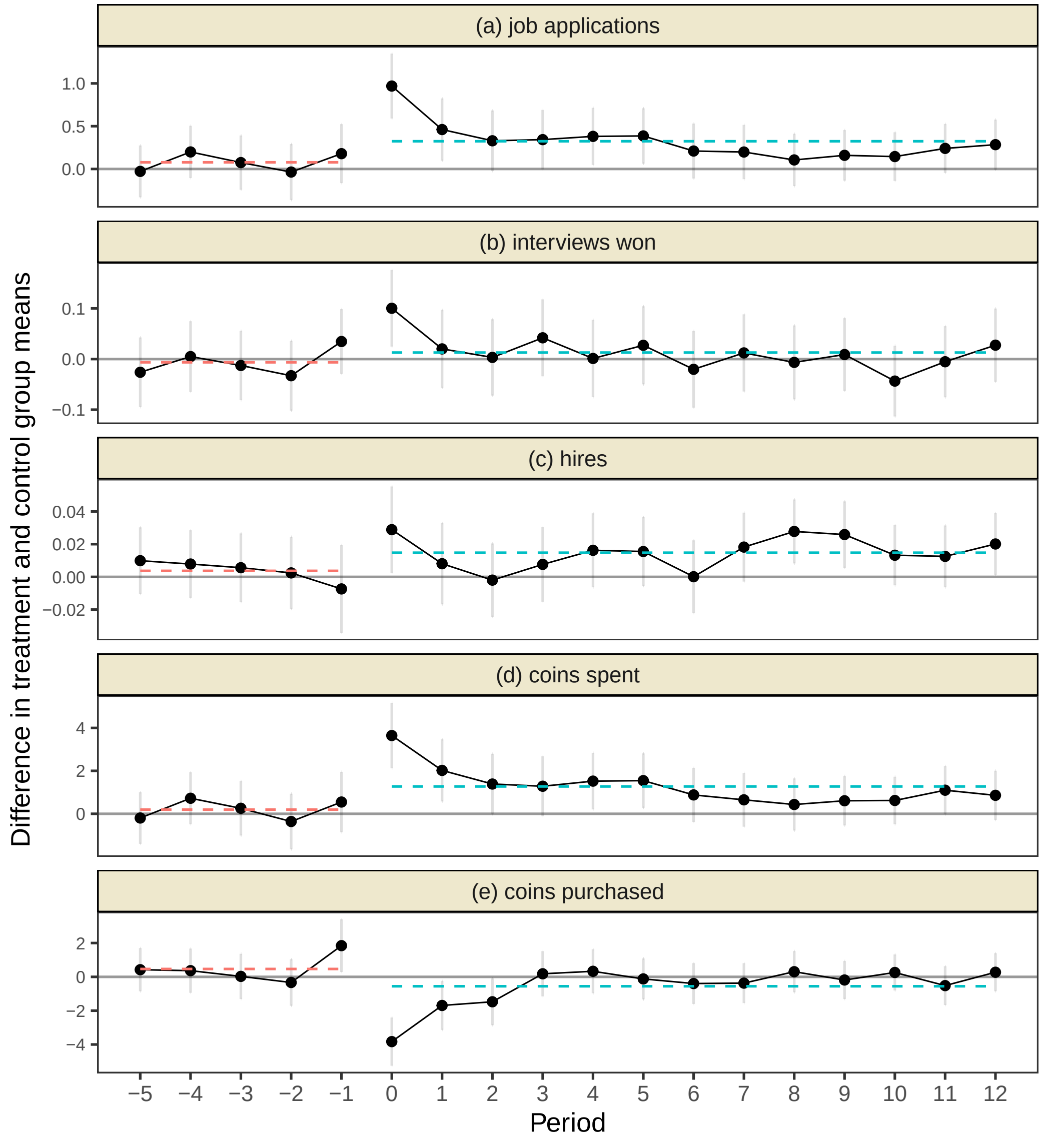
As users were assigned to either the treatment or the control group, we observe only one of the two potential outcomes for a user. Hence, analyses had to be conducted on a between-subjects basis. However, a simple linear regression of the buying and consuming outcomes on a treatment indicator allows us to obtain estimates for $\beta_B = y_t^B - y_c^B$, and $\beta_C = y_t^C - y_c^C$. The ratio of the two effects is

$$\frac{|y_t^C - y_c^C|}{|y_t^B - y_c^B|} = \frac{\text{MPC}}{1 - \text{MPC}}, \tag{1}$$

and the population MPC is equal to $1/(1 + |\beta_B|/\beta_C)$. Note that if the effect of the transfer on consumption is larger than the effect on buying, then the estimated MPC will be greater than 0.5.

Our estimation strategy assumes that the transfer has been acted upon in some way by the time we measure the buying and spending outcomes. That is, the transfer has increased

Figure 2: Panel estimates of the effects of the treatment.



Notes: This figure plots the differences in per-period user outcomes between the treatment and the control group. We plot point estimates of the mean difference for each period, along with a 95% confidence interval. The dashed orange line depicts the pre-allocation mean difference, and the dashed blue line depicts the post-assignment mean difference. The distribution of each dependent variable is winsorized at the 99% level. More details on the construction of the panel are provided in Section 3.2.

consumption, reduced buying, or done both. However, some users may have neither bought nor consumed coins after the transfer. For example, a user may have exited the platform before the transfer took place, and the transfer was not large enough to induce her to "restart." For these users, the transfer will "show up" as higher coin balances, but not as changes in buying and consuming coins. These users would have the same outcome had they received a zero-coins transfer. Note that A is removed from the estimation process of Equation 1, or equivalently, this "deflation" of A affects both the buying and the consuming estimates. As such, it does not affect our estimates, which is an advantage of our formulation.

One complication in any MPC exercise is defining the period of interest, as both consuming and buying take time. For this reason, we report results for increasing period lengths and then use the estimate once the results have seemingly stabilized. We first obtain estimates of the MPC using aggregate user outcomes using windows ranging from 1 to 12 periods post-allocation. We compute aggregate buying and consumption outcomes for each period length for each user. We then, simulate 1000 draws from the population of the experiment, selecting 5000 users with replacement for each of the 5000 draws. For each draw, we compute an MPC estimate using Equation 1.

Figure 3a plots the results of the bootstrap estimation process. Each point represents an estimate of the MPC for a different post-allocation window. The 95% confidence interval is plotted around those estimates. The MPC estimates range from 0.62 to 0.75, with a mean of 0.7 (indicated by the horizontal dashed line). Estimates appear to be stable by about week 7.

An alternate approach employs panel data to estimate the MPC via the treatment effect on per-period buying and consuming. The panel regression specification is

$$y_{it} = \beta \left(\textsc{Post}_t \times \textsc{Trt}_i \right) + \alpha_i + \delta_t + \epsilon_{it} \tag{2}$$

where y_{it} is some period-specific outcome, $\textsc{Post}_t$ indicates whether period is greater than or equal to 0, $\textsc{Trt}_i$ indicates that user i was allocated to the treatment group, α_i is a user specific fixed effect and δ_t is a period-specific effect. One advantage of this specification is that individual fixed effects absorb some of the individual-specific variation that is a nuisance.

Figure 3b plots the estimated MPCs using a pre-allocation window of 10 periods, and post-allocation windows ranging from 1 and 12 periods. The MPC estimates range from 0.47 in the second period to 0.66 at the end of the panel, with a mean of 0.59 (as indicated by the horizontal dashed line). Estimates also appear to be stable by about week 6 or 7.

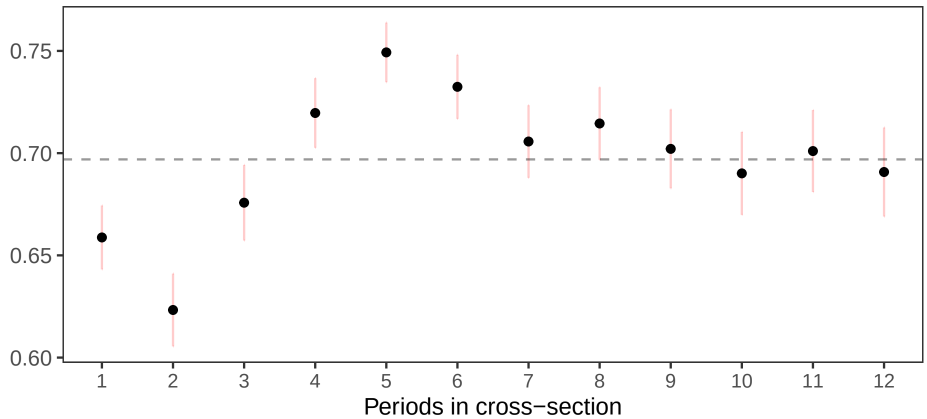
Together, the cross-sectional and panel estimates of the MPC show that transfers substantially increase consumption, and displace purchases but much less than one-for-one. While we do have a range of estimates for MPC, 0.60 is conservative and is the main estimate we use going forward when decomposing the sources of increased consumption.

5 Sources of the consumption increase

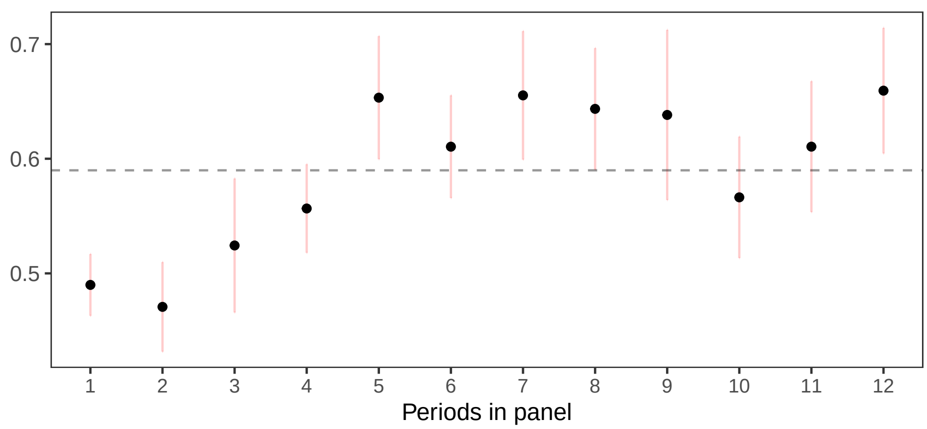
The treatment increased coin consumption well beyond what we would have expected if the targeted population were all Gs—active buyers and consumers. The first step to understanding where the increased consumption came from is to recognize that many recipients were not Gs. Some had *de facto* left the market (Es); others were about to (Fs).

Figure 3: Experimental estimates of the implied MPC.

(a) Estimates using cross-sectional data.



(b) Estimates using panel data



Notes: This figure plots estimates of the MPC using cross-sectional and panel data. Each point represents an estimate of the MPC for a different post-allocation window, and a 95% bootstrap confidence interval is plotted around it. For each panel, the dashed lines indicate the mean estimate across post-allocation windows. For more details on the estimation strategy, see Section 4.1.

It is useful first to sketch out the possible ways that users could respond to an unexpected coin transfer. Long ago Baumol (1952) and Tobin (1956) assessed how consumers should hold cash balances, given the transaction cost of securing cash, and the interest foregone when holding it. It was a classic inventory problem. Our problem is related. Individuals buy coins and then consume from their stockpile. There is no foregone interest, but there is a transaction cost of time to purchase coins. Following the standard framing of the inventory problem (and Baumol (1952)/Tobin (1956)), we initially posit that users have a constant rate of consumption. Unlike the standard framework, users in our setting may stop consuming coins at any time for reasons such as getting a job on or off the platform, deciding that the platform does not fit them well, leaving the workforce, and so on.

Figure 4 illustrates the possible patterns of a users buying or consuming coins past the time of allocation to the experiment. The downward-sloping portion of the curve indicates the user's consumption of coins when applying for jobs. The vertical segments indicate coin replenishments. The vertical line indicates the time of allocation to the experiment. Three scenarios are drawn.[3]

Panel 4a depicts the "Exit" scenario. Users in this scenario had already exited the market by the time of allocation.[4] The coin transfer might stimulate these E users to become active again, spending down the transfer and then returning to an exited status. When this occurs, we observe an MPC equal to 1.[5] E users who consume their transfers should show up empirically as an increase in the fraction of users consuming any coins in the post period.

In terms of buying coins, no additional purchases could be expected: if E users did not find replenishing coins worth it in the control group, then free coins should not make purchasing coins more likely. However, some form of "pump priming" could conceivably lead to purchases by Es receiving the coin transfer.

Panel 4b shows the "Fumes", "Fumes", scenario. In this scenario, users would consume but not buy additional coins, post-allocation. We do not know who these users are in the treatment group, but we observe who they are in in the control group.[6] The transfer should cause these users not merely to increase consumption, but also to extend the period of time they are actively consuming, as they are spending down a larger balance. F users have an MPC of 1, and do not contribute to an extensive margin effect on consumption.

Panel 4c depicts the "Gangbusters", G, scenario. In this scenario, the transfer has the potential to crowd-out purchases as it is directed to users who were going to consume and buy post-allocation. If treated Gs do not alter their consumption rate, they will simply defer their first post-transfer replenishment by the amount of time required to completely consume the transfer at their standard consumption rate. As such, we should observe an MPC equal to 0 for G.[7]

[3]The scenario where users buy coins but do not consume seems to be missing. This effectively did not happen in our data: only about 0.89% users in the control group exhibited this behavior. One explanation for this rare behavior is that the users got lucky with a job shortly after buying their coins.

[4]This scenario may seem implausible or exceedingly rare: why would a user who exited the market win an interview? In practice, this could happen, for example, because employers can choose to interview days and even weeks after an application was sent. Users have little incentive to self-report that they have exited.

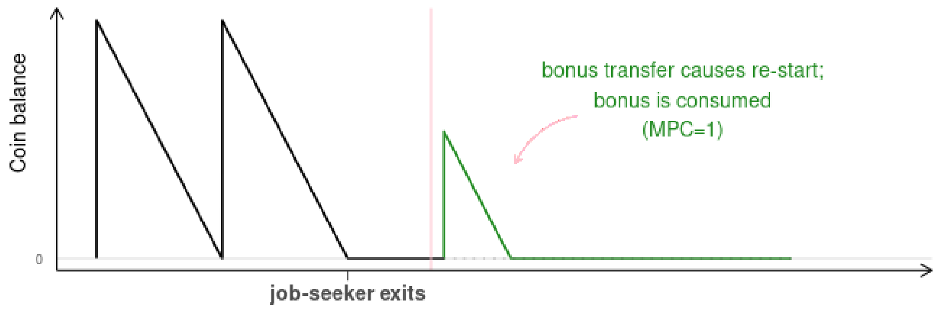
[5]It is worth noting that some treated E users will still not bother to consume. Of course, a user might be a "sleeping beauty" and return to use their allocated coins eventually, but presumably, few will do so. These users are equivalent to the user who receives the apple voucher but throws it in the trash. If coins were a physical good, this would be wasteful, but because of the coin properties described in Section 2, unused coins represent no real claim on the platform's resources.

[6]Note, we make no assumptions on users' planning *ex ante* knowledge; rather, we simply state that in the control counterfactual, these users would consume but not purchase coins.

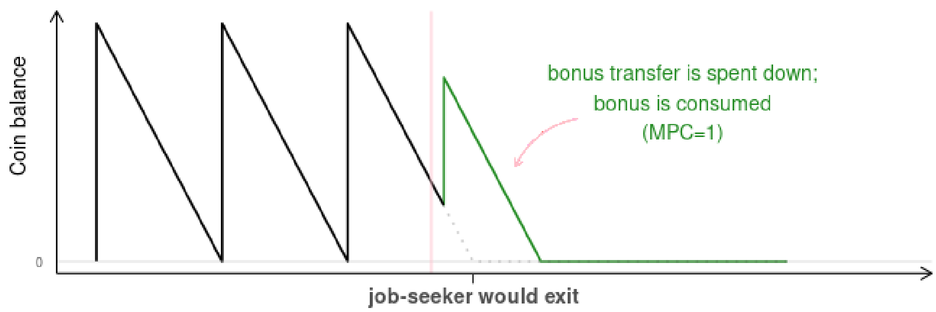
[7]With a fixed cost of replenishment in addition to the unit price, it is also possible that treated users will make fewer replenishments. If the transfer is sufficiently large, they might never replenish. This could potentially reduce consumption more than the transfer amount, thereby creating a negative MPC.

Figure 4: Possible effects of an in-kind transfer to individuals

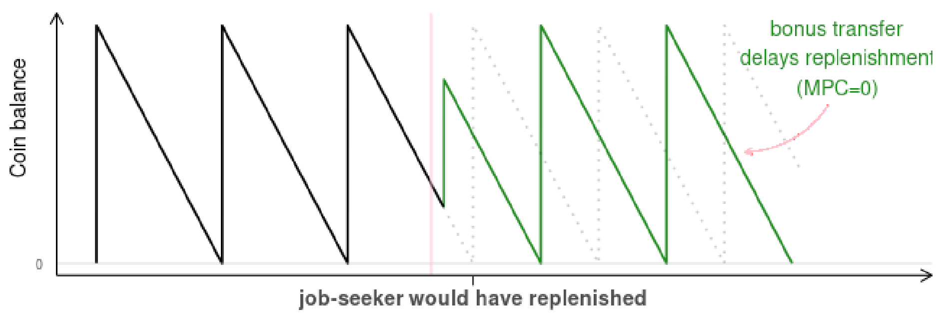
(a) The "Exit" scenario: already stopped consuming.



(b) The "Fumes" scenario: consuming but no planned buying.



(c) The "Gangbusters" scenario: consuming and planned buying.

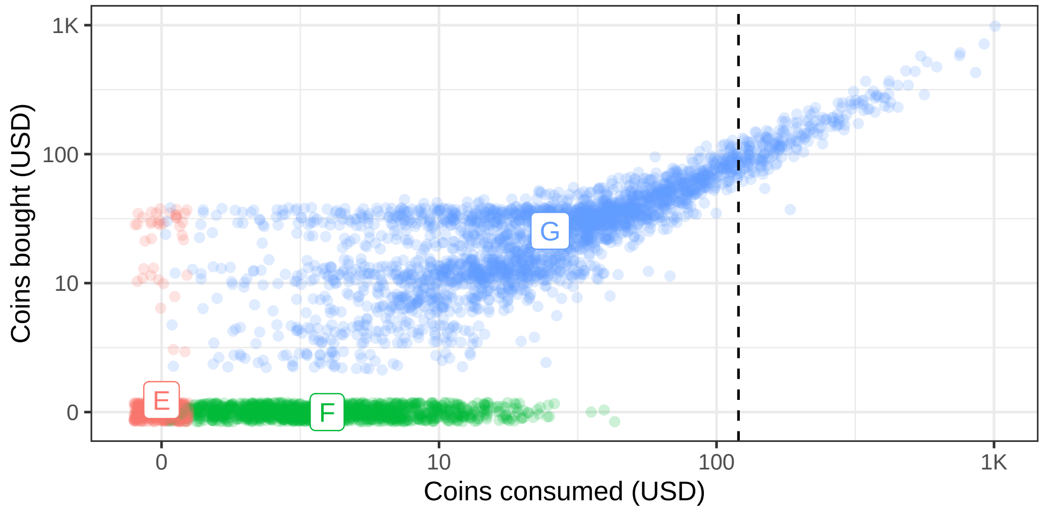


Notes: This figure illustrates the possible patterns for users who are willing to either purchase or consume coins past the time of allocation to the experiment. We assume that users follow a Baumol (1952)/Tobin (1956) framework where coins are bought and then consumed at a constant rate. The vertical red dashed line indicates the time of allocation to the experiment. The black and green solid lines indicate users's behavior before and after the coin transfer. The gray dashed line indicates the counterfactual behavior, had the transfer not taken place. There would be variability in the pace of decline, obviously, if the consumption rate was stochastic, for example in response to job opportunities as they arose. However, the qualitative features of the model, including the effects of transfers, would remain.

5.1 Identifying consumption types empirically

Using the control group, it is possible to classify users into our E, F, and G categories. Figure 5 shows a scatterplot of the coins bought and consumed by control group users. Each point represents a user's cumulative outcomes during the post-period. The points are "jittered" by adding a small random noise to prevent over-plotting. The dashed vertical line indicates the 95th percentile of the coin consumption distribution. The results are plotted on a logarithmic scale.

Figure 5: Coins bought and consumed by control group users.



Notes: This figure plots a scatterplot of the number coins consumed and bought by control group users. Each point represents a user's buying and consumption during the experimental period, with points jittered to prevent overplotting. The colors correspond to the user's type (see Section 5). The horizontal dashed line indicates the 95th coin consumption percentile. The results are plotted on a logarithmic scale.

Had we not "jittered" the data, the F-type users who consumed but did not buy would simply lay on a line. Note that the E-F-G framing neglects the possibility that a user could buy but not consume in the post-period. We do see a handful of Es at 0 on the x-axis but with positive values on the y-axis. However, such behavior is rare—only about 40 users' classified as Es bought coins in period -1, but consumed none in period 0.

Coin buying and consuming are strongly correlated in the G group, as would be expected. That was particularly true at the highest consumption levels. Indeed, only Gs engaged in high buying and high consumption. No user had a pre-existing balance large enough to sustain a high consumption rate without purchasing additional coins.

5.2 Effect of the treatment on consumption patterns

To examine how the treatment affected consumption patterns for Es, Fs, and Gs, we regress indicators for our three types on the treatment indicators. Table 3 reports these estimates.

The outcome in Column (1) is for Es. These users neither consumed nor purchased in the post-period. In the control group, 12.55% of users consumed no coins in the post-period. The treatment increased the fraction of users that consumed at least some coins by about 1.99%, roughly one-sixth of its original value.

Some of these Es might have consumed their transfers and then exited (the behavior of F types)—and perhaps some went on to both consume and purchase coins due to "pump priming" (the behavior of G types). However, on net, the treatment greatly increased membership in F and decreased it in G. If these transfers created any movement from E to G, that effect was likely swamped empirically by the effect of control G types deciding not to purchase coins, turning them into F types. In the control group, 60.14% of users both consumed and bought coins. In the treatment group, that percentage fell to 55.76%, basically losing one in seven of the original consumers and buyers.

Table 3: Cross-sectional estimates of the treatement effects on coin consuming and buying.

	Dependent variable:				
	E	F	G	coins spent	coins purchased
	(1)	(2)	(3)	(4)	(5)
Control	0.125***	0.273***	0.601***	191.992***	158.073***
	(0.005)	(0.007)	(0.007)	(6.166)	(4.726)
Treatment	−0.020**	0.064***	−0.044***	24.985*	−2.207
	(0.007)	(0.010)	(0.011)	(10.345)	(7.716)
Observations	8,721	8,721	8,721	8,721	8,721
R^2	0.001	0.005	0.002	0.001	0.00001
Adjusted R^2	0.001	0.005	0.002	0.001	−0.0001

Notes: This table reports regressions where the dependent variables are (i) whether any coins were consumed, (ii) whether any coins were bought, (iii) the number of coins consumed, and (iv) the number of coins bought. The dependent variable is a treatment indicator. Significance indicators: $p \leq 0.1 : \ddagger$, $p \leq 0.05 : *$, $p \leq 0.01 : **$, and $p \leq .001 : ***$.

The transfer representing the treatment reduced the size of the E and G groups and increased the number of Fs by the amount that the Es and Gs lost. That outcome was expected. Quite simply, more users consumed some coins without purchasing (turning Es into Fs), and some users consumed but reduced purchases (turning Gs into Fs).

5.3 Adding-up estimate of MPC for Gangbusters

Some consuming users who would have purchased in the post-period (making them Gs) chose not to purchase at all once given their transfers (making them Fs). Maintaining the same assumptions about MPCs and group fractions, we obtain an MPC estimate of 0.44 based purely on movements between groups. These group-based MPC estimates are substantially smaller in magnitude than the direct estimates that we estimated in Section 4.1. Because the type-based estimates assume that E- and F-type users have an MPC equal to one This MPC of 1 is a conservative assumption if seeking the minimum MPC for Gs. It follows that Gs had to have a positive MPC for this to occur.

5.4 Additional evidence for higher consumption for Gs

The comparison between type- and consumption-based estimates of MPC shows that G-type users increased their consumption. Assuming, consistent with traditional inventory models and Baumol/Tobin, that for active users and purchasers the size of one's stockpile does not affect one's consumption, the Gs "should" have an MPC of 0. Their significantly positive MPC is surprising. Thus, we present two additional pieces of evidence for this result.

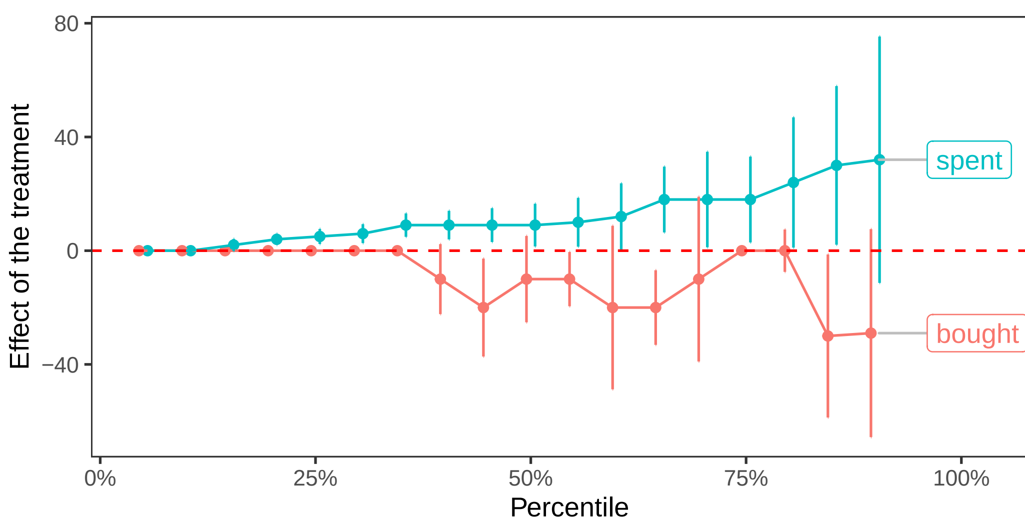
5.4.1 Quantile treatment effects

Figure 6 reports quantile treatment effect estimates on coin buying and consumption. For each percentile estimate, we report a 95% confidence interval using bootstrapped standard errors. The estimated effects on spending and increasing across a range of percentiles. In contrast, the estimated effects on buying are 0 for the bottom half of users, but turn negative past the 50th percentile.

As Table 3 showed, nearly half of the job-seekers in the control group bought no coins post allocation. As such, there can be no effect on total buying at low percentiles.

Importantly, consumption increases, even at the highest percentiles. That is obviously inconsistent with an MPC of 0 for G users. The MPC for Gs must be greater than 0.

Figure 6: Estimates of the treatment effect on buying and consumption by percentile.



Notes: This figure reports estimates of the treatment effects on coin consumption and purchasing by percentile. We compute cumulative buying and spending outcomes for each user using a cross-section that covers the experimental period (periods 0 to 12 in our panel). For each estimate, we report a 95% confidence interval using bootstrapped standard errors.

6 Explaining the positive MPC for habitual buyers

The experiment demonstrated that many users who were actively consuming and buying—going Gangbusters—still have a non-zero MPC from the transfer. On the face of it, it would seem that an in-kind transfer of this good would have no effect on net consumption. If users are already consuming at their preferred rate of consumption, why would a transfer impact that rate?

The users were already participants on the platform; thus, coins were not an experience good (Bawa and Shoemaker, 2004; Narasimhan, 1984). Furthermore, it is not the kind of good where the stock of past consumption might increase future marginal utility of consumption, as say with classical music (Becker and Murphy, 1988). If anything, users with more on-platform experience typically find it easier to get more work (Pallais, 2013). If so, the future demand for coins a diminishing function of past consumption. Ruling out these two explanations, a behavioral explanation might leap to the fore. For example, "windfall" coins might be valued less and thus spent more freely (Thaler, 1990; Milkman and Beshears, 2009; Hastings and Shapiro, 2013). However, as we show below, quite apart from behavioral explanations, a positive MPC out of a transfer may be sensible even among those habitually buying and consuming.

The related Baumol (1952)/Tobin (1956) model shows a "saw tooth" pattern for consumption and replacement. Consumption - in their context, the use of cash, proceeds at a constant rate. There is then periodic replacement.

Our model differs from traditional inventory models, and Baumol (1952)/Tobin (1956) by assuming that the size of one's stock affects the pace of one's spending. In particular, more stock induces faster spending. This turns the straight edges of a saw tooth pattern into curved teeth as shown in Figure 6.5.

If consumption does follow this pattern, then any boost to the stock will raise the MPC, since the slope of the curve is steeper at higher stock levels. What factors motivate faster spending at higher stock? The answer is that users face a risk of no longer wanting to use the site, say if they get a long-term or permanent job, or fall ill. Their inventory will then be worthless. The greater stock, the longer until it gets used up. The increased time till stock exhaustion, the more likely that coins on hand will not be needed. We first show this fact in our data. Even considering only jobs secured on this site, getting hired strongly decreases demand for coins. We turn now to develop a model based on this insight. We then show empirically that, consistent with this prediction, the rate of consumption does indeed decline as users' balances get closer to 0.

6.1 Coin consumption and replenishment after a demand shock

In our empirical setting, by contrast, job search comes in bunches: users actively apply to jobs for a spell, and then drop away. This matters practically, as it means that a user might find coins in his stock to be worthless, depending on changed circumstances such as deciding to exit or getting a job. This risk of worthlessness should and does affect the decision-making about balances and consumption rates. Before showing this formally, we first establish the episodic nature of job search and how it in turn affects the demand for coins.

Empirically, individuals searching for a job one week are quite likely to be searching as well for a job the next week. Hence, we would expect strong week-to-week auto-correlation in users' consumption. Getting hired should interrupt this process, however. Measures of job search will fall, perhaps to 0, in subsequent time periods. In our context, that would reduce both coin consumption and coin purchases in those time periods.

We explore this point empirically by examining week-to-week dynamics. The goal is to offer reduced-form evidence for the basic relationship: getting hired reduces the need for coins. Let y_{it} be user i's purchase or consumption of coins in period t. We assume the specification

$$y_{it} = \beta_1 y^C_{i(t-1)} + \beta_2 y^B_{i(t-1)} + \beta_3 h_{i(t-1)} + \alpha_i + \delta_t + \epsilon_{it},$$

where y^C is the number of coins consumed, y^B is the number of coins bought, h_i indicates the individual is hired, α_i is an individual fixed effect, and δ_t is a period fixed effect.

Table 4 reports estimates from this specification. Column (1), assesses the number of coins bought each week. Users who consumed many coins in the previous period need to buy new ones to replenish their balance. As expected, users who bought many coins in the previous period are less likely to buy more. However, the coefficient on past buying is much smaller in absolute magnitude than the coefficient on past consumption. That is because large previous purchases are still being consumed down. Getting hired has a large negative effect on coin purchases in the relatively near-term future; coins are not needed.

In Column (2), the outcome is the number of coins consumed each week. We observe strong auto-correlation and a positive and statistically significant coefficient on both the number of coins consumed and the number of coins bought in the previous period: users who bought many coins in the previous period are consuming them down in the next period, and users who consumed many coins in the previous period are still consuming many coins in the next period. This highlights the episodic nature of job search on the platform. The effect on getting hired, however, is sharply negative, as we would expect. Job search plummets in value when one gets hired. This reduces consumption in the next period.

6.2 A traditional inventory model with transfers

Suppose a user chooses to spend an instantaneous flow r on coins and gets a flow utility $v(r)$, with $v'(\cdot) > 0$ and $v''(\cdot) < 0$. Value is concave either because users exhaust their best alternatives, or because they have limited capacity to take on more work. The coins have a unit price p.

Table 4: The dynamics of coin buying and consuming, and the effect of getting hired

	Dependent variable:	
	Coins bought (USD)	Coins consumed (USD)
	(1)	(2)
$y^C_{i(t-1)}$, lag consumed	0.520^{***}	0.473^{***}
	(0.020)	(0.021)
$y^B_{i(t-1)}$, lag bought	-0.166^{***}	0.042^{***}
	(0.011)	(0.012)
$h_{i(t-1)}$, lag hired	-1.391^{***}	-2.099^{***}
	(0.404)	(0.392)
Worker FE	Y	Y
Period FE	Y	Y
Observations	148,751	148,751
R^2	0.451	0.636
Adjusted R^2	0.434	0.624

Notes: This table reports regressions where the dependent variable is the number of coins bought (column 1) and the number of coins consumed (column 2) by users during the entire period of our analysis. The independent variables are the number of coins consumed in the previous period, the number of coins bought in the previous period, and an indicator variable for whether the user was hired in the previous period. Standard errors are clustered at the user level, and the sample includes users allocated to both the treatment and the control groups. Significance indicators: $p \le 0.1 : \ddagger$, $p \le 0.05 : *$, $p \le 0.01 : **$, and $p \le .001 : ***$.

Assume that users have to hold a balance of coins that they need to replenish, at a fixed cost per replenishment, C. They have to pick some replenishment cycle, purchasing B coins at $t = 0$ for a price pB, then consuming them down at a rate $r(t)$ until they reach $t = T$ and the balance has fallen to zero, at which point they replenish. The optimization problem is to choose T and $r(t)$ to maximize average flow utility, or

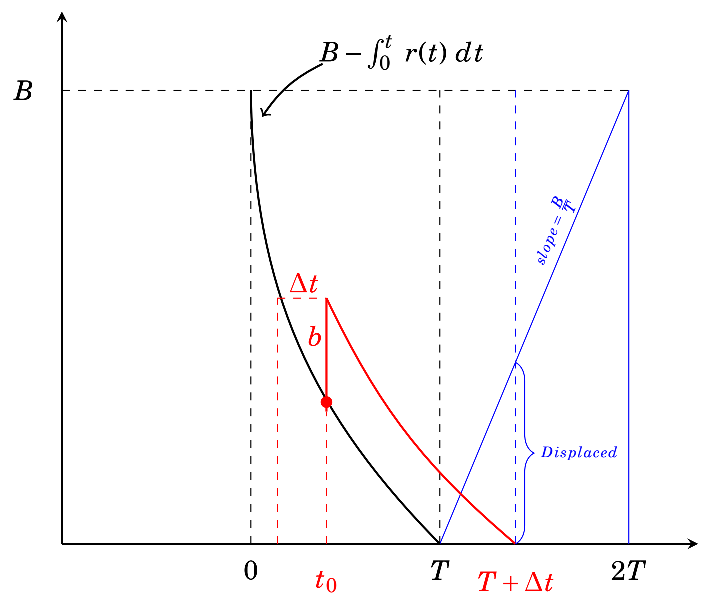
$$U^* = \arg\max_{r(t),T} \left(\int_0^T [v(r(t)) - pr(t)]dt - C \right) / T,$$

subject to the constraint that $\int_0^T r(t)dt = B$.

Figure 7 shows the one-period consumption behavior of a job-seeker. At time $t = 0$, the job-seeker starts with a balance, B. They draw it down at a rate $r(t)$ until it is completely exhausted at time $t = T$. At T, they replenish.

Note that if holding had no cost, there was zero time preference and no liquidity constraints, and replenishment incurred a cost, the optimal solution would be to simply buy a lifetime supply of coins and consume them down at a rate $v'(r) = p$ forever, thereby avoiding all costs but the purchase at the start. However, we observe finite balances everywhere. Indeed fewer than 2% of users at a point in time have more than XX coins in their stock. Hence, there

Figure 7: Effects of an unexpected transfer on a consumption plan



must be some cost to holding a balance. Positive time preference is an implausible explanation, given that 90% of coins purchased are spent within YY days. Liquidity constraints are no more plausible. XX coins, the coin holdings of the 98th percentile worker, cost only \$XX x \$0.15. We posit a different explanation, which we label inventory risk. Particularly when the stock in inventory is large, coins purchased might prove to be of zero value. Assume the job-seeker might exogenously no longer need to consume coins, as might happen if they get hired on or off the platform, or they simply decide to exit the platform. When their consumption stops, their balance on hand is lost.

We explore this phenomenon in the simplest case of a constant stopping hazard λ. The distribution of stopping times, t, has pdf $f(t)$ and cdf $F(t)$. Note that if there was a risk to purchasing and thereby holding a balance, but it cost nothing to replenish ($C = 0$), the job-seeker would just trickle in coins at a continuous rate to support $v'(r) = p$, thereby avoiding any inventory risk.

The probability that the job-search is *not* "shut off" in the next dt unit of time is $\frac{f(t)}{1-F(t)} dt = 1 - \lambda dt$. As such, we can write the intertemporal optimality condition as

$$\frac{v'(r(t))}{v'(r(t) + r'(t)dt)} = 1 - \lambda dt.$$

Assume a CARA utility function, and so $v''/v' = -\alpha$. We can obtain an expression for the consumption rate as

$$\frac{v'}{v' + r'(t)dtv''} = 1 - \lambda dt$$
$$r(t) = -\frac{\lambda}{\alpha}t + k_0. \tag{3}$$

With $\lambda/\alpha > 0$, $r'(t) < 0$. This implies that a rational job-seeker consumes at the highest rate immediately after replenishment, and then gradually reduces that rate until the balance is consumed at time T. They then replenish.

Note that α only matters when $\lambda > 0$. The α term tells us how costly it is to increase consumption, as it captures is the convexity of the utility function. When α is high, all else equal, the job-seeker would like to consume more slowly because of the greater foregone utility.

From Equation 3, we know the consumption rate declines over time. To pin down the rate, we still need to find the constant, k_0. When there is no inventory at risk (i.e., the job-seeker has a balance of 0), the job-seeker wants to consume at their preferred rate, such that the marginal utility is p. This gives a boundary condition, $v'(r(T)) = p$ that allows us to solve for k_0 in terms of T. This leaves T as the only unknown.

To actually solve for T^*, we can consider the optimal replenishment amount, B, such that a marginal change in B leaves the average flow utility unchanged. At the optimum choice of T^*, the marginal utility at $t = 0$ from consumption equals the average utility with the costs included:

$$u(r(0)) - pr(0) = \frac{\int_0^T u(r(t)) - pr(t))dt - C}{T^*}.$$

With the user's problem and solution characterized, we now consider what happens when they get an unexpected transfer while implementing their solution, as happened in the experiment.

Assume the job-seeker is actively buying and consuming and plans to continue to do so into the future. The job-seeker receives an unexpected transfer of size b at t_0. Let the time it takes to consume down a transfer of size b be Δt. Note that we already "know" how a job-seeker expecting no future transfer consumes at this new balance. This precise balance occurred Δt ago. As such,

$$\int_{t_0 - \Delta t}^{t_0} r(\tau)d\tau = b. \tag{4}$$

Note graphically, it is as if we take the curve from $r(t - \Delta t)$ and shift it forward. The forward shift delays replenishment by Δt. Even though the consumption rate varies over time, a delay of time in *purchasing* simply displaces Δt of the average buying rate. Hence, the transfer reduces the number of coins purchased by $\Delta t \frac{B}{T}$. The marginal propensity to consume out of the transfer is

$$\text{MPC} = 1 - \frac{B}{T}\frac{\Delta t}{b}. \tag{5}$$

This Δt term depends on both b and t_0, the time—and hence balance—once the transfer arrives. Note that if $r(t)$ was instead a constant, as in the Baumol (1952)/Tobin (1956) model of cash demand, $r(t) = B/T$, so MPC $= 0$ i.e., a transfer would impact consumption not at all.

Recall from Table 6 that in the G group, in the treatment, the consumption *rate* increased by about 25% immediately after the transfer. If we let $x = B/T$, then by Equation 5, MPC $= 1 - x/(1/1.25x) \approx 0.2$. This is not far from the 0.17 MPC we estimated for this group, which accords with the overall MPC of 0.6. This does not prove that the rational model explains the increase—any increase in MPC in the G group would lead to a higher consumption rate. But it does suggest that the approach leading to Equation 5 could be sensible.

6.3 Direct evidence on balance-contingent consumption rates

With the model of decision-making sketched out above, it will be optimal for users to spend down coins quickly when their balances are high, but then, [slacken][taper] their spend rate as the balance moves lower. To see whether practice meets prescription, we turn to the data. To make this assessment, we need to measure the time between job applications, which we label an interval. Our model predicts that as the number of applications sent thus far in an interval rises, the pace of application will slow.

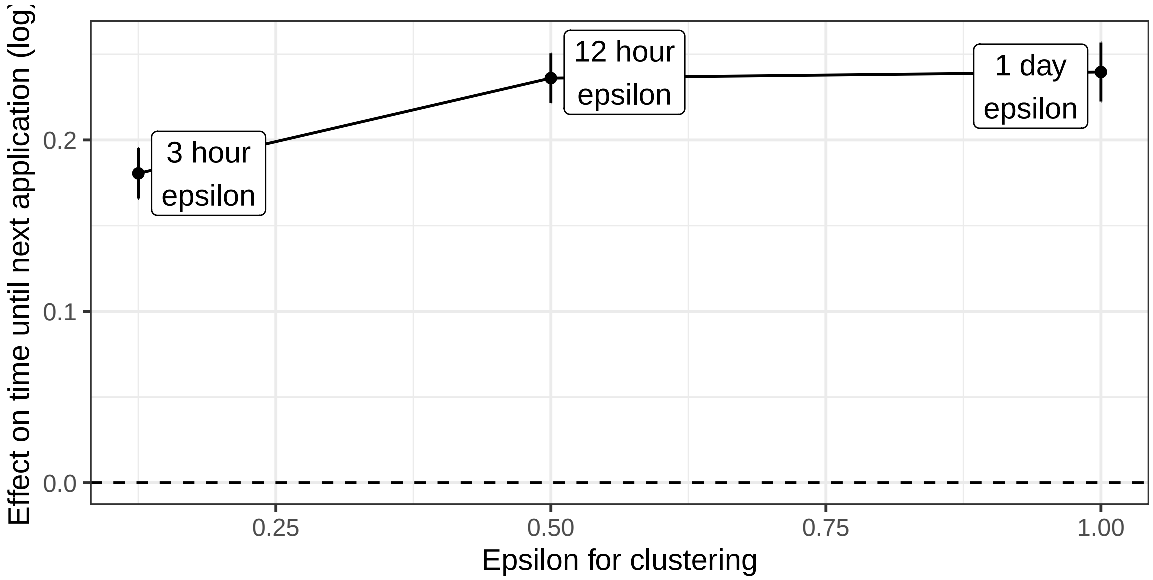
Job search happens in "spells" and we need some way to identify those spells. Otherwise, we would have large times between applications that simply reflect the start and end of spells rather than the job-seeker slowing down or speeding up consumption rates.

One approach is to cluster together job applications such that all jobs in a cluster are less than ϵ distance apart from each other. Then, within those clusters, we can compute differences. Suppose that job-seeker i has a cluster $k(i)$. They send applications $j = 1, 2, \ldots J_{ik}$ Let t_{ikj} be the time of application j by user i to cluster $k(i)$. Dropping subscripts, let $\Delta_j = t_{j+1} - t_j$ be the difference in times between a user's j and $j+1$ applications. With this difference computed, we can then estimate

$$\Delta t_{ik} = \beta \left(\frac{j}{J_k}\right) + \gamma_{ik} + \eta_i \tag{6}$$

where γ_{ik} is a job-seeker with a cluster-specific fixed effect and j/J is the quasi-percentile rank of that particular application. If $\hat{\beta} > 0$, it implies that on average, coins are spent more slowly as the balance gets closer to zero. Figure 8 shows the estimates of β computed with various values of ϵ. Standard errors are clustered with clusters.

Figure 8: Association between consumption rate and coins balance



Notes: This shows the relationship between a job application's position in a "spell" and the time until the next application.

If we take the 1-day epsilon, the point estimates show that the time between the last applications in a spell is about 20% longer than the first in a spell. The effect is highly significant. The magnitude is also ball-park consistent with our finding that the transfer increased the consumption rate by 25% in the G-group if we think of the transfer as shifting users from a low-to-high balance rate of consumption (recall the modal coins balance was around just 3). The choices of ϵ seem to matter somewhat. That is not surprising given that a smaller ϵ is more likely to split up true spells into tinier pieces, reducing differences between these pseudo-starts and ends. Although not causal, the regression sketches descriptively a declining rate of consumption. Its curvature supports the hypothesis that transfers for Gs have a significant MPC.

6.4 Further implications of the model

The MPC of a transfer is not merely positive, but it is increasing in size. If we differentiate Equation 4 with respect to the transfer amount, b, we get

$$\frac{d\Delta t}{db} = \frac{1}{r(t_0 - \Delta t)} \geq 0, \tag{7}$$

and so $\frac{dMPC}{db} > 0$ if $\Delta t > b\frac{d\Delta t}{db}$. Using Equation 7 and using the fact that $r(\cdot)$ is declining in t, we can write this condition as

$$
\begin{aligned}
r(t_0 - \Delta t)\Delta t &> \bar{r}\Delta t \\
&> b.
\end{aligned}
$$

where $\bar{r}$ is the average consumption rate. Note the implication is not that a larger transfer leads to more consumption, though that is true. Rather, that the lesson is that the proportion of a transfer consumed increases with the size of the transfer. In short, the larger the transfer, the greater the MPC. However, the user gets decreasing returns to utility, given that $v(\cdot)$ is concave.

The MPC of a transfer is increasing in the balance held by the recipient. A higher balance means a lower t_0, or the time when the transfer was received. The effect on the marginal propensity to consume from a lower balance (higher t_0) is

$$\frac{dMPC}{dt_0} = -\frac{B}{bT}\frac{d\Delta t}{dt_0}.$$

If we differentiate Equation 4 with respect to the arrival time of the transfer, t_0, we get

$$\frac{d\Delta t}{dt_0} = 1 - \frac{r(t_0)}{r(t_0 - \Delta t)} > 0.$$

and so $\frac{dMPC}{dt_0} < 0$, or a lower balance has a lower MPC.

$$\frac{d^2\Delta t}{db^2} = \frac{r'(t_0 - \Delta t)}{r(t_0 - \Delta t)^2}\frac{d\Delta t}{db} \leq 0.$$

Note that if the slope were constant, $r(t_0) = r(t_0 - \Delta t)$, then the location of t_0 would not matter—the Δt would be a constant, which would give us the flat "saw tooth" and MPC = 0.

7 Conclusion

The main conclusion of the experiment is that in-kind transfers can bolster even users who are already buying and consuming. Despite their ability to stimulate consumption, it is clear that at least some of these transfers simply displace planned consumption. Obviously, if transfers are targeted based on past behavior, that can improve their bang for the buck. That bang comes from increased consumption and decreased crowding out of purchases.

In a range of economic development settings, free goods have a real cost. Consequently, giving goods to low-value users may present a poor use of resources. In our case, real resource coins that are transferred but not used are not a resource cost; their production cost is 0. Redeemable vouchers for some good might be a better strategy, as redeeming the voucher can be enough of an ordeal to foster better targeting (Nichols and Zeckhauser, 1982).

Consider that giving someone $100 in ride credits for Uber that can be used forever is unlikely to change much for habitual Uber users. In contrast, giving them $100 in credits that have to be spent in the next week, and can only be used for a trip to, say, a specific retail mall, is less likely to be used, but is more likely to increase net consumption. The more likely a participant is to find the subsidy useful, the less desirable they are as a target, positing that increased consumption is the goal. A managerial implication is that for any free or even heavily discounted good, there are probably numerous ways to increase MPC through combinations of targeting and variations in expiry date or other constraints.

The model makes it clear that there is a trade-off between MPC and user utility. Creating "uncomfortable" balances, as the name implies, imposes some individual utility loss relative to what a social planner unconcerned with the externalities would prefer. While uncomfortable inventory examples might seem particular to this context, inventory plan disturbances leading to increased consumption rates but with an attendant utility loss happen frequently even in ordinary life. Consider the gift card that soon expires; the drink that cannot be taken through airport security; the vegetables that are at risk to wilt. The challenge we face in light of some "excess" inventory is reducing our stock as best we can, given the time constraints.

The acceleration in consumption almost certainly imposes some utility loss relative to the same amount of consumption at the user's preferred rate of consumption. All else equal, we would rather not consume $100 of pizza and arcade games in the next two days, before our Chuck E Cheese voucher expires; we would rather enjoy our coffee leisurely, not guzzle it down over an airport trashcan; we would rather not eat Brussels sprouts for successive dinners before they spoil in our refrigerator. As a more serious example, Pollack and Zeckhauser (1996) explain in theory, and Liebman and Mahoney (2017) provide evidence, there is a considerable degree of end-of-year, likely wasteful government expenditure due to expiring funds.

Of course, the question of how to spend out of a stock of a resource depends on one's expectations, and in this case, one's expectations concerning future transfers. This study left to future experiments examining how such expectations are created and their impacts. The transfers here were windfalls from the blue. The platform had never previously made nor publicly discussed transfers like these. Were such transfers to become commonplace, on this platform or on different platforms, recipients would surely alter their consumption patterns. If participants see themselves as playing a game with the platform, with transfers contingent on behaviors, strategic behaviors would surely be implemented.

A planner seeking to create uncomfortable balances as a consumption-boosting measure would do some seemingly odd things. For example, it would give transfers to people who were already near their preferred maximum balance—say giving coins to people who just bought coins—or give a much larger transfer to one person rather than two smaller transfers to two people. More generally, it would favor a narrow definition of the "good"—to create the declining marginal flow utility. It would make the transfer expire, ideally in a hard-to-predict way (e.g., until a certain level of transfers are used across the firm)—to create the hazard that the transfer will become useless. Giving vouchers for transfers to people who appear to have a low valuation of the good has an advantage. Such recipients have a low probability of exercising the voucher. Accordingly, they have little expected claim on the planner/platform's resources—but when they are acted upon, the consumption is likely additive. Some of these ideas are prescriptive, but they also have positive implications describing some of the marketing/promotional behavior. The implications would be particularly germane in two-sided marketplaces, where the platform benefits greatly from increased activity. The bottom line of this study is that transfers of goods can increase consumption. In a significant field experiment, they did so. They even had this consumption-boosting effect on users who were actively consuming and purchasing the good.

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A Additional details and results for the experiment

A.1 Internal Validity

One way to assess whether the randomized assignment was performed correctly is to try to detect systematic differences in observable pre-treatment characteristics between users assigned to the control and the treatment groups. In Table 5, we perform two-sided t-tests for various job-post level attribute averages in the time preceding the experiment which is covered in the panel (for more details, see Section 3.2). We find no evidence of systematic differences between the experimental groups.

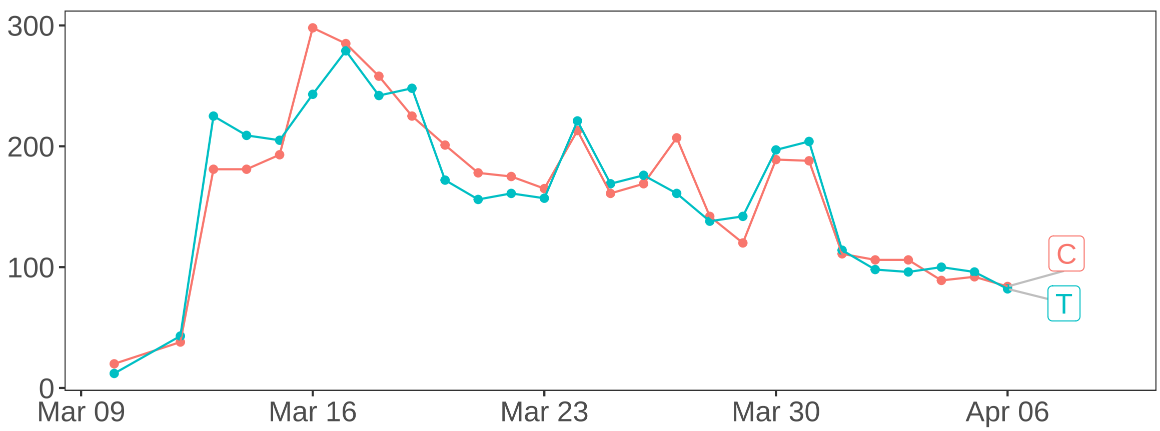
Another way to assess the correctness of the randomize assignment is to plot the raw number of users allocated to the control and the treatment arm(s) each day of the allocation period. This also allows us to inspect visually the intensity of allocations over time. Figure 9 plots the number of employers allocated to the experimental cells over time. The allocation period began on March 10, 2020 and ended on April 6, 2020. We find no evidence of systematic differences in the number of users allocated to the control and treatment cells.

Table 5: Balance test table

	Control mean $\overline{X}_{CTL}$	Treatment mean $\overline{X}_{T}$	p-value
User characteristics			
email length	22.53	22.54	0.906
US-based	0.13	0.14	0.092
UK-based	0.02	0.02	0.513
tenure	3.08	3.14	0.347
Pre-experiment Outcomes			
job applications	26.85	27.14	0.768
interviews won	5.69	5.62	0.723
number of hires	1.04	1.07	0.429
coins spent	98.07	98.81	0.851
coins purchased	82.58	83.48	0.789
Observation counts	4,375	4,346	0.756

Notes: This table reports averages and p-values of two-sided t-tests for various pre-treatment observables, for users assigned to the control group and to the treatment group. The reported attributes are (i) the number of characters in the user's registration email, (ii) whether the user is based in the United States of America, and (iii) whether the user is based in the United Kingdom. (iv) the number of coins the user purchased, and (v) the number of job applications the user placed. Outcomes (iv) - (v) are cumulative outcomes computed for the period of January 1, 2020 to March 1, 2020.

Figure 9: Users allocated to the control and treatment groups over time



Notes: This figure plots the number of users allocated to the control and treatment groups each day of the allocation period. The allocation period began on March 10, 2020 and ended on April 6, 2020.

A.1.1 Consumption rates

We can also so where the excess consumption came from by examining coin consumption rates directly. This may seem unnecessary given the quantile regression results, but as we will show later, differences in consumption rates have an interesting interpretation in light of the model we develop.

We define the consumption rate as per-period average consumption up to the last period of consumption in the experimental period. For users who consumed no coins, that rate is zero.

Table 6 reports regressions where the outcome is the consumption rate and the independent variable is a treatment indicator. Column (1) covers the full sample (i.e., E, F and G). We can see that that treated users overall had a 10.41% higher coin consumption rate than control users.

Possibly, all of that increase came solely from the treatment turning Es to Fs, with this compositional change explaining the observed treatment effect. E.g., perhaps those E-to-F users had a very rapid consumption rate. However, Column (2) shows that this is not the case. For this regression, we drop E users from our sample (from both treatment and control). This if, of course, a selected sample (we know the treatment affects group composition) but if the treatment was due purely to selection, the effect should disappear. The treatment effect on consumption rates diminishes by about one quarter to about 7.95%.

Table 6: Cross-sectional estimates of consumption rates

	Dependent variable:			
	Consumption rate			
	(1)	(2)	(3)	(4)
Treatment	1.674**	1.463*	1.975***	3.110***
	(0.572)	(0.630)	(0.379)	(0.885)
Constant	16.085***	18.393***	5.347***	24.318***
	(0.404)	(0.447)	(0.281)	(0.613)
Population	E+F+G	F+G	F	G
Observations	8,721	7,713	2,659	5,054
R^2	0.001	0.001	0.010	0.002
Adjusted R^2	0.001	0.001	0.010	0.002

Notes: This table reports regressions where the dependent variable is the coins consumption rate by users during the experimental period, and the independent variable is a treatment indicator. The outcome distribution is winsorized at the 99% level Each column reports estimates for different user subopopulations based on their types. Significance indicators: $p \le 0.1 : \ddagger$, $p \le 0.05 : *$, $p \le 0.01 : **$, and $p \le .001 : ***$.

Column (3) restricts the sample to F-type users. The intercept is the mean consumption rate for F-type users in the control group. In Column (4), we restrict the sample to G users. This restriction gives us the mean consumption rate for G-type users in the control. It only gives us part of the treatment effect, as the treatment reduced buying on the extensive margin. Hence, some treated G users are missing.

Recall that 60.14% of control users were in group G, and that the treatment reduced the fraction buying by 4.38 percentage points (7.29%) to 55.75% (see Table 3). Noting that the consumption rate of G-type users was 24.32 in the control group and 24.32+3.11 = 27.43 in the treatment group. From that, we can calculate the consumption rate of exiting users that would be required to explain this result purely by selection. Let r_S = 27.43 be the consumption of G users who bought coins during the experimental period, and r_E be the consumption rate of those users the ones who left the G group. Then

$$24.32 = 0.927 * r_S + 0.073 * r_E,$$

which yields that r_E = -15.18, which is impossible.