# How Do Past Privacy Choices Shape the Future?*

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**Abstract**

Consumers face frequent and consecutive digital privacy choices, but each choice is not necessarily independent. This paper demonstrates that past privacy choices affect consumers' current privacy choices. Such state-dependent choices suggest that privacy choices can have externalities within a platform in which one app's data requests can affect the ability of other apps to collect data. Specifically, I use an individual-level consumer panel to investigate data consent decisions by consumers on Alipay, a major digital platform that connects users and third-party apps. Leveraging a natural experiment that encourages users to accept data requests, I find that the probability of rejecting the next request declines 15%. This effect decays over time; it is larger when preferences for the app at that moment are relatively weak, categorized by large language models (LLMs); and for users whose privacy preferences are relatively weak. The effect does not differ by whether the specific data requested in consecutive data consent decisions is the same. Overall, these results suggest that the externalities arising from state-dependent data consent choices are temporary. Nevertheless, this temporary effect incentivizes platforms to encourage apps to provide consumer-friendly data request designs.

*Keywords: digital privacy, data sharing, state-dependence, platforms*

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# 1 Introduction

There is a well-documented tradeoff between privacy protection and the sharing of personal data (Acquisti et al. (2016)). The current platform ecosystems and privacy policy debates recognize that firms trade off the competing goals of data acquisition, which generates value for both firms and consumers (Farboodi and Veldkamp (2021)), and privacy protection. Yet, most empirical evidence has been focused on consumers' independent data-sharing decisions (Tucker (2024); Lin (2022); Kummer and Schulte (2019)). However, externalities, both across users and across firms, matter in data markets (Bonatti (2024)). Within a platform environment, consumer privacy decisions with one service provider might affect their decisions with other service providers, particularly if privacy decisions are state dependent. In this paper, I explore connections between data consent decisions, state dependence, and externalities in a platform environment where consumers make repeated privacy decisions.

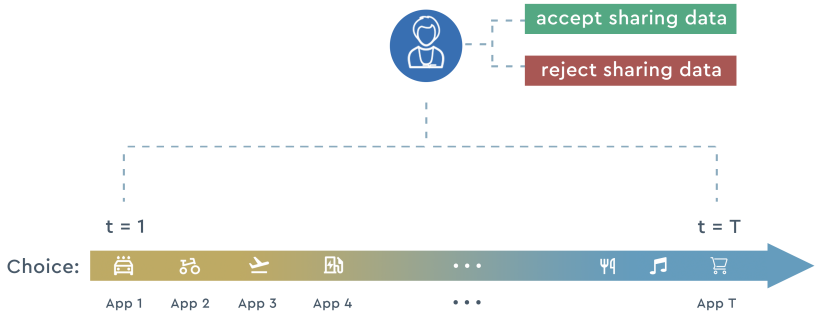
This paper investigates whether consumer privacy choices affect their subsequent privacy choices using large-scale panel data from Alipay, a major digital platform that serves over a hundred million active monthly users and connects users and third-party apps. In other words, this paper examines whether structural state dependence exists in consumer data-consent decisions in this online sequential binary choice setting. In the marketing literature, structural state dependence is a phenomenon where there is a causal effect of past behaviors on current behaviors (Heckman et al. (1981); Dubé et al. (2010)).

I identify structural state dependence by leveraging a quasi-experiment in which consumers are highly likely to accept data requests. This is a similar approach to prior work that identifies structural state dependence using stockout-style variation (Goldfarb (2006); Sudhir and Yang (2014); Levine and Seiler (2023)). Specifically, I study sequential privacy choices made by consumers on third-party apps within the Alipay platform. On this platform, portable power bank apps, which are designed for consumers needing to charge their devices on the go, generate near-universal data consent. These power bank app brands deploy their charging stations at shopping malls, bars, restaurants, hotels, tourist attractions, and other public locations. Notably, the average rejection rate for data requests from these portable power bank apps is below 1%. In contrast, the rejection rates for data requests from other types of apps on the platform are much higher, ranging from 5 percent to 50 percent.

Leveraging this quasi-experiment, I document that structural state dependence exists: an acceptance of data terms when using a battery app reduces the probability of rejecting a subsequent data request by approximately 15%, with the effect persisting for at least three consecutive choices before gradually dissipating. This pattern of significant, but temporary, structural state dependence indicates that consumer privacy decisions can generate a positive or negative externality across apps within the platform ecosystem–if the users accept one app's data request, this will make them more likely to allow other apps to collect data. This is a similar externality to the one identified in Nosko and Tadelis (2015), which documented that bad eBay buyer experiences with one seller had a negative impact on the platform overall.

I examine these privacy choices using a panel of approximately 358,000 randomly sampled users from March 2020 to September 2022. On Alipay, consumers repeatedly face binary data-consent decisions from third-party apps—"accept" or "reject"—with no option for partial consent. Only by accepting the whole bundle of data types can the consumers use the services–without customization options. Since most apps on the platform are third-party, data-consent decisions represent an exchange between the consumer and the app provider, conditional on already consenting to share data with Alipay. Figure 1 illustrates the sequential nature of these choices across different apps and contexts over time. The unit of observation is the user-app-decision.

Figure 1: Consumers make sequential data-consent decisions




Note: This figure illustrates the sequential privacy decisions in my empirical data. A user need to make a binary decision of "accept" or "reject" data sharing from the app request. Each data consent choice is on a different app instead of on the same app, on a large platform (i.e. same interface for every decision).

Outside of the Alipay context, such binary accept-or-reject choices are not unusual. For

example, "cookie walls" on websites similarly force an all-or-nothing choice.[1] Such cookie walls are prevalent in Europe, in regions like the Netherlands, France, and the UK, with differing guidance from authorities,[2] and are permitted under CCPA.[3] Similar dynamics occur with Facebook's third-party plug-ins and cookie banners on first visits to websites (online appendix Figure B1).

I then explore potential explanations of the observed state dependence. I find evidence consistent with two key factors. First, drawing on the accessibility-diagnosticity framework of Feldman and Lynch (1988), I demonstrate that the estimated state dependence decays with each choice after the initial shock; the shorter the interval between decisions, the larger the estimated effect size. Second, I consider the role of the strength of preference for one choice or another (Webb et al. (2024); Roy et al. (1996)). I find suggestive evidence that state dependence is more pronounced in contexts characterized by weak preferences, where consumers have little urgency or clear valuation for the service. In other words, the data consent decision is more likely to be affected by past choices for consumers when preferences for the app at that moment and preferences for privacy are relatively weak.

To explore the strength of preference for the app, I use the application program interface (API) of a large language model (LLM) to categorize each app in terms of whether the app is only useful if the consumer is physically present at the location where the services are provided and whether there is not another easy option for consuming the services besides using the app. In those cases, I categorize this as a context of stronger preference which suggests a high urgency for accepting the data request. Otherwise, I categorize the app as weaker preference context and a low urgency for accepting the request. Measured structural state dependence is only statistically and economically significant for low-urgency apps, suggesting that past choices are more impactful on the more deliberative current choices.

To further explore the role of the strength of preference, but the preference for the cost of data sharing, I distinguish between consumers who have read a privacy agreement before data consenting and consumers who do not. I find that consumers who have read privacy agreements do not exhibit structural state dependence in their choices. I interpret this to suggest that consumers with stronger preferences for privacy, or bearing higher cost of data sharing, exhibit less state

[1] Definition and overview of cookie wall: https://cookie-script.com/blog/cookie-wall (available on Jan 24, 2025).
[2] Deloitte Cookie Benchmark Study (Original PDF, archived). Scribd database(available at Jan 24, 2025).
[3] Examples of the interface could be found here: https://www.cookieyes.com/blog/cookie-wall/ (available at Jan 24, 2025).

dependence.

I also rule out alternative explanations. Evidence is inconsistent with state dependence driven by data permanence (Bonatti (2024)) or by shifts in risk perception (Tucker (2024)). If data permanence drove state dependence, the effect would persist over time, particularly when past and future requests involve the same types of data. In contrast, I find that state dependence is short-lived. Moreover, the effect occurs even with new data types requested in the latter choice. By shifts in risk perception, I mean that the state dependence could be due to consumers accepting future data requests after experiencing no negative consequences of accepting an initial data request. To explore this hypothesis, I compare users who are more likely to reject data requests prior to having used the battery app with those who are less likely to. I find no significant difference in terms of state dependence for these groups, suggesting that users who might anticipate a high risk from sharing data do not adjust more than those who do not anticipate such a risk.

Broadly, this structural state dependence suggests an externality from the data requests of some apps to the ability of other apps to collect data. By explicitly recognizing the connection between privacy choices over time, platforms should recognize the positive benefits of consumer-friendly privacy terms and the negative effects of terms that consumers are likely to reject. These externalities are particularly strong for apps and contexts where the consumer is unlikely to have a strong preference. This implies that platforms can strategically refine recommendation algorithms to enhance data consent rates for apps characterized by weaker preferences or exploratory usage. Specifically, these apps could be recommended immediately after users consent to data sharing for promoted apps. On the flip side, current regulatory actions and lawsuits on consumer data are addressing individual app misconduct, but attentions on app collusion is less prominent.

The remainder of the paper is organized as follows. In Section 2, I connect my results to literature on privacy and on structural state dependence. I combine insights from these two streams of research to develop a conceptual framework (Section 3) for state-dependent consumer privacy choices. In Section 4, I describe the empirical strategy for the identification of state dependence, the institutional background, the construction of the dataset, and suggestive evidence of state dependence and heterogeneity in privacy choices. Section 5 presents the model estimates, documenting significant structural state dependence. Section 6 examines the nature of this structural state dependence, and provides suggestive evidence that memory and preference strength play an important role in explaining the magnitude of structural state dependence. I conclude with a

discussion of limitations and the managerial implications for the platform. The online appendix contains additional supporting materials in more detail.

## 2 Literature

This paper relates to several strands of literature. The findings of this study contribute to our understanding of individual consumer privacy choices, addressing a gap where empirical evidence from the field has been limited. This study measures state dependence in a new context, digital privacy choices. It highlights that considering state dependence in consumer choices is crucial not only for brand promotion strategies in retailing but also for platform strategies aimed at retaining active users.

**I. Consumer Privacy.** First, this study relates closely to the literature on establishing an empirical framework to understand consumer privacy choices. Empirical evidence consistently indicates that consumers do care about their privacy. For example, holding price and functionality constant, demand is lower for apps that request sensitive data (Kummer and Schulte (2019)). Similarly, under the Ad-Choices program, users opt out of behavioral advertising leaving the costs to the publishers and the exchange (Johnson et al. (2020)). In addition to the documented evidence of the demand shift in response to privacy-related changes, recent frameworks quantify consumer privacy valuation. Lin (2022) proposes a dual-preference model incorporating intrinsic and instrumental components, measuring privacy preferences from psychological and economic perspectives. From the field data, Tang (2019) quantifies the monetary value of distinct data types in an online borrowing context, such as social network ID, and employer contact information. Building on this literature, my work contributes by treating privacy choices as dynamic rather than static, thereby examining consumers' sequential decisions to share data across time using individual-level field data.

Context–especially in offline environments–can shape consumers' online decisions. Liaukonyte et al. (2015) demonstrate that television advertising encountered offline can indeed influence online shopping behaviors. More recently, in mobile advertising settings, context has become more critical. By the nature of the mobile phone, the context changes accordingly when people move around. Marketers have found it more efficient to adopt contextual mobile advertising (Andrews et al. (2016)). The contextual information not only improves targeting effectiveness but also can generate

additional revenue by nudging customers toward new behavioral patterns (Ghose et al. (2019)). Earlier field experiment work has shown that people's privacy evaluations are sensitive to contextual factors (Acquisti et al. (2013)). Factors such as consumer ignorance, lack of attention, and perceived control, affect privacy choices (Tucker (2014); Burtch et al. (2015)). However, whether the change of contexts directly influences sequential digital privacy decisions remains under-explored. My findings on contextual state dependence offer parallels with research on advertising, suggesting that the platform could boost the data consent rate for apps whose value-cost tradeoff is unclear by leveraging users' state dependence in similar privacy choices. This also relates to an emerging literature on choice architecture for privacy (Lin and Strulov-Shlain (2023); Fradkin et al. (2024)).

Nosko and Tadelis (2015) show that reputational externalities emerge when eBay buyers extrapolate their experiences with individual sellers to form impressions about the overall eBay market, and those who have a negative experience with a single seller show a decreased likelihood of returning to the platform. Jaffe et al. (2017) find similar results on Airbnb.

The externalities in the privacy literature currently fall into three main themes.[4] First, an individual's choice to share data can directly impact others within their network. For instance, a social media feed contains information about both the account owners' posts and likes and those of their connections. Second, information about one individual provides probabilistic information about others since preferences and behaviors are correlated (Choi et al. (2019); Bonatti (2024)). Third, consumers' decisions to withhold information can also expose their types through market interactions (Bergemann and Bonatti (2015)). This paper finds a new form of externality in the platform, where consumers' data consent choices to a specific app can positively or negatively impact the ability of other apps to collect data.

**II. State Dependence.** This work is also rooted in the state-dependence literature in marketing and economics. The empirical regularity of state dependence is explained in two ways: structural state dependence, where experiencing an event alters future preferences, and spurious state dependence, which arises from unobserved consumer preferences. Since the 1980s, a consensus in the quantitative marketing literature has been growing that there is substantial evidence of persistent choices of consumers in the consumer packaged goods (CPG) industry (Seetharaman et al. (1999); Dubé et al. (2010); Simonov et al. (2020)). Recently, Levine and Seiler (2023) found no evidence of such persistence when leveraging an exogenous shock that disrupted a single

[4]Goldfarb and Que (2023) provides a more comprehensive overview of the positive and negative externalities.

purchase occasion. Drawing on Heckman et al. (1981), these papers provide frameworks to assess whether such persistence measures a causal effect of past choices on current purchase behavior (structural state dependence), instead of the time-invariant preference heterogeneity (spurious state dependence).

In addition to the econometric models that quantify the state dependence, there is extensive research in consumer psychology on memory-based choices. The accessibility-diagnosticity framework by Feldman and Lynch (1988) posits that memory input—in my setting, a prior data-consent choice—affects subsequent related behavior based on two factors: accessibility and diagnosticity. The observed diminishing effect of state dependence that I document aligns with the theory that past choices, recent and more accessible, exert a stronger influence. Furthermore, the results in preference strength indicate that individuals with lower diagnosticity of current information are more influenced by past choices.

Next, I build a conceptual framework and empirical strategy that draws on ideas from both the privacy literature and the state dependence literature.

# 3 Conceptual Framework

To provide intuition for my empirical analysis and identification strategy, I consider a simple model[5] of consumer privacy choices that allow for individual privacy preference heterogeneity as well as structural state dependence.

A consumer can choose to accept or reject the privacy terms of an app. Let $d \in \{0, 1\}$ to represent this decision, where $d = 1$ is acceptance and $d = 0$ is rejection. A consumer can only access the app's service by agreeing to the data-consent request. Upon downloading the app, consumer $i$ decides to accept ($d = 1$) or reject ($d = 0$) the data-consent request from the app provider. Acceptance leads to the app being installed and opened, while rejection prevents the app from opening. The flow utility consumer $i$ obtains from their decision $d_{it}$ at choice $t$ is given by:

$$u_{it}(d_{it})^j = \begin{cases} \underbrace{\delta_{it}^j - c_{it}^j}_{\text{value-cost tradeoff}} + \underbrace{\gamma_A \times \mathbf{1}(d_{i,t-1} = 1)}_{\text{state dependence}} + \quad \varepsilon_{it}^1, & \text{if} \quad d_{it} = 1 \\ \underbrace{\gamma_R \times \mathbf{1}(d_{i,t-1} = 0)}_{\text{state dependence}} + \quad \varepsilon_{it}^0, & \text{if} \quad d_{it} = 0 \end{cases} \tag{1}$$

[5]Note that I do not estimate a model of consumer utility. Instead, my analysis is a reduced form of this framework.

When accepting at the current choice ($d_{it} = 1$), consumer $i$ incurs a privacy cost of data consent, which is specified as $c_{it}^{j}$. This cost of data included in the model consists of a consumer-specific privacy preference, which is persistent individual heterogeneity, and a specific cost on observing the app-datatype pair. Therefore, in my setting, unlike Lin (2022), I am not able to separately identify the intrinsic preference from the instrumental. $\delta_{it}^{j}$ captures the value of the app $j$ when consumer $i$ accepts the data consent and adopts it at choice $t$. Together, consumer $i$ faces the value-cost tradeoff of $\delta_{it}^{j} - c_{it}^{j}$ at every choice $t$, that is app-value and cost-of-data specific. The second term captures state dependence by allowing the utility to differ when the acceptance choice was also made in the last choice occasion $(t-1)$. $\mathbf{1}(d_{i,t-1} = 1)$ is a dummy variable indicating whether or not the consumer accepted the data-consent request in the last choice. $\gamma_A$ reveals the degree of state dependence of accepting if accepted last time, and $\gamma_R$ reveals the degree of state dependence of accepting if rejecting last time. In the main results section below, the degree of state dependence, measured by $\gamma_A$ and $\gamma_R$, is assumed to be constant. In discussing potential explanations, I allow the degree of state dependence to vary across user groups and across contexts. Finally, $\varepsilon_{it}^{1}$ and $\varepsilon_{it}^{0}$ are logit taste shocks that are independent across consumers and choices. When rejecting ($d_{it} = 0$) at the current choice, consumer $i$ does not enjoy the app value $\eta_{i}^{j}$ or incur any cost of data consent. However, the individual receives the utility of state dependence if they rejected it in their last choice $(t-1)$. This is captured by $\gamma_R$.

Following Heckman et al. (1981), structural state dependence and heterogeneity can be confounded. A core challenge in identifying structural state dependence lies in the fact that observed patterns of choice persistence could be produced by both preference heterogeneity and structural state dependence, making it difficult to distinguish between the two solely based on choice persistence data. In the data-consenting context of this study, the key challenge is to separately identify state dependence from consumers' underlying privacy preferences. Therefore, the key idea of the identification strategy that I detail below is that the privacy choice behavior following a shock is different if state dependence exists.

In the absence of structural state dependence, consumers' privacy decisions should be independent from one choice to the next. Consequently, the distribution of rejection share ($Pr(d_{it} = 0)$) for each consumer is the same for each choice. Thus, if the average persistence in choices does not immediately return to its baseline following a shock that forces users into acceptance or rejection, it suggests that the null hypothesis (that decisions across different time periods are independent)

is not rejected. I specifically focus on the external shock that forces users into acceptance. In this regard, I derive the probability of rejection ($Pr(d_{it} = 0)$) at choice $t$ with respect to the condition where the consumer accepts the data request in their last choice ($d_{i,t-1} = 1$).

Specifically, a consumer chooses to reject ($d_{it} = 0$) if and only if $u^j_{it}(0) \geq u^j_{it}(1)$. The probability of consumer $i$ choosing rejection ($d_{it} = 0$) is

$$\begin{aligned}
Pr(d_{it} = 0) &= Pr\Big(u^j_{it}(0) \geq u^j_{it}(1)\Big) \\
&= Pr\Big(\gamma_R \mathbf{1}(d_{i,t-1} = 0) + \varepsilon^0_{it} \geq -\delta_i + \gamma_A \mathbf{1}(d_{i,t-1} = 1) + \eta^j_i + \varepsilon^1_{it}\Big) \\
&= Pr\Big(\gamma_R \mathbf{1}(d_{i,t-1} = 0) - \gamma_A \mathbf{1}(d_{i,t-1} = 1) + \delta_i - \eta^j_i + \varepsilon^0_{it} - \varepsilon^1_{it} \geq 0\Big) \\
&= Pr\Big(\gamma_R \big(1 - \mathbf{1}(d_{i,t-1} = 1)\big) - \gamma_A \mathbf{1}(d_{i,t-1} = 1) + \delta_i - \eta^j_i + \varepsilon_{it} \geq 0\Big) \\
&= Pr\Big(-\big(\gamma_R + \gamma_A\big)\mathbf{1}(d_{i,t-1} = 1) + \gamma_R + \delta_i - \eta^j_i + \varepsilon_{it} \geq 0\Big)
\end{aligned}$$

Conceptually, without structural state dependence, the rejection probability ($Pr(d_{it} = 0)$) at choice $t$ would be independent, and solely depends on $\eta^j_i - \delta_i$. However, if structural state dependence exists, the distribution of rejection share ($Pr(d_{it} = 0)$) for each consumer would change. The state-dependence ($\gamma_R + \gamma_A$) will be estimated in an analysis in Section 4 using the change that low-battery mandatory-acceptance shock imposes on the probability of rejection ($Pr(d_{it} = 0)$). Note that in this study, I am not able to disentangle $\gamma_A$ and $\gamma_R$ because only a "mandatory-acceptance" shock is leveraged in the reduced-form analysis. Therefore, I combine these into a parameter $\gamma(= \gamma_R + \gamma_A)$.

# 4 Empirical Strategy

In this section, I introduce the institutional context and describe my empirical approach. I detail the construction of the dataset used for the primary analysis and how the key variables are defined, and provide summary statistics alongside motivating evidence. Subsequently, I introduce a staggered difference-in-differences design to assess the causal impact of the shock.

## 4.1 Institutional Background

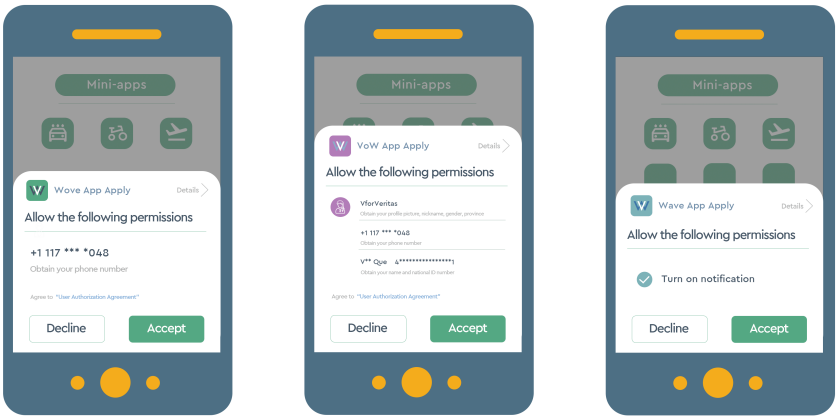
I analyze proprietary individual-choice-level panel data from Alipay. The data contain information on user demographics and on the apps. With millions of data points, this data set allows for rich

analysis of user data consent choices in an important real-world setting.

Alipay is a Chinese digital payment and lifestyle platform with more than a hundred million monthly active users. It is one of the largest digital platforms in the world. The platform supports hundreds of thousands of third-party apps for extensive digital products and services, such as tickets, food orders, health applications, personal tools, coupons and offers, life services, rent and resell, and entertainment activities (online appendix Figure B2).

By using Alipay for apps, consumers avoid the friction of separately downloading, installing, and managing each app on their mobile phones. This concept is similar to third-party plug-ins found on Facebook or extensions available on the Chrome browser. Note that such explicit data consent rule is similar to the Android app ecosystem, while it does not exist in Apple's iOS.

Figure 2: Interface: binary choices & different data combinations




Note: The data types requested by apps vary. An app could request phone number only (p1), others could ask for a combination of basic profile information, phone number and national ID (p2). Requests for items unrelated to core personal data (i.e. privacy)—such as notification allowances—are classified as "others" (p3).

To access products and services provided by the apps on the Alipay platform, users need to authorize certain types of data when they first download and install an app. When prompted with a bundle request of data types, consumers have two options– to accept the data-sharing request by the app or to reject it (Figure 2). Only by accepting the whole bundle of data types can the consumers use the app. No partial acceptance or data customization is allowed, making the data-for-service exchange explicit. Once a consumer accepts the request, no further privacy decision is

required for that app.[6] Note that all apps are free at the time of installation on the platform.[7]

The bundle of data types requested varies across apps. It is the app provider's individual decision on what data types to request upfront, and there is considerable variation in the information requested. For example, some apps ask for general profile information only, others ask for credit information, and others request access to the step count.

Alipay standardized this mandatory upfront data consent rule in 2019.[8] Therefore, this form of data-service consent decision was still new to consumers during the time period I study. The panel data underlying my analysis begins on March 1, 2020, marking the onset of a clean, comprehensive dataset with minimal missing values and full population coverage.

## 4.2 Identification: A Quasi-Experiment

In the absence of structural state dependence, consumers' privacy choices should remain independent from one choice to the next.

To identify structural state dependence described in Section 3, I leverage a quasi-experiment. Specifically, to separate the user's state dependence from heterogeneity, I use acceptance of data requests from the portable power bank apps as an exogenous shock (as did in Levine and Seiler (2023); Sudhir and Yang (2014); Goldfarb (2006)). The purpose is to identify whether the "mandatory" acceptance brought by the shock impacts a user's data-consent decision in the choices that follow.

### 4.2.1 Industry Background of Portable Power Bank

In China, mobile devices are central to daily life, with digital wallets and QR codes enabling seamless transactions in various settings.[9] The infrastructure of portable power banks is extensively developed across China, reaching urban and rural areas alike, with over 200 million users per week

[6] At choice T, if it is the initial download and installation of app $J$ for consumer $I$, the individual must make data-consent decisions. If the individual rejects the data request, access to app $J$ is denied; alternatively, if this is not the consumer's first interaction with app $J$, meaning they have previously engaged with the data request, consumer $I$ can continue using app $J$ without making further privacy decisions.

[7] Apps monetize through in-app transactions.

[8] This payment platform was originally launched in 2004, and the large-scale third-party app ecosystem on Alipay was built in 2015.

[9] See example: www.brookings.edu/wp-content/uploads/2020/04/FP_20200427_china_digital_payments_klein.pdf (accessed Jan 24, 2025)

Figure 3: An Illustration of Portable Power Bank Use Case




Note: A low-battery shock occurs when a user's phone battery depletes at public facilities (p1; source). To address this, users can rent a portable power bank by scanning a QR code at any station and installing the associated app. Upon accepting the privacy terms, users can remove a power bank (p2; source) and use it on the go (p3; source). Once charging is complete, they can return the power bank to any station of the same brand (p4; source).

by 2021.[10] Charging stations are prevalent in public spaces, including but not limited to shopping malls, bars, restaurants, hotels, and tourist attractions. Figure 3 illustrates how the portable power bank works. When a user experiences low battery on their phone, they can scan a QR code at a portable power bank station, install the app, and grant data consent to access a physical power bank. After charging, they return it to any station of the same brand and are billed based on usage time.

By 2022, 280 brands operated in China, with the top three holding nearly half the market share,[11] and 263 of which are covered in my dataset. While portable power banks have experienced significant growth in China, their market is not confined to the country, with a global market of USD 7.1 billion in 2021.[12]

### 4.2.2 Identification Assumptions

The core identification assumption is that the out-of-battery shock affects the subsequent choice only through its impact on the currently treated choice. In other words, the treatment is not correlated with any unobserved demand shocks to the next choice $t$.

For this assumption to be violated, either the low-battery situation should directly reduce the rejection probability on the subsequent choice or some omitted factor is leading to low rejection

[10] "Portable Charger Sharing Industry Appears to Have a Better Future Than Bike Sharing – But Still Holds Risks", chinamktginsights.com, January 7, 2021 (available on Jan 24, 2025)
[11] Category "Portable Power Bank" in Appendix A Table C8
[12] Spherical Insights & Consulting Report.(available on Jan 24, 2025)

probabilities on consecutive choices.

I argue that both of these are unlikely in the context of portable power bank apps. For the first argument, the direct causal impact is unlikely because once the battery is charged, there is no longer an urgent need to rush decisions to accept the next. The situation would be resolved. For the second argument, an omitted factor is unlikely for two reasons. First, I will show that consecutive decisions rarely occur over a short time period and are not driven by time-of-day or day-of-week effects. Second, I observe no substantive changes in the variety or frequency of downloaded app categories before and after the treatment, suggesting treated users do not use the portable power bank apps because of a sudden change in preferences.[13].

#### 4.2.3 Portable Power Bank Apps as the Instrument

Absent state dependence in a user's sequence of data-consent decisions, the "mandatory" acceptance of the power bank app during a low-battery emergency should not influence subsequent choices. The treatment is an increase in the likelihood that a consumer says "yes" in the middle of a digital privacy choice sequence. I argue that this allows me to separate structural state dependence in choices from persistent heterogeneity. I assume the shock happens to the treated users randomly in the position of their choice sequence, thereby enabling separate measurement of the state dependence from the underlying the sources of heterogeneity.

As illustrated in online appendix Figure B5Panel A, the average rejection rate for portable power banks is markedly lower than for any other app type. This low rate of rejection holds across brands, with no brand having a rejection rate higher than 2 percent. The different portable power bank apps, however, do have substantial variation in the data requested, with some asking for more than one type of data and others only requesting basic profile information, or phone number. This consistently low average rejection rate regardless of the specific data requested supports the identifying assumption that the use of portable power bank apps is indeed urgent.

[13]For the treated and not-yet-treated users in the analysis, I calculate the downloading percentage of each category before and after the shock. The percentage numbers are calculated by the number of apps in this category over the total number of apps downloaded for each group. For a balancing purpose, I focus on three choices before and three after the shock for the treated users; the 2nd, 3rd, 4rd choices (comparable to the pre-shock period), and the 6th, 7th, 8th (comparable to the post-shock period) for the not-yet-treated ones. Results are shown in Online Appendix Table C1.

### 4.3 Data and Descriptive Evidence

The main data set is constructed from a panel of user data-consent decisions across all app categories on the platform. The dataset begins March 1, 2020, and ends September 30, 2022, covering a total of 31 months. A potential concern is that COVID lockdowns could affect app adoption and authorization. Most cities ended their lockdowns on March 25, 2020.[14] The potential concern is that the treatment effects could vary between lockdown and non-lockdown periods. However, this issue is not present in the data, as all treatments (i.e. the mandatory acceptance shock) began after March 25, 2020. The data also shows a combination of indoor and outdoor apps before and after the staggered shocks. From the active user base during this period, I randomly sampled 800,000 users.

I described the shock I will use for the difference-in-differences research design above. To identify the structural state dependence in a sequence of consumer data-consent choices, the analysis includes only users who made at least 8 data-consent decisions throughout the sample period (for a burn-in period and for validating parallel trends pre-treatment), ultimately yielding a dataset of 475,139 active users. Furthermore, the primary analysis targets a subset of users who have both rejected and accepted a data request at least once, ensuring that their probabilities of rejection and acceptance are non-zero. This subset comprises 358,004 users.[15] All individuals are de-identified before entering into the data analysis process.

The data includes three main categories of variables: user behaviors (privacy decisions and reading privacy legal term decisions), user demographics, and app characteristics. Below are the main components of my data:

**Data-consent decisions.** An individual can choose to "accept" or "reject" the data request the first time they interact with an app. This is the focal decision I look into across time, which is observed on the user-app-choice level.

**Reading privacy agreements decision.** When consumers encounter a data consent pop-up during an app download, they may review the privacy legal terms before choosing "accept" or "reject". I observe the review timestamp and the associated app request, but not the content of the legal terms or the review duration. Reading the agreement is optional. Only 5% of the

[14] China's lockdown timeline is summarized in Figure 4, Xu et al. (2020)(available on Jan 24, 2025).

[15] Main sample of 358,004 users is used for main results in Section 5.1, while full sample of 475,139 users is used for the first robustness check in Section 5.2.

population read any agreement.

**Demographic data.** I observe the consumer's demographic information such as age, gender, education level, residential area (eg. province, city, city tier[16]), and the average balance on the account.

**App data.** I observe an app's unique app ID, and its merchant category code, assigned by credit card companies to classify the business by the type of goods or services it provides. I also observed the datatype combination an app requests, which may include any of the following, individually or in combination: user profile information (such as profile picture, nickname, province, and gender), phone number, address, credit history on the payment platform, national ID, student information, and daily step counts.

**Firm and service description.** I complement the panel data from Alipay with service description data from Qichacha[17]. Qichacha provides detailed information on Chinese companies, including product offering details, registration, financial records, and legal proceedings. The platform aggregates data from various official sources to ensure the accuracy and reliability of the information provided[18]. I gather detailed descriptions of all firms operating on least one app on the Alipay platform. These descriptions provide the operational contexts and use cases.

### 4.3.1 Sample Characteristics

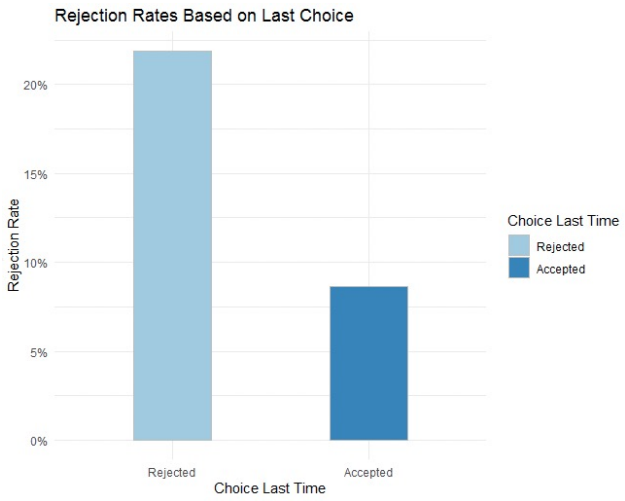
The summary statistics of the users are shown in online appendix Table C2Panel A. The table summarizes the age, gender, education, occupation, city degree, and asset balance for all users and the treated users, respectively, in the main sample. The sample of the treated users exhibits similar distributions in all demographic dimensions as the main sample. One-third of the population has ever been treated. Online appendix Table C2Panel B summarizes the characteristics of 7,321 apps

[16]In China, cities are often categorized into five tiers based on a variety of criteria such as economic performance, population size, and infrastructure development. First-tier cities are the largest and most developed cities with significant international influence and advanced infrastructure. First-tier cities include Beijing, Shanghai, Guangzhou, and Shenzhen. Second-tier cities are regional economic hubs with rapidly growing economies and improving infrastructure. They include provincial capitals and major coastal cities such as Chengdu, Hangzhou, and Nanjing. Third-tier cities include smaller provincial capitals and other significant cities that have a moderate pace of economic development, such as Qingdao, Dalian, and Xiamen. Fourth-tier cities generally have smaller populations and less developed infrastructure. Fifth-tier cities are often referred to as county-level cities or small towns.

[17]https://www.qcc.com/

[18]Including government databases such as National Enterprise Credit Information Publicity System (www.gsxt.gov.cn), China Judgements Online (wenshu.court.gov.cn), China's Enforcement Information Disclosure Website (zxgk.court.gov.cn), China National Intellectual Property Administration (www.cnipa.gov.cn), Trademark Office of China National Intellectual Property Administration (sbj.cnipa.gov.cn), National Copyright Administration (www.ncac.gov.cn)

Figure 4: Rejection Rates by Previous Choice




Note: The raw data reveal that users are more likely to reject a current option following a previous rejection (light blue) than following an acceptance (dark blue). This pattern may reflect either a structural state dependence or heterogeneous privacy preferences, motivating the identification.

in the main sample of 358,000 users. The average number of users per app is 6,116, with a median of 202, indicating a highly skewed distribution. While the largest app has over 333,000 users, the smallest has only one, highlighting that a few apps dominate user engagement, while most have relatively few users.

### 4.3.2 Motivating Analysis

This section documents three aspects of the data that motivate the main analysis. First, that the rejection rate for the current choice is lower when the preceding choice was acceptance rather than rejection. Second, that there is considerable heterogeneity across demographic groups in terms of data-consent choices. Third, that there is considerable heterogeneity across app categories in terms of the rejection rate, with the lowest rate for the portable power bank category–serving as the identification strategy.

**Rejection Rates in Sequence.** The starting point for a study of structural state dependence is an assessment of the observed correlation between current and past choices. In my data sample, on average, users generally exhibit lower rejection rates if they accepted the last choice (as illustrated in Figure 4). This correlation suggests a potential state dependence effect.

Thus, Figure 4 indicates that either a consumer's current choice may be influenced by their

previous choice or there may be underlying preference heterogeneity.

**Heterogeneous Consumer Privacy Choices.** Consumers reject requests for data use permission approximately 13% of the time in the data sample. In order to get access to the apps, consumers need to authorize certain types of data during the initial download and installation. Partial acceptance or customization of data permissions is not available. Consequently, if consumers encounter a privacy-intrusive situation that makes them uncomfortable sharing specific data types, they will reject the data request and forgo using the app. This tendency to reject data requests from an app is heterogeneous across consumers, as shown in online appendix Figure B3Panel A, and shows significant variation by age (online appendix Figure B4Panel A) and education (online appendix Figure B4Panel A), aligning with existing literature (Goldfarb and Tucker (2012); Tang (2019)).

**Heterogeneous Rejection Rates Across Apps and Contexts.** Figure B5Panel A in online appendix shows rejection rates by app category, with onsite-service-related apps like food delivery, restaurants, ride-sharing, bike-sharing, parking, and vending machines having relatively low rejection rates. Notably, the "Portable Power Bank" category stands out with an exceptionally low rejection rate, below 1%. Other categories range from 5% for gas stations to 30% for coupons. Rejection rates also vary by data type requested–such as profile information (profile picture, nickname, province, and gender), phone number, address, credit history, national ID, student information, or step counts[19]–with no clear monotonic relationship between request scope and rejection rates (online appendix Figure B5Panel B).

Online Appendix Table C8 provides further details on rejection rates and market size by category, revealing significant variability across categories and across top apps in each category.

### 4.4 Staggered Difference-in-Differences Design

I define $D_{it}^{l} := \mathbf{1}\{t - E_i = l\}$ to be an indicator for user $i$ being $l$ periods away from initial treatment at their choice $t$. For never-treated users, $E_i = \infty$ and $D_{it}^{l} = 0$ for all $l$ and all $t$. The conventional two-way fixed effect (TWFE) framework adopts a static specification, estimating a single treatment

[19]Here, type "other" means any request that is not related to a consumer's personal information. For example, the requests of "turning on notification of this app", or "allow transitioning to the payment system" are both considered in type "other".

effect that is time invariant:

$$Pr(d_{it} = 0) = \alpha_i + \lambda_t + \gamma \sum_{l \geq 0} \cdot D_{it}^{l} + \nu_{it} \tag{2}$$

However, this specification is problematic in its interpretability under staggered rollout of the treatment as pointed out by Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Sun and Abraham (2021), Borusyak et al. (2024), and De Chaisemartin and d'Haultfoeuille (2020). Specifically, $\gamma$ as an aggregated parameter, recovers a weighted average of some underlying treatment effect parameters but could put negative weights on some of these parameters. Even in the case where weights are not negative, they are sensitive to the size of each group, the timing of treatment, and the total number of time periods (see Goodman-Bacon (2021) for an explanation of the Decomposition Theorem underlying this concern).

#### 4.4.1 Callaway Sant'Anna's Estimator

To address the issue of heterogeneous treatment effects, the above-mentioned papers propose alternative DiD estimators to avoid the pitfalls associated with TWFE. I adopt the approach from Callaway and Sant'Anna (2021), focusing on a disaggregated causal parameter called the "group-time average treatment effect", where a "group" is defined by the time period when units are first treated. The Callaway-Sant'Anna (CS) estimator is most suitable in my setting since it has weaker assumptions on a) whether the parallel trends assumption holds after conditioning on covariates; and b) whether the possible aggregations of group-time average treatment effects are flexible, which allows treatment effect heterogeneity both on cohort groups and across time.

Callaway and Sant'Anna (2021) describe several aggregation schemes, among which I focus on a summary parameter that circumvents the issue of changing the composition of groups by balancing the sample with respect to event time. Specifically, in my analysis, this treatment effect parameter is local to groups that participated in the treatment for at least 3 periods before and after. In this sense, the treated users in the main analysis are those who have their first choice for the burn-in period and at least 3 choices before and after the treatment. The unit of observation in my estimation sample is at the user($i$)–choice($t$) level. From Section 3, the state-dependence effect on rejection probability on choices could be assessed through reduced-form analysis after treatment.

Under the assumptions of conditional parallel trends and limited anticipation, I capture the effect of state dependence at each choice after the shock as:

$$Pr(d_{it} = 0) = \alpha_i + \lambda_t + \left( \sum_{\tau \in \{-3,\ldots,3\}} \gamma_\tau \cdot D_{i,\tau} \right) + \epsilon_{it} \tag{3}$$

where $Pr(d_{it} = 0)$ is the probability of user $i$ rejecting at choice $t$. $D_{i,\tau}$ is the indicator that equals to one if user $i$ was at $\tau$ periods from the user $i$'s initial treatment choice. $\alpha_i$ and $\lambda_t$ are user and choice fixed effects. The user-fixed effect $\alpha_i$ captures time- and choice-invariant individual characteristics that would affect the privacy choices, while the choice-fixed effect $\lambda_t$ captures user-invariant, choice-order-dependent factors that would affect the decisions. $\gamma_\tau$ measures the causal effect of the exogenous shock on privacy choices, which is the state dependence of the last choice on the current choice. Finally, $\epsilon_{it}$ represents unobserved idiosyncratic shocks for user $i$ at choice $t$ with mean zero.

# 5 Results

My primary analysis focuses on the main sample of users who have rejected a data request at least once,[20] ensuring their likelihood of rejection is not zero. Qualified treated users must have at least three choices both before and after the treatment, allowing me to test parallel trends and assess state dependence. Robustness checks are also performed on the full sample and several subsets of users.

## 5.1 Main Effect

The main results are reported in Table 1. Columns (1)-(3) report the Callaway and Sant'Anna (CS) estimator of the regression. I used the choice order as the time-fixed effect.[21] Columns (4)-(6) in Table 1 capture the difference-in-differences estimates under traditional two-way fixed-effect (TWFE) regressions with user and choice fixed effects, as robustness checks.

[20] The sub-sample consists of 358,004 users.

[21] The rationale is that, unlike traditional consumer shopping behaviors where seasonal effects are salient, calendar time in privacy preferences would probably not be the first priority. Instead, due to the hypothesis that there is state dependence across choices, then the main relevant time dimension is likely to be the choice order. As shown in Appendix A Figure B6Panel A, the rejection rate is going downward smoothly for all the users in the sample and the qualified users with more than 8 choices.

The CS estimates show effects of similar magnitude in the baseline model. Column (1) shows the basic CS estimator with user and choice fixed effects. Compared to the baseline rate, the rejection rate declines 2.2 percentage points, or 15% after the treatment, suggested by the CS estimator.[22] Column (2) and (3) add the calendar time controls, and category and datatype controls, respectively, to the model.[23] TWFE in column (4) shows a 2.2 percentage-point (15%) drop from the 14.5% baseline. Column (5), with calendar-month fixed effects, yields similar results, suggesting calendar time matters less than choice order. Column (6), adding category and datatype FEs, yields an estimated effect of 1.6 percentage points.

The CS estimator avoids using already treated users as controls, enabling reliable analysis with not-yet-treated users as the control group, as shown in Table 1. The CS estimates in this table do not suffer from the concern that users who are treated by the out-of-battery shock are inherently different from the users in the control group. This approach ensures comparability, as control users face the same likelihood of out-of-battery situations. For robustness, Table C3 in online appendix includes both never-treated and not-yet-treated users in the control group, with no significant shift in estimates.[24]

Table 1: Traditional TWFE & Callaway and Sant'Anna Estimates (Control Group: Only Not-yet-treated Users)

| | (1) CS | (2) CS | (3) CS | (4) TWFE | (5) TWFE | (6) TWFE |
|---|---|---|---|---|---|---|
| Treated × 1st Choice Post | -0.022*** | -0.037*** | -0.019*** | -0.022*** | -0.021*** | -0.016*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Treated × 2nd Choice Post | -0.017*** | -0.039*** | -0.016*** | -0.018*** | -0.018*** | -0.014*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Treated × 3rd Choice Post | -0.015*** | -0.041*** | -0.013*** | -0.015*** | -0.015*** | -0.013*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 603,631 | 603,631 | 603,631 | 603,631 | 603,631 | 603,631 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | Yes | No | No | Yes | No |
| Category FE | No | No | Yes | No | No | Yes |
| Datatype FE | No | No | Yes | No | No | Yes |
| Clustered error | user | user | user | user | user | user |
| *Note:* | baseline rejection rate before treatment is 14.5% | | | | | |

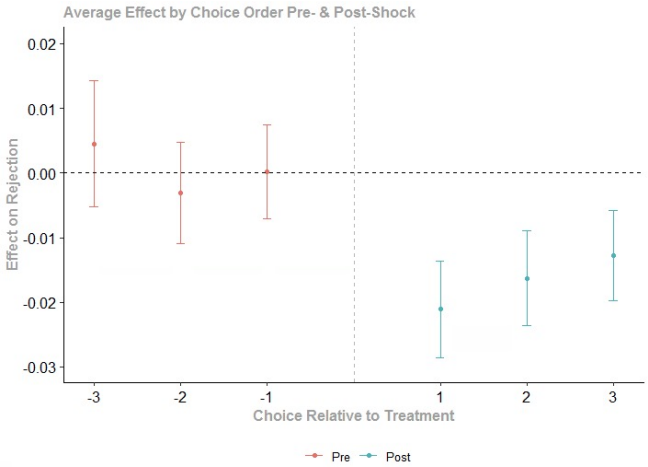
Figure 5 shows the CS estimators display parallel trends pre-treatment, and reveal a 2.2

[22]The pre-treatment rejection rate is 14.5% and the percent change (ratio of 2.2 to 14.5) is 15%.

[23]Note that in Callaway and Sant'Anna (2021), the additional fixed effects are not possible. Therefore, the additional terms in column (2) and (3) are added in as controls.

[24]The TWFE estimates in Table 1 and in online appendix Table C3 are not reliable because they include the already treated in the control. I include them for completeness.

Figure 5: Event-Study Plot of Callaway and Sant'Anna Estimates




Note: This figure plots the coefficients and 95% confidence intervals from event study regression of change in rejection probability using Callaway and Sant'Anna (2021). The specification includes user and choice-order fixed effects. Standard errors are clustered at user level. Additional specifications for robustness see Table 1.

percentage-point (15%) drop in rejection at the first post-treatment choice, persisting for at least three choices.

## 5.2 Robustness Checks

In this section, I present several robustness checks for the main results.[25]

**Including Users Whose Rejection Probability is Zero.** This robustness check includes users who never reject in the whole panel.[26] The CS estimates are summarized in online appendix Table C4Panel B (control group consists of users both never-treated and not-yet-treated) and Table C4Panel A (only not-yet-treated users as the control group). Including the users who have zero likelihood of rejection decreases the pre-shock baseline rejection rates, but the relative drop remains consistent. Additionally, I apply the Sun & Abraham estimator (Sun and Abraham (2021)) to both the main and full samples, using varying control groups and fixed effects (eg. calendar time, app category, and datatype). Results across all specifications (online appendix Table C5Panel A, Table C5Panel B) remain statistically and economically significant, confirming the robustness of the main findings.

[25]Results of robustness checks are presented in the Online Appendix.

[26]Recall that in the main analysis in Table 1, I include the users who have at least rejected the data request once in their lifetime. In this robustness check, I include these users whose rejection probability is zero.

**Alternative Difference-in-Differences Estimators.** Table C9 in the Online Appendix summarizes the traditional TWFE and alternative estimators (Sun and Abraham (2021); Roth and Sant'Anna (2023); Gardner (2022)), aligning with the main Callaway and Sant'Anna (2021) estimator. Using the same sample as Table 1 and restricting controls to not-yet-treated users,[27] all regressions, with minimal variation in point estimates and standard errors, indicate robust and consistent treatment effects across models.

**Validating the Assumption of No Simultaneity.** The identification of state dependence rests on the assumption that the out-of-battery shock influences subsequent choices solely through its effect on the current one, with no direct link to the next choice's rejection probability. To address concerns that treated users might accept portable power bank apps solely to enable future app acceptance, I analyze a subsample where post-shock choices occur on a different day than treatment.[28] This reduces observations by 2% (from 603, 631 to 590, 386), showing most post-shock choices are not immediate. Results in online appendix Table C6, using Callaway and Sant'Anna (2021), TWFE, and Sun and Abraham (2021) estimators, closely align with main results in different models (online appendix Table C9), confirming robustness when usage contexts differ.

**Time of Day and Day of the Week.** An important assumption is that no time-based omitted variable causes low post-shock rejection rates. Potential omitted variables—such as time of day or day of week (e.g., battery depletion coincides with a higher evening acceptance rate)—could explain lower next-choice rejection among treated users. Yet, online appendix Figure B6Panel B shows stable rejection from 9 am to 7 pm, with only slight variation at night, likely biasing measured effects downward. In addition, online appendix Figure B6Panel B shows a constant rejection rate across the day of a week, suggesting minimal time-of-day or day-of-week confounds.

**Using a Subsample of Newly Registered Users on the Platform.** Although this data-consent format is new to users at the start of my analysis period (March 2020), I do not observe their first data-consent decisions (initial condition). To check robustness, I examine 10,032 newly registered users midway through the sample period with no prior platform experience,[29] of whom 1,829 were treated (i.e. have ever encountered the out-of-battery shock).[30] Table C7 in

[27]This ensures the results are not compromised by the concern that individuals exposed to the out-of-battery shock may be systematically different from those who have never used portable power bank apps.

[28]In this case, the usage scenarios and contexts are different for the treated choice and the subsequent one.

[29]Online Appendix Figure B9 shows the overall distribution of the timing of the first choice for each user in the main sample, most of which joined prior to the data period start.

[30]Newly registered users are rare since the platform has its prevalent establishment in China for more than 15

the Online Appendix shows a slightly larger effect than in the main sample in all specifications, confirming state dependence despite less statistical precision due to a small sample size.

This section documented that there exists state dependence after the use of a portable power bank app. Next, I explore possible explanations for this state dependence.

# 6 Potential Explanations

The main results show state dependence exists after the shock. That is, in consumers' online sequential data-consent decisions, their current choices are affected by their past choices. After being treated by the need to use a portable power bank app, consumers reduce their rejection rate on subsequent data requests and are more willing to give out data in the following privacy decisions.

There is not a literature focused on state dependence in privacy per se. There are, however, a number of papers that provide possible mechanisms for the state dependence that I document. In this section, I demonstrate that the established state dependence effect 1) is consistent with a psychological theory where consumers retrieve a prior similar choice from their memory as a signal of their preferences; and 2) is more significant when the individual has a weak preference for the app at the time of decision. Equation (1) in the conceptual framework provides the intuition that when a consumer has well-defined preferences in data-sharing deliberation, the incremental utility from state dependence on decision at time $t$ is minimal. I will also provide evidence that indicates this effect is not a change in privacy preferences, data protection efforts, or risk perceptions.

**Memory: Decaying Effect.** Lynch and Srull (1982) introduce the concept of "stimulus-based choice" to describe situations where all relevant information is available and processed at the time an individual makes decisions. On the contrary, a pure "memory-based choice" happens when individuals make decisions without any present information, relying solely on memory. However, most real-world choices are a blend of both. That is, the customer uses a combination of remembered information and options, and information available at the moment of decision-making. A rich stream of literature has shown that past attitudes and elicitation of preferences, as well as choices, can influence subsequent behavior. A particularly relevant theoretical model for understanding these dynamics is the Accessibility-Diagnosticity framework (Feldman and Lynch (1988)), which

years. Only 1,829 users have been treated, while a larger portion of the newly registered users were never treated in the sample period.

underlines that past choices, when highly accessible, can serve as indicators of preferences and thus be repeated (Biehal and Chakravarti (1983)). The accessibility of past choices declines as memory recall fades over time, thereby reducing the likelihood of repeating these choices, thus attenuating state dependence (Alba et al. (1992)). For accessibility, I demonstrate that state dependence intensifies when the temporal gap between a past choice[31] and the current decision is shorter.

**Strength of Preference.** The role of preferences is a simple reflection of the marginal impact of state dependence on choice. If preferences are strong, then a one-time shock will have little impact on behavior. This is consistent with the discussion in Webb et al. (2024), that persistent preferences and behaviors may take time to form, and that a short-term interruption in consumption will not sufficiently disrupt established habits. In choice settings, Roy et al. (1996) lay out a similar framework where state dependence incentivizes trials, while strong habit persistence (i.e. strong preferences) hinders consumers from switching brands. Similarly, Levine and Seiler (2023) found that strong preferences persisted despite an exogenous shock disrupting a single purchase. The focus on marginal effects remains on whether the strong preferences are driven by the benefits or the costs of data sharing. Thus, if consumers appear to have strong preferences in their data consent decisions or hold a clear stance in privacy deliberation then measured state dependence is likely to be small because a one-time shock should have little effect on their following choices.

**Data Permanence.** This relates to the idea that once a consumer discloses a particular type of data on the platform, the consumer may no longer see a benefit to protecting that data further. As Bonatti (2024) notes, data is non-rival and so secondary usage can be repeated without degrading the quality of the initial data disclosure. Therefore, if consumers perceive that disclosing data to one app effectively implies disclosure to all apps, then consumers will only be privacy-protective the first time they disclose a particular type of data.

**Risk Perception.** The lowered rejection rates could emerge from consumers observing that data sharing carries no adverse effects, thus lowering their risk perception after the shock. Lin (2022) shows consumers' beliefs on the consequences of sharing data matter for understanding context-dependent privacy preferences. Risk perception refers to the user's perception of potential negative consequences after data disclosure (Tucker (2024)).

[31] In my empirical setting, the past choice for treated users is their choice during the treatment.

### 6.1 Memory: Decaying Effect

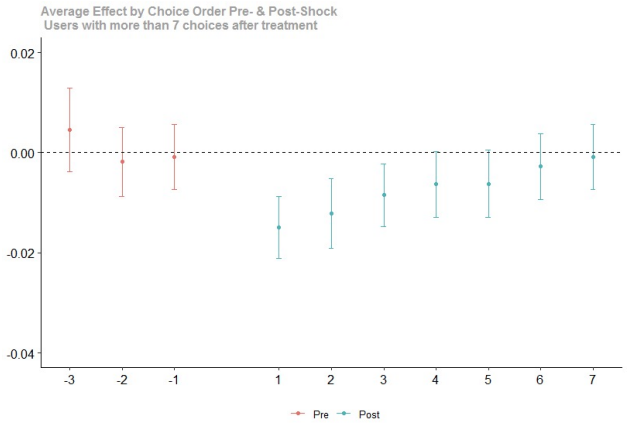
The Accessibility-Diagnosticity framework (Feldman and Lynch (1988); Lynch et al. (1988)) predicts that diminished memory recall over time reduces the likelihood of repeating these choices, and thus, the magnitude of the state dependence effect. The magnitudes of the estimators in my findings decrease with the number of choices post-treatment (Figure 5 and Table 1). This is consistent with what the framework predicts.

To further investigate this decreasing trend and the length of this effect, I sub-sampled users with more choices after the treatment in the data period. Specifically, the subsample with users making more downloads, and therefore the corresponding data consent decisions, follow the same analysis using Callaway and Sant'Anna (CS) estimation:

$$Pr(d_{it} = 0) = \alpha_i + \lambda_t + \left( \sum_{\tau \in \{-3,\ldots,7\}} \gamma_\tau \cdot D_{i,\tau} \right) + \epsilon_{it} \tag{4}$$

Figure 6 shows that the average effect size decreases with each choice after the treatment. In this sub-sample of users having more than 12 choices, the average effect goes to null after the 7th choice post-treatment. Therefore, consistent with a role for memory, the effect of the treatment decreases over time.

Figure 6: Decaying estimate with more choices




Note: This figure plots the coefficients and 95% confidence intervals of the impacts of acceptance in out-of-battery shock on rejection probability using Callaway and Sant'Anna (2021). The estimates decay across choices post-shock.

In addition to showing the decaying effect of choices, I also look into the role of memory by examining the duration between choices. I classify users into "short-duration" versus "long-

duration" groups, depending on whether their first post-treatment choice occurs within a week.[32]

Assigning control groups to these "short" and "long" groups presents challenges, as it is difficult to pinpoint the first post-treatment choice for control individuals. To address this, I narrowed the focus to treated users who received their treatment at their fifth choice. Consequently, control users can be divided into two duration categories based on the interval between their fifth and sixth choices.

Figure B7 in online appendix demonstrates that the main effect is much larger for the users who have a short duration between the shock and their first choice following the shock. More specifically, if the next choice following the shock happened within a week, then the magnitude of the effect ($-0.0382$) is more than twice the magnitude of the group with duration over a week ($-0.0185$).

As the choice of one week as a threshold is necessarily arbitrary, I present the coefficients of CS estimators in Table 2 to account for the threshold with various durations between the treatment and the first post-treatment choice. For three days as the threshold (82% of observations), the rejection rate drops by 20% (i.e. a 2.2% drop from the 11% baseline); for seven days (75% of observations), it drops by 17.6%; for three weeks (50% of observations), the effect remains; but by five weeks the effect is insignificant.

Table 2: Decaying effect with duration after the shock

| | (1) 1st (same day) | (2) 1st (> 1 day) | (3) 3rd (< 2 weeks) | (4) 3rd (≥ 2 weeks) |
|---|---|---|---|---|
| Treated × 1st Choice Post | -0.049*** | -0.018*** | -0.044*** | -0.021*** |
| | (0.008) | (0.003) | (0.014) | (0.003) |
| Treated × 2nd Choice Post | -0.018** | -0.016*** | -0.035*** | -0.016*** |
| | (0.009) | (0.003) | (0.014) | (0.003) |
| Treated × 3rd Choice Post | -0.011* | -0.013*** | -0.034*** | -0.012*** |
| | (0.010) | (0.003) | (0.015) | (0.003) |
| Observations | 60,053 | 706,162 | 36,799 | 729,416 |
| Individual FE | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes |
| Clustered error | user | user | user | user |
| *Note:* | | | | baseline rate pre-shock: 14% for all groups |

Thus, the magnitude of the state dependence effect diminishes with each subsequent choice following the initial shock; and the effect size decreases as the time interval between a previous and

[32]The rationale for defining "short" versus "long" duration as one week stems from the distribution of intervals across all observations. The instances where the duration is shorter than one week account for approximately 25% of the observations. The median duration is three weeks.

current decision increases. These findings, which highlight the decaying nature of the effect over time, suggest the role of memory in state dependence and that the effect is temporary.

## 6.2 Strength of Preference

The strength of preferences directly influences the marginal effect of state dependence on choice. Webb et al. (2024) suggests that a single, isolated shock will have minimal impact on switching behavior when preferences are habitual and enduring. The Accessibility-Diagnosticity framework also implies that individuals are more likely to retrieve past choices to help current decision when they do not have a strong or clear preference in the current context. To evaluate the role of preference strength, I examine the heterogeneous effects across both app usage contexts and user attributes.

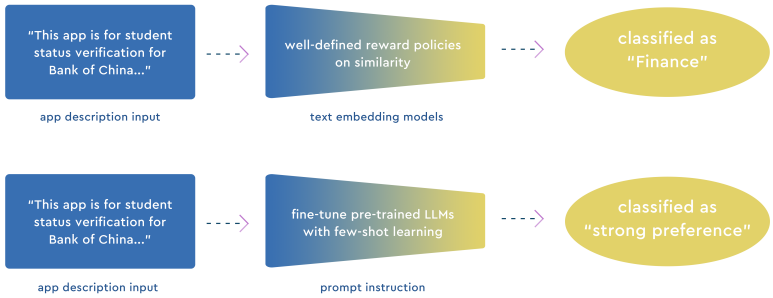
### 6.2.1 State Dependence is Stronger When the First Post-Shock Choice is in Weak Preference Contexts

The existing descriptive statistics show varying rejection rates across app categories (online appendix Table C8), suggesting that individuals could follow different decision strategies depending on the context. To further explore the mechanism of this observed state dependence between choices, I next examine whether and how the magnitude of the estimated state dependence shifts when the context differs, specifically, based on a proxy for the strength of preferences.

The context of the decision may affect the estimated state dependence. In scenarios involving data-consent decisions in app downloads and installations, relevant context factors may include situational factors such as location, use case, and urgency. In my analysis, I use large textual data of firms' service descriptions to create a proxy for the likely strength of preference for the particular app conditional on being sufficiently interested in the app to download it.

In order to do this large-scale classification task, I leverage recent advances in large language models (LLMs). With LLMs, it is now possible to classify large quantities of text through clearly articulated prompts. Unlike other machine learning techniques to categorize mobile apps such as Random Forest classifier (Ershov (2024)), or embedding techniques such as Sentence-BERT (Reimers and Gurevych (2019)) to measure the similarity between patents and app descriptions, LLMs leverage in-context learning, or few-shot learning, where the model learns a natural language instruction and a few demonstrations of the task. This new learning paradigm allows the model

Figure 7: An Illustration of the Advantages of LLMs on Capturing Situational Factors




to adapt to further instances of the task. The primary advantage of GPT-3 and GPT-4 is that the models are trained to follow instructions presented in prompts, shifting the researcher's task to instruction tuning (Ouyang et al. (2022)).

This flexibility in LLMs allows me to develop a nuanced definition of situational differences in privacy decisions in interactions with apps. Specifically, current embedding models have their own well-defined reward policies to measure bag of words and to provide similarity measures. However, keyword similarity does not always correspond to the same level of user preference strength.

For instance, a restaurant's app for menu exploration and payment might show high textual similarity with a restaurant discovery app, yet these contexts are categorized as different levels of preference strengths in my rubric (detailed below). A restaurant discovery app might exhibit low textual similarity with a job search app, but my rubric groups them together as same group of preference strengths due to their functional and situational similarities. Figure 7 illustrates an example of how the app for student status verification purposes could be classified as Finance, where most apps in this category fall in weak preference context in my definition. On the contrary, with detailed prompt instructions and examples, the LLMs classified the app as strong preference context.

At the time of labeling, my analysis included models in the GPT-3.5, GPT-4, and GPT-4o families. The difference between the models lies in the contextual handling. GPT-4 and GPT-4o family handles subtle distinctions and complex decision trees more effectively, which is essential for my task where multi-step classification rules need to be applied accurately. Generally, in-context learning models provide better tools for understanding the nuances between contexts. I follow similar steps in Eloundou et al. (2024) for instruction tuning and data labeling.[33]

[33]The rubric is derived from (Brand et al. (2023)), using ChatGPT as a research tool.

To construct my primary classification to proxy for preference strength conditional on downloading, I collected both human annotations and GPT classifications, using a prompt tuned for agreement with a sample of labels from three individuals (including the author). I observe similar agreement levels in GPT responses and between human and machine evaluations.

- Human Ratings: The author and two other human annotators (undergraduate and graduate research assistants) labeled an extensive set of service descriptions to the context of preference strength.[34] Three human annotations are mapped to a single classification standards.
- GPT-3.5 Ratings: Agreement rate is 72.8%.
- GPT-4 Ratings: Agreement rate is 84.0%.
- GPT-4o Ratings: Agreement rate is 90.0%.

I propose a new rubric for service contexts and the summary of the rubric is listed below, while the complete rubric can be found in Online Appendix Appendix A. I divide apps into strong preferences, weak preferences, and lottery. I include lottery as a separate category because I anticipated that they would not fit well into the descriptions I gave for either strong or weak preferences.

**Summary of Service Rubric**

- **Strong Preferences if:**
  - services under strong preferences are often on-site necessities services, which cover consumers' standardized tasks and are largely repetitive;
  - the exclusive justification for downloading the designated app arises when a user is physically present at the specified facility, as the services provided by the app are confined to this particular location and are not applicable elsewhere;
  - if a consumer is at the facility, there is no space for outside options – meaning the main activities can not be achieved without using the app.
- **Weak Preferences if:**
  - services under weak preferences are often value-added services, which provide additional

[34] Human annotations are performed on a sample of 800 apps. This sample is labeled to check the agreement rates. The full sample contains 7,321 apps in total.

benefits and supplementary value but are not critical for routine life;

- there is no need to be physically present at a specified facility to justify the app download;
- if a consumer is at the facility, there is relative space for outside options – meaning the main activities can still be achieved without using the app.

- **Lottery if:**
  - apps under this context are the ones for marketing campaigns, coupon, cashback, or lottery purposes;
  - this category is specialized in a lottery and gambling nature that does not fit in either strong or weak preferences

Table 3: Heterogeneity in Context – Weak versus Strong Preferences

| | (1) Weak Preference | (2) Strong Preference | (3) Lottery |
|---|---|---|---|
| Treated × 1st Choice Post | **-0.037*** ** | **0.008** | **0.014** |
| | (0.007) | (0.005) | (0.016) |
| Observations | 147,349 | 299,586 | 54,114 |
| Individual FE | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes |
| Clustered error | user | user | user |
| *baseline pre-shock rej_rate | 11.80% | 10.98% | 14.31% |

Note: Preference strength (weak vs. strong) was based on categorization by a large language model (LLM) on service description.

I present Table 3, grouping users by the preference strength of their first post-shock app choice according to the defined rubric.[35] Similar to the between-choice duration analysis, identifying the "first post-treatment" choice for control individuals is challenging. To address this, I focus on treated users who encountered their treatment at their fifth choice. Consequently, the control for their "first post-treatment" choice is the sixth choice from the control group.

Column (1) shows the state dependence effect is statistically and economically significant for users interacting with weak-preference apps in their sixth choice – the low-battery acceptance shock reduced their rejection probability by an average of 3.7% (31.4% reduction from the baseline). Column (2) reveals that the state dependence effect is absent for strong-preference apps. I include

[35]Online Appendix Figure B10 presents an illustration of the scenarios of the classification where most apps in a category fall in. Pictures are generated by GenAI.

column (3) for completeness.[36] This difference in rejection rates is about the apps rather than the users. There are no systematic differences in the pre-shock baseline rejection rate between individuals in the weak and strong preference groups at their sixth choice.

Overall, column (1) and (2) in Table 3 imply that strong habitual preferences hinder consumers from switching, where we would not expect state dependence to happen after one shock (Roy et al. (1996)). The impact of state dependence is most pronounced among consumers who are on the margin. In weak preference contexts, users are more likely to base their decisions on their most recent choices. This aligns with psychological theories that for decision problems requiring deliberation (i.e., weak preferences in the current setting), individuals are more likely to rely on heuristics derived from past choices (Payne et al. (1993)).

### 6.2.2 State Dependence is Stronger for Users Who Do Not Have Strong Preference on Privacy

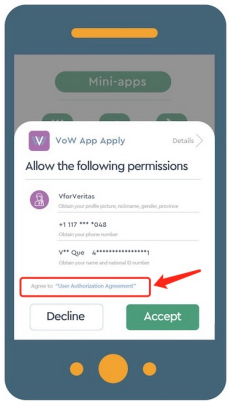
This section explores the heterogeneous effect of the observed state dependence by the preference strength for privacy by users. I proxy for user privacy preferences with whether the user clicked on the "user authorization agreement" (illustrated in Figure 8) ever before any data consent decision. The "user authorization agreement" specifies the legal terms that regulate the apps on the data they collect. Users who clicked consist of 5% of the population. This preference strength for privacy is treated as a fixed trait across individuals.

Table 4 shows that the state dependence effect is insignificant for users who read privacy terms (column 1), while the significance and magnitude of the effects hold for the rest of the users. Note that users who examined privacy information have a slightly higher baseline rejection rate (19.5% vs 14.3%), but online appendix Figure B3Panel A shows they are not systematically located on the right tail of the distribution.

In summary, the heterogeneity in users' level of engagement with privacy deliberation, serves as a critical indicator of strong or weak preferences for privacy. In line with Webb et al. (2024), consumers with stickier and stronger preferences are expected to exhibit negligible state dependence effects.

[36] I urge caution in interpreting the result in Column (3) due to the high variance in rejection rates within this category, which may contribute to the statistical insignificance.

Figure 8: An Illustration of the Privacy Agreement that Attentive User Clicks On




Note: This figure demonstrates the interface of "user authorization agreement" (i.e. privacy agreement). This link appears in blue color above the binary choice buttons. It appears on every consent window.

Table 4: State dependence by whether user read privacy terms

| | (1) Users who read privacy terms | (2) Users who did not read privacy terms |
|---|---|---|
| Treated × 1st Choice Post | -0.007 | -0.022*** |
| | (0.016) | (0.003) |
| Treated × 2nd Choice Post | -0.011 | -0.016*** |
| | (0.015) | (0.003) |
| Treated × 3rd Choice Post | -0.005 | -0.013*** |
| | (0.017) | (0.003) |
| Observations | 31,241 | 734,974 |
| Individual FE | Yes | Yes |
| Choice-order FE | Yes | Yes |
| Clustered error | user | user |
| *Note:* | baseline rate pre-shock: 19.5% (attentive) and 14.3% (inattentive) | |

## 6.3 Alternative Explanations for the Observed Effect

I explore potential alternative explanations for the observed effect where treated users lower their rejection rates post-treatment.

Consumers may reduce data protection efforts for information already exposed to third-party apps, as the perceived cost of sharing diminishes after initial disclosure, reflecting the non-rivalry of data (Bonatti (2024)). Risk perception, which refers to users' beliefs about the potential negative consequences of data disclosure in my setting, plays a role in shaping context-dependent privacy preferences (Acquisti et al. (2013); Lin (2022)). A shift in risk perception, influenced by the

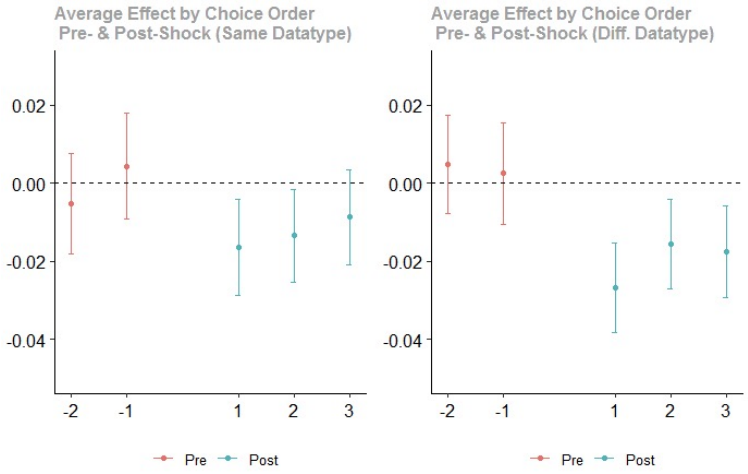
absence of negative consequences after providing data, may also increase willingness to disclose data. However, I do not find empirical support for the arguments that state dependence is related to data permanence and risk perception.

### 6.3.1 Data Permanence

If consumers believe disclosing data to one app implies sharing it with all, they are likely to protect privacy only during the first disclosure of a specific data type due to its non-rival nature. To test if state dependence stems from data permanence, I analyze whether consumers guard previously shared data less closely in subsequent decisions after the treatment. I hypothesize that state dependence should only apply to the same data type in the treatment ($data_{d_j}$) in future choices, not different types ($data_{-d_j}$). I examine this hypothesis in two separate analyses.

First, I categorize users into two groups: those encountering the same or fewer data types ($d_g$ where $g \in J$) as in the treatment ($d_J$ where $J = \{1, ..., J\}$), and those facing new data types ($d_k$ where $\exists k_0$ s.t. $k_0 \notin J$). To address the challenge of assigning controls for the first post-treatment choice, the analysis is confined to users who received their treatment at their fifth choice. I compare their sixth choice. If consumers relinquish protection after compulsory consent, treatment effects should be stronger for the same data types, and the effect is expected to be small or potentially negligible in scenarios containing new data types. However, results in Figure 9 show significant effects across both circumstances (and, if anything, effects are smaller when the same data type is requested).

Figure 9: Test for data permanence: State dependence exists for both conditions




Note: This figure displays event study coefficients and 95% confidence intervals for changes in rejection probability, using Callaway and Sant'Anna (2021). The left panel shows the treatment effect for users whose post-shock choice includes the same or fewer data type combinations than those requested in the treatment, while the right panel reflects the effect for users with additional or different combinations.

Second, I examine decisions on choices with all other data types without focal data types—profile information, phone number, and credit information—requested in the portable power bank app treatment. Using staggered difference-in-differences estimation, I analyze pre- and post-treatment choices excluding the focal data type asked in the treatment. Results in online appendix Figure B8 show state dependence effects persist in subsequent choices when data types other than profile information or phone number (in the treatment) are asked. Overall, they suggest state dependence in sequential privacy decisions is data-type agnostic.

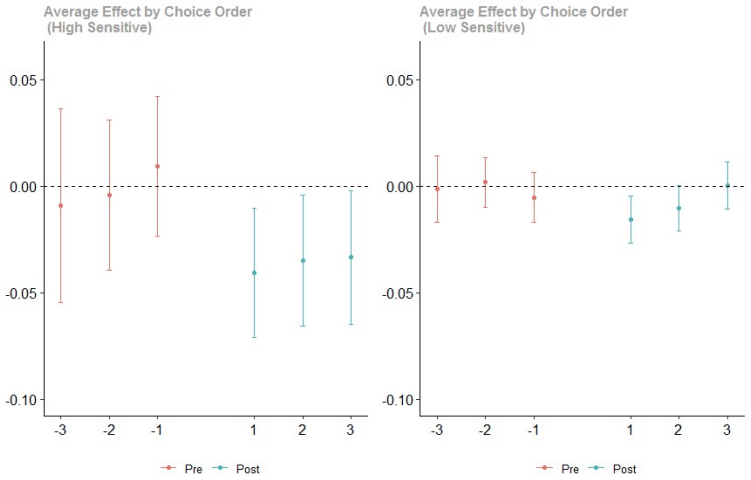
Additionally, if data permanence drove this effect, the main effect should not decay with choice order and duration. Overall, I do not find evidence supporting data permanence as the basis for state dependence.

### 6.3.2 Risk Perception Adjustment

This explanation posits that consumers who share data with portable power bank apps and observe no adverse outcomes may reduce their perceived risk, leading to lower rejection rates in subsequent data-consent decisions. If the theory holds, the treatment should exert a more substantial impact on those with higher risk perceptions. To test this, I proxied risk perception using historical pre-

panel rejection rates (Sep 2019–Feb 2020),[37] categorizing users who previously ever rejected data requests as high-risk (18% of the sample).

Figure 10: Similar magnitudes of effect across groups




Note: This figure plots the coefficients and 95% confidence intervals from event study regression of change in rejection probability using Callaway and Sant'Anna (2021). Left panel: treatment effect on high-risk-perception users; right panel: treatment effect on low-risk-perception users.

Event-study plots (Figure 10) show larger absolute effects for the high-risk group, but relative drops in rejection rates are similar (17.2% for high vs. 15.8% for low). The result reveals that the magnitude of adjustment does not differ significantly between users with initial high and low rejection rates. The decaying treatment effect further implies that the mechanism does not involve a permanent change in beliefs about the riskiness of providing data.

Together, the evidence does not support risk perception changes as the primary mechanism.

# 7 Conclusion

This paper provides empirical evidence that consumers' digital privacy choices exhibit structural state dependence. To address the challenge where researchers rarely observe individual data-consent decisions from field data, I use large-scale individual-level data from Alipay, in which consumers consistently engage with apps from third-party providers. To identify state dependence, I leverage a quasi-experiment in which consumers are highly likely to accept data requests due to the need

[37]The firm began recording consent data in the initial months, but due to missing values, this data was excluded from the main analysis—though it is sufficient to assess the risk perception among recorded users.

of immediate charging. The staggered difference-in-differences estimates suggest a 15% reduction in the rejection rate of subsequent data-consent choices, and this effect is significant for at least 3 choices after treatment. The shock leads to a temporary change in the consent decisions of the data. The rejection rate eventually goes back to baseline, suggesting that people will ultimately hold on to their well-calibrated privacy preferences.

I examine the roles of consumer memory and preference strength in driving the identified state dependence effect. First, I provide evidence that the magnitude of state dependence is greater when the current choice is closer to the treatment choice, aligning with the accessibility-diagnosticity framework. Second, I find that users with apparently weak preferences in app usage contexts exhibit significant state dependence following the shock, while those in app contexts with apparently well-calibrated preferences do not show this effect. Furthermore, the role of preference strength is supported by the analysis of user traits, revealing that users who examine privacy terms do not demonstrate state dependence after the shock.

Alternative explanations could explain the decline in rejection rates among treated individuals. One theory suggests that data permanence leads consumers to abandon data protection efforts for data types previously exposed to third-party apps. However, state dependence persists even for unexposed data types, indicating that consumers do not completely abandon data protection efforts. Another explanation assumes that consumers adjust their perception of risk when they experience no negative consequences after data disclosure. However, comparable effects across high- and low-risk groups discount the notion that the effect is driven by shifts in risk perception. Overall, the decaying effect over time suggests that state dependence is not a function of abandoning data protection or changing risk perception.

Recognizing state dependence in consumers' privacy decisions suggests an important aspect of platform privacy strategy. Data consent decisions on one app may change the ability of other apps to collect data from the same user. App-specific data decisions create an externality similar to the reputation externality highlighted in Nosko and Tadelis (2015). The presence of structural state dependence implies that early consumer interactions can shape the trajectory of data sharing across the ecosystem.

Any discussion of specific strategies to address this externality is necessarily speculative. For example, leveraging the result on preference strength, when the externality is negative, the platform could incentivize apps in consumers' strong preferences to mitigate the effect. Conversely, when the

externality is positive, the platform could promote apps in low-urgency contexts, such as bundling apps in sequence. Such strategic designs, which could be implemented in the app recommendation algorithms, can serve as a lever to balance the tradeoff between data collection and consumer privacy. While I did not test these specific data strategy designs, future theoretical and empirical research in this area is welcome. More generally, the state dependence documented in this paper suggests an incentive for platforms to encourage consumer-friendly data request designs.

To the extent that the state dependence across apps would also apply to data consent decisions within an app, the results may also affect scenarios where a single app engages the same consumer with multiple sequential data requests. For example, an app may benefit from asking for data in sequence rather than asking for data all in once.

Furthermore, the temporary nature of the estimated state dependence suggests that underlying privacy preferences may be relatively stable, at least for the Alipay users and the decision environment studied here. This contrasts with assertions by various policymakers and legal scholars that consumers do not a have well-established understanding of privacy, and therefore privacy preferences fluctuate rapidly (Tucker (2024)).

I acknowledge several limitations to the study. First, the main data lacks records of the initial choices for all consumers. Therefore, the analysis does not build from true initial conditions. That is, the platform did not start collecting consumer decision data when it introduced the current data-consent rule where consumers exchange their data explicitly for app services. Second, for the main analysis, the identified state dependence is observed only in the three subsequent choices following the treatment. Examining the long-term effect is therefore limited to a relatively low-powered analysis of a subsample of the main data. Furthermore, the implications for platform strategy with respect to externalities are inferred, rather than directly tested.

This paper demonstrated that past privacy choices affect current privacy choices. Such state dependence implies an externality in the data requests across apps. Ignoring this externality may affect the ability of apps and platforms to collect data. Overall, the evidence presented in this paper suggests a new perspective on managing consumer data consent designs in platform ecosystems—one that accounts for both individual decision processes and the externalities across apps.

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# Online Appendix: Supplementary Materials

April 8, 2025

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# A Service Rubric

# Service Taxonomy

As an analytical expert in service categorization, your task is to classify the main services provided by the given company descriptions into one of three categories (S0, S1, S2).

Use the following context and information for categorization:

Assume you are a consumer with an average income level and have been living in your city for more than 5 years. You have a strong preference for services (S0) that, conditional on your downloading, you are in an urgent situation to use them, such as public transportation, bike sharing, or car sharing; vending machines, utility payment channels, or electric car charging stations. However, for other services, here denoted as services with weak preference (S1), probably recommended by your friend, you may find them value-added, but not with strong preferences at the moment downloading– meaning you will find it valuable or time-saving to use, but it won't disturb your life if you finally decide do not use them.

To distinguish between services of strong preference (S0), services of weak preference (S1), and lottery-nature services (S2), use the following thought process:

- Step 1: Determine whether physically presenting at the facility of the focal app is the only way that makes sense to justify downloading the app.

- Step 2: If the consumer is already standing in front of the facility, determine how much outside option they have if they decide not to use the app.

In Step 1, if the user must be physically present to justify the download, move to Step 2. If not, and this is not a lottery-nature app, the service can be under weak preference (S1). In Step 2, if users have relative space for outside options, the app is labeled S1. If not, it's labeled S0.

In Step 1, there are two scenarios about you downloading an app: First, you might be standing in front of a service infrastructure. Without downloading and using the app, you are not able to use the service. Therefore, the use of the app directly leads to the viability of your next move. Second, you might be sitting at home downloading and exploring the app. The app does not directly lead to the viability of your next move.

For example, you are downloading an e-commerce app. You could be either downloading it at some random place outdoors or sitting at home exploring the brand. According to Step 1, this e-commerce app could be directly categorized as S1.

For instance, if you are downloading a car charger app. It only makes sense to download it when you need to use a specific brand of car charger at a random place. It does not make sense that you download it at home since you won't predict your car is running out of battery. Therefore, the car charger app belongs to the first scenario only. According to Step 1, being physically present at the facility is the only way that justifies the download, we could move to Step 2. In Step 2, not downloading the car charger app leaves little space for outside options. Therefore, the car charger app should be categorized as S0. For another example, let us look at an app for a mall directory or the electronic record system (ERS) of a hospital, the apps can make information search or data storage more efficient for the user. According to Step 1, it fits the description and we move to Step 2. The user can still achieve the original purpose of the focal activities, such as shopping in the mall or seeing a doctor, without using the app. Then the mall directory app and the ERS app are S1.

The key distinction is that apps under strong preference are integrated into the user's regular habits and serve essential needs, while apps under weak preference offer supplementary benefits or services that enhance the overall digital experience when required, but are not critical for the routine life operation.

Here is the taxonomy:

- ## S0 – Strong Preferences
  - Services under strong preferences are often on-site ncessities services. Following the two-step process above, if a firm's description of its main service can be seen as 1) physically presenting at the facility of the focal app is the only way to justify the download; and 2) without using the app, the user cannot continue his next step which is the direct intention of downloading the app. By satisfying the 2 steps, then it should be classified as S0.
  - This includes services such as: Electric Car Charger, Coffee and Dessert stores (QR codes for orders), Restaurants (QR codes for orders), Lockers or Vending Machines, Food or Floral Delivery, Parking, Public Transportation, Sharing bikes, Sharing cars, Car Rent, Utility Payment Channels, COVID QR code for mandatory checking.
- ## S1 – Weak Preferences
  - Services under weak preferences are often on-demand value-added services. These services provide additional benefits and supplementary value but are not critical for routine life. Following the two-step process above, if a firm's description of its main service can be seen as 1)

physically presenting at the facility of the focal app is not the only way to justify the download – meaning the user can be sitting at home exploring; OR 1) physically presenting at the facility of the focal app is the only way to justify the download but 2) the user has some space for outside options even without the app at the facility they attempt to download it.

  - This includes services such as: Brand online stores, Courier Service Tracking, E-Commerce, Financial Services, Gas Stations, Labor Service Search, Information Search, Productivity, Health Management, Hospital ERS, Luxury Rent, Mall Directory, Charity, Resell, Social, Tourist Spots Guide, SME Brands and Stores.

- ## S2 – Lottery-nature Services

  - Lottery-nature Services are the ones for advertising, marketing campaigns, or coupon purposes. People download the apps for lottery or play games to receive coupons or cashback. This category is less valuable in utility compared to S0 and S1, but is more hedonic.
  - This includes services such as: Entertainment, Marketing, Coupon, Games, and Lottery.

Please take into consideration the examples attached below:

## Annotation Examples:

Example 1:

Description: zFrontier Equipment Frontline is a geek equipment purchase and interactive community platform. By directly cooperating with designers and manufacturers, the platform allows members to enjoy high-end products at exclusive preferential prices. Users can also discuss and share geek equipment through the platform. Questions and answers are available online.

Label (S0/S1/S2): S1

Explanation: This app is an online community connecting designers and manufacturers. Implementing the 2-step process, physically presenting at the facility of the focal app is not the only way to justify the download – meaning the user can be sitting at home exploring. Therefore, this could be categorized as S1. Moreover, according to the taxonomy of S1, this app is mostly related to “Labor Service Search”.

Example 2:

Description: The name 85°C comes from ”coffee tastes best at 85°C.” Founded in 2003, ”85°C” is a fashionable catering company in Taiwan, China, focusing on coffee, cakes, and baking.

Label (S0/S1/S2): S0

Explanation: The app is a coffee chain shop. Implementing the 2-step process, without a need to order in a coffee store, a consumer would never download a specific app for this coffee chain. Therefore, we move from Step 1 to Step 2. Since ordering coffee is the only purpose to download this app, then a consumer does not have much space for outside options. Therefore, this could be categorized as S0. Moreover, according to the taxonomy of S0, this app is mostly related to "Coffee and Dessert stores (QR codes for orders)".

Example 3:

Description: Shengdianhua APP is an online shopping APP released by Guangzhou Nuomici Information Technology Co., Ltd. Shengdianhua cooperates with mainstream e-commerce platforms such as Taobao, Tmall, and Pinduoduo. Users can not only receive coupons for shopping, but also enjoy shopping cash-back, life privileges, and other rights after becoming members.

Label (S0/S1/S2): S2

Explanation: The app is a coupon/cashback app. Implementing the 2-step process, physically presented at the facility of the focal app is not the only way to justify the download, and the app is a lottery-dominated app. Therefore, this could be categorized as S2. Moreover, according to the taxonomy of S2, this app is mostly related to "Marketing" or "Coupon".

Example 4:

Description: Atour, a company with a comfortable smile. Atour is a new lifestyle hotel brand that advocates a humanistic, warm and interesting "on the road" fourth space lifestyle. The brand has four major products: Atour Hotel, ATHouse, THE DRAMA, and long-term rental apartments. Atour, providing guests with a high-quality life experience covering all areas such as accommodation, e-commerce, tourism, and finance. ATHouse is based in the urban core, leading the dynamic social space of urban travel. Each ATHouse is located in the core blocks of first-tier cities and provincial capitals. The public area of the hotel is named United Park (abbreviated as UP public area). It is not limited to the broad concept of socializing and urban living room. It will become a gathering place for young people to share the urban trend culture of travel life. This is a place that gathers trend culture, urban activities and active young groups. It provides coffee, wine, food and accommodation, shares and selects sales content, and

provides young people with novel travel experiences and quality life on the road. THE DRAMA is the first drama-themed hotel in China, dedicated to building a Shanghai Renaissance distribution center. THE DRAMA is located in the Jing'an Temple business district, located in SMG Shangyan Valley, adjacent to the world-famous immersive drama "Sleep No More". As an epoch-making hotel product, THE DRAMA brings together art peripherals such as drama, vinyl, and movies, and redefines hotel accommodation space through art exhibitions, markets, performances, etc.

Label (S0/S1/S2): S1

Explanation: This app is an all-round service of the Atour facilities. Implementing the 2-step process, without physically being at the Atour facility, a consumer would not download the app. Therefore, we move from Step 1 to Step 2. Different from Example 2, a consumer in this case does have some space for outside options. They could enjoy shopping and other activities even without this app. Therefore, this could be categorized as S1. Moreover, according to the taxonomy of S1, this app is mostly related to "Mall".

# B Additional Figures

Figure B1: Example screenshots of data-consent decisions

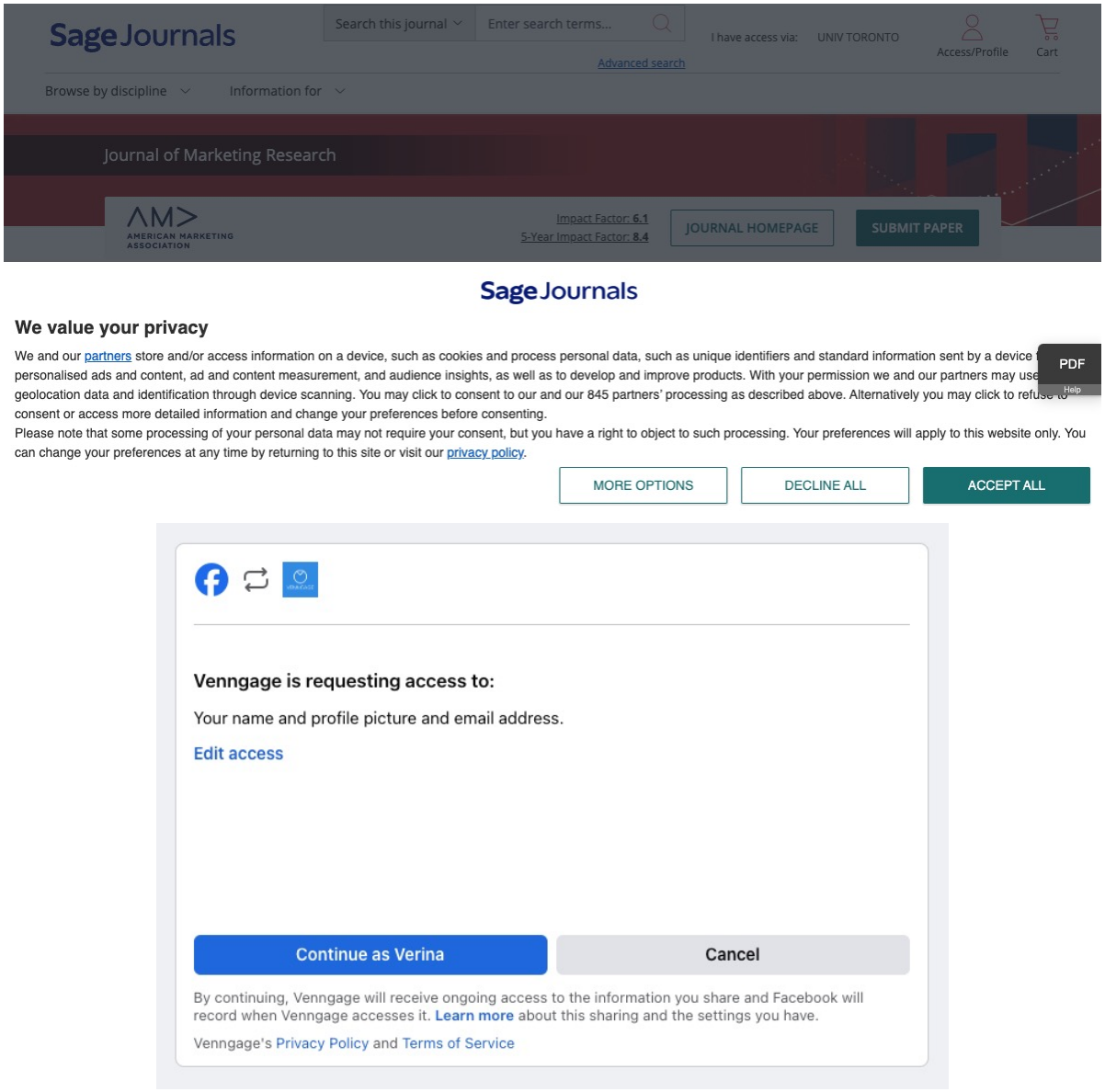
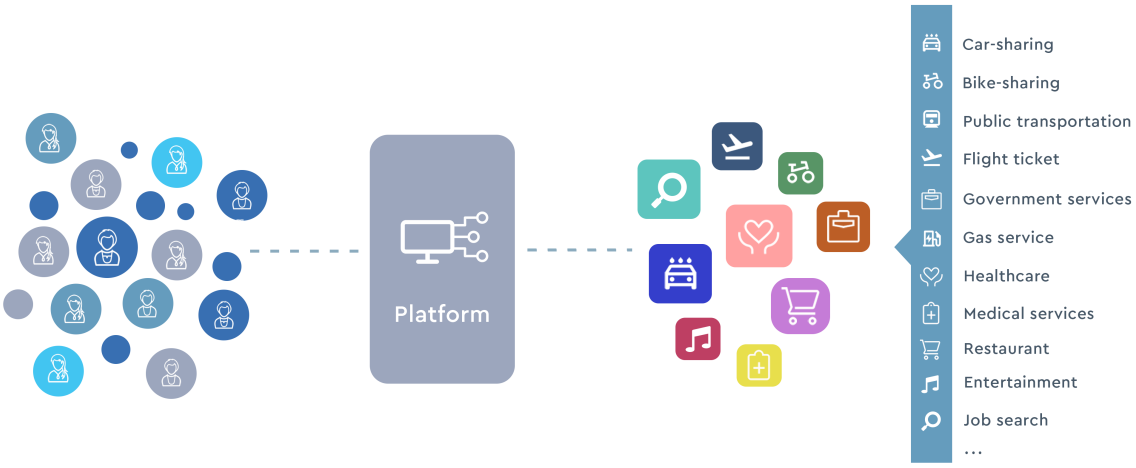


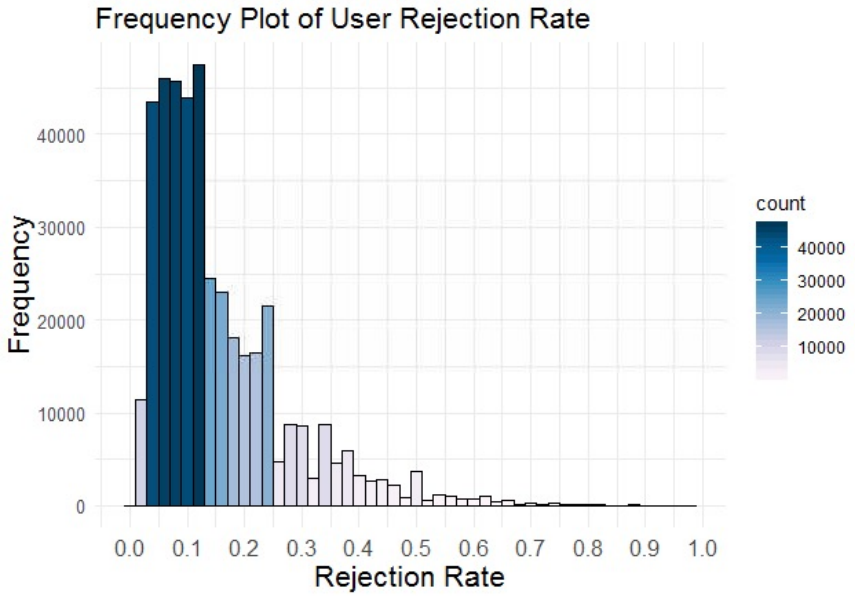
Figure B2: Empirical setting: the Alipay, with consumers and third-party apps on both ends




Note: The platform is the Alipay platform, which has hundreds of millions of consumers on one end, and millions of third-party apps on the other. The apps have a wide range of functionalities. The app providers range from big tech giants to individual merchants. The Alipay platform provides sophisticated infrastructure for small business owners or individuals to create apps on the platform.

Figure B3: Distribution of User Behaviors – User Heterogeneity

Panel A: distribution of user rejection rates




Panel B: distribution of number of apps a user downloads

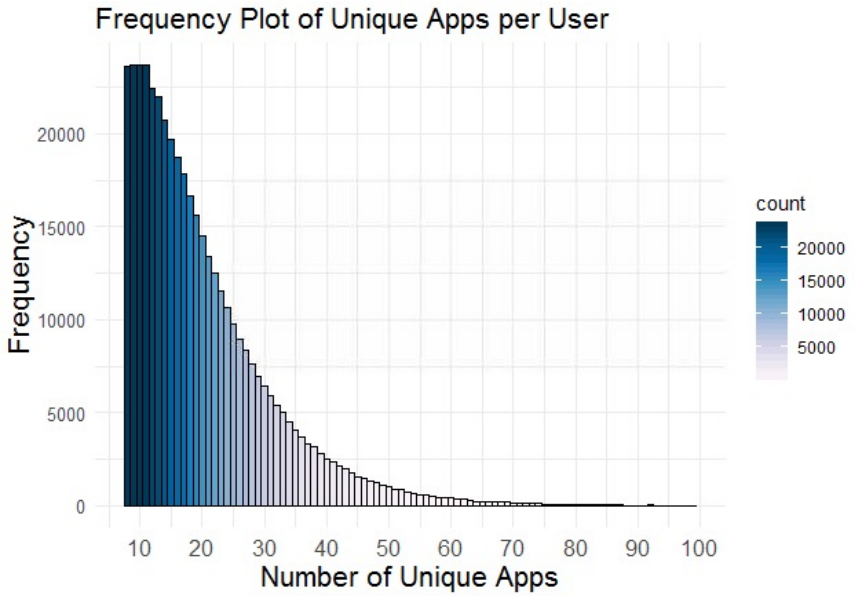
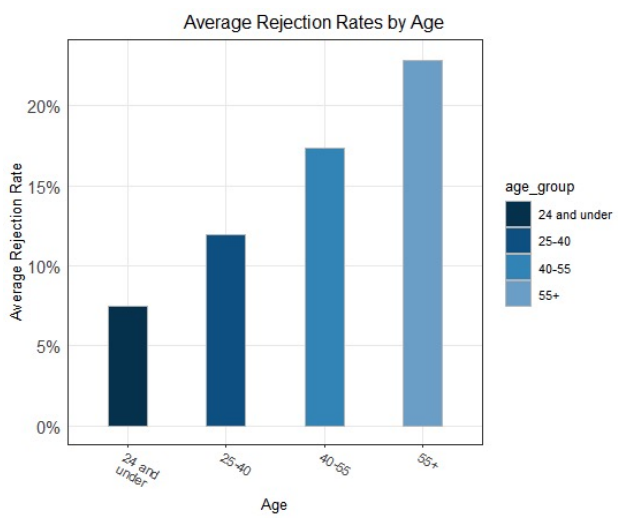
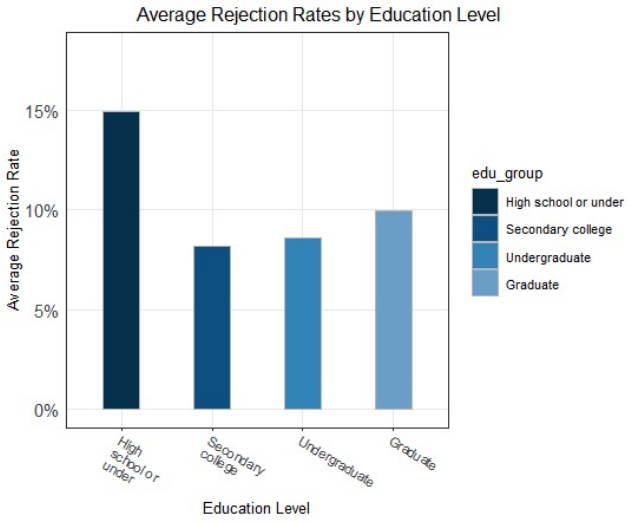


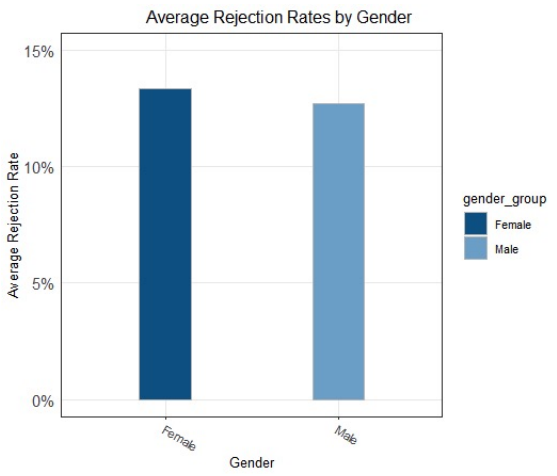
Figure B4: Heterogeneous Privacy Choices by User Demographics

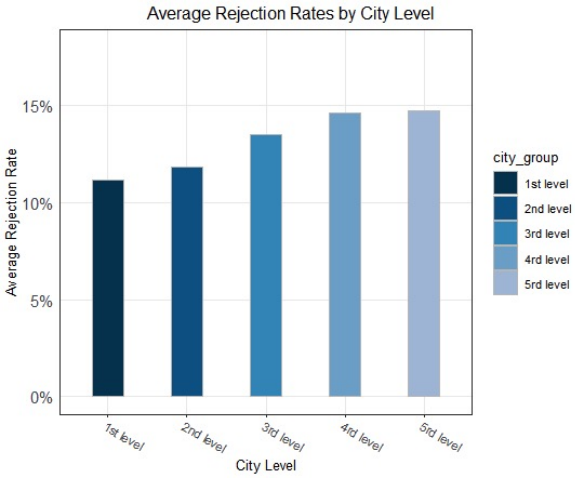
Panel A: Consumers are Heterogeneous in Privacy Choices across Age & Education Groups




Panel B: Consumers are Not Heterogeneous in Privacy Choices across Gender, City or Asset level



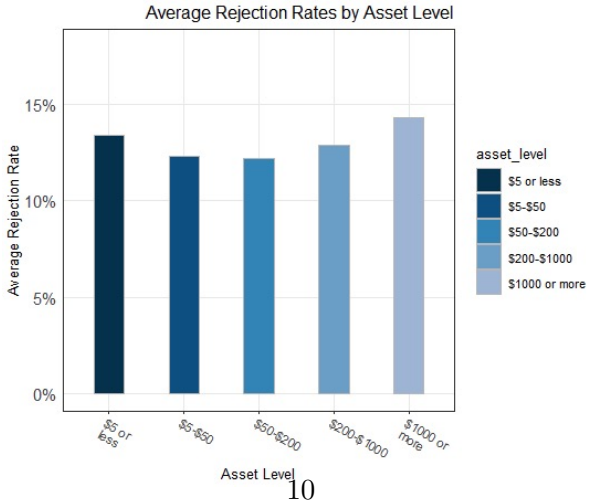
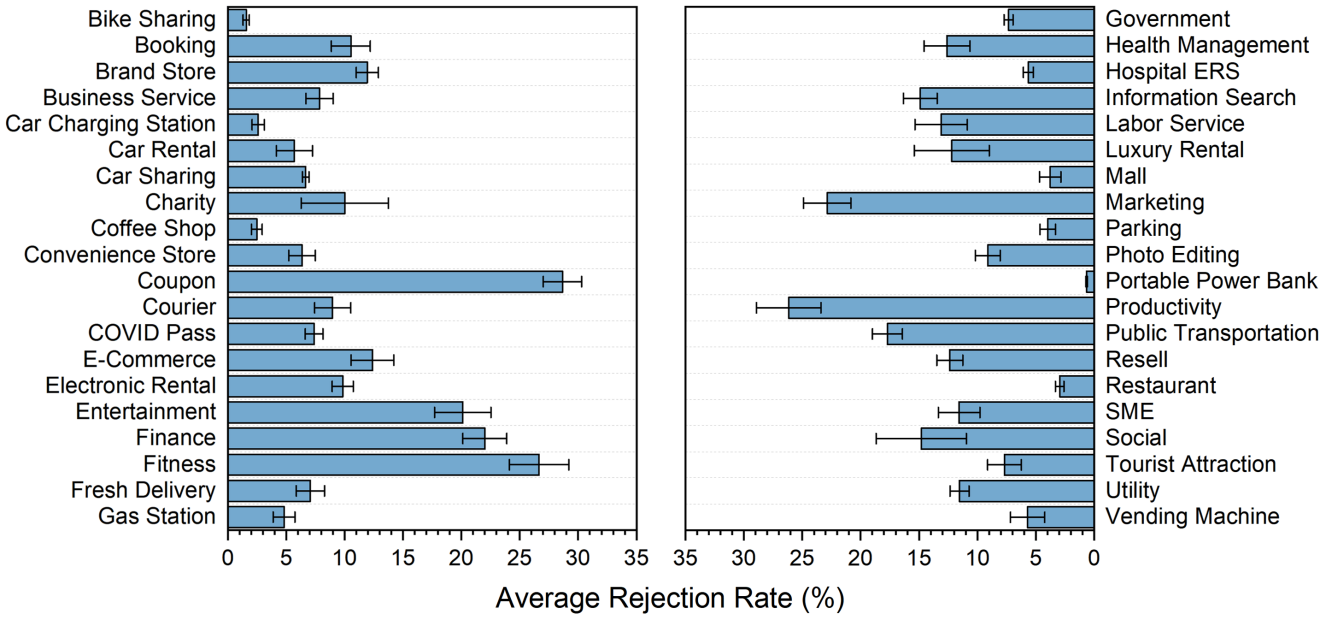



Figure B5: Consumer's Privacy Choices are Context-Dependent

Panel A: average rejection rate by app functionalities




Panel B: average rejection rate by datatype combinations

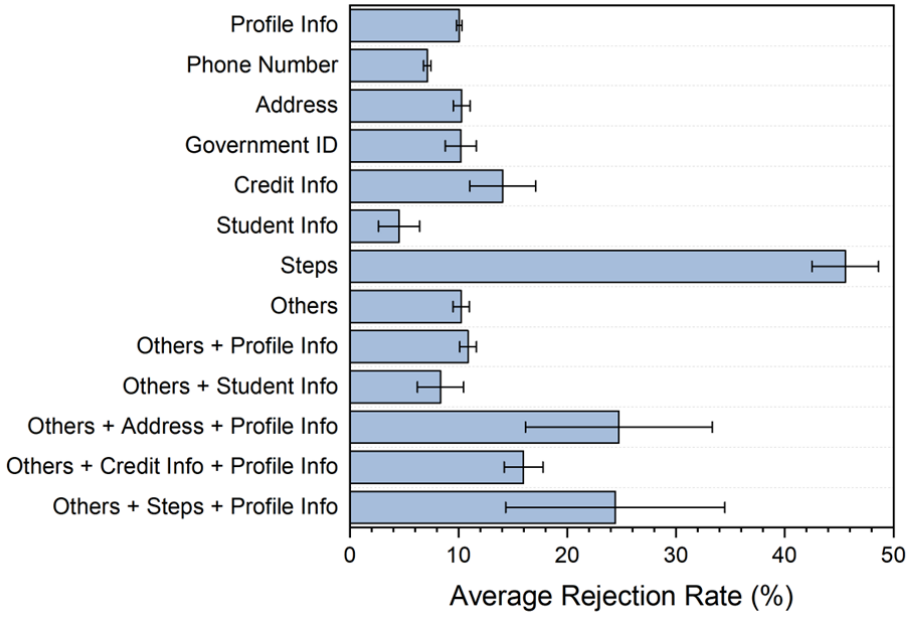
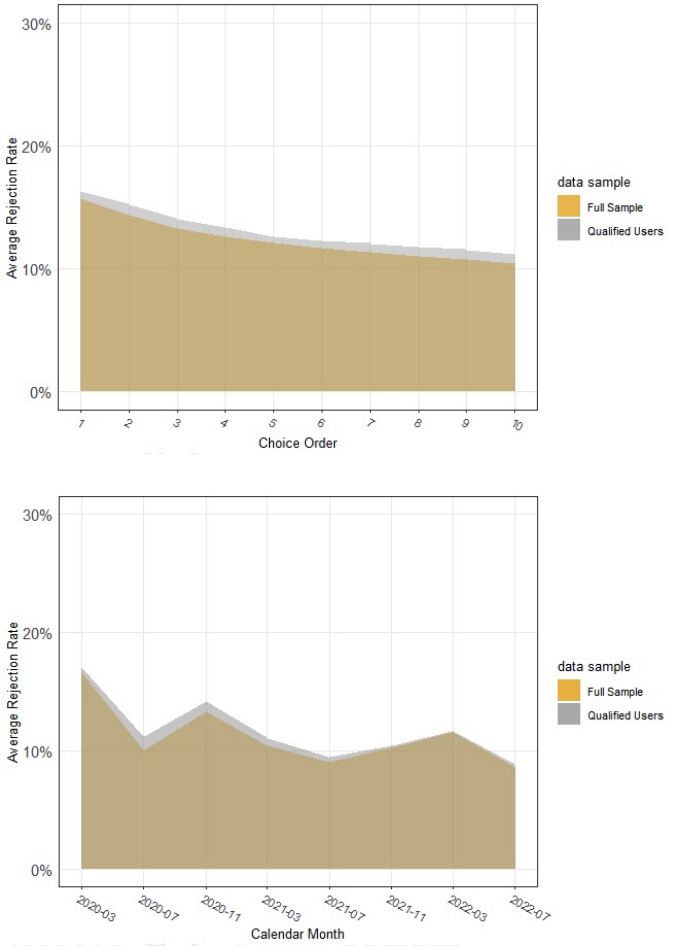


Figure B6: Average Rejection Rate Across Time and Choices

Panel A: average rejection rate across choice order & calendar time (month)




Panel B: average rejection rate by hour in a day & day in a week

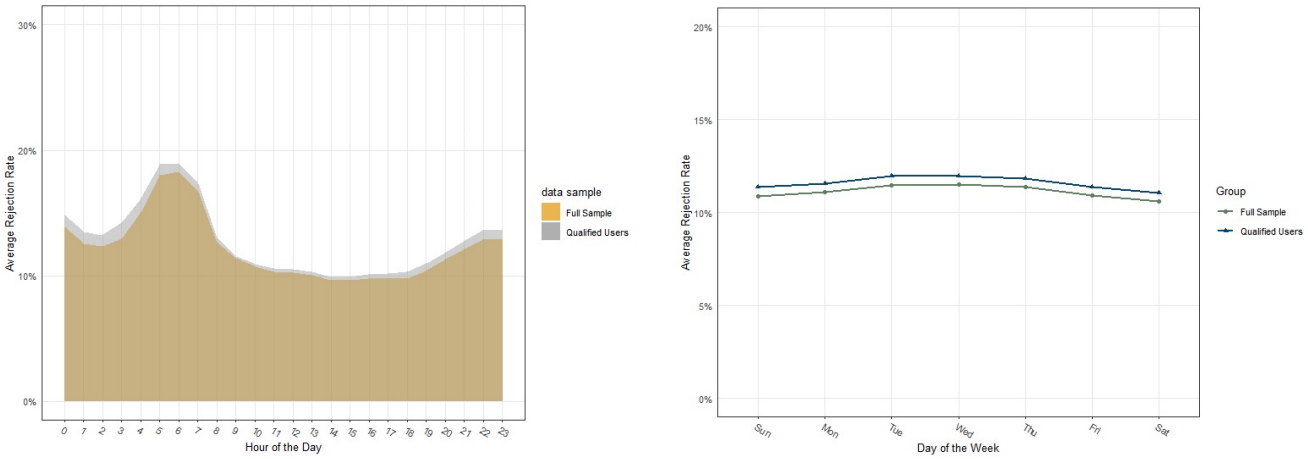
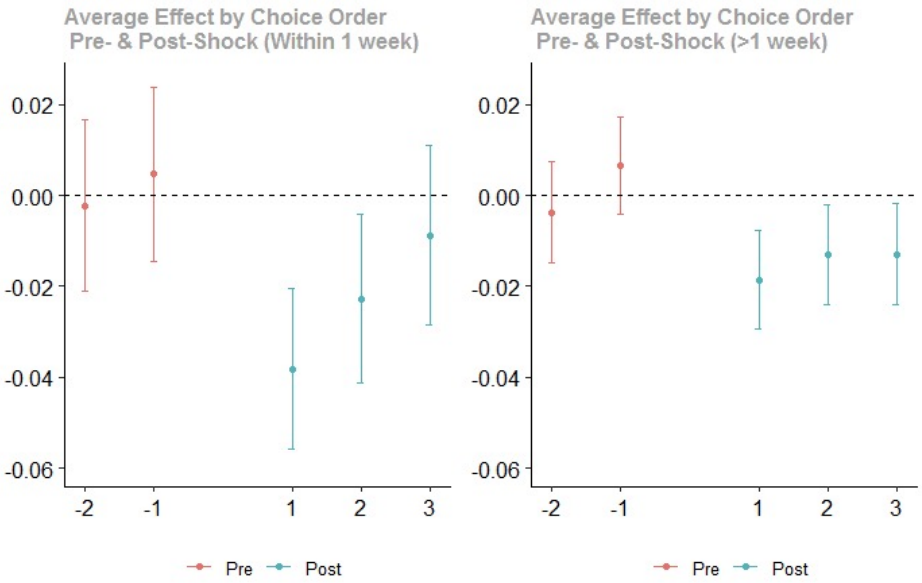
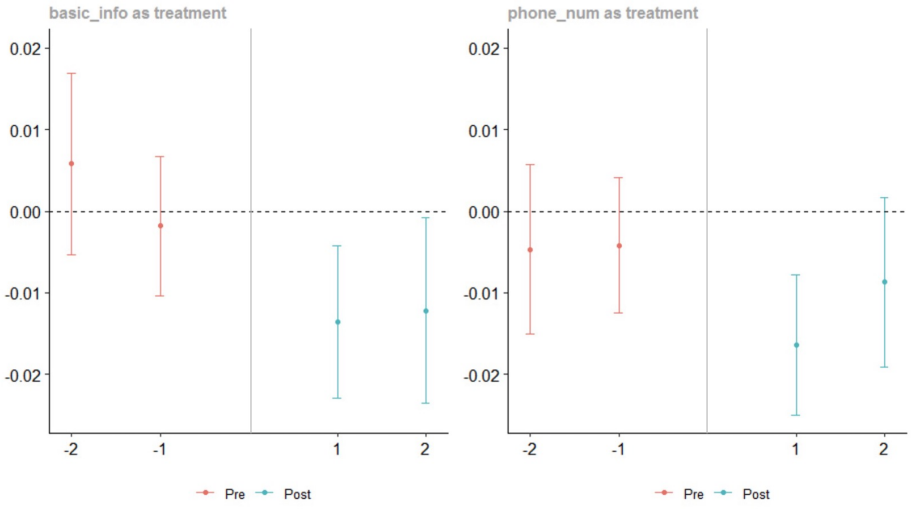


Figure B7: Decaying estimate over time




Note: This figure plots the coefficients and 95% confidence intervals from event study regression of change in rejection probability using Callaway and Sant'Anna (2021). Left panel displays the treatment effect on users who make their subsequent privacy decision within a week post-shock. Right panel shows the treatment effect on users who make their subsequent privacy decision more than a week post-shock. The specifications include user and choice-order fixed effects. Standard errors are clustered at user level.

Figure B8: Test for data permanence: Treatment as another data type




Note: This figure plots the coefficients and 95% confidence intervals from event study regression of change in rejection probability using Callaway and Sant'Anna (2021). Left panel displays the treatment effect on users who have "basic profile information" datatype requested in their treatment, while all other choices are without "basic profile information". Similarly, right panel shows the treatment effect on users who have "phone number" datatype requested in their treatment, while other choices are without "phone number". The analysis results for the scenario where credit information is requested in the treatment are not shown due to sparse data in this estimation. The specifications include user and choice-order fixed effects. Standard errors are clustered at user level.

Figure B9: Distribution of time when the first choice happened

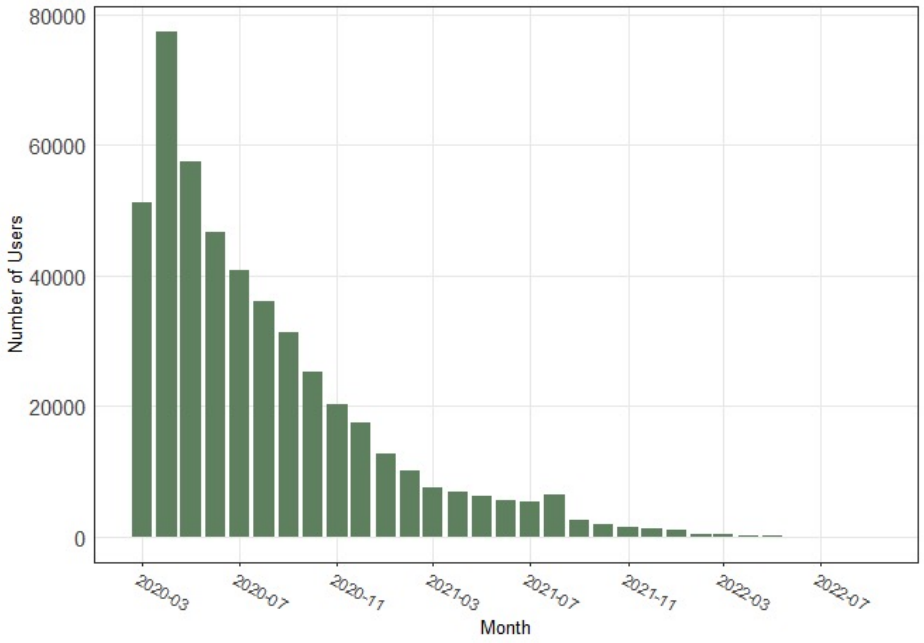



Figure B10: Context Categorization Under LLMs



**Weak Preference**

Examples of exploratory scenarios: e-commerce, job-search, financial services, productivity, ...




**Strong Preference**

Examples of necessity scenarios: vending machines, public transportation, bike-sharing, car charger, ...

# C Additional Tables

Table C1: Percent Apps Downloaded by Category Pre/Post Portable Power Bank App

| | Treated | | Control | |
|---|---|---|---|---|
| category | 3 choices pre-shock | 3 choices post-shock | choice 2,3,4 | choice 6,7,8 |
| Bike Sharing | 4.1% | 3.2% | 2.6% | 2.6% |
| Booking | 4.1% | 3.4% | 3.5% | 4.4% |
| Brand Store | 1.2% | 0.9% | 1.6% | 1.2% |
| Business Service | 0.8% | 0.7% | 0.7% | 0.7% |
| Car Charging Station | 0.4% | 0.4% | 0.3% | 0.3% |
| Car Rental | 0.9% | 0.7% | 0.8% | 1.0% |
| Car Sharing | 5.3% | 4.1% | 4.5% | 4.7% |
| Charity | 1.2% | 0.8% | 1.8% | 1.4% |
| Coffee Shop | 3.3% | 3.3% | 3.4% | 4.0% |
| Convenience Store | 1.2% | 1.5% | 0.9% | 1.4% |
| Coupon | 6.0% | 4.6% | 7.6% | 7.6% |
| Courier | 3.8% | 3.7% | 3.7% | 4.5% |
| COVID Pass | 6.4% | 6.9% | 6.2% | 4.6% |
| E-Commerce | 2.0% | 1.8% | 2.2% | 2.5% |
| Electronics Rental | 1.9% | 1.5% | 2.1% | 2.3% |
| Entertainment | 1.2% | 0.7% | 1.6% | 1.1% |
| Finance | 1.1% | 1.1% | 1.2% | 1.4% |
| Fitness | 0.8% | 0.8% | 1.6% | 1.7% |
| Fresh Delivery | 5.5% | 5.0% | 4.3% | 5.9% |
| Gas Station | 1.1% | 1.3% | 0.8% | 1.2% |
| Government | 16.2% | 15.0% | 14.4% | 14.2% |
| Health Management | 1.2% | 2.1% | 0.9% | 1.9% |
| Hospital ERS | 0.4% | 0.6% | 0.3% | 0.5% |
| Information Search | 1.1% | 1.0% | 1.3% | 1.4% |
| Labor Service | 0.3% | 0.3% | 0.4% | 0.4% |
| Luxury Rental | 0.03% | 0.02% | 0.03% | 0.03% |
| Mall | 0.4% | 0.2% | 0.6% | 0.4% |
| Marketing | 0.3% | 0.2% | 0.6% | 0.5% |
| Parking | 0.9% | 1.0% | 0.7% | 0.9% |
| Photo Editing | 0.9% | 0.9% | 1.0% | 1.2% |
| Productivity | 3.4% | 1.6% | 4.6% | 2.5% |
| Public Transportation | 0.9% | 0.6% | 0.9% | 0.8% |
| Resell | 0.9% | 0.8% | 1.5% | 1.5% |
| Restaurant | 4.8% | 5.0% | 4.4% | 5.6% |
| SME | 0.3% | 0.3% | 0.2% | 0.3% |
| Social | 1.1% | 0.1% | 2.3% | 0.2% |
| Tourist Attraction | 0.1% | 0.2% | 0.1% | 0.2% |
| Utility | 8.2% | 6.6% | 8.3% | 8.2% |
| Vending Machine | 1.1% | 1.0% | 1.1% | 1.1% |

Table C2: Summary Statistics

Panel A: Summary Statistics for Users

| | N | Mean | Std | Min | Median | Max |
|---|---|---|---|---|---|---|
| All Users | | | | | | |
| $age_i$ (in year) | 358,000 | 34.58 | 10.00 | 16.00 | 34.00 | 79.00 |
| $male_i$ | 358,000 | 56.02% | n.a | n.a | n.a | n.a |
| $higher_edu_i$ | 358,000 | 29.28% | n.a | n.a | n.a | n.a |
| $blue_collar_i$ | 358,000 | 26.72% | n.a | n.a | n.a | n.a |
| city $degree_i$ (1-5) | 358,000 | 2.66 | n.a | 1.00 | 3.00 | 5.00 |
| avg daily account $balance_i$ (in USD) | 358,000 | 1251.67 | 4946.96 | 0.01 | 77.87 | 478,894.31 |
| Treated Users | | | | | | |
| $age_i$ (in year) | 119,864 | 31.93 | 8.14 | 16.00 | 31.00 | 78.00 |
| $male_i$ | 119,864 | 58.29% | n.a | n.a | n.a | n.a |
| $higher_edu_i$ | 119,864 | 38.65% | n.a | n.a | n.a | n.a |
| $blue_collar_i$ | 119,864 | 21.19% | n.a | n.a | n.a | n.a |
| city $degree_i$ (1-5) | 119,864 | 2.48 | n.a | 1.00 | 2.00 | 5.00 |
| avg daily account $balance_i$ (in USD) | 119,864 | 1229.26 | 4834.52 | 0.01 | 94.98 | 315,092.91 |

Panel B: Summary Statistics for Apps

| | N | Mean | Std | Min | Median | Max |
|---|---|---|---|---|---|---|
| App Stats for Full Sample | | | | | | |
| total user $count_j$ | 7321 | 6115.84 | 38017.51 | 1.00 | 202.00 | 333256.00 |
| life $length_j$ (in months) | 7321 | 14.82 | 9.90 | 3.00 | 13.00 | 31.00 |
| monthly average $user_j$ | 7321 | 566.79 | 5548.56 | 1.00 | 15.65 | 228051.00 |
| monthly average $duration_j$ (in hours) | 7321 | 61.62 | 1812.56 | 0.006 | 1.00 | 108423.65 |
| monthly average $launch_j$ | 7321 | 5162.72 | 63284.96 | 0.33 | 59.78 | 2853103 |

Table C3: Traditional TWFE & Callaway and Sant'Anna Estimates (Including Never-treated Users in the Control Group)

| | (1) CS | (2) CS | (3) CS | (4) TWFE | (5) TWFE | (6) TWFE |
|---|---|---|---|---|---|---|
| Treated × 1st Choice Post | -0.021*** | -0.024*** | -0.020*** | -0.018*** | -0.014*** | -0.027*** |
| | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) |
| Treated × 2nd Choice Post | -0.016*** | -0.020*** | -0.015*** | -0.013*** | -0.009*** | -0.019*** |
| | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) |
| Treated × 3rd Choice Post | -0.010*** | -0.017*** | -0.010*** | -0.010*** | -0.006*** | -0.015*** |
| | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) |
| Observations | 2,943,504 | 2,943,504 | 2,943,504 | 2,943,504 | 2,943,504 | 2,943,504 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | Yes | No | No | Yes | No |
| Category FE | No | No | Yes | No | No | Yes |
| Datatype FE | No | No | Yes | No | No | Yes |
| Clustered error | user | user | user | user | user | user |
| *Note:* | baseline rejection rate before treatment is 18.5% | | | | | |

Table C4: Traditional TWFE & Callaway and Sant'Anna Estimates in Full Sample (Including Users Whose Rejection Probability is Zero)

Panel A: Control Group: Only Not-yet-treated Users

| | (1) CS | (2) CS | (3) CS | (4) TWFE | (5) TWFE | (6) TWFE |
|---|---|---|---|---|---|---|
| Treated × 1st Choice Post | -0.015*** | -0.025*** | -0.013*** | -0.013*** | -0.012*** | -0.008*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Treated × 2nd Choice Post | -0.011*** | -0.026*** | -0.010*** | -0.009*** | -0.008*** | -0.006*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Treated × 3rd Choice Post | -0.009*** | -0.027*** | -0.008*** | -0.006*** | -0.006*** | -0.004* |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Observations | 787,177 | 787,177 | 787,177 | 787,177 | 787,177 | 787,177 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | Yes | No | No | Yes | No |
| Category FE | No | No | Yes | No | No | Yes |
| Datatype FE | No | No | Yes | No | No | Yes |
| Clustered error | user | user | user | user | user | user |
| *Note:* | baseline rejection rate before treatment is 10.8% | | | | | |

Panel B: Control Group: Both Never-treated & Not-yet-treated Users

| | (1) CS | (2) CS | (3) CS | (4) TWFE | (5) TWFE | (6) TWFE |
|---|---|---|---|---|---|---|
| Treated × 1st Choice Post | -0.015*** | -0.018*** | -0.013*** | -0.012*** | -0.008*** | -0.019*** |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) |
| Treated × 2nd Choice Post | -0.011*** | -0.015*** | -0.011*** | -0.008*** | -0.005*** | -0.014*** |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) |
| Treated × 3rd Choice Post | -0.008*** | -0.013*** | -0.008*** | -0.006*** | -0.003* | -0.010*** |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) |
| Observations | 3,888,286 | 3,888,286 | 3,888,286 | 3,888,286 | 3,888,286 | 3,888,286 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | Yes | No | No | Yes | No |
| Category FE | No | No | Yes | No | No | Yes |
| Datatype FE | No | No | Yes | No | No | Yes |
| Clustered error | user | user | user | user | user | user |
| *Note:* | baseline rejection rate before treatment is 13.6% | | | | | |

Table C5: Robustness Check: Sun and Abraham Estimator (main sample & full sample)

Panel A: Main sample: users with rejection probability larger than 0

| | late-treated as control | | never- & late-treated as control | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated × 1st Choice Post | -0.022*** | -0.016*** | -0.021*** | -0.033*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Treated × 2nd Choice Post | -0.017*** | -0.014*** | -0.017*** | -0.026*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Treated × 3rd Choice Post | -0.015*** | -0.013*** | -0.013*** | -0.023*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 603,631 | 603,631 | 2,943,504 | 2,943,504 |
| Individual FE | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | Yes | No | Yes |
| Category FE | No | Yes | No | Yes |
| Datatype FE | No | Yes | No | Yes |
| Clustered error | user | user | user | user |
| *Note:* | $^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$ | | | |

Panel B: Full sample: all users (including those who never reject)

| | late-treated as control | | never- & late-treated as control | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated × 1st Choice Post | -0.015*** | -0.011*** | -0.015*** | -0.024*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Treated × 2nd Choice Post | -0.011*** | -0.010*** | -0.011*** | -0.019*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Treated × 3rd Choice Post | -0.009*** | -0.008*** | -0.008*** | -0.015*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Observations | 787,177 | 787,177 | 3,888,286 | 3,888,286 |
| Individual FE | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | Yes | No | Yes |
| Category FE | No | Yes | No | Yes |
| Datatype FE | No | Yes | No | Yes |
| Clustered error | user | user | user | user |
| *Note:* | $^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$ | | | |

Table C6: Robustness Check: When First Post-shock Choice Is on the Next Day or Later

| | CS | TWFE | | SA | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated × 1st Choice Post | -0.020*** | -0.021*** | -0.020*** | -0.021*** | -0.019*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Treated × 2nd Choice Post | -0.018*** | -0.019*** | -0.018*** | -0.018*** | -0.017*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Treated × 3rd Choice Post | -0.016*** | -0.016*** | -0.016*** | -0.016*** | -0.015*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Observations | 590,386 | 590,386 | 590,386 | 590,386 | 590,386 |
| Individual FE | Yes | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | No | Yes | No | Yes |
| Clustered error | user | user | user | user | user |
| *Note:* | | $^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$ | | | |

Table C7: Robustness Check: Main Effect on Newly Registered Users

| | CS | TWFE | | SA | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Treated × 1st Choice Post | -0.033** | -0.028** | -0.028** | -0.024* | -0.025* |
| | (0.011) | (0.010) | (0.010) | (0.010) | (0.010) |
| Treated × 2nd Choice Post | -0.036** | -0.028** | -0.030** | -0.023* | -0.024* |
| | (0.012) | (0.010) | (0.010) | (0.011) | (0.011) |
| Treated × 3rd Choice Post | -0.041** | -0.028* | -0.029** | -0.028* | -0.028* |
| | (0.012) | (0.011) | (0.011) | (0.012) | (0.012) |
| Observations | 18,475 | 18,475 | 18,475 | 18,475 | 18,475 |
| Individual FE | Yes | Yes | Yes | Yes | Yes |
| Choice-order FE | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | No | No | Yes | No | Yes |
| Clustered error | user | user | user | user | user |
| *Note:* | | $^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$ | | | |

**Note:** Baseline rejection rate before treatment is 10%.

Table C8: Summary Statistics for App Categories

| | | | | Top1 App | | Top3 Apps | |
|---|---|---|---|---|---|---|---|
| category | avg rejection | app # | user # | avg rejection | market share | avg rejection | market share |
| Bike Sharing | 1.58% | 131 | 235,614 | 5.62% | 79.30% | 5.34% | 86.17% |
| Booking | 10.53% | 104 | 101,657 | 3.82% | 14.57% | 7.25% | 39.42% |
| Brand Store | 11.94% | 307 | 95,373 | 8.55% | 13.52% | 8.96% | 34.46% |
| Business Service | 7.86% | 162 | 47,482 | 2.45% | 17.70% | 2.36% | 41.61% |
| Car Charging Station | 2.60% | 102 | 24,946 | 3.25% | 26.66% | 2.21% | 53.45% |
| Car Rental | 5.71% | 48 | 28,679 | 1.66% | 20.33% | 2.09% | 49.19% |
| Car Sharing | 6.67% | 36 | 210,080 | 2.21% | 32.58% | 7.60% | 71.80% |
| Charity | 10.01% | 28 | 55,380 | 10.77% | 54.53% | 8.25% | 77.10% |
| Coffee Shop | 2.48% | 177 | 143,259 | 13.49% | 15.45% | 7.96% | 42.98% |
| Convenience Store | 6.36% | 188 | 101,676 | 16.67% | 24.62% | 10.21% | 52.02% |
| Coupon | 28.66% | 307 | 241,660 | 22.60% | 15.11% | 27.12% | 39.63% |
| Courier | 8.98% | 66 | 193,826 | 14.51% | 34.84% | 10.36% | 60.62% |
| COVID Pass | 7.39% | 95 | 397,510 | 10.01% | 30.08% | 10.39% | 43.95% |
| E-Commerce | 12.29% | 124 | 134,409 | 22.19% | 20.37% | 13.41% | 47.39% |
| Electronics Rental | 9.84% | 290 | 95,552 | 18.23% | 35.34% | 15.01% | 51.90% |
| Entertainment | 20.12% | 109 | 65,348 | 1.90% | 22.17% | 17.57% | 56.90% |
| Finance | 22.01% | 196 | 69,716 | 4.93% | 9.76% | 14.78% | 25.43% |
| Fitness | 26.66% | 82 | 72,603 | 47.87% | 29.16% | 33.35% | 55.18% |
| Fresh Delivery | 7.07% | 142 | 235,211 | 14.09% | 32.92% | 15.79% | 67.35% |
| Gas Station | 4.81% | 176 | 88,416 | 6.17% | 27.67% | 3.19% | 66.97% |
| Government | 7.33% | 434 | 445,000 | 12.26% | 15.04% | 8.68% | 30.25% |
| Health Management | 12.59% | 93 | 151,798 | 32.31% | 26.59% | 21.51% | 53.70% |
| Hospital ERS | 5.63% | 700 | 78,366 | 3.97% | 16.85% | 7.03% | 30.31% |
| Information Search | 14.89% | 194 | 417,541 | 13.42% | 59.43% | 13.35% | 82.67% |
| Labor Service | 13.10% | 112 | 21,033 | 5.39% | 33.42% | 3.91% | 59.82% |
| Luxury Rental | 12.20% | 14 | 1,210 | 25.67% | 30.19% | 27.93% | 68.24% |
| Mall | 3.77% | 87 | 26,389 | 26.83% | 39.67% | 20.92% | 71.64% |
| Marketing | 22.87% | 154 | 30,908 | 14.72% | 20.19% | 31.12% | 35.31% |
| Parking | 3.98% | 112 | 50,768 | 1.54% | 23.95% | 3.82% | 59.02% |
| Photo Editing | 9.12% | 189 | 84,505 | 8.05% | 37.05% | 9.53% | 55.94% |
| Portable Power Bank | 0.65% | 241 | 128,971 | 1.97% | 18.84% | 1.70% | 47.97% |
| Productivity | 26.16% | 137 | 154,996 | 11.11% | 47.26% | 23.40% | 77.40% |
| Public Transportation | 17.71% | 158 | 50,818 | 2.27% | 22.14% | 12.99% | 38.24% |
| Resell | 12.37% | 115 | 74,916 | 22.36% | 23.28% | 20.93% | 46.22% |
| Restaurant | 2.96% | 896 | 192,431 | 2.53% | 17.32% | 7.95% | 32.85% |
| SME | 11.57% | 159 | 12,984 | 0.95% | 20.73% | 3.90% | 42.88% |
| Social | 14.80% | 22 | 35,141 | 54.68% | 93.75% | 53.84% | 97.41% |
| Tourist Attraction | 7.69% | 154 | 16,945 | 3.70% | 26.49% | 2.59% | 44.80% |
| Utility | 11.53% | 448 | 339,195 | 17.80% | 13.72% | 36.70% | 31.33% |
| Vending Machine | 5.70% | 126 | 83,371 | 3.89% | 47.16% | 5.43% | 68.89% |

Note: The top 1 and top 3 apps are defined as those with the largest and top three market shares, respectively, within each category, listed alphabetically.

Table C9: Main Effects: Rejection Probabilities of Post-Shock Choices, Alternative Difference-in-Difference Estimators

| | Point Estimate | Standard Error | Lower Bound 95% Confidence Interval | Upper Bound 95% Confidence Interval |
|---|---|---|---|---|
| TWFE-OLS | | | | |
| Treated × 1st Choice Post | -0.022 | 0.003 | -0.028 | -0.017 |
| Treated × 2nd Choice Post | -0.018 | 0.003 | -0.024 | -0.013 |
| Treated × 3rd Choice Post | -0.015 | 0.003 | -0.021 | -0.010 |
| Callaway-Sant'Anna | | | | |
| Treated × 1st Choice Post | -0.022 | 0.003 | -0.027 | -0.016 |
| Treated × 2nd Choice Post | -0.017 | 0.003 | -0.023 | -0.012 |
| Treated × 3rd Choice Post | -0.015 | 0.003 | -0.020 | -0.009 |
| Sun-Abraham | | | | |
| Treated × 1st Choice Post | -0.022 | 0.003 | -0.027 | -0.016 |
| Treated × 2nd Choice Post | -0.017 | 0.003 | -0.023 | -0.011 |
| Treated × 3rd Choice Post | -0.014 | 0.003 | -0.020 | -0.009 |
| Roth-Sant'Anna | | | | |
| Treated × 1st Choice Post | -0.018 | 0.002 | -0.022 | -0.014 |
| Treated × 2nd Choice Post | -0.011 | 0.002 | -0.016 | -0.006 |
| Treated × 3rd Choice Post | -0.010 | 0.003 | -0.016 | -0.003 |
| Gardner | | | | |
| Treated × 1st Choice Post | -0.022 | 0.002 | -0.027 | -0.018 |
| Treated × 2nd Choice Post | -0.018 | 0.002 | -0.022 | -0.013 |
| Treated × 3rd Choice Post | -0.015 | 0.002 | -0.020 | -0.010 |
| Observations | 603,631 | | | |
| Individual FE | Yes | | | |
| Choice-order FE | Yes | | | |
| Clustered error | user | | | |

**Note:** This table shows the main effect of state dependence on the three choices post-shock, using robust difference-in-differences estimators introduced in Callaway and Sant'Anna (2021); Sun and Abraham (2021); Roth and Sant'Anna (2023) and Gardner (2022). All regressions include user and choice-order fixed effects. The standard errors are clustered at the user level.

# References


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