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Abstract

This paper examines how misleading advertising of borrowing costs affects consumer choices in the high-interest, short-term loan market. We assemble a comprehensive dataset on online cash-advance apps that combines detailed advertising content, app downloads and usage, and actual and consumer-perceived borrowing costs. We first document that many high-cost apps increasingly advertise “no interest” claims over time, despite their misleading nature. We then show that these “no interest” claims make advertising nine times more effective in driving existing app users’ continued app usage. Using a survey experiment, we find that these claims lead a substantial fraction of respondents to mistakenly believe the loans are costless, even among those with prior usage experience. These findings suggest that “no interest” claims distort consumer borrowing decisions and cause harm, directly informing the ongoing policy debate on strengthening regulation in this fast-growing market.

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1 Introduction

Advertising is a primary channel through which firms proactively provide product information to consumers. To attract consumers, firms often have strong incentives to exaggerate product features or make misleading claims, potentially distorting consumer choices and causing harm. This vulnerability is especially pronounced for financial products, which are complex and difficult to evaluate, even after purchase (Bertrand and Morse, 2011). Moreover, misleading advertising for financial products can be particularly harmful, because financial decisions are high stakes and poor choices can lead to severe, lasting consequences such as household financial distress (Melzer, 2011).

In this paper, we empirically examine the impact of misleading advertising in the cash advance app market—an emerging, app-based market for high-interest, short-term loans. The market has reached a total transaction volume of $32 billion dollars by 2022 (Consumer Financial Protection Bureau, 2024a). While cash advance apps offer similar services as traditional payday loans at comparably high costs, their fee structures are more complex and obscure, making true borrowing costs harder for consumers to discern. Moreover, the mobile app format allows lenders to seamlessly convert viewers of mobile ads into users—often requiring just a single click. As a result, consumers are particularly vulnerable to misleading advertising in this market due to the fee complexity and frictionless conversion. Indeed, anecdotal evidence suggests that cash advance app advertising may lead consumers to underestimate the true cost of borrowing. For example, one user reported: "I definitely didn't think about the payback time and the interest... They just portray it as being so simple and so easy."[1]

To examine misleading advertising's impact, we assemble a unique dataset that combines daily data on cash advance apps' new downloads and active users, their true fee structures, ad impressions (the number of times ads are displayed to viewers), and the original advertising

[1] Source: NBC News, "Millions use Earnin to get cash before payday. Critics say the app is taking advantage of them." https://www.nbcnews.com/tech/internet/millions-use-earnin-get-cash-payday-critics-say-app-taking-n1034071.

materials in images and videos. We identify common financial claims in these ads, such as "no interest" and "no credit check," and use machine learning methods to extract these claims alongside other ad content. We complement these data with surveyed measures of consumer beliefs about their borrowing costs. Using these data and measures, we assess whether the advertised claims are misleading and measure their impact.

We first examine whether the advertised claims reflect the true borrowing costs of cash advance apps, focusing on the prevalent "no interest" claim. A typical app charges approximately \$10 for a \$100 cash advance with a two-week term—comparable to the costs of traditional payday loans and equivalent to an annual percentage rate (APR) exceeding 260%. Nevertheless, 36% of apps advertise "no interest," even though all such apps charge high borrowing costs and are more likely to include less salient fees. These findings suggest that cash advance apps indeed advertise misleading information about their true costs. Further, the prevalence of "no interest" claims has risen sharply over time, increasing by 23 percentage points within 4.7 years. This finding suggests that firms not only have strong incentives to misrepresent their borrowing costs, but may also have learned that such strategies are effective in attracting users.

We next examine whether misleading "no interest" claims actually influence consumer behavior. Although these claims misrepresent true costs, their impact on consumer decisions is ultimately an empirical question. If consumers recognize the claims as misleading, they should have little effect on app usage. However, if the claims shape consumers' perceptions of borrowing costs—even after they have experienced the actual costs—then "no interest" advertising would strongly increase usage. Concern is especially warranted if this effect persists among experienced users, as it would indicate that consumers remain misled despite direct product experience.

To examine the causal effect of misleading advertising claims, field experiments are generally infeasible due to ethical concerns and the absence of firm incentives to participate, making observational data the only viable approach. However, estimating advertising effects with observational data is inherently difficult, as it requires carefully isolating exogenous variation to avoid severe endogeneity concerns (Shapiro, Hitsch and Tuchman, 2021). The challenge is even greater when estimating the causal impact of a specific advertised message, rather than the overall effect of advertising exposure.

To address these challenges, we carefully design an identification strategy that exploits useful variation in our context to estimate the advertising effect of "no interest" claims. Specifically, we leverage within-app-month variation in advertising impressions while holding ad content fixed at the app-month level for identification. We show that this variation does not correlate with major demand shifters, such as app updates and within-month paycheck cycles, suggesting that this variation is not targeted to predictable demand shocks and is thus plausibly exogenous. We further show that "no interest" claims are neither correlated with ad impression volumes given the fixed effects nor targeted to specific user demographics, suggesting that the variation in such messages is plausibly exogenous. Finally, we employ generalized random forests (Athey, Tibshirani and Wager, 2019) to non-parametrically estimate the effect of "no interest" advertising claims while controlling for other ad features.

We demonstrate that "no interest" claims in advertising are particularly effective in driving app usage. While a 1,000 increase in advertising impressions without "no interest" claims generates 0.98 additional daily active users, the same increase with these claims generates 9.12 additional users—approximately nine times the advertising effect. In comparison, other advertised claims and ad features explain little of the advertising effect, including claims such as "no hidden fee," the true borrowing costs, actor characteristics (gender, age, ethnicity, facial expressions), and the advertising format. We also find that "no interest" advertising primarily induces existing users to continue using the apps, with smaller effects on attracting new users and negligible effects on stealing users from competitors. These findings suggest that "no interest" advertising creates a perception of low borrowing costs, significantly increasing consumer usage of cash advance apps.

Notably, the finding that the "no interest" ad effect persists even among existing users with prior app experience suggests that consumers do not correct their misperceptions of borrowing costs as they gain experience. We present further evidence for this lack of correction in two ways. First, we show that the effect of "no interest" claims remains consistent regardless of the actual borrowing costs. Second, we find that these claims are not associated with lower user retention rates. Together, these findings demonstrate that even after using the apps and observing actual costs, consumers fail to recognize that "no interest" claims are misleading.

We further conduct an online survey experiment to assess borrower beliefs after exposure to "no interest" claims. We randomize subjects into three conditions: no ad exposure, exposure to a "no interest" ad, and exposure to a similar ad without the "no interest" claim. The "no interest" ad misleads a considerable share of subjects into believing that the apps have no borrowing costs, even among those with prior experience and those exposed to highly salient cost information. These findings provide direct evidence supporting our empirical results that "no interest" ads distort consumer beliefs about borrowing costs and that consumers fail to correct these distortions even with experience.

Our findings that "no interest" ads significantly distort consumer beliefs and choices—and that these misperceptions are difficult to correct through experience—suggest an important role for regulation in this market. According to our survey, users of cash advance apps typically have alternative borrowing options that would allow them to borrow at much lower costs. Therefore, a regulation banning "no interest" ads could substantially save borrowing costs for these consumers. Based on our estimates, such a policy could save consumers up to $18 million dollars annually. This finding highlights the importance and urgency of detecting and banning misleading advertising in high-interest consumer loan markets.[2]

Our paper contributes to two streams of the literature. First, we contribute to the literature on the role of borrowing cost information in short-term high-interest consumer loan

[2]In practice, such a policy could require lenders to either report all borrowing costs transparently or refrain from mentioning costs altogether, a policy that is currently implemented by CFPB in other loan markets. See our discussion in Section 7.

markets.[3] Bertrand and Morse (2011) show that presenting information illustrating how loan fees accumulate over time and compare to alternative options reduces consumer borrowing of payday loans—an insight that later inspired Texas legislators to mandate such disclosures (Wang and Burke, 2022). In contrast, we study advertising effects, where firms voluntarily disclose information and have limited incentive to present information truthfully and transparently. Our paper demonstrates that misleading advertising of borrowing costs is prevalent and has a substantial impact on consumer perception of borrowing costs and their repeated engagement with high-interest loan apps. We also demonstrate in our survey experiment that simply mandating transparent cost information, following the direction of Bertrand and Morse (2011), only partially corrects misleading advertising's effect on consumers. Our results, therefore, offer direct support for regulatory efforts that directly restrict lenders' misleading advertising of borrowing costs (Consumer Financial Protection Bureau, 2024b).

Second, it contributes to a small but growing literature on consumers' responses to misleading advertised information. Several studies estimate the impact of policies that ban or discourage misleading information on product sales (Rao and Wang, 2017) and the volume of misinformation (Avery, Cawley, Eisenberg and Cantor, 2013; Chiou and Tucker, 2018). Closest to our paper, Fong, Guo and Rao (2024) design an online survey experiment to estimate the effect of misinformation and its debunking on consumers' stated choices. In contrast, we assemble new field data on advertising material and consumer choices to estimate the effect of advertised misleading information.[4]

The rest of the paper is organized in the following way. Section 2 discusses the institutional background and the data used in the paper. Section 3 compares the advertised costs

[3] A number of papers show the negative impact of such loans (Melzer, 2011; Carrell and Zinman, 2014; Gathergood, Guttman-Kenney and Hunt, 2019) and consumers making suboptimal borrowing decisions (Agarwal, Skiba and Tobacman, 2009).

[4] This paper is also broadly related to the literature on how advertising affects financial decisions—including choices of mortgages (Gurun, Matvos and Seru, 2016; Kim, Jiang and Thomadsen, 2023), loans (Bertrand, Karlan, Mullainathan, Shafir and Zinman, 2010), and funds (Jain and Wu, 2000; Mullainathan and Shleifer, 2005; Reuter and Zitzewitz, 2006)—and how product advertising affects stock valuation and returns (Grullon, Kanatas and Weston, 2004; Gurun and Butler, 2012; Lou, 2014).

with the actual costs charged by cash advance apps. Section 4 discusses the empirical design and identification strategy. Sections 5 and 6 present the empirical analysis results and the survey experiment results, respectively. Section 7 discusses policy implications and Section 8 concludes.

2 Institutional Background and Data

2.1 Institutional Background

Cash advance apps. Online cash advance applications (apps) allow users to borrow a small amount of money from their next paychecks before the paychecks arrive. The typical loans have a maturity of two to four weeks, and the average loan duration is 10 days (California Department of Financial Protection and Innovation, 2023). The maximum borrowing amounts generally fall between \$100 to \$500, though users often receive less, with an average loan amount of \$106 (Consumer Financial Protection Bureau, 2024b). Similar to payday loans, cash advance apps typically do not require a credit check and do not impact users' credit scores (Bhutta, 2014; Li, 2022). However, they often require users to authorize access to their bank accounts or provide payroll information, allowing the apps to withdraw repayments directly once paychecks arrive. Unlike traditional payday lenders, which operate with physical store fronts, cash advance apps operate online and often entirely through the smartphone app. Due to their convenience, these apps have seen a multiple-fold increase in downloads and active users in recent years (see Figure 1), establishing themselves as important players in the consumer loan market.

Fee structure. Cash advance apps typically charge users high fees, and these fees can take several forms. The first form is an upfront cost, typically charged as a flat monthly subscription fee to gain access to loans, such as Albert (\$14.99 per month), Brigit (\$9.99), and Empower (\$8.00). A few apps charge fees as a fixed percentage to the loan amount, such as Varo (see Appendix Figure B1).

The second form is an "instant fee," a per-loan charge that reduces wait time for funds (also called "express fee," "fast fee," or "lightning fee" by some apps). Without this fee, borrowed funds typically arrive in 2 to 3 business days; with it, funds arrive the same day. Many customers opt for the instant fee, as a 2-3 day wait is substantial relative to the average cash-advance duration of only 10 days.

The third form is tips, which some apps encourage users to pay in addition to other costs. According to a 2023 investigation, users pay tips 73 percent of the time when the option is available (California Department of Financial Protection and Innovation, 2023).

Combined, these fees suggest that borrowing from cash advance apps is costly. In fact, our analysis later shows that these fees can amount to a borrowing cost of more than $10 for a $100 cash advance for a term of only two weeks. These borrowing costs are comparable to those in traditional payday loan markets, often translating to effective APRs of several hundred percent.

Regulations. The Truth in Lending Act (TILA, or Regulation Z, 12 CFR Part 1026) makes specific transparency requirements about how lenders can advertise their credit terms. Section § 1026.16 (Advertising) requires lenders to "clearly and conspicuously" present all cost information if they were to make a "negative or affirmative reference" about their loan terms. The section gives specific examples including when the lender advertises "no interest" (an instance when it makes a negative reference), it must transparently present all fee components, including all fixed and variable fees that are required to access the loan. According to Section § 1026.4 (Finance charge) and CFPB's new interpretive rule issued in July 2024 (Consumer Financial Protection Bureau, 2024b), monthly subscription fees, instant fees, and tips are all considered as finance charges for cash advance apps.

Whereas CFPB has taken enforcement actions against misleading advertising in similar markets,[5] it has not enforced TILA in the cash advance app market. However, the FTC

[5]For example, in 2016 CFPB took enforcement actions against LendUp, an online payday loan company

has pursued a few cases against these apps for deceptive claims about the amount of cash advances they provide, the instant fees and tips they charge, and the difficulty of canceling subscriptions (FTC v. Brigit, 2024; FTC v. Dave, 2024; FTC v. FloatMe, 2024).

2.2 Data

We assemble a novel dataset by integrating four sources: (1) a panel dataset providing daily app-level measures of active users, new downloads, and total advertising impressions (i.e., the number of times an ad is shown to users of other apps); (2) all advertising materials—images and videos, referred to in the industry as "ad creatives"—along with their ad impressions; (3) manually collected data on the fee structure of each app; and (4) survey data on consumer beliefs. Below, we present each of our data sources and the sample screening criteria.

Daily data on active users and new downloads. The primary data on mobile apps is from Sensor Tower, a leading provider of global mobile app analytics and performance metrics in the industry. The Sensor Tower database contains a comprehensive collection of information on millions of mobile apps across more than 100 countries on iOS platform.

In this paper, we use mobile app data for cash advance apps from July 2017 to February 2022. For a given cash advance app in the sample, we obtain its daily numbers of downloads and active users. Sensor Tower combines actual data provided by their corporate partners (app developers and publishers), as well as app rankings and metadata information from the App Store to estimate each app's downloads and active users. Downloads are recorded at the account level, and importantly, re-downloads by the same account (even across devices) are not counted in this measure. Thus, app downloads capture the number of new, first-time adopters. In contrast, active users include both new users and repeated users.

that falsely advertises lower future interest rates if consumers were to repeatedly borrow.

Advertising impressions and creatives. We collect advertising data for each cash advance mobile app from Sensor Tower. The raw data comes in the format of daily impression shares across different advertising campaigns for overlapping short time windows. An impression share represents the percentage of times an ad appears on a user's screen relative to all other ads. We assemble the impression shares across all advertising campaigns and dates and normalize them by a proxy of the total number of impressions among iOS users, which gives us the daily impressions for each advertising campaign. Appendix A details the construction process of the ad impression data.

Importantly, we obtain information on the advertising creatives for each campaign. These ad creatives include videos and images, which we obtain in their native formats (e.g., mp4 and png). To illustrate the typical content featured in these ad creatives, we present two examples of video ads and two examples of image ads in Figure 2. Panel (a) shows four screenshots that show key moments in a video ad by the app Albert. The ad starts with the main character sitting in the car in need of cash, and the text prompt shows "Download Albert. They can spot you up to $250." Then the video turns to large-font text that repeats the previous message and highlights "0% interest. No late fees. No credit check." The video ad ends with the main character holding cash in his hand. Similar to other mobile ads, cash advance apps' ad creatives often use text prompts to highlight the main messages in the ads, including financial claims such as the maximum borrow amount ($250) and claims of "no interest," "no late/hidden/overdraft fees," and "no credit checks," which can also be seen in panel (c) (an image ad by Brigit). Panel (b) shows a similar video ad as panel (a) by Earnin, which features human actors that show facial expressions of distress as they need money, and expressions of excitement after they receive money from the app. Different from Albert's ad in panel (a), Earnin's ad in panel (b) highlights the instant speed but does not make the "no interest" claim. Finally, panel (d) shows an image ad by MoneyLion which emphasizes "early" and, other than hinting the maximum borrow amount, does not make other claims about its cost structures and features. To use the rich information contained in these creatives, we extract advertising features using various machine-learning methods, which we discuss later.

Costs of using cash advance apps. We manually collect the fee structures of each cash advance app. As introduced earlier, these fees can be categorized into three broad types: (1) upfront costs, (2) instant fees, and (3) tips. This information is collected from the financial information aggregator *finder.com*, which reviews most large cash advance apps, and various other information websites. To standardize each fee component and make them comparable across apps, we assume that the upfront cost per advance includes half of the monthly subscription fee and any percentage-based borrowing costs (equivalent to each consumer taking two loans per month). We also assume that consumers require same-day money delivery and leave an average $5 tip if the app encourages tipping. Based on these fees, and assuming a typical borrowed amount of $100 per loan, we can compute an implied APR for each app, which we use as an alternative measure of app fees that can be compared across markets.

Sample screening. To define cash advance apps in the sample, we start with the universe of apps classified as finance apps. Then, we construct a preliminary list of cash advance apps that consists of the list under the "payday loan" categorization from Sensor Tower and the list of ChatGPT-identified cash advance apps. Taking the union of the two sets, we arrive at a list of 92 apps. Then, we remove small apps of which we do not have complete data. Of the 92 apps, 63 are too small for Sensor Tower to collect active user data reliably, leaving us a sample of 29 apps. Further, we manually check all apps and remove three that are mis-categorized by either Sensor Tower or ChatGPT. Lastly, we remove four apps that never advertised in our sample and 20% app-year level observations with zero advertising, because we can only measure advertising content for apps that advertise. We thus arrive at the final sample of 22 apps and 25,791 app-day level observations.

Survey on consumer beliefs. We conduct a pre-registered online survey experiment on Prolific to directly measure how "no interest" advertising influences borrower beliefs. We sample $N = 1,800$ U.S. adults, screening on those who have a job, have income below $\$100,000$, and express interest or recent experience with loans. The final sample consists of 1,760 participants who passed an attention check.[6] The survey randomly assigns participants to different experimental conditions and elicits their beliefs about borrowing costs, along with information on their financial experiences and literacy. Section 6 presents the experiment design and results.

2.3 Extracting Advertising Features

The primary focus of this paper is the communication of borrowing costs in cash advance apps' advertisements. We extract this information, along with other financial features, from the advertising creatives data. We also extract other visual and auditory features that may affect the advertising effect, such as the actor's ethnicity and facial expression.

We start from a set of 27,529 ad creatives in our sample, which include 15,605 images and 11,924 videos. For each video, we capture one frame per second to convert the video into a set of images. We then extract text and human faces from each image and analyze the soundtracks of the videos, which we detail below.

In mobile app advertising, text is critical as it precisely conveys the message the app wants to communicate to users. Even for videos, since users often use their phones in silent mode, apps rely on highlighted text to display the video scripts. Thus, text contains essential information for each ad creative. We extract text from each image using *pytesseract*, a state-of-the-art optical character recognition tool. Next, we use regular expressions to identify keywords, including phrases like "no interest," "no ... fee," "no overdraft," "quick," "FDIC insured," "no credit check" and mentions of whole dollar amounts (e.g., "$250"). We group

[6] The sample size is large relative to experimental studies in the household finance literature. For example, Beshears et al. (2017) recruit a total of 597 participants and Lian et al. (2019) have 400 participants for each of their experiments.

all phrases of the form "no ... fee" with fewer than three words between "no" and "fee" as "no hidden fee."

Human faces and emotions are also common elements in cash advance app advertising. As we have seen in Figure 2, a common theme in these ads features a short story where a person in need of cash gets an advance through the app and displays a happy face. Previous research has demonstrated that character demographics (Kim, Jiang and Thomadsen, 2023) and emotions (Small and Verrochi, 2009) can play a role in advertising effectiveness. Thus, we recognize human faces in each image using *DeepFace*, which extracts gender, age, race, and facial emotions from each face. Users may identify with the demographics of the characters and be influenced by the emotions. We then use the characteristics of the largest face in each ad creative as the representative human characteristics and emotions of the creative.

Additionally, we extract the soundtrack from each video-format creative. Using *moviepy*, we isolate the soundtrack in mp3 format from the videos and then separate the foreground from the background sound using *librosa*. We identify blank audio and classify the soundtracks into different audio types using VADNet and YAMNet. We also convert the soundtracks into a 180-dimensional embedding using *librosa*, following Gorodnichenko, Pham and Talavera (2023). We then use principal component analysis (PCA) to reduce the soundtrack features to two principal dimensions for each video.

In total, we extract seven text-based advertising features, seven image-based facial and emotional features, two audio features, and three features capturing ad format. The text features indicate whether the ad claims a high borrowing limit, no hidden fee, no interest, no overdraft fees, quick to get funds, FDIC insured, and no credit check. The image features capture the presence of human actor(s), and whether the actor is young, female, African American, or displays happiness, sadness, or fear. The audio features consist of the first two principal components of the soundtrack classifier outputs and embeddings. The ad format features indicate whether the ad is full-screen, is a video ad, or displays a clickable button.

After extracting textual, facial, soundtrack, and format features from each ad creative, we aggregate these creative-level features up to the app-month level using their impression shares. This aggregation is motivated by two reasons, which we elaborate in Section 4.2. We find that app-month fixed effects explain 67% of the daily variation in "no interest" claims, suggesting that the app-month level features are a good approximation of the app-date level features.

2.4 Summary Statistics

Table 1 presents summary statistics for the major variables across the 22 cash advance apps in our field data. On average, these apps have about 56,000 active users on an average day and about 127,000 active users on their most popular days. The number of active users differs significantly across apps, as the 10th to 90th percentile of daily active users is between 1,000 and 202,000. Over the 4.7-year sample period, the average total downloads per app is 3.2 million, reaching nearly 8 million at the 90th percentile. About 5% of downloads convert to regular users who persistently use these apps, as measured by the 90-day retention rate.[7] These apps have also heavily advertised, with an average of 784 million total impressions during this period.

In terms of the fee structure, the average upfront cost per paycheck cycle (two weeks) is $3, the instant fee is $3, and, assuming $5 tip if the app encourages tips, the average tips is $2. Fees vary significantly across apps, with some apps charging high fees for one type (e.g., $9 for instant fee) and zero for others. When we cast these fees in terms of APRs, the typical APR averages 270%, reaching 455% at the 90th percentile.

Table F1 and Figures F1 and F2 in Appendix F present summary statistics of the survey participants. In the survey, 58% of participants indicate that they have used a cash advance app before. Interestingly, these users have the same median income as non-users at $55,000. Among experienced users, 93% own a credit card and 68% of them report typically having at least $100 in available credit at the end of their billing cycle. These findings suggest

[7]The retention rate is an estimate of the fraction of users who, after 90 days since their initial download of the app, continue to use it. The 5% retention rate is comparable to that of typical mobile apps.

that cash advance app users are not necessarily in severe financial distress and often have access to alternative borrowing options, implying that many of them may not need to rely on cash advance apps at all. These findings also contrast with common perceptions of traditional payday loan users, who generally have lower incomes (Lawrence and Elliehausen, 2008; Bertrand and Morse, 2011), likely because cash advance apps are more convenient and appeal to a broader population. Finally, we find that participants significantly underestimate the apps' APRs, mirroring the misperceptions observed among payday loan borrowers (Bertrand and Morse, 2011).

3 Advertised Costs and Actual Costs

Typical cash advance apps charge high fees for each cash advance. Figure 3 presents the fees for borrowing $100 in one advance for each app in our sample.[8] As we can see, 21 out of the 22 apps charge a fee for their cash advance services, varying between $2 to $17.5 across different apps. Remarkably, Albert, the app that advertises "no interest" in our first example in Figure 2, charges $17.5 per advance. There is also a large variation in the type of fees charged across apps. In the graph, the blue bar represents upfront costs, the green bar represents instant fees, and the red bar represents tips. Most apps charge upfront costs for borrowing, with some charging as high as $10 for a two-week advance. About half of them charge an instant fee, with some more than $10. Several apps "encourage" tipping to the apps for borrowing. Overall, the total fees charged by the online cash advance apps are of the same order of magnitude compared to the traditional payday lenders (Melzer, 2011).

Next, we compare the advertised claims made by these apps versus their actual fees. As we discussed in Section 2, in markets such as credit cards and mortgages, provisions in Regulation Z mandate that lenders present all components of borrowing costs in a transparently

[8]Many of these apps also offer features beyond cash advances, such as budgeting tools and financial insights. Reviewing the features offered by each app across subscription tiers, we find that most of these additional services are typically free to use and therefore not included as part of the paid subscription benefits. Access to cash advances remains the primary paid feature, with Albert being the only app whose subscription also includes a rewards debit card.

manner in their advertising material if they were to advertise some components of costs. However, these regulations currently do not apply to online cash advance apps. In Figure 4, we plot the average fraction of ads with "no interest" claims against the total costs in the form of implied APR and each of the upfront costs, instant fees, and tips. As we can see, 8 out of the 22 apps advertise "no interest," but these apps all have high total costs with an implied APR of more than 200%. Thus, although there is no cost component called "interest," the "no interest" claims are potentially misleading and may suggest a low borrowing cost. The advertisements violate the typical advertising disclosure requirements for financial products by advertising "no interest" without disclosing other cost components. In addition, the fraction of ads claiming "no interest" is positively and significantly correlated with instant fees and tips (panels (c) and (d), t = 3.66 and 3.05), but not with total costs or upfront costs (panels (a) and (b), t = 1.17 and −0.53). This result suggests "no interest" claims are linked to hidden tactics such as charging instant fees and tips. Our results also complement the findings in Gurun, Matvos and Seru (2016) that more expensive lenders advertise more.

In addition, the "no interest" claims have become more prevalent over years, as shown in Table 2. Over the 4.7-year sample period, their prevalence rose by 23 percentage points, with the number of apps making such claims quadrupling from two to eight.

This evidence demonstrates that the advertised "no interest" claims do not reflect true borrowing costs. Such claims *may* mislead consumers into using the apps and making sub-optimal borrowing decisions. We next turn to how these claims actually affect consumer decisions.

4 Measuring the Effect of "No-Interest" Advertising

In this section, we examine how "no-interest" advertising influences consumers' downloads and usage of cash advance apps. If consumers recognize that "no interest" claims are misleading, these claims should not increase advertising effects. However, if these claims shape consumers' perceptions of borrowing costs—even after they have experienced the true costs—then "no interest" claims would strongly increase app usage, even among existing users.

We start by outlining our empirical framework for measuring heterogeneous advertising effects. This framework addresses potential endogeneity concerns caused by using observational data and controls for other advertising features that may influence ad effect beyond the "no interest" claim. We then describe our identification strategy and estimation approach based on generalized random forests.

4.1 Framework

We specify a model where the log number of daily active users of app j on date t is a function of j's ad stock and fixed effects:

$$\log(\text{users}_{jt}) = \beta(x_{j,m(t)}) \log(\text{ad stock}_{jt} + 1) + \alpha_{j,m(t)} + \epsilon_{jt}, \tag{1}$$

where $\beta(x_{j,m(t)})$ is the ad effect on the log number of active users, ad $\text{stock}_{jt} = \sum_{\tau \leq t} \delta^{t-\tau} \times$ ad $\text{impressions}_{j\tau}$ is the discounted sum of ad impressions and captures potential lagged effects of ad exposure, and $\alpha_{j,m(t)}$ are app-month fixed effects. The ad effect $\beta(x_{j,m(t)})$, commonly referred to as "ad elasticity" in a log-log model, captures the percentage change in active users in response to a percentage change in the ad stock (Shapiro et al., 2021). We also estimate the ad effects on the log number of downloads, $\log(\text{download}_{jt})$, using the same specification.

We specify the advertising effect $\beta(x_{j,m(t)})$ as a function of app and ad features $x_{j,m(t)}$ measured at the app-month level. These features include "no interest" claims, other financial claims in advertising (e.g., "no hidden fees"), non-financial content (e.g., the actor's race), ad format (e.g., a video ad), and app characteristics—features that may also affect consumer responses to advertising.

We aggregate features to the app-month level—the same level as the fixed effects—for two reasons. First, this aggregation allows us to perform a within transformation to take out the fixed effects $\alpha_{j,m(t)}$ and to apply generalized random forest algorithms to estimate heterogeneous effects. Second, holding $x_{j,m(t)}$ constant at the same level as the fixed effects ensures that identification relies on within-app-month variation in ad impressions rather than variation in ad content. In other words, we identify ad effects from variation in ad impressions (ad stock_{jt}) for a given set of features $x_{j,m(t)}$, rather than comparing users with different $x_{j,m(t)}$. The ad effect $\beta(x_{j,m(t)})$ hence represents the average advertising effect among users exposed to ads with features $x_{j,m(t)}$ and can be interpreted as the conditional average treatment effect on the treated (CATT).

4.2 Identification

We identify the advertising effect function $\beta(x_{j,m(t)})$ using within-app-month variation in ad impressions, while holding fixed ad features $x_{j,m(t)}$. In this section, we illustrate the identifying variation in the data, state the two key identifying assumptions, present evidence supporting these assumptions, and examine when we can interpret our estimate as the causal effect of "no-interest" claims.

Illustration. Before formalizing our identifying assumptions, we first present an illustration in Figure 5, which shows how an app's number of users and downloads vary with its advertising campaign within app-month and net of time trend.[9] For illustrative purposes, we define the first day within an app-year with positive ad impressions as the start of an ad campaign and focus on campaigns that begin at least 20 days after the start of the year and at least two months before year-end. Figure 5 demonstrates that campaigns typically begin with a high volume of impressions on day 1 and then gradually die out over the next

[9]For illustration purposes, we take out the time trend in users and downloads (see Figure 1), which allows us to visualize their co-movements with ad impressions.

two months. In response, the number of users and downloads spike on the first day of the campaign. Their subsequent peaks occur after one week, two weeks, and a month, aligning with typical paycheck cycles when users either take the next advance or repay a previous one. In addition, there is some carryover in advertising effects on the number of users and downloads, but this impact is not long-lasting. Overall, Figure 5 provides initial suggestive evidence that within-app-month variation in ad impressions drives usage and downloads, potentially with some carryover effects.

Identifying Assumptions. To identify $\beta(x_{j,m(t)})$ as the conditional average advertising effect given content and app characteristics $x_{j,m(t)}$, we require two identifying assumptions.

The first assumption is the exogeneity of advertising impressions to demand shocks within app-month:

Assumption 1 (Strict Exogeneity of Advertising): For any $t, t' \in m(t)$,

$$\mathbb{E}[\epsilon_{jt'} \,|\, \log(\text{ad impressions}_{jt} + 1), \alpha_{j,m(t)}] = 0. \tag{2}$$

The primary challenge in estimating advertising effects using observational data is that firms often increase advertising when demand is high. In our setting, demand for cash advance apps has grown rapidly over the sample period (see Figure 1), raising a concern that apps may gradually increase advertising spending in response to, or in anticipation of, their own growth paths. To address this, we include app-month fixed effects $\alpha_{j,m(t)}$ to absorb any persistent differences across apps and capture app-specific trends over time. As noted in Choi, Mela, Balseiro and Leary (2020), roughly 65% of online display ads are sold through guaranteed channels that involve advance sales, suggesting that app-month fixed effects $\alpha_{j,m(t)}$ may account for a large part of app-specific demand shifters that are targetable at the time of ad purchase. Our identifying variation thus comes from the residual ad impression variation within app-months.

One may imagine that advertising may be targeted toward other demand shocks within an app-month. In Appendix C, we examine two other important demand shifters to which advertising may be targeted. First, apps may choose to advertise more on days when consumers are further from their previous paycheck and thus more likely to need cash, which could induce an upward bias through within-month paycheck cycles. To assess this, we examine whether ad impressions correlate with the number of days until the end of the month and find no correlation (see Appendix Table C2), even though the number of users increases toward month-end (see Appendix Figure B2). This suggests that apps do not systematically time their advertising around paycheck cycles. Similarly, we later show in Table 3 and Appendix Section E that our advertising effect estimates remain virtually identical when date fixed effects are included.

One may also imagine that advertising may target dates when the app introduces new features through version updates—events that substantially increase usage (see evidence documented by Huang et al. (2023)). However, Appendix Table C1 shows that advertising impressions do not respond to version updates, despite these being large demand shocks to the apps.

Finally, one may imagine that cash advance apps may target advertising in response to recent demand shocks by increasing ad spending after observing a surge in downloads or usage. If demand shocks are serially correlated, such targeting could lead to an upward bias of the advertising effect estimates. We show in Appendix Table E3 that adding lagged downloads or users does not change the estimates of advertising effects, implying that past shocks have little impact on current ad spending.[10]

Taken together, this evidence supports the assumption that, within an app-month, advertising impressions are not targeted to demand shifters in $\boldsymbol{\epsilon}$.

The second assumption is the orthogonality between advertising content and impressions:

[10]We use four-day and seven-day lags, rather than shorter lags, to avoid mechanical correlation between the lagged variable and the ad stock variable by definition.

Assumption 2 (Mean Independence of Ad Content and Impressions):

$$\mathbb{E}[\beta(x_{jt}) - \beta(x_{j,m(t)}) | \log(\text{ad impressions}_{jt} + 1), \alpha_{j,m(t)}] = 0 \quad (3)$$

In our framework, we model $\beta(x_{j,m(t)})$ as a function of an app's monthly advertising content. Approximately 67% of ad content variation is absorbed by app-month fixed effects, leaving 33% as daily variation within app-month. A potential concern is that this daily variation in ad content—causing deviations in the daily advertising effect from the monthly effect $(\beta(x_{jt}) - \beta(x_{j,m(t)}))$—may be correlated with ad impressions, leading to a bias in the estimated ad effect. This could occur if firms allocate more impressions to campaigns they expect to be more effective, or if advertising algorithms deliver more effective ads to a larger audience. The deviation resembles the optimal prediction error described by Hyslop and Imbens (2001). We demonstrate in Appendix Table C3 that "no interest" claims—the primary ad content driving variation in β—are uncorrelated with daily ad impressions given the fixed effects, suggesting that cash advance apps do not increase advertising volume when making "no interest" claims.

Given these two assumptions, the conditional average advertising effect on the treated, $\beta(x_{j,m(t)})$, can be identified from within-app-month variation in advertising impressions.

Interpretation of $\beta(x_{j,m(t)})$. To interpret the conditional average advertising effect $\beta(x_{j,m(t)})$ as the causal effect of ad content—rather than the advertising effect specific to users exposed to "no interest" ads—requires the variation in ad features x_{jm} to be quasi-random. We thus assess the extent of ad content targeting in our setting. We have shown that cash advance apps do not increase advertising impressions when making "no interest" claims (see Appendix C), suggesting limited targeting of such claims. In addition, the "no interest" claims are not systematically delivered to publisher apps with different user demographics (see Appendix C). Many practitioners acknowledge that designing new ad creatives requires substantial time and effort (Altstiel, Grow and Jennings, 2019). Our interviews with practitioners further reveal that ad creatives and budget targeting are typically managed by different teams within a company during the sample period. Moreover, in our data, ad features vary little within campaigns, suggesting limited ad feature targeting from algorithms. Taken together, these findings imply that the targeting of "no interest" claims is limited in our context, and the estimates of $\beta(x_{j,m(t)})$ can be safely interpreted as the causal effect of ad content.

4.3 Estimation

To estimate the heterogeneous advertising effect function $\beta(x_{j,m(t)})$, we model it as a random forest and estimate it using the generalized random forests estimator (Athey, Tibshirani and Wager, 2019). The estimation is implemented in the R package "grf," developed by Athey, Tibshirani and Wager (2019). Because the package does not permit simultaneous estimation of fixed effects, the persistence parameter δ embedded in ad stocks, and the heterogeneous advertising effect function, our estimation strategy proceeds in three steps.

First, we conduct a grid search to find the best-fitting persistence parameters δ, assuming constant advertising effect $\beta(x_{j,m(t)}) = \beta_0$. We identify the optimal δ's by minimizing the mean squared errors. Using the best-fitting δ's, we construct ad stocks for subsequent analyses and obtain the average ad elasticity β_0.

Second, we conduct a within-transformation to take out fixed effects $\alpha_{j,m(t)}$ from equation (1). Specifically, we run separate linear regressions of the outcome variables and the log ad stocks on the fixed effects. We then use the residuals from these regressions in the following steps (see Appendix C for detailed derivation). Note that we leverage the specific model structure that $\beta(x_{j,m(t)})$ does not vary within the fixed effects $\alpha_{j,m(t)}$, which implies that we can partial out fixed effects from log ad stocks directly.

Finally, we estimate $\beta(\cdot)$ using the generalized random forests estimator. We compute bootstrap standard errors clustered at the app-week level, which allows for error correlation within a time window comparable to the median duration of an advertising campaign.

4.4 Discussions

Does Rival Advertising Play an Important Role? A key question is whether advertising primarily attracts people who would otherwise borrow from competing cash advance apps or those who would not borrow from these apps at all (the outside option). This distinction is crucial for policy implications. If advertising mainly attracts people who would not otherwise borrow from cash advance apps, regulations on how these apps can advertise could meaningfully reduce the total amount of high-interest loans. However, if advertising mainly shifts users between competing apps, such regulations would have little effect on total app usage and total loans.

To examine this question, we estimate a specification that incorporates both own and rival advertising stocks, assuming homogeneous advertising effects:

$$\log(\text{users}_{jt}) = \beta_0 \log(\text{own ad stock}_{jt} + 1) + \beta_1 \log(\text{rival ad stock}_{jt} + 1) + \alpha_{j,m(t)} + \epsilon_{jt}, \tag{4}$$

where rival ad stock$_{jt} = \sum_{j' \neq j} \sum_{\tau \leq t} \delta^{t-\tau} \times$ ad impressions$_{j'\tau}$ is the sum of rival advertising impressions with the same geometric decaying effect over time.

Do "No Interest" Claims Associate with Lower Retention Rates? To assess whether users attracted by "no interest" claims continue using the app in the long run, we examine whether apps that advertise such claims have lower retention rates. Our data directly measure the retention rate for each app in each quarter, defined as the share of users still using the app 90 days after their initial download. Because this measure is quarterly, we cannot apply our main identification strategy to analyze its response to daily variation in advertising impressions. Instead, we provide suggestive, correlational evidence on the relationship between advertising claims and app retention. Specifically, we regress

$$\text{retention}_{jq} = \beta_0 + \beta_1 \text{ share ``no interest'' claims}_{jq} + \alpha_j + \lambda_q + \epsilon_{jq}, \tag{5}$$

where retention$_{jq}$ and share "no interest" claims$_{jq}$ are retention rates and the fraction of impressions with "no interest" claims at the app j, quarter q level, and α_j and λ_q are app and quarter fixed effects. We interpret β_1 as the changes in retention rate explained by quarterly variation in an app's tendency to advertise "no interest" claims, controlling for the fixed effects. We also estimate a similar specification at the app level, where we leverage across-app variation in advertising content and control for observed characteristics.

5 Empirical Results

In this section, we present empirical results demonstrating that "no interest" ads are particularly effective in driving existing users' app usage, and that these users do not learn the true borrowing costs from experience, implying a strong misleading effect of these ads. Before presenting these results, we first report estimates of the average advertising effect and its persistence.

5.1 Baseline Advertising Effects and Distribution

Average ad effects and their persistence. Table 3 presents the baseline results for outcome variables downloads (columns (1)-(3)) and users (columns (4)-(6)). These results are obtained by estimating equations (1) and (4) without allowing for heterogeneity over $x_{j,m(t)}$. Columns (1) and (4) present our main and preferred specifications, columns (2) and (5) add rival advertising effects, and columns (3) and (6) add date fixed effects.

On average, we find an advertising elasticity of 0.036 for downloads and 0.011 for active users (columns (1) and (4)). These estimates imply that, if an app increases 10% of discounted sum of impressions (ad stock), it should lead to a 0.36% increase in new downloads and a 0.11% increase in active users on average. At the sample averages of new downloads and active users, these estimates imply that every 1,000 advertising impressions convert to a 0.12 increase in new downloads and a 1.06 increase in active users. As the increase in active users is much higher than the increase in new downloads, advertising primarily increases the usage frequency of returning users. The estimated advertising elasticities are virtually identical when we control for date fixed effects (columns (3) and (6)). The average elasticities are comparable to those documented in the broader advertising literature, including studies on consumer packaged goods (Shapiro, Hitsch and Tuchman, 2021).

We also find that the ad effects on downloads and active users are short-lived, as they decay to less than 3% of their initial values in two days.[11] These short-lived effects are expected, as most ads are clickable, allowing viewers to directly access the app store to download the focal app if they have not installed it, or open the app if they have. As a result, one should expect that most of the ad responses occur immediately after the viewers have seen the ad.

Further, we find that rival advertising effects are negligible in this market (columns (2) and (5)). The average advertising elasticity of rival ad stocks is -0.003 for new downloads and 0.001 for active users, with both effects statistically insignificant. Importantly, the own ad stock elasticities remain virtually unchanged controlling for rival ads. These findings suggest that rival advertising effects are negligible, thus implying that the primary advertising effect is to drive app usage for people who would otherwise not use cash advance apps.

We conduct two robustness checks for the average advertising effects. First, we examine whether our results are robust to a linear (rather than log) specification of equation (1). Second, we investigate whether our results are sensitive to alternative fixed effects specifications by including app-specific quadratic time trends on top of app-month and date fixed effects, allowing for richer controls of predictable demand shocks. Our results remain robust in both alternative specifications, as shown in Appendix E.

The distribution of ad elasticities Figure 6 presents the distribution of the heterogeneous advertising effects from equation (1), estimated by the generalized random forests and projected onto the observed covariates $x_{j,m(t)}$. We find that the average ad elasticities across

[11] $0.16^2 = 0.0256$, $0.11^2 = 0.0121$.

observations are 0.035 for downloads and 0.011 for users, closely aligning with the baseline estimates reported in Table 3.

On top of these averages, we find significant heterogeneity in ad elasticities across ad content and app characteristics. The inter-quartile range for ad elasticities is [0.006, 0.053] for downloads and [0.001, 0.017] for users. This large discrepancy suggests that some ad creatives or app characteristics are more effective than others in driving downloads and usage. We will next investigate the extent to which each of these features explains the discrepancies in the ad treatment effects.

5.2 "No Interest" Claims Significantly Increase Advertising Effects

To understand what drives the heterogeneous advertising effects, we first present the variable importance for all dimensions of advertising content and app characteristics, $x_{j,m(t)}$, separately for the download and user models. Variable importance is measured as the share of tree splits that occur on a specific variable's node among all tree splits in the random forests. Because generalized random forests select tree splits that maximize the heterogeneity in advertising effects ($\beta(x)$), the frequency with which variables are chosen for splitting provides a measure of their importance in explaining $\beta(x)$—hence the term "variable importance."

We show in Figure 7 that the fraction of ad impressions with a "no interest" claim consistently takes high importance of 0.55 for downloads and 0.46 for active users. Other than "no interest" claims, the advertised borrow limit, the use of human actors, video format, and the app's upfront cost are also important explanatory variables. On the other hand, factors such as other financial claims, the characteristics and emotions of human actors, soundtrack, clickable buttons, and the app's instant fee and tips do not explain much of the variation in $\beta(x)$. In particular, the message "no hidden fees" explains little advertising effects, which highlights the importance of "no interest" claims and suggests that these claims influence consumers beyond simply reassuring them about hidden costs.

Next, we show how "no interest" claims drive the advertising effect. Ad effect $\beta(x)$ is a function of multiple ad and app features, which could be correlated with "no interest" claims. To isolate the marginal effect of "no interest" claims, we need to hold other features constant when predicting $\beta(x)$. To this end, we vary the fraction of "no interest" ads within the 2.5–97.5 percentile range [0, 0.54], holding other features at their empirical joint distribution. We then predict $\beta(\cdot)$ for each vector x, integrate over other features, and present the average $\hat{\beta}$("no interest") for each fraction of "no interest" ads. We also present confidence intervals and statistical tests of these results based on bootstrap standard errors clustered at the app-week level.

Figure 8 shows that advertising effects increase in the fraction of "no interest" ads, with a sharp jump around the 30% mark. Holding other features constant at their empirical distribution, ad campaigns without "no interest" claims have advertising elasticities 0.033 for downloads and 0.010 for users. These effects imply that 1,000 ad impressions would convert to 0.11 new downloads and 0.98 active users. In contrast, if half of the ads communicate "no interest," the elasticities are much higher, at 0.250 for downloads and 0.097 for users, implying that the same 1,000 ad impressions will convert to 0.87 new downloads and 9.12 active users. Therefore, ads with "no interest" claims are at least 8 to 9 times more effective than ads without "no interest" claims. Because the increase in active users far exceeds the increase in new downloads, most of the advertising effect comes from existing users.

We examine the statistical significance of these findings by testing against a series of null hypotheses $H_0 : \beta(\text{“no interest”} = 0) = \beta(\text{“no interest”} = x)$ for $x \in (0, 0.54]$. As shown in Appendix Figure D2, we find that the advertising effects are statistically significantly higher when the share of "no interest" claims exceeds 0.35.

We conduct two robustness checks for the results of the "no interest" claims. First, we examine whether our results from the generalized random forests are robust to adding date fixed effects to equation (1). Second, we estimate a linear regression model interacting the "no interest" variable with advertising impressions. Our results remain robust in both alternative specifications, as shown in Appendix E.

In summary, our findings demonstrate that, all else equal, “no interest” claims substantially increase the advertising effect and greatly enhance cash advance apps’ ability to use advertising to attract new and returning users. In particular, “no interest” advertising is nine times more effective in driving existing users’ continued app usage compared to otherwise similar ads without the “no interest” claim. Consistent with anecdotal evidence, these findings suggest that “no interest” claims create a perception of low borrowing costs, leading users to make suboptimal borrowing decisions.

5.3 Do Existing Users Learn about True Borrowing Costs?

We further ask: If consumers are uninformed about true borrowing costs and are influenced by “no interest” claims, can they learn the true borrowing costs and self-correct? This question is crucial for understanding the welfare implications of misleading advertising. If consumers quickly learn that “no interest” claims do not correspond to low borrowing costs and adjust their behavior accordingly, such advertising may only temporarily distort their decision-making. However, if consumers fail to learn even after repeated use, misleading advertising could lead to persistent harm.

Our finding that “no interest” ads primarily affect existing users rather than new users suggests that users do not learn to correct their beliefs after observing the true borrowing costs. We conduct three empirical tests to further support this claim. First, we examine whether the effect of “no interest” ads varies with actual borrowing costs. If users learn from experience, these ads should be less effective for high-cost apps, where the gap between advertised and true costs is larger. Second, we examine whether users attracted by “no interest” ads show higher attrition rates—evidence they discover the misleading nature of advertised claims and exit. Third, we use survey evidence to directly measure whether user beliefs about borrowing costs are distorted by “no interest” claims even after exposure to actual fees.

Across all three analyses, the evidence consistently points to the same conclusion: consumers fail to recognize the misleading nature of "no interest" claims and do not self-correct even after experiencing the true costs. This is likely because such claims create a strong and persistent impression of low borrowing costs—one that is difficult for users to detect or correct. In what follows, we present the first two findings based on observational data and discuss the survey evidence in the next section.

The Effect of "No Interest" Claims Does Not Associate with True Costs. First, we examine whether the effect of "no interest" advertising varies with the true borrowing costs. Figure 9 presents three sets of graphs that illustrate how the effect of "no interest" ads depends on the level and type of app costs. Each set of graphs isolates one cost component and plots the estimated "no interest" ad effects at different cost levels for that component, while holding the other two at zero. For upfront costs and instant fees, we divide the sample into quartiles; for tips, we distinguish between apps that encourage tipping and those that do not. To control for other ad features, we integrate over their empirical distributions, following the approach in Section 5.2. These graphs can be viewed as slices of the overall contour plots in Appendix Figures D3 and D4, which show the predicted advertising effect $\hat{\beta}(x)$ as a function of the share of "no interest" claims and app cost.

We find that the effect of "no interest" claims does not vary with either the level or the type of actual borrowing costs. Advertising effects remain consistently higher for ads with "no interest" claims than for those without. This finding suggests that existing users attracted by "no interest" claims do not connect such claims with the actual costs and fail to recognize the claims as misleading. Instead, they appear to respond to the false impression that borrowing costs are low.

"No Interest" Claims Do Not Associate with Low Retention Rates. Second, we examine the relationship between "no interest" advertising and user retention in the long run. If users realize they have been misled about borrowing costs, they should be more likely to leave the app, leading to lower retention rates for apps using "no interest" claims. Retention is measured at the app-quarter level in our data, so we examine its relationship with "no interest" advertising both across apps and within apps across quarters (see Section 4.4). The within-app specification helps address potential selection bias, as apps that use "no interest" claims may have other quality attributes that enhance retention despite misleading advertising. However, as documented in Section 3, apps making "no interest" claims are in fact less attractive as they tend to have higher total borrowing costs and are more likely to employ hidden fee tactics such as aggressive tipping prompts.

Table 4 shows no significant relationship between retention rates and "no interest" claims, whether comparing across apps (controlling for actual fees) or within the same app across quarters. These findings suggest that even users initially attracted by deceptively low advertised costs do not exhibit higher attrition after 90 days of adoption, reinforcing the conclusion that consumers influenced by misleading "no interest" claims fail to learn about the true borrowing costs.

6 Survey Experiment Results

In this section, we present the results of the online survey experiment, which directly examines how "no interest" advertising influences borrower beliefs. Section 5 has shown that "no interest" ads are effective in driving existing users' continued usage, that their effect does not vary with true borrowing costs, and that the use of such claims is not associated with lower retention rates. Together, these findings suggest that "no interest" advertising shapes existing users' beliefs about borrowing costs and that users do not seem to recognize these claims as misleading. Here, we directly measure consumer beliefs through a pre-registered online survey experiment and demonstrate that "no interest" ads significantly distort these beliefs, further confirming that many existing users do not realize the misleading nature of these claims.

Design. In the survey, we first introduce an anonymous cash advance app to subjects, including its costs of a \$9.99 monthly subscription fee, a \$3.99 optional instant fee, and optional tips.

We then randomly assign subjects to one of six experimental conditions in a 3×2 design. Subjects are exposed to either (1) no advertising, (2) a "no interest" ad, or (3) a similar ad without the "no interest" claim. In addition, half of the subjects (a) view the app's description, including borrowing costs, before seeing the ad, whereas the other half (b) *also* view a visual comparison of fees between the cash advance app and a credit card before seeing the ad. These three advertising conditions (1–3) and two information conditions (a–b) together form the 3×2 experimental design. Figure 10 presents the advertising design and the visual fee comparison.

After viewing the assigned materials, subjects report their beliefs about the app's borrowing costs (in dollars) and compare the costs to credit card costs. Finally, we collect information on subjects' financial experiences, financial literacy, and credit card ownership as an alternative borrowing option.

Results. Our main result focuses on condition (a), in which subjects are shown an ad (or no ad, as the baseline) right after being informed about the app's borrowing costs. We find that exposure to a "no interest" ad significantly lowers subjects' perceived borrowing costs and leads a substantial share to mistakenly believe that the loans are costless (see Table 5). The "no interest" ad decreases the average perceived cost by \$2.27, 16% of the baseline. Remarkably, the "no interest" ad increases the share of subjects who believe the cost is zero by 8.9 percentage points—an increase of 524% over the baseline. In contrast, subjects exposed to a similar ad without the "no interest" message report beliefs that are not different from those in the baseline group, indicating that the effect is driven specifically by the "no interest" claim.[12]

[12]We note that, when it comes to perceived borrowing cost relative to credit cards, we do not find evidence that the "no interest" claim has an additional effect beyond general advertising. Interestingly, 41.1% of subjects in the baseline group believe that the cost of using cash advance apps is similar or lower

We further examine whether prior experience with cash advance apps helps consumers form more accurate beliefs about borrowing costs and mitigates the effect of "no interest" advertising. Evidence in Section 5 suggests that consumers influenced by "no interest" claims fail to learn the true borrowing costs. Here, we directly test whether subjects who report prior experience with cash advance apps respond differently to "no interest" ads. We find that "no interest" ads are 42% *more* effective at misleading subjects with prior experience into believing that the borrowing cost is zero, compared to subjects without prior experience—although the difference in effect is not statistically significant ($p = 0.32$; see Figure 11). This finding suggests that existing users may, if anything, be more susceptible to "no interest" advertising and cannot discern its misleading nature, echoing our earlier findings that users attracted by "no interest" claims do not learn to correct their misperceptions even after repeated exposures.

Finally, we examine whether providing additional cost information can mitigate the misleading effects of "no interest" ads. We contrast the "no interest" effect in condition (a) with condition (b), where subjects are additionally shown visual cues about borrowing costs that make the true information more salient (following the approach in Bertrand and Morse (2011)). Our results show that this information treatment significantly reduces—but does not eliminate—the impact of "no interest" ads. Specifically, with this information treatment, "no interest" ads are 45% less effective at leading subjects to believe borrowing cost is zero ($p = 0.03$; see Figure 12). This finding implies that mandated disclosure policies, such as those suggested by Bertrand and Morse (2011) and Wang and Burke (2022), can help mitigate the harm caused by misleading advertising. However, "no interest" ads remain effective even under this strong information intervention, indicating that their influence is substantial and that direct regulation banning such claims may be a more effective policy approach.

than the cost of using credit cards, in line with our findings that subjects significantly underestimate the apps' APR.

7 Policy Implications

Our findings have important implications for regulating cash advance app advertising. Despite the apps' high fees, the "no interest" advertising claim distorts consumer beliefs about borrowing costs and increases their cash advance app usage—an effect that persists among experienced users and is difficult to correct. These findings suggest that the misleading "no interest" ads distort consumer finance decision-making and harm consumers.

A potential regulatory response is to prohibit the use of "no interest" claims in cash advance app advertising. Under TILA, which governs advertising in other loan markets, lenders must either report all borrowing costs transparently or refrain from mentioning costs altogether in their advertising.[13] Applying these standards to cash advance apps would likely render "no interest" advertising illegal unless all cost components were fully revealed—a practice lenders are unlikely to adopt. As a result, such regulations would likely lead apps to avoid mentioning any costs or fees in their advertising.

To quantify the potential impact of such a regulatory change, we conduct a simple back-of-the-envelope counterfactual analysis. We simulate a scenario where all "no interest" ads are replaced with ads that do not make such claims. Using our estimated $\beta(x)$, we compute the counterfactual advertising effect and the resulting total number of active user-days in this market. Our calculations show that removing "no interest" claims reduces the number of user-days by 32.7 million in 2021, which is 46.7% smaller compared to all user-days currently driven by advertising.

We further estimate the potential impact of this policy on loan volumes. While we cannot directly estimate the effect of advertising on loan counts, we provide a back-of-the-envelope calculation using our estimates and industry statistics. Our data shows that the total number of active user-days on cash advance apps reached 774 million on iOS in 2022. According to Consumer Financial Protection Bureau (2024a), the total number of loans in 2022 was 86 million. If half of these loans were taken by iOS users, this translates to roughly

[13] See 12 CFR § 1026.16 and 12 CFR § 1026.24

one loan per eighteen user-days.[14] Assuming that the marginal user induced by advertising borrows the same amount as the average user, we project that a ban on "no interest" ads would have reduced the number of loans by about 1.8 million in 2021.[15] This decrease could potentially save users up to $18 million dollars in total borrowing costs, depending on what users would have done had they not borrowed from cash advance apps.[16] These numbers could only become bigger since 2021 due to the increasing trend in both the consumer usage of cash advance apps and the advertiser's use of "no interest" claims. These calculations highlight the substantial impact that regulating misleading advertising in the cash advance app industry could have on consumer behavior and financial outcomes. They also support the CFPB's broader and ongoing efforts on regulating fee transparency in this market.

8 Conclusion

This paper examines misleading advertising in the cash advance app market and studies its impact on consumers' beliefs about borrowing costs and cash advance app usage. To achieve this, we compile a unique dataset that combines mobile app utilization data, detailed advertising content, and actual and consumer-perceived costs of online cash advance apps.

We first show that cash advance apps make misleading "no interest" claims about their borrowing costs in the advertising while their actual costs are high. We then demonstrate that these "no interest" claims significantly increase advertising effects by eight-to-nine times, bringing in new downloads and increasing the daily number of active users. In addition, we conduct an online survey experiment that shows such claims increase the share of consumers who believe the borrowing cost is zero.

Our findings highlight the importance of regulations requiring transparent presentation of

[14] $774 * 2/86 = 18$

[15] $32.7/18 = 1.8$

[16] Our survey shows that most cash advance app users still have remaining credit available at the end of their billing cycle. This suggests that, even if they have to borrow $100, they could do so by using their credit card instead. If they repay the balance by their next paycheck, the credit card loan would typically incur no cost, as it remains within the grace period. Even if they are unable to repay by the next paycheck, borrowing through a credit card would still incurs much lower costs than using a cash advance app.

borrowing costs in advertising, such as the Truth in Lending Act in traditional credit markets. Our back-of-the-envelope calculation suggests that such regulation could save consumers up to $18 million dollars annually in borrowing costs in the cash advance app market. Our findings also provide direct evidence supporting the CFPB's ongoing consideration of fee transparency requirements for cash advance apps (Consumer Financial Protection Bureau, 2024b).

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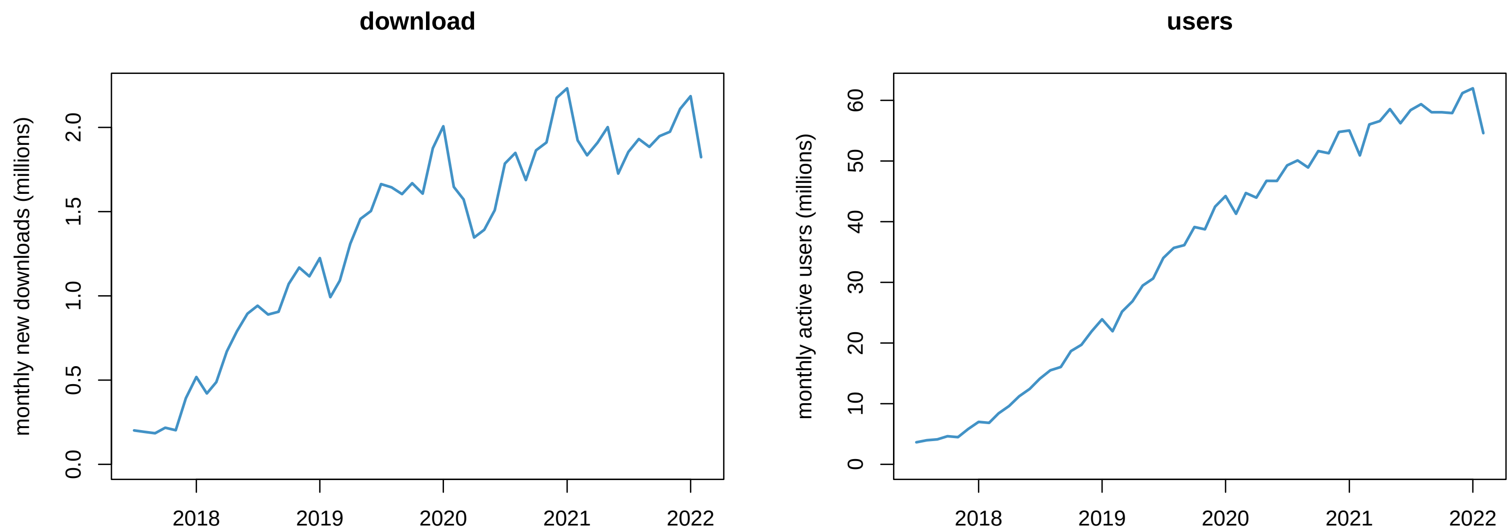
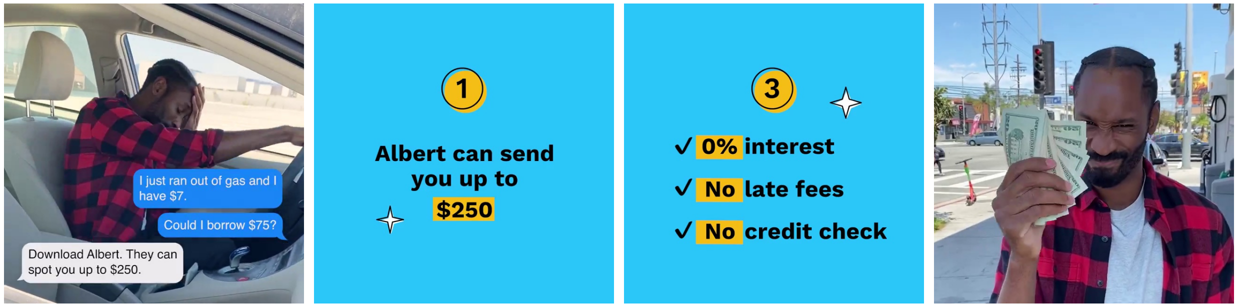
Graphs and Tables

Figure 1: Downloads and Active Users over Time

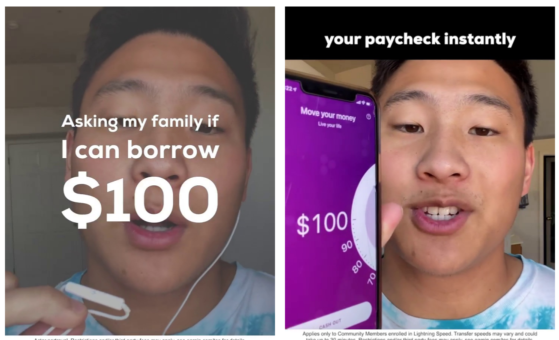


This figure shows the monthly total number of active users and downloads for the cash advance apps in our sample.

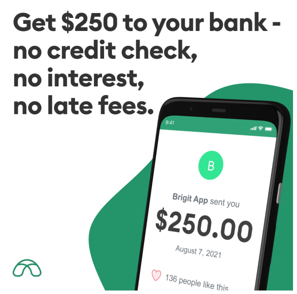
Figure 2: Advertising Examples



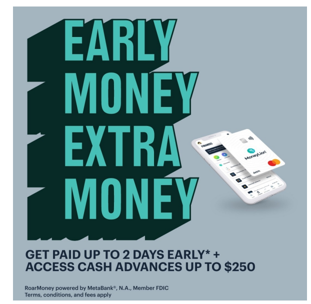
(a) Albert



(b) Earnin



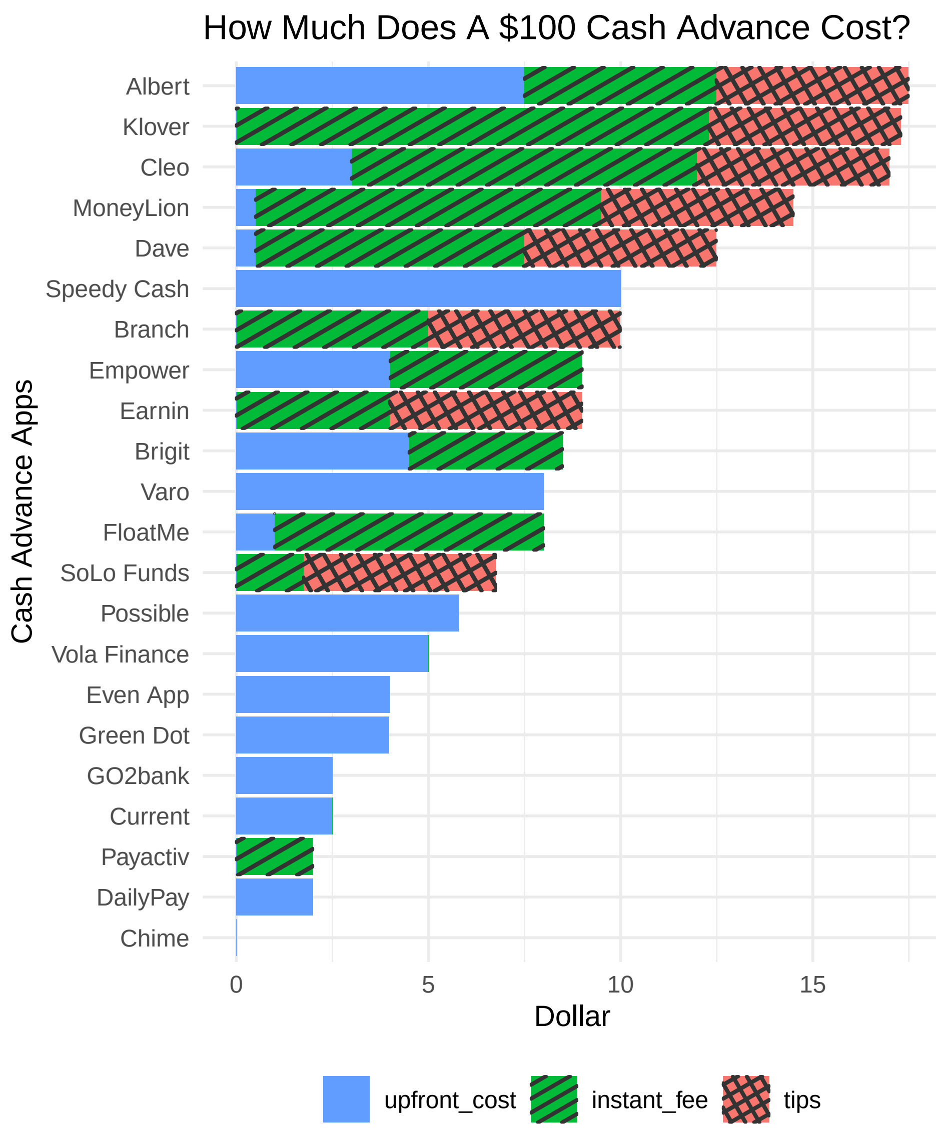
(c) Brigit



(d) MoneyLion

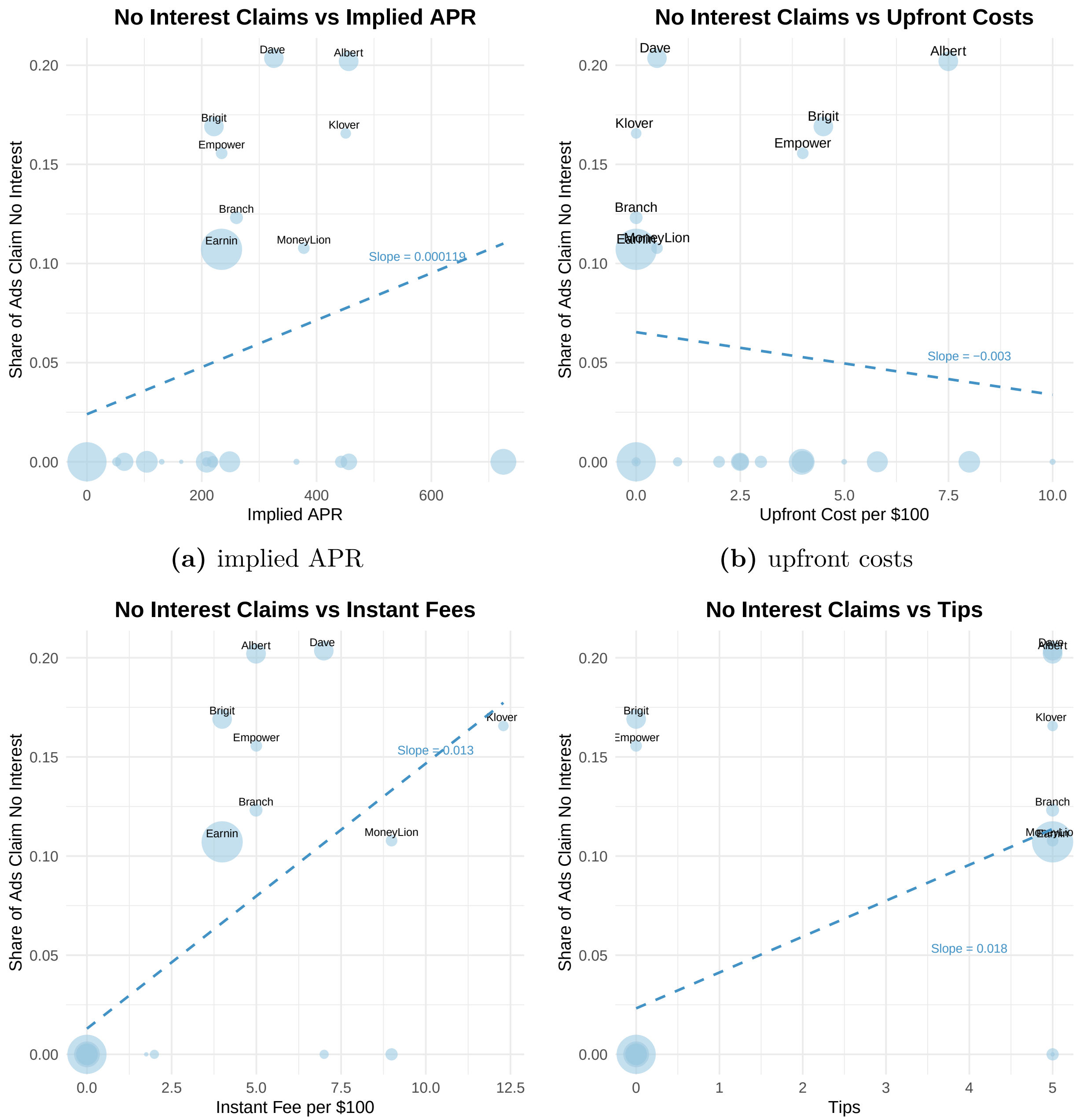
This figure gives examples of typical ads in the sample. Panel (a) is extracted from an Albert's video ad, panel (b) is extracted from an Earnin's video ad, panel (c) is a Brigit's image ad, and panel (d) is a MoneyLion's image ad.

Figure 3: Cash Advance Costs



This figure shows the implied costs of borrowing $100 for once from each cash advance app in the sample. The cash advanced apps are sorted from high to low by the implied costs. The blue, green, and red colors represent upfront costs, instant fees, and tips. The upfront cost includes half of the monthly subscription fee (assuming two advances per month) and cost per advance, if any. Since Chime collects tips in a non-intrusive way, we classify it as a free app.

Figure 4: Relationship Between Ad and App Features



(a) implied APR

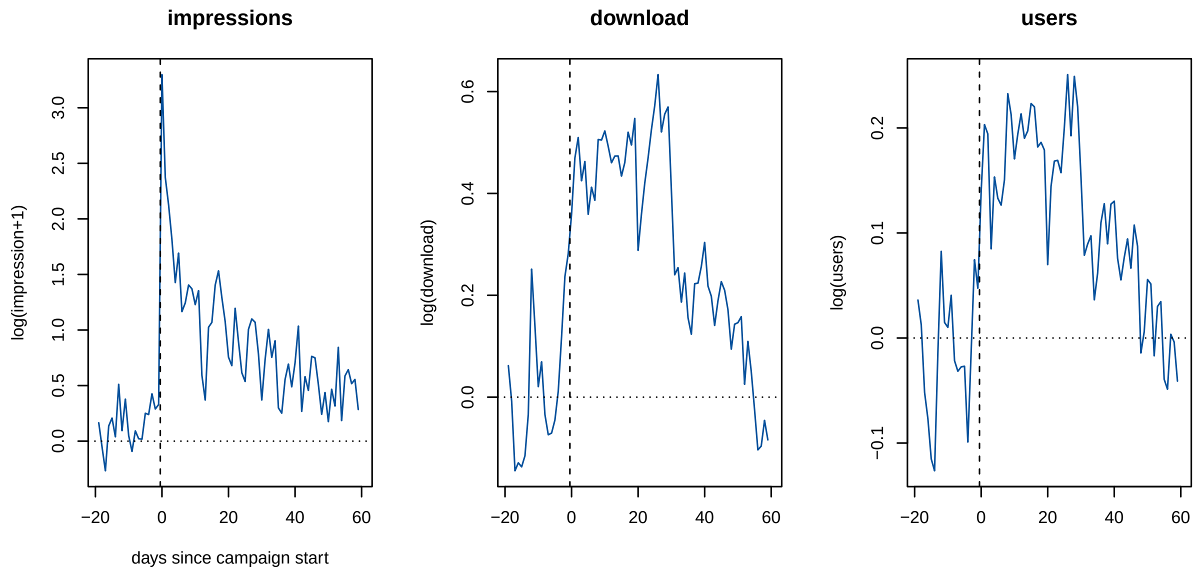
(b) upfront costs

(c) instant fees

(d) tips

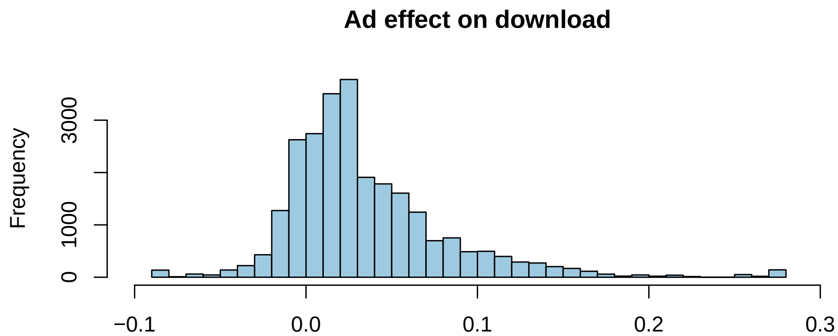
This figure shows the relationship between the average fraction of no interest by apps and the different app features. The app features include the implied APR per $100, upfront cost per $100, instant fee per $100, and tips. The size of the circles represents the size of the apps and the dashed lines are the OLS fitted lines.

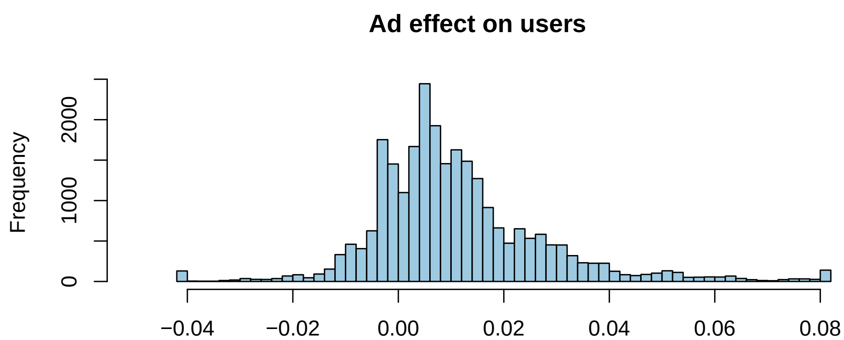
Figure 5: Illustration of Identification



This figure shows the logged number of impressions, logged number of active users, and logged number of downloads around the start of a campaign.

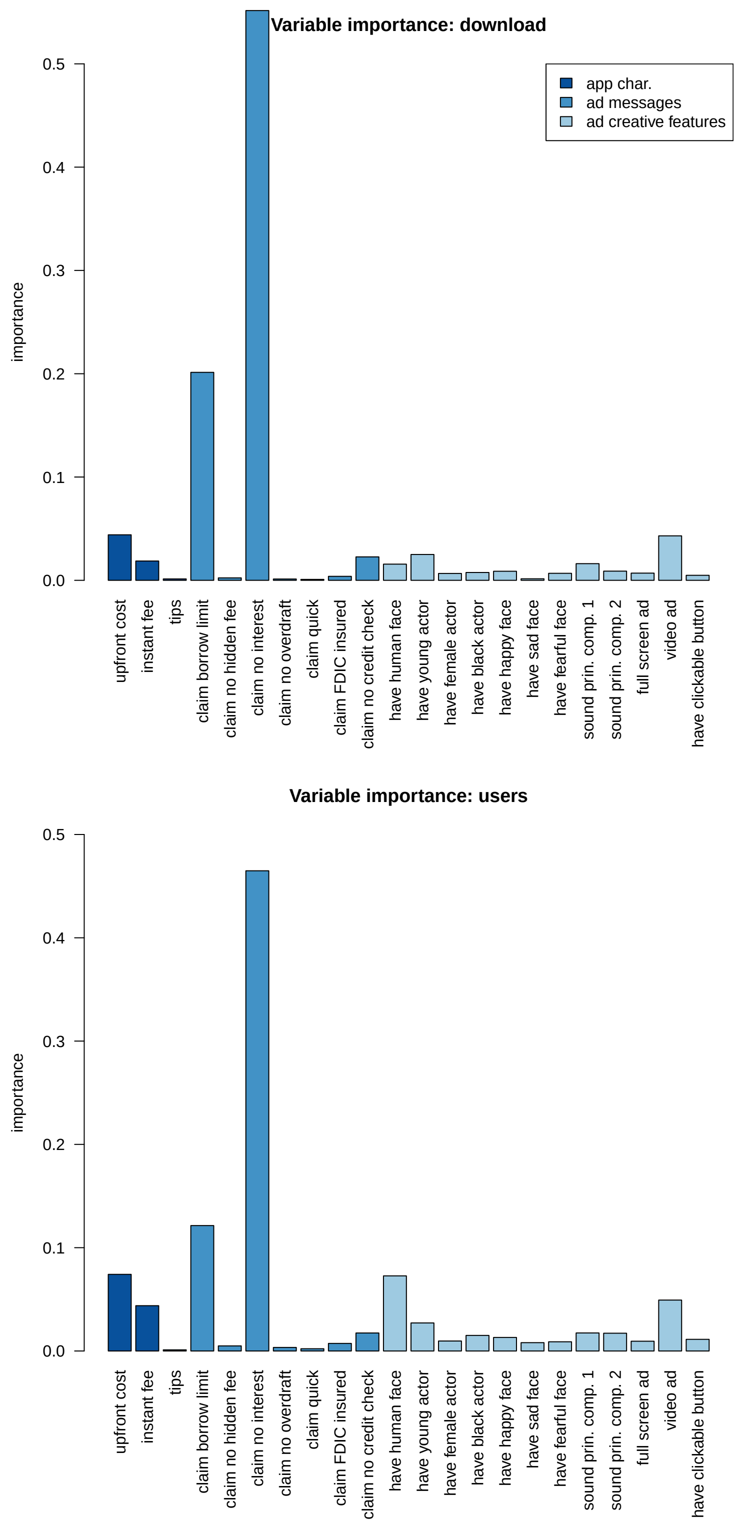
Figure 6: Ad Elasticities at Observed Data Points





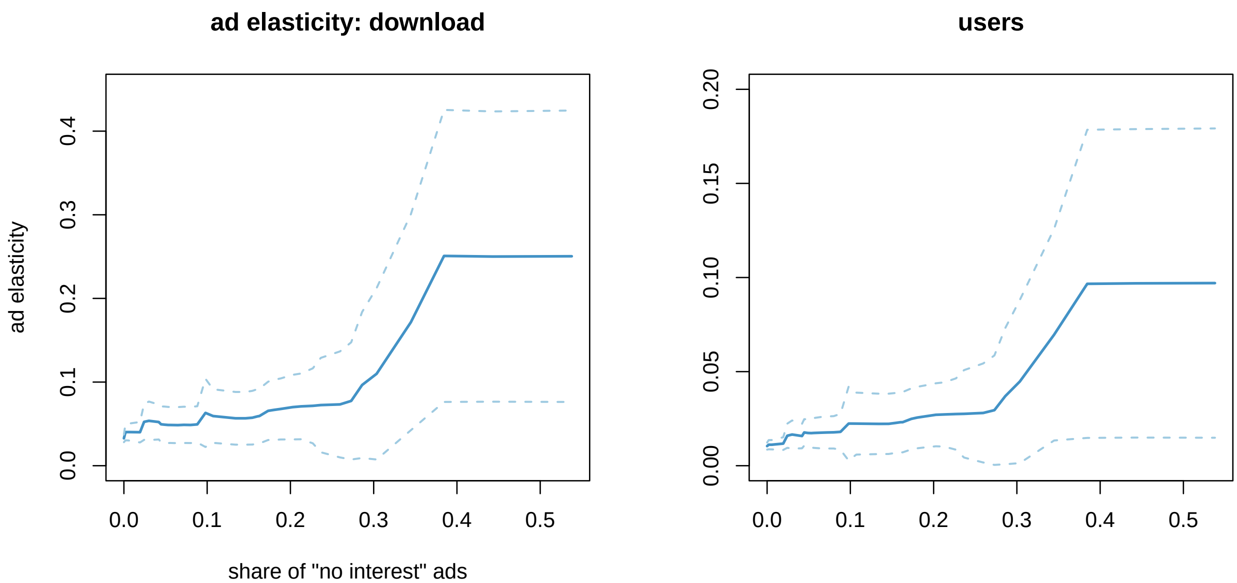
This figure shows the distribution of advertising elasticities estimated by the generalized random forests and projected onto the observed covariates.

Figure 7: Importance Weights of Different App and Ad Features



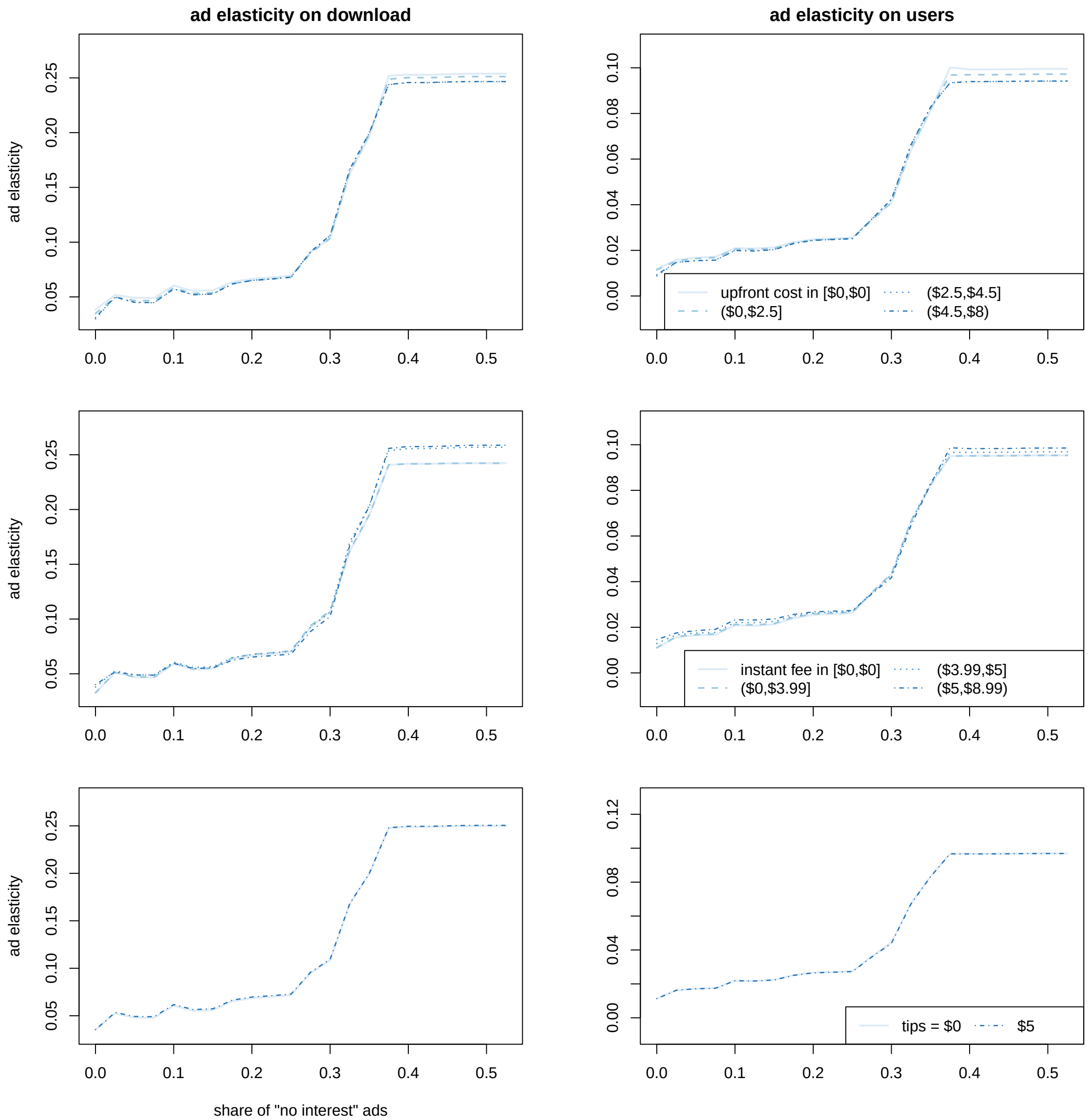
This figure shows the importance weights of different app and ad features in the generalized random forest estimation. The importance of a variable is calculated as the share of tree splits on that variable's node among all tree splits. A variable takes a high importance if much of the differences in $\beta(x)$ relies on the forest splitting on this variable.

Figure 8: Ad Elasticities as a Function of "No Interest" Claims



This figure shows the predicted advertising effects $\beta(x_{j,m(t)})$ for apps where the share of "no interest" claims in the campaign varies between 0 and 54%. At each value of "no interest" share, we integrate over all other characteristics from their joint empirical distribution. Dashed lines mark 95% confidence intervals. The results imply that an increase of 1,000 ad impressions will convert to 0.87 new downloads and 9.12 active users. Since the increase in active users far exceeds the increase in new downloads, most of the advertising effect comes from existing users.

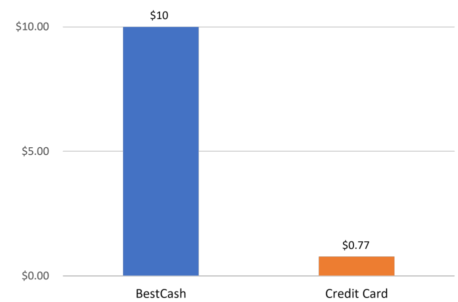
Figure 9: Are Ad Elasticities Affected by the App's True Costs?



This figure shows the predicted advertising effects $\beta(x_{j,m(t)})$ for apps where the share of "no interest" claims in the campaign varies between 0 and 54%. In each panel, one specific fee component varies between four quartiles of its support whereas other components are held at zero.

Figure 10: Survey Conditions: 2 × 3 design

(a) 2 (cost comparison)

No comparison	Cost comparison with credit cards
No comparison page	

(b) 3 (ad)

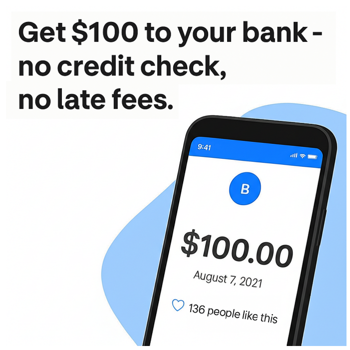
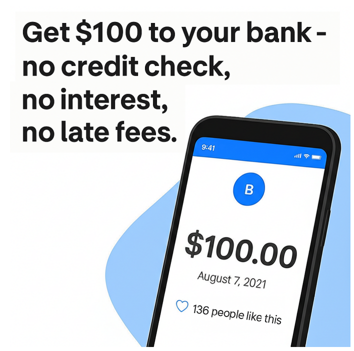
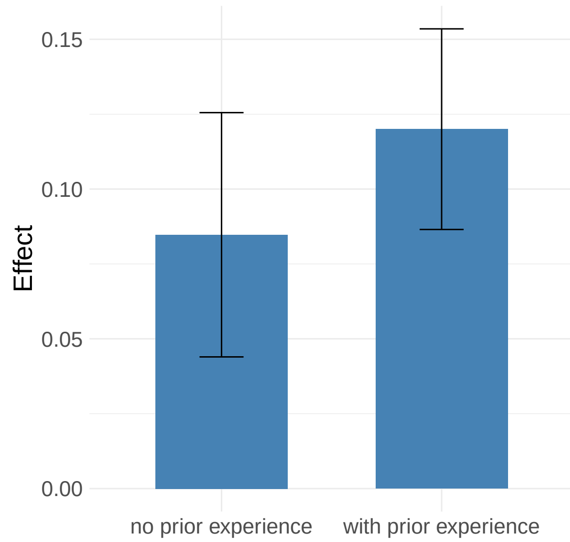
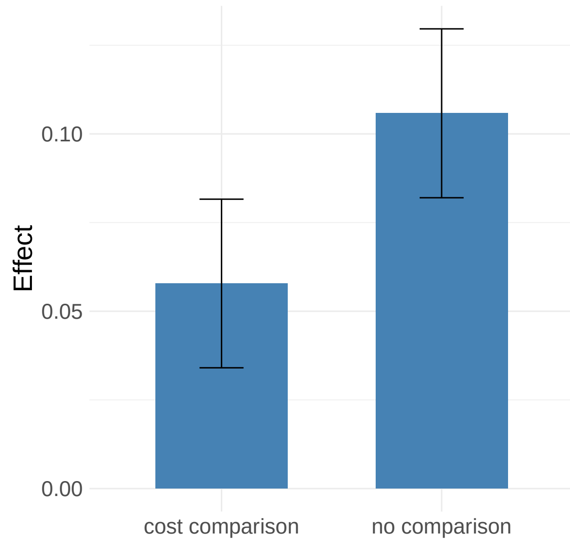
Control	Other ad	"No interest" ad
No ad page		

Figure 11: Effect of "No Interest" Ad by Prior Experience



This figure shows the effect of "no interest" ad on the share of participants believing the cost is zero by prior experience.

Figure 12: Effect of "No Interest" Ad by Cost Comparison



This figure shows the effect of "no interest" ad on the share of participants believing the cost is zero by cost comparison.

Table 1: Summary Statistics

	mean	10%	50%	90%
average daily users (in thousands)	56	2	26	103
peak daily users (in thousands)	127	10	58	202
total download (in thousands)	3,233	387	2,227	7,896
90-day retention rate (%)	5	4	5	8
total impressions (in millions)	784	0	143	2,455
upfront cost (in dollar)	3	0	2	7
instant fee (in dollar)	3	0	2	9
tips (in dollar)	2	0	0	5
APR typical case (%)	270	69	235	455

This table shows the summary statistics of the major variables across the 22 cash advance apps.

Table 2: Fraction of "No Interest" Claims Over Time

	fraction of no interest claims	
time trend	0.186***	0.230***
	(0.006)	(0.005)
Constant	-0.028***	
	(0.003)	
App FE	No	Yes
Observations	13,634	13,634
R^2	0.077	0.278
Note:	*p<0.1; **p<0.05; ***p<0.01	

This table shows the positive time trend in the fraction of "no interest" claims in cash app advertising. The "time trend" variable is defined between 0 and 1.

Table 3: Average Advertising Elasticities

	log(download) (delta = 0.16)			log(users) (delta = 0.11)		
log(impression stock+1)	0.036***	0.036***	0.036***	0.011***	0.011***	0.011***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
log(impression stock opponent+1)		-0.003			0.001	
		(0.004)			(0.002)	
App-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	No	No	Yes	No	No	Yes
Observations	25,791	25,791	25,791	25,791	25,791	25,791
Note:				*p<0.1; **p<0.05; ***p<0.01		

This table presents the average advertising elasticities estimated by fixed effects regressions. At the sample averages of new downloads and active users, these estimates imply that every 1,000 advertising impressions convert to a 0.12 increase in new downloads and a 1.06 increase in active users.

Table 4: Retention Rates and the Fraction of "No Interest" Claims

	90-day retention rate		
share of no-interest ads	-0.006	0.030	0.001
	(0.014)	(0.044)	(0.067)
share of no-interest ads x log(impression + 1)		-0.003	
		(0.004)	
log(impression + 1)		0.0004	
		(0.0004)	
upfront cost			-0.002
			(0.001)
instant fee			-0.002
			(0.002)
tips			-0.001
			(0.002)
Constant			0.066***
			(0.008)
App FE	Yes	Yes	No
Quarter FE	Yes	Yes	No
Observations	285	285	22
R^2	0.736	0.738	0.172

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows regression coefficients between apps' retention rates and the fraction of "no interest" ads. Column 1 estimates equation (5) using app-quarter level data and controlling for app and quarter fixed effects. Column 2 adds the number of advertising impressions and its interaction with the fraction of "no interest" ads. Column 3 regresses the two variables using app-level averages and controls for the three fee measures. All columns show that there is no relationship between retention rate and "no interest" claims.

Table 5: **Effects of "No Interest" Ad on Consumer Cost Belief in Survey**

	dollar cost average	dollar cost =\$0	relative cost average	relative cost cost ≥ credit card
No interest ads	-2.268***	0.089***	-0.228**	0.098**
	(0.610)	(0.019)	(0.100)	(0.041)
Other ads	-0.706	0.025	-0.230**	0.091**
	(0.615)	(0.019)	(0.100)	(0.042)
Baseline	14.469***	0.017	3.664***	0.411***
	(0.432)	(0.013)	(0.070)	(0.029)
Observations	868	868	868	868

Note: *p<0.1; **p<0.05; ***p<0.01

This table presents the effects of "no interest" ad on consumer cost belief in the survey. The dollar cost refers to the absolute borrowing cost of the cash advance app (answers in a \$0 - \$30 7-point likert scale). The relative cost refers to the borrowing cost of the cash advance app relative to a typical credit card (answers in a 5-point likert scale).

Appendix

A Assembling of ad impression data

The advertising impression data is from Sensor Tower, a leading data provider in the mobile industry. We collect daily impression share data from the company's API. The data comes in small chunks - each chunk records campaign-level normalized impression shares for 100 apps for a 29-day window. We then need to assemble the impression shares across all apps and dates and re-scale the normalization across chunks. We take the following steps to assemble this data:

1. Define each 100 apps as a batch. First, for each batch, we calculate inter-date ratios based on the overlapping date between chunks.

2. Using the ratios, we re-scale and combine data across dates for each batch.

3. To re-scale the impression shares across batches, we use a separate dataset of monthly advertising impression shares collected across batches.

4. We match the daily and monthly level data to calculate the inter-batch ratios. For cases that there is no advertising on the overlapping date for inter-date ratio calculation, we also match the daily and the monthly level data to calculate the inter-date ratios.

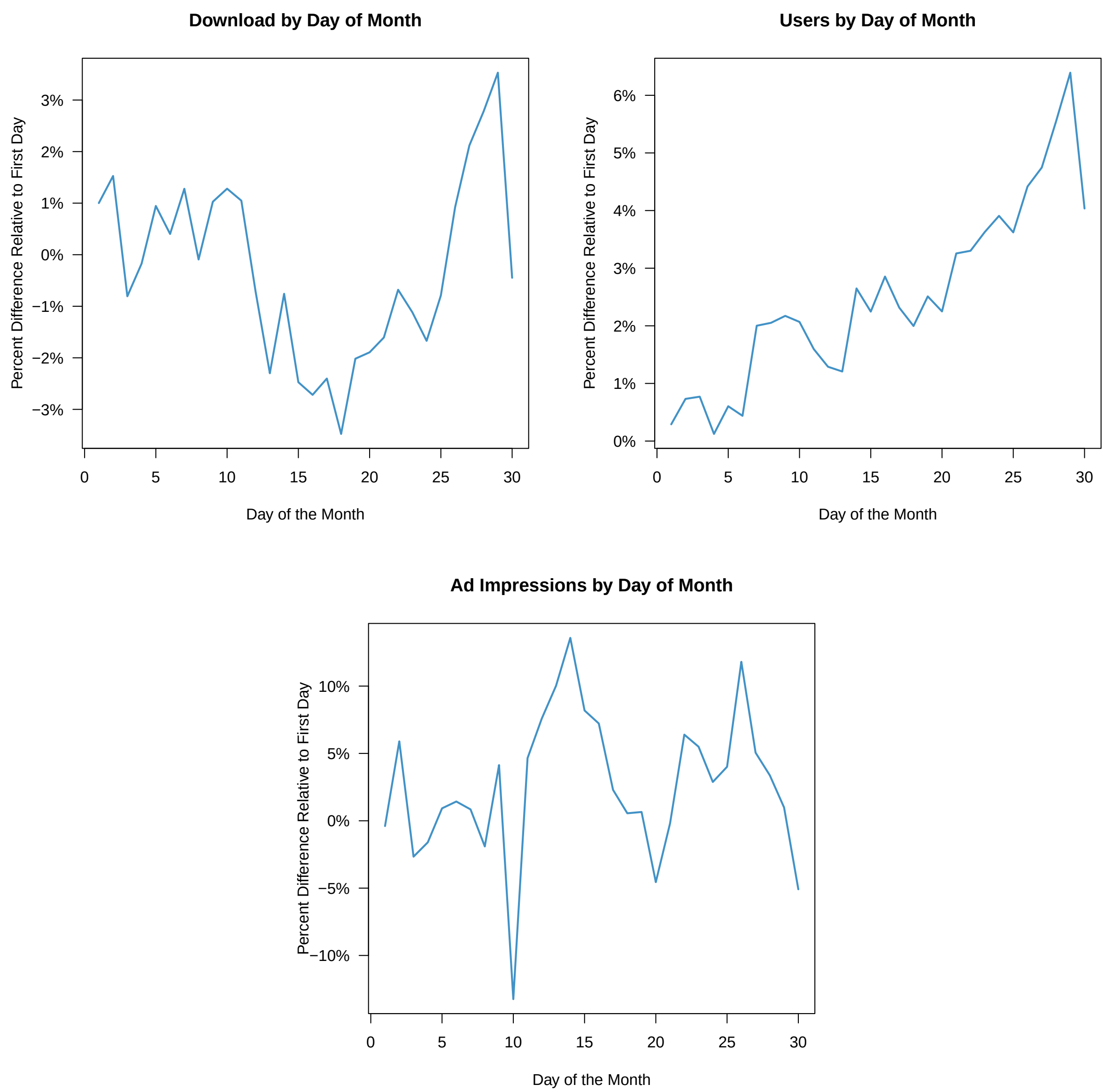
5. Using the inter-batch ratios, we re-scale and combine data across batches. We then save them as final files of impression shares.

6. We then multiple these shares by the total number of impressions among iOS users to gett the daily impressions for each advertising campaign.

B Additional data patterns

Figure B1: Upfront Cost of Varo

Figure B2: Download, Usage, and Ad Impressions by the Day of Month



This figure plots the percentage difference in downloads, active users, and ad impressions relative to the first day of the month. As shown, the number of users increases in the day of the month, while there is no specific trend for downloads. Ad impressions appear uncorrelated with either downloads or users.

Table B1: Publisher App Features

	Ads with 'no interest' claims	Other ads from apps that advertise 'no interest'	Other ads from other apps
Female (%)	49.5	48.9	48.5
Average age	31.8	32	33.3

This table presents the characteristics of publisher apps for ads with "no interest" claims, other ads by advertisers that ever advertise "no interest" claims, and other ads by the other apps. Publisher apps are apps that show ads to users and get paid by ad exchanges.

C Estimation and Identification under a General Model with Daily Advertising Features

In this section, we first outline a general model in which advertising features vary daily, denoted x_{jt}. We then show how to transform the model to one with app-month specific advertising features x_{jm} and how to handle fixed effects. Lastly, we present the identifying assumptions under which using x_{jm} still provides consistent estimates of the advertising effect. The general model is:

$$\log(\text{users}_{jt}) = \beta(x_{jt}) \cdot \log(\text{advertising}_{jt} + 1) + \alpha_{j,m(t)} + u_{jt} \quad (1)$$

where j indexes products, t indexes days, and $m(t)$ denotes the month of day t.

C.1 Estimation

Our main specification models advertising effects as a function of the average ad feature at the product-month level, $x_{j,m(t)}$:

$$\log(\text{users}_{jt}) = \beta(x_{j,m(t)}) \cdot \log(\text{advertising}_{jt} + 1) + \alpha_{j,m(t)} + \epsilon_{jt} \quad (2)$$

where the composite error term is:

$$\epsilon_{jt} = [\beta(x_{jt}) - \beta(x_{j,m(t)})] \cdot \log(\text{advertising}_{jt} + 1) + u_{jt} \quad (3)$$

This error captures both the original idiosyncratic error and the deviation from using month-level instead of daily-level effects.

We first apply within-transformation at the app-month level to eliminate fixed effects $\alpha_{j,m(t)}$. Define $\widetilde{y}_{jt} = y_{jt} - \mathbb{E}[y_{jt}|j, m(t)]$ as the deviation of variable y from its conditional mean at the app-month level. Applying this within-transformation to equation (2):[17]

$$\widetilde{\log(\text{users}_{jt})} = \beta(x_{j,m(t)}) \cdot \widetilde{\log(\text{advertising}_{jt} + 1)} + \widetilde{\epsilon}_{jt} \quad (4)$$

Next, we use generalized random forests to estimate equation (4), which approximates $\beta(x_{j,m(t)})$ with random forests while minimizing deviations from the identifying moment $\mathbb{E}[\widetilde{\log(\text{advertising}_{jt} + 1)} \cdot \widetilde{\epsilon}_{jt}] = 0$. In this second step, we use Athey et al. (2019)'s R package.

[17] Equation (4) follows equation (2) because $\beta(x_{j,m(t)})$ is constant within each product-month:

$$\begin{aligned}
\beta(x_{j,m(t)}) \cdot \widetilde{\log(\text{advertising}_{jt} + 1)} &= \beta(x_{j,m(t)}) \cdot \log(\text{advertising}_{jt} + 1) - \mathbb{E}[\beta(x_{j,m(t)}) \cdot \log(\text{advertising}_{jt} + 1)|j, m(t)] \\
&= \beta(x_{j,m(t)}) \cdot \log(\text{advertising}_{jt} + 1) - \beta(x_{j,m(t)}) \cdot \mathbb{E}[\log(\text{advertising}_{jt} + 1)|j, m(t)] \\
&= \beta(x_{j,m(t)}) \cdot \widetilde{\log(\text{advertising}_{jt} + 1)}.
\end{aligned}$$

C.2 Identifying Assumptions

This estimation routine obtains consistent estimates of $\beta(x_{j,m(t)})$ under the following two identifying assumptions:

Assumption 1 (Strict Exogeneity of Advertising):

$$\mathbb{E}[u_{jt'} \,|\, \log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)}] = 0, \tag{5}$$

for any $t, t' \in m(t)$. This is a standard assumption requiring that advertising impressions within a month is not correlated to any day's unobserved demand shock u. We later demonstrate that, within app-month, daily variation in advertising impressions does not seem to be correlated with some targetable observable demand shifters, such as the proximity to typical paycheck dates, and whether the app provides an update.

Evidence Supporting the Assumption 1. We present evidence that supports the Assumption 1. Whereas Assumption 1 is not directly testable, we demonstrate that daily advertising impressions are not targeted to observable demand shifters—namely, updates and proximity to paycheck dates. Huang et al. (2023) document that update is a major demand shifter in the mobile app market. If apps were strategic and able to precisely time their advertising releases, they would likely concentrate impressions around app updates. Similarly, demand for cash advance apps may fluctuate with paycheck cycles and may peak at the end of the cycle. If advertising impressions were targeted to align with paycheck timing, we would expect a negative correlation between advertising impressions and the number of days remaining until the end of the month.

Table C1 demonstrates that, for a subset of 7 apps for which we observe daily update information, daily impressions have a negligible correlation with whether an update appears on that day. Table C2 further shows that impressions do not have a strong correlation with the number of days until paycheck dates. These results support the Assumption 1.

Table C1: Ad Impressions Not Correlated with Updates

	log(impression + 1)	any impression
release update	0.0002	0.009
	(0.051)	(0.010)
App-month FE	Yes	Yes
Observations	7,575	7,575
R^2	0.833	0.727
Note:		*p<0.1; **p<0.05; ***p<0.01

This table shows that the number of advertising impressions is not correlated with app updates. Note that update information is only available for a subset of apps.

Table C2: Ad Impressions Not Correlated with Days until Paycheck Dates

	log(impression + 1)	any impression
days until end of month	-0.002	-0.0004
	(0.005)	(0.001)
... squared	0.00003	0.00002
	(0.0002)	(0.00003)
App-month FE	Yes	Yes
Observations	25,791	25,791
R^2	0.850	0.756
Note:		*p<0.1; **p<0.05; ***p<0.01

This table shows that the number of advertising impressions is not correlated with the number of days until the end of month.

Assumption 2 (Mean Independence of Ad Features and Impressions):

$$\mathbb{E}[\beta(x_{jt}) - \beta(x_{j,m(t)})|\log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)}] = 0 \tag{6}$$

Assumption 2 states that within app-month, daily variation in ad-effect driving features, $\beta(x_{jt})$, are uncorrelated with advertising impressions. It rules out strategic behavior where advertisers systematically deploy more effective creative content on days when they increase advertising intensity. This could be violated if, for example, advertisers use their best-performing ads during high-stakes periods with large advertising budgets.

Evidence Supporting the Assumption 2. While the Assumption 2 might not be directly testable as the shape of the function $\beta(x_{jt})$ is unknown, we can alternatively test whether $\mathbb{E}[x_{jt} - x_{j,m(t)}|\log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)}] = 0$. This is a good approximation when either (1) the variation in x_{jt} can be largely explained by x_{jm}, or (2) the function $\beta(x_{jt})$ is close to a linear shape. We find that the app-month fixed effects can explain 67% of the daily variation in the no interest$_{jt}$ variable and 61%-74% variation in the other ad feature variables, suggesting that x_{jm} is a good approximation of x_{jt}. The alternative test follows the idea of Hyslop and Imbens (2001) for optimal prediction error in linear regression models.

As we show empirically in Table C3, daily variation in the fraction of "no interest" ads—the most important ad feature in our context—appears orthogonal to advertising impressions within app-month. This result provides support for the Assumption 2.

Table C3: Ad Impressions Not Correlated with "No Interest"

	log(impression + 1)
$x_{jt} - x_{jm}$	0.231
	(0.155)
App-month FE	Yes
Observations	13,634
R^2	0.802
Note:	*p<0.1; **p<0.05; ***p<0.01

This table shows that the number of advertising impressions is not correlated with the use of "no interest" claims. Note that the share of "no interest" ads can only be measured on dates where the number of impressions is not zero.

Under the Assumptions 1 and 2. Under the Assumptions 1 and 2, we derive the orthogonality condition required for obtaining consistent estimates of $\beta(x_{j,m(t)})$. Consider the conditional expectation of the composite error:

$$
\begin{aligned}
&\mathbb{E}[\epsilon_{jt} | \log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)}] \\
&= \mathbb{E}\left[[\beta(x_{jt}) - \beta(x_{j,m(t)})] \cdot \log(\text{advertising}_{jt} + 1) + u_{jt} \middle| \log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)} \right] \\
&= \log(\text{advertising}_{jt} + 1) \cdot \mathbb{E}[\beta(x_{jt}) - \beta(x_{j,m(t)}) | \log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)}] \\
&\quad + \mathbb{E}[u_{jt} | \log(\text{advertising}_{jt} + 1), \alpha_{j,m(t)}] \\
&= \log(\text{advertising}_{jt} + 1) \cdot 0 + 0 = 0
\end{aligned} \tag{7}
$$

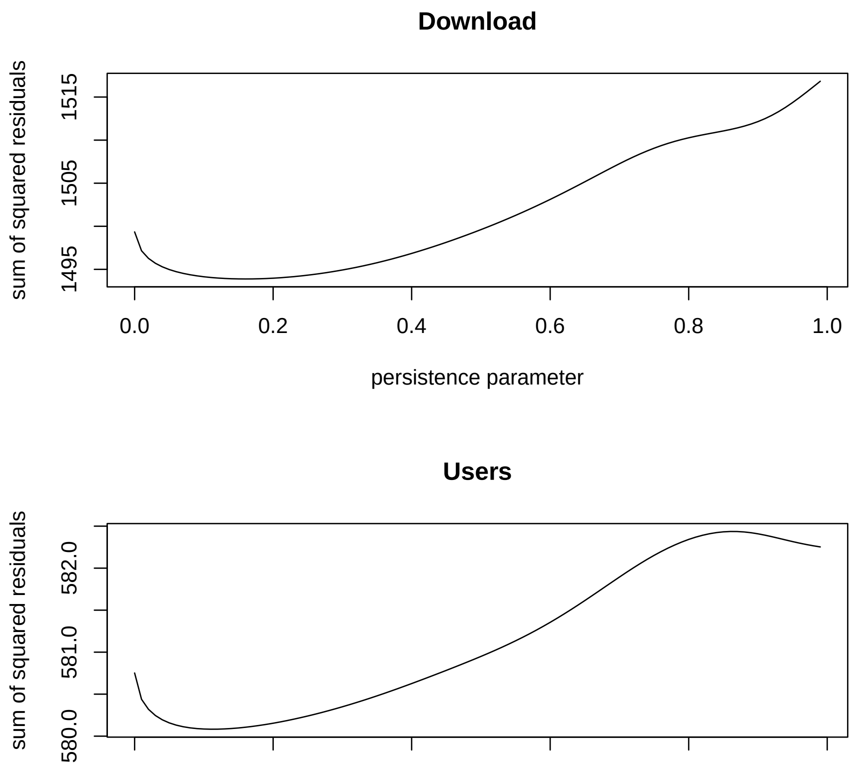
where the last equality follows from Assumptions 1 and 2.

Since the within-transformation preserves conditional mean independence, we arrive at:

$$
\mathbb{E}[\widetilde{\epsilon}_{jt} | \log(\widetilde{\text{advertising}}_{jt} + 1)] = 0. \tag{8}
$$

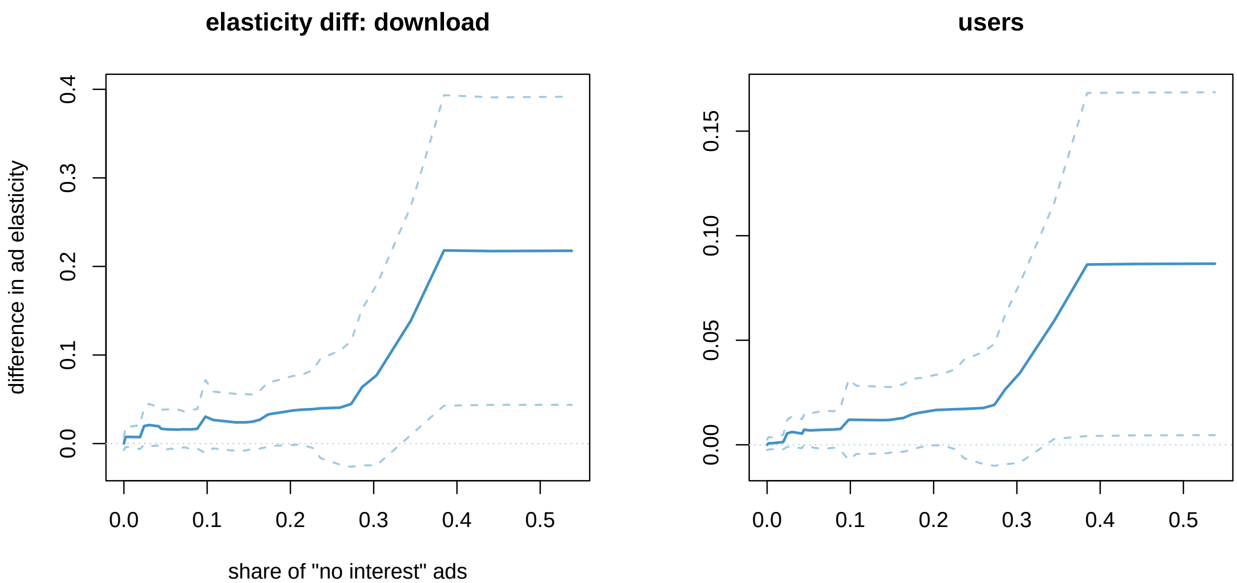
D Additional figures and tables for results

Figure D1: Grid Search for the Best-Fitting Persistence Parameters



This figure shows the sum of squared residuals from estimations for different persistence parameters.

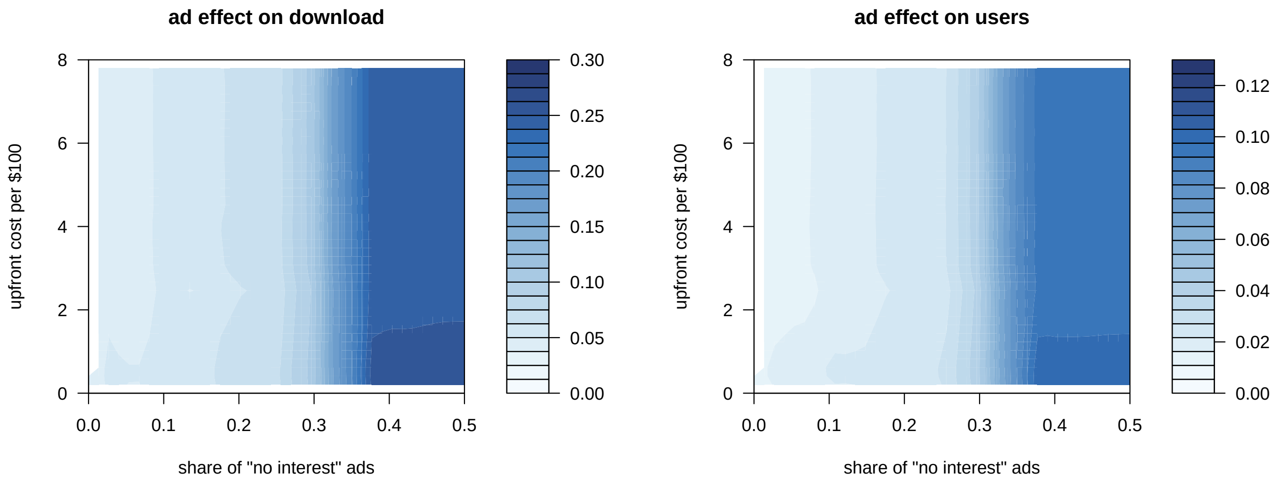
Figure D2: Statistical Tests of Null $\beta(x) = \beta(0)$



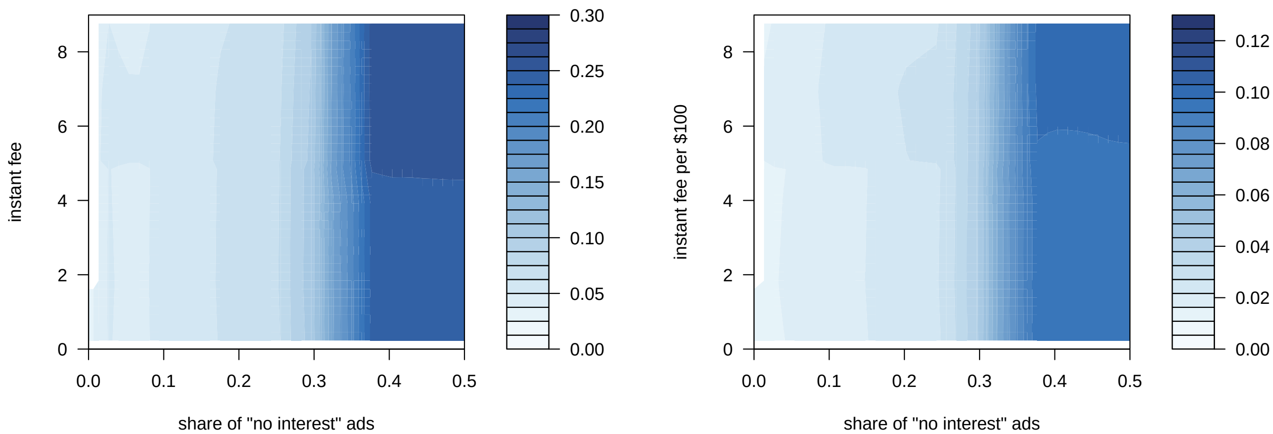
This figure shows the difference $\beta(x) - \beta(0)$ where $x \in (0, 0.5]$ is the share of "no interest" claims. The $\beta(x)$'s are averaged across the empirical distribution of other app and ad characteristics. Dashed lines present 95% confidence intervals. The dotted horizontal line is at 0, representing the null hypothesis that advertising effects under "no interest" claims are the same as those without such claims.

Figure D3: Are Ad Elasticities Affected by the App's True Costs?

(A) upfront cost



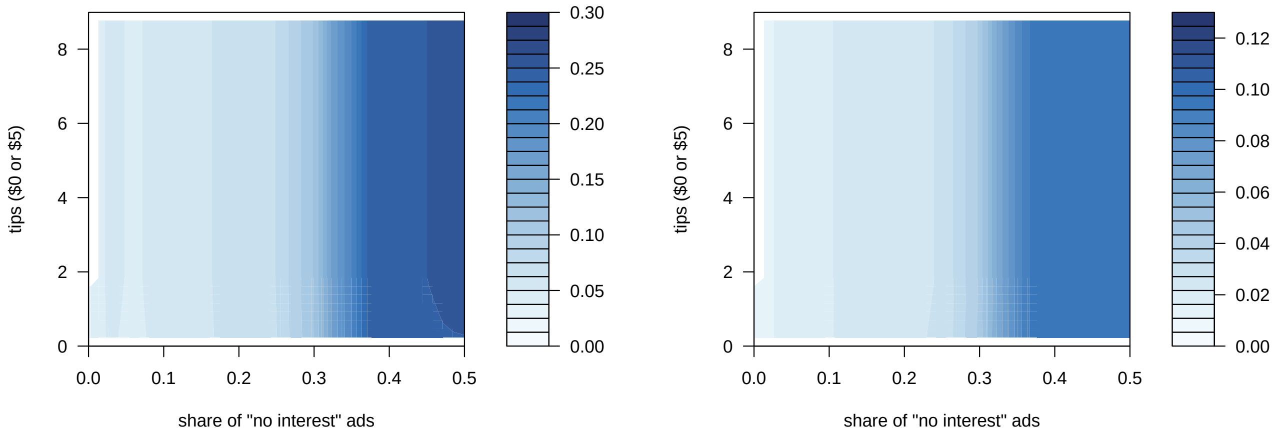
(B) Instant fee



This figure shows the predicted advertising effects $\beta(x_{j,m(t)})$ for apps where the share of "no interest" claims in the campaign varies between 0 and 54% (x-axis). In panel A, we also vary the upfront cost per \$100 (y-axis). In panel B, we vary the instant fee (y-axis).

Figure D4: Are Ad Elasticities Affected by the App's True Costs? (Con'd)

(C) Tips



This figure shows the predicted advertising effects $\beta(x_{j,m(t)})$ for apps where the share of "no interest" claims in the campaign varies between 0 and 54% (x-axis). In panel C, we vary the tips (y-axis).

E Robustness checks

Table E1: Robustness to Level Specification

	download (delta = 0.16)			users (delta = 0.11)		
impression stock	0.035***	0.035***	0.035***	0.167***	0.169***	0.170***
	(0.007)	(0.007)	(0.007)	(0.043)	(0.043)	(0.042)
impression stock opponent		-0.001			-0.007	
		(0.001)			(0.011)	
App-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	No	No	Yes	No	No	Yes
Observations	25,791	25,791	25,791	25,791	25,791	25,791
Note:				*p<0.1; **p<0.05; ***p<0.01		

This table presents the average advertising elasticities estimated with the linear (rather than log) specification.

Table E2: Robustness to Including Alternative Fixed Effects

	log(download) (delta = 0.16)			log(users) (delta = 0.11)		
log(impression stock+1)	0.036***	0.036***	0.036***	0.011***	0.011***	0.011***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
App-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Date FE	No	No	Yes	No	No	Yes
App-quadratic Trend	No	Yes	Yes	No	Yes	Yes
Observations	25,791	25,791	25,791	25,791	25,791	25,791
Note:				*p<0.1; **p<0.05; ***p<0.01		

This table presents the average advertising elasticities estimated with app-month, app-specific quadratic trend, and date fixed effects.

Table E3: Robustness to Including Lagged Download and Users

	log(download)			log(users)		
log(ad stock+1)	0.036***	0.035***	0.035***	0.011***	0.011***	0.012***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)
log(download t-4)		0.091***				
		(0.019)				
log(download t-7)			0.083***			
			(0.016)			
log(users t-4)					0.003	
					(0.008)	
log(users t-7)						0.069***
						(0.012)
App-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,791	25,791	25,791	25,791	25,791	25,791

Note: *p<0.1; **p<0.05; ***p<0.01

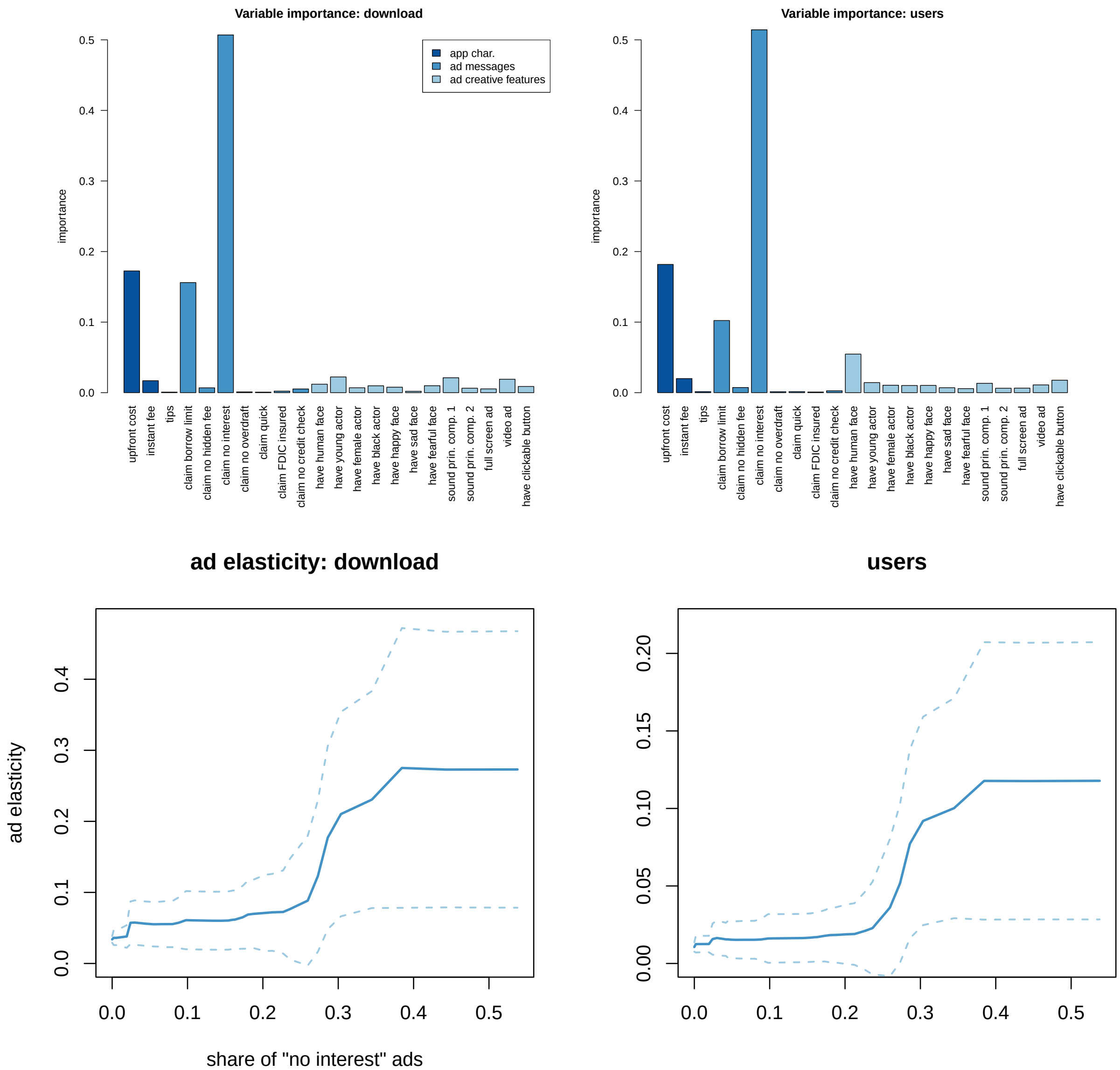
This table presents the average advertising elasticities estimated with app-month fixed effects and lagged 4-day or 7-day outcome variables (log users and downloads).

Table E4: Regression Results with Interaction Terms

	log(download)			log(users)		
log(impression stock+1)	0.028***	0.018	0.022	0.008***	0.007	-0.015
	(0.003)	(0.027)	(0.020)	(0.001)	(0.009)	(0.017)
frac no interest x log(impression stock+1)	0.164***	0.181**	0.261***	0.056**	0.089**	0.060*
	(0.059)	(0.076)	(0.078)	(0.026)	(0.037)	(0.036)
Including other feature interactions	No	Yes	Yes	No	Yes	Yes
Including app dummy interactions	No	No	Yes	No	No	Yes
App-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,791	25,791	25,791	25,791	25,791	25,791
Note:				*p<0.1; **p<0.05; ***p<0.01		

This table presents linear regression estimates of the advertising effects of “no interest” claims.

Figure E1: Robustness to Including Date Fixed Effects



This figure shows the estimates using generalized random forests with app-month and date fixed effects.

F Survey

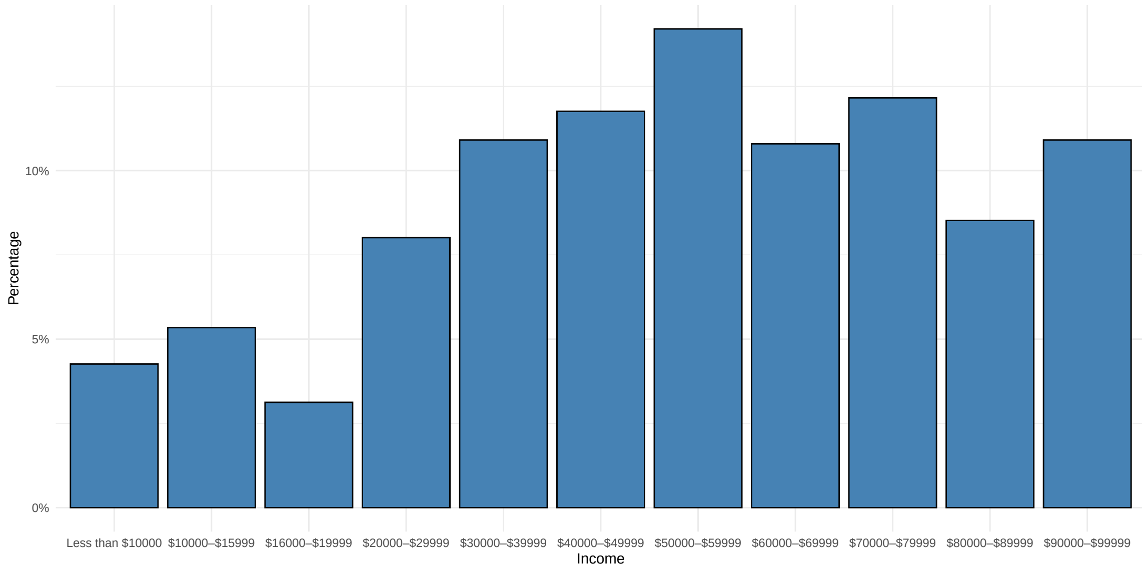
Table F1: Survey Summary Statistics

	All participants	Conditional on prior usage
Times used cash advance apps before (%)		
0	42	
1-2	22	
3-5	18	
6-10	9	
10+	9	
Perceived APR		
Median	24	24
APR > 100 (%)	16	23
Has a credit card (%)	95	93
Credit card remaining credit (%)		
<$10 (max out)	9	10
$10-$100	15	20
$101-$200	16	23
$201-$500	17	20
$500+	44	25
Median income ($)	55000	55000
Median age	36	35
Female (%)	57	57
Observations	1,760	1,025

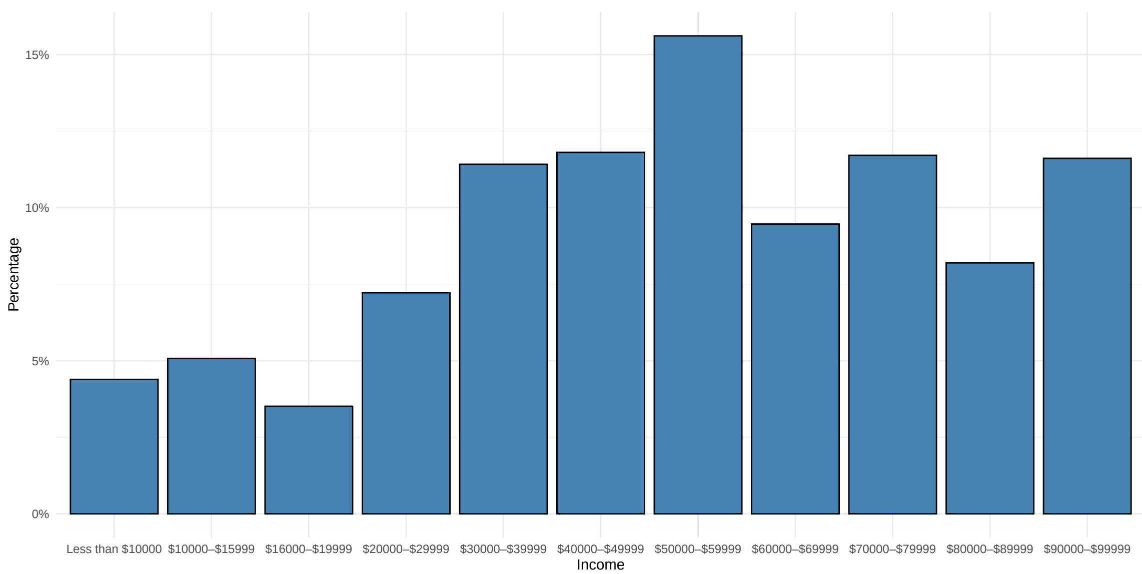
This table presents summary statistics for all survey participants (left) and for those with prior cash advance app usage (right).

Figure F1: Income Distribution of Survey Participants

(a) All participants



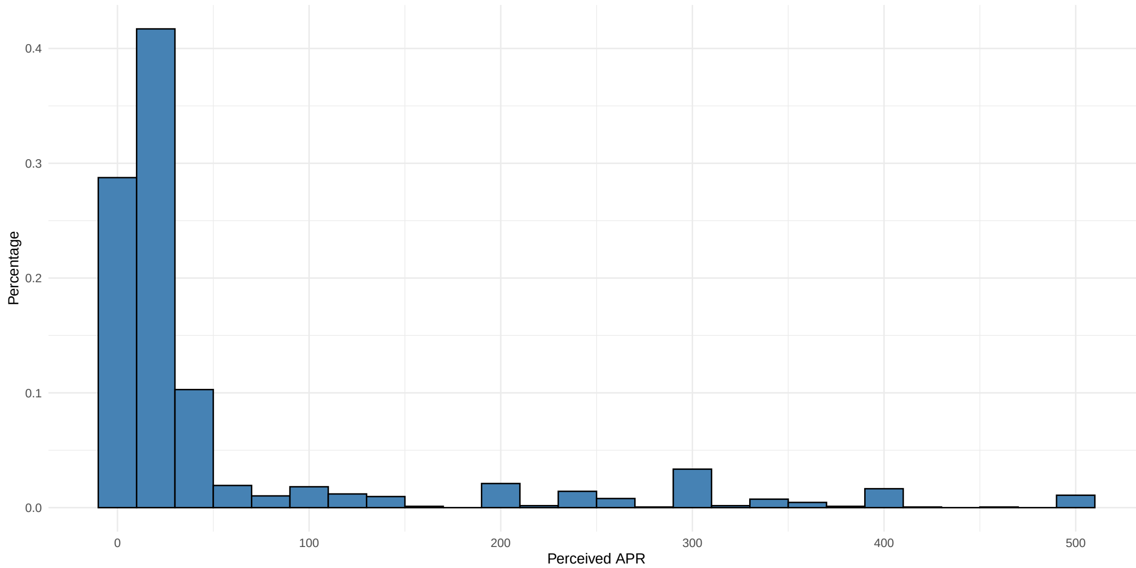
(b) Participants with Prior Experience of Cash Advance Apps



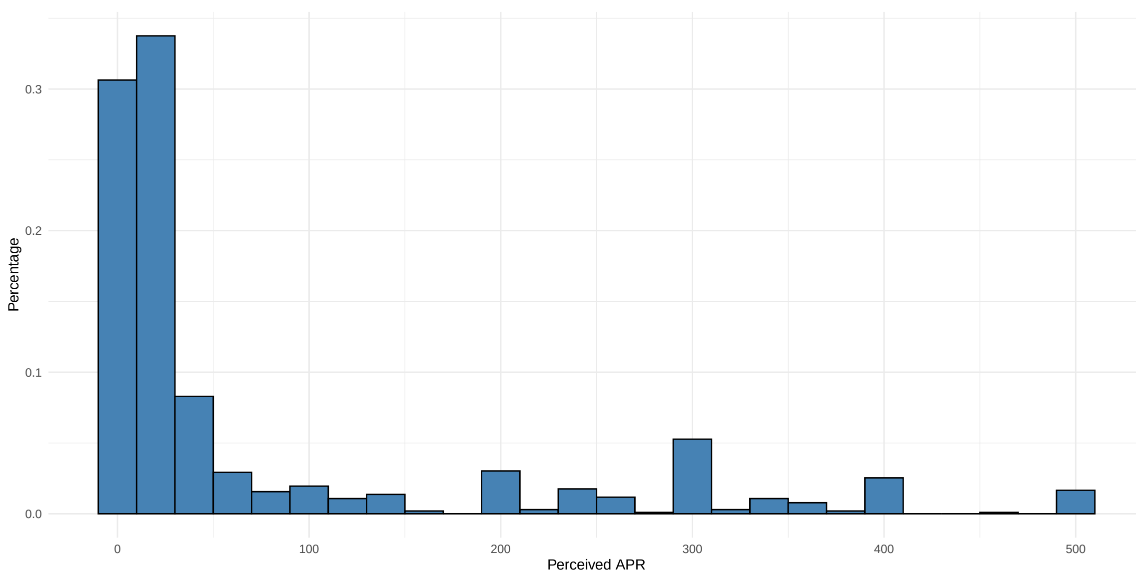
This figure shows the income distribution for all survey participants (top) and for those with prior cash advance app usage (bottom).

Figure F2: Perceived APR Distribution of Survey Participants

(a) All participants



(b) Participants with Prior Experience of Cash Advance Apps



This figure shows the perceived APR distribution for all survey participants (top) and for those with prior cash advance app usage (bottom).