Evaluating The Impact of Privacy Regulation on E-Commerce Firms: Evidence from Apple's App Tracking Transparency[*]

Guy Aridor [†]   Yeon-Koo Che [‡]   Brett Hollenbeck[§]

Maximilian Kaiser [¶]   Daniel McCarthy [‖]

June 2024

Abstract

In the backdrop of heightened data privacy concerns, governments and firms have introduced various privacy regulations. Assembling novel datasets on online advertiser spending, performance, and revenue, we quantify the economic effects of Apple's App Tracking Transparency (ATT) privacy policy on e-commerce firms. Our paper extends prior literature by uncovering the effect of ATT on not only advertising effectiveness but also changes in firm-side strategy to mitigate the effects of ATT and the resulting net impact on overall firm revenue. We show that the performance of conversion-optimized Meta advertisements, which critically depend on third-party data for targeting and measurement, significantly worsened after ATT with a 37% reduction in click-through rates compared to click-optimized campaigns. Although advertisers substitute away from Meta for the Google ecosystem, those with a higher baseline dependence on Meta nevertheless experience a 39% reduction in firm-wide revenue. We find that this is driven primarily by steeper declines at smaller firms in the acquisition of new customers vis-a-vis retaining existing customers.

[†]Northwestern University, Kellogg School of Management; guy.aridor@kellogg.northwestern.edu

[‡]Columbia University; yeonkooche@gmail.com.

[§]UCLA Anderson School of Management; brett.hollenbeck@anderson.ucla.edu

[¶]Hamburg University, Grips Intelligence; maximilian.kaiser@gripsintelligence.com

[‖]Emory University Goizueta Business School; daniel.mccarthy@emory.edu

1 Introduction

Digital advertising comprises the largest share of advertising spending at U.S. firms, surpassing both TV and print advertising in 2019, and reaching $522 billion in total spending worldwide in 2021.[1] Online advertising owes its success to two key features. First, online advertising allows firms to target consumers precisely using granular data on their online behavior, including their past purchases. Second, it allows for direct response and measurement, enabling firms to track the performance of their advertising spending to a greater degree than ever before and optimize their targeting using rapid feedback from prior campaigns.[2] The targeting enabled by this advertising technology lowered consumer acquisition costs and purportedly created a large ecosystem of ecommerce firms that sell products and services directly to consumers (DTC). This technology, therefore, has potentially large positive welfare ramifications as it enables the existence of these firms by cheaply and efficiently matching them to their focal consumers.

However, many consumers and privacy advocates have criticized this operating model. Their critiques have centered around the tracking of consumers' behavior by online platforms and advertising intermediaries, including third parties to whom consumers may not have explicitly consented to collect their data. These privacy concerns have led to several privacy policies, both by regulators and industry, such as the EU's General Data Protection Regulation (GDPR), Apple's App Tracking Transparency, Google's Privacy Sandbox, and the European Data Protection Board restricting the ability of digital platforms to use behavioral advertising. These privacy-oriented policies reduce firms' access to information about customers' online activity and, thus, their abilities to deliver targeted advertising to their consumers. Consequently, online retail firms, particularly DTC firms, may suffer higher costs in reaching their target consumers, entailing potential welfare loss for these firms and their customers.

---

[1]https://www.statista.com/statistics/237974/online-advertising-spending-worldwide/

[2]This is as opposed to general brand advertising, which does not feature a direct response to measure and is designed to build brand equity over time (Borkovsky et al., 2017).

In this paper, we quantify the economic costs to direct-to-consumer firms of one such privacy policy – Apple's App Tracking Transparency (ATT) – which allows Apple iOS users to opt out of data sharing across their apps (third-party data sharing) by prompting users to either allow or disallow data sharing. Press reports and tech analytics firms suggest that the vast majority (roughly 80-85%) of users shown this prompt opted out of data sharing (Wagner, 2021, Chen, 2021, Laziuk, 2021) and, without this data, it was difficult for advertising platforms to effectively measure and target advertisements.

Our investigation focuses on three broad questions. First, to what extent is advertising effectiveness impacted across various online advertising platforms? While experimental evidence has established that losing access to the off-site third-party data could substantially curtail the effectiveness of Meta advertising (Wernerfelt et al., 2022), the true effects of ATT on advertisers and their magnitude are unclear since both advertisers and ad networks such as Meta would adjust behavior to mitigate them.[3] Second, how do advertisers substitute across advertising platforms in the aftermath of differential changes in advertising effectiveness across platforms? Third, to what extent did ATT impact firm revenues and customer acquisition? Even if certain targeted advertising channels are adversely impacted, advertisers substituting other forms of advertising for customer acquisition could limit the revenue impact. To understand the ultimate economic costs of ATT, our empirical analysis, therefore, focuses primarily on quantifying the impact on firm revenue.

We combine several unique sources of data to answer these questions. First, we use data from Grips Intelligence, a data analytics and market intelligence firm that observes all data available through Google Analytics for thousands of ecommerce firms worldwide. This provides us with a uniquely detailed global view of firm-level traffic and revenues by device type, operating system, and traffic source. Second, we use data from an anonymous data provider that enables a granular view of advertising spend and performance across Meta,

---

[3]Here and throughout, we use the term Meta advertising to refer to advertising done on Facebook, Instagram, and the Meta Audience Network as we do not distinguish between these in our analysis. In principle, other mobile app advertising platforms such as Snapchat, Tiktok, and Pinterest are also impacted by the ATT policy change, but as Meta is the dominant player in this industry we focus on it.

Google, and TikTok, as well as revenue data from Shopify that differentiates between new and returning consumers. The combination of these datasets allows us to precisely measure the extent to which targeting was impacted and to quantify the downstream revenue costs of ATT.

To answer our first question we use the advertising data to establish whether and to what extent the effectiveness of targeting was impacted. By comparing advertising performance across campaign objectives (which we observe), we first test whether Meta advertising was impacted by ATT. Specifically, we perform a within-advertiser comparison between advertising campaigns that optimize for a third-party (off-platform) objective to those that optimize for a first-party (on-platform) objective and find that this type of advertising was severely impacted. We find a 37% reduction in click-through rates for third-party optimized campaigns relative to first-party campaigns. Second, we show evidence that firms reallocated their advertising budgets away from Meta towards other channels and in particular shift roughly 5 percentage points of market share to Google ads, which was expected to be less impacted relative to Meta. This reallocation suggests that more Meta-dependent firms attempted to mitigate the reduced effectiveness of Meta advertising by reducing spend or moving to other advertising channels, and, as such, in order to fully characterize the impact of the policy, we need to understand its impact on downstream revenue.

Our empirical strategy is to compare firm revenues before and after the ATT policy, accounting for unrelated revenue trends by comparing firms with differing levels of exposure to the ATT policy. We stratify sellers in terms of ATT exposure using the extent to which they rely on Meta advertising for revenue generation and then perform a difference-in-differences estimation using the April 2021 rollout as the treatment date and estimate the effects for the 18 months following the policy.[4] We find that more Meta-dependent firms had a significant reduction of total orders by 22.3% and overall revenue by 39.4%. Furthermore, we find

---

[4]We also consider a specification where firms are stratified according to the share of their revenue that comes from iOS users and find similar results. Both reliance on Meta advertising and reliance on iOS users expose firms more to the ATT policy change, but, as these are not perfectly correlated, capture ATT exposure in different ways.

that this effect is disproportionately driven by the smaller firms within our sample. We use the additional data from Shopify to both validate the robustness of our results as well as document that the revenue decrease is primarily driven by a decrease in new customer, as opposed to repeat customer, orders.

Our results have several important policy and managerial implications. The large and negative impact on revenues indicates that opt-in privacy regulation has a significant economic cost for firms that rely on targeted advertising for revenue generation, and especially for smaller firms. In particular, the magnitude of the revenue reductions suggests that privacy regulation can threaten the viability of business models, such as those of DTC firms, relying on targeted advertising. While a full welfare analysis of this policy would consider the consumer welfare gains to added privacy protection, our results suggest there may be countervailing effect on consumer welfare through this change of composition of firms that can succeed in the product market. Furthermore, as our analysis quantifies the effect of ATT based on pre-ATT measures, a key managerial implication is that the costs to starting up a DTC firm are substantially higher as a result of ATT. Finally, even though we observed substitution away from Meta advertising, the persistent revenue losses point to other forms of advertising as being weak substitutes. This indicates both that Meta advertising created significant value for advertisers and that Meta had considerable market power over advertisers.

Related Literature Our research joins a growing literature studying the economics of online privacy (Acquisti et al., 2016; Goldfarb & Que, 2023; Miklós-Thal et al., 2024), and exploring the impact of privacy regulation specifically (Johnson, 2023).

Our work is most closely related to several papers that also study the impact of ATT. Wernerfelt et al., 2022 use internal access to Meta to run large-scale field studies studying the effectiveness of ad targeting in which they compare the performance of "offsite conversion optimized" ad campaigns utilizing offsite data with the performance of ad campaigns treated

with "link-click optimization" that make no use of offsite data. They find that removing the offsite data from targeting decreases targeting effectiveness and increases the median cost per incremental customer by 37%, with especially large effects for small businesses. Our work extends and complements these findings by measuring the comprehensive effects of ATT using observational data.

Studying the impact using observational data after ATT went into effect has several advantages. First, such a study accounts for the "true" effect of ATT that takes into consideration the adjustments made by both ad networks and advertisers in targeting algorithms in response to the loss of offsite data. In the field experiments run by Wernerfelt et al., 2022, the treatment group is link-click optimized, so it does not correspond to the post-ATT ad campaigns that are still offsite conversion optimized. Depending on Meta's success in the post-ATT reoptimization for offsite conversion, the treatment effect found by the experiment is likely to overstate the effect of ATT. Second, an observational study captures potentially important general equilibrium effects held constant by the experiment, such as advertisers' substitution across ad channels, the resulting changes in the prices of different types of ads, and the propensity for consumers to actually decide to opt out of data sharing. To account for these effects, we focus on total revenue, which is ultimately a more important outcome measure for firms. The changes in revenue that we observe are all conditioned on firms responding as best they can to the changing environment brought on by ATT. Furthermore, understanding how much of the overall revenue effect is driven by the impact on new customer acquisition versus demand from existing customers provides insight into how ATT affects firms that are relatively more or less reliant upon new customer acquisition, increasing the generalizability of our findings.

In contemporaneous work Cecere and Lemaire, 2023 also study the effect of ATT on predicted, aggregated ad outcomes and find that ATT reduced targeting efficiency on Meta. We complement this work by using platform-observed advertising data to similarly find a reduction in targeting efficiency and use our revenue data to quantify the downstream eco-

nomic costs of reduced targeting efficiency. Several other papers (Cheyre et al., 2023; Kesler, 2022; Kollnig et al., 2022; Li & Tsai, 2022) also study the impact of Apple's App Tracking Transparency (ATT), but largely focus on the supply-side response of iOS applications to the regulation. These papers largely find that ATT had a negative impact on app downloads and on the incentives to develop new applications as well as that some applications shifted from advertising revenues to charging for their apps. We complement these papers by studying the effect on the advertisers themselves – as opposed to the application's advertising revenues.
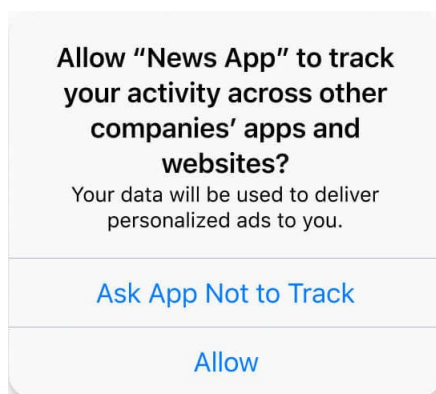
Earlier work has studied the effects of other prominent privacy regulations such as the EU's Cookie Law in 2009 (Goldfarb & Tucker, 2011), the 2010 AdChoices program (Johnson et al., 2020), the iOS privacy nutrition labels (Bian et al., 2021), the EU's General Data Protection Regulation (GDPR) in 2018 (Johnson, 2023), and the proposed EU ePrivacy Regulation (Miller & Skiera, 2023). ATT most closely relates to the GDPR as a key provision of both policies was to enable consumers to opt in to data tracking. The existing literature has studied the effects of GDPR on business investment and app creation (Janssen et al., 2022; Jia et al., 2021), consumer search (Zhao et al., 2021), data storage (Demirer et al., 2024), and market concentration for web trackers (Johnson et al., 2023; Peukert et al., 2022). The most closely related papers within this literature are Goldberg et al., 2024 and Aridor et al., 2023, which study the effects of GDPR on customer acquisition and e-commerce/advertising revenues. Relative to these papers, ATT provides a cleaner empirical characterization of the effects of opt-in privacy regulation, as it neither involves compliance issues (Ganglmair et al., 2023) nor suffers from heterogeneity in the design of opt-in prompts (Utz et al., 2019) that are endemic to GDPR. Furthermore, ATT only impacts iOS, unlike GDPR which impacts data processing across all operating systems. As such, ATT, in contrast to GDPR, provides an exogenous shock to the efficacy of Meta advertising and thus provides a measure of its economic value. Furthermore, it provides a measure of substitution patterns from Meta to other advertising platforms and thus sheds light on an important component of antitrust

debates around Meta (Aridor, Forthcoming; Competition and Markets Authority, 2020; Morton & Dinielli, 2022).

2 Data and Context

2.1 Background on App Tracking Transparency

Figure 1: ATT Details and Adoption

Allow "News App" to track your activity across other companies' apps and websites?
Your data will be used to deliver personalized ads to you.

Ask App Not to Track

Allow

In the Fall of 2020, Apple announced to app developers that when the next generation of its mobile operating system iOS 14, was rolled out in 2021, it would include a new feature that prompts users to explicitly consent to tracking by each app.[5] This was followed by a period of confusion by app developers as the details were slowly clarified before the update was rolled out starting April 25, 2021.

Before this update, app publishers had access to an "identifier for advertisers" (IDFA), which was available by default on Apple devices and provided access to user-level data. The update removed default access to this and instead prompted users, "Allow [app name] to track your activity across other companies' apps and websites?" (see Figure 1). For users selecting "Ask App Not To Track," the app can no longer use tracking to observe what those users did after leaving the app. Unlike other privacy regulations such as the GDPR, there were neither compliance issues (Ganglmair et al., 2023) nor heterogeneity in the design

---

[5]https://techcrunch.com/2020/06/22/apple-ios-14-ad-tracking/

of the opt-in prompt (Utz et al., 2019). In particular, if an application wanted to remain listed on the App Store, they were required to comply with the policy by including this prompt in their application, and the prompt design was the same across applications as per Apple's requirements. Furthermore, consumers can fully opt out across all applications in the ecosystem. Figure 2 shows that while the launch date was on April 25th, 2021 – iOS 14.5 adoption amongst users was rather gradual, but with a sudden increase in adoption rate roughly 5 weeks after the policy when Apple began to nudge users to adopt the new operating system.

Figure 2: ATT Adoption Over Time
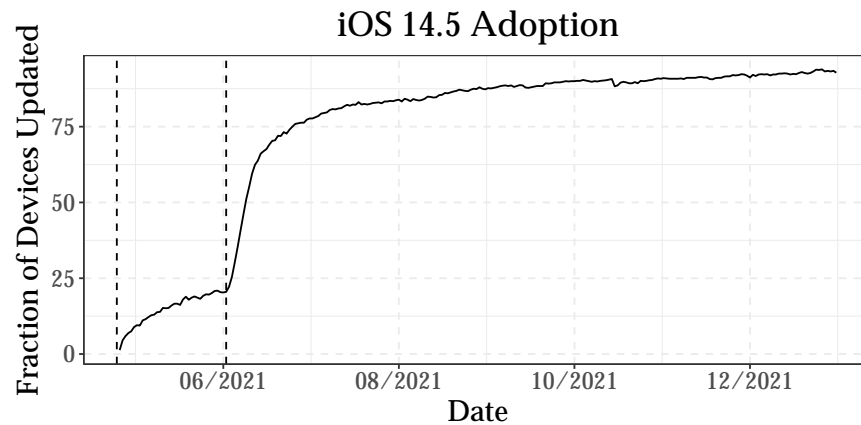
NOTES: Figure represents the estimated percentage of iOS devices that updated to iOS 14.5 over time. The first vertical dotted line represents April 25, 2021, when Apple first introduced iOS 14.5. The second vertical dotted line represents June 1 2021, when Apple began encouraging iOS users to update their operating systems. Source: Gupta Media, https://lookerstudio.google.com/u/0/reporting/3d5dda40-37ea-4b9f-bd91-bb8df8e12620/page/aDUJC?s=kTs6iab_AhQ

The IDFA had two primary uses for mobile display advertising via platforms such as Meta. First, it provided a view of the consumer activity across applications, which could serve as an input for targeting. For instance, an advertiser could target an advertisement based on the applications that a consumer had used. Second, it enabled Meta to link conversions to advertisements more easily. As an example, suppose a consumer saw an advertisement on Meta for a Nike shoe, clicked on the ad to go to the Nike application, and subsequently purchased through the application. Nike would pass back the conversion information that includes the consumer's IDFA, allowing Meta to link the conversion to

the advertisement. If a consumer opts out through ATT, however, then Meta is unable to link ad impressions or clicks to purchases. This also means that Meta is limited in its ability to accurately report conversions to advertisers. Indeed, following ATT, Meta rolled out Aggregated Event Measurement where they replaced actual observed conversions with "modeled" conversions for users that opted out and provided changes to the type of events that advertisers could monitor.[6] Thus, both the loss in off-platform data and conversion measurement issues contribute to an overall degradation in targeting by reducing the data observed by advertisers (Johnson et al., 2022; Runge & Seufert, 2021).

2.2 Data Overview

To study the impact of ATT, we use detailed data on advertising and revenues for thousands of firms. This data comes from two sources, each of which contain advertising and revenue data for separate sets of firms, but with some differences in what specific features the respective data sources contain. The first data source is Grips Intelligence (Dataset A), which contains first-party Google Analytics tracking data from direct data partnerships with the domain operators. It consists of detailed traffic and revenue metrics for 1,801 domains operating globally, at the domain-device-operating system-day level. The second source (Dataset B), which prefers to remain anonymous, provides information on 2,442 advertisers, with granular data on their Meta, Google, and TikTok advertising expenditures and performance as well as their Shopify e-commerce revenues, where applicable.

In both cases, the data provide a highly granular view of advertising performance and revenue for the types of firms most likely impacted by ATT, namely, e-commerce firms that typically rely on targeted advertising for demand generation. Both datasets encompass a broad range of firms, including many smaller, direct-to-consumer firms. They provide complementary views of firm performance. Dataset A provides a firm-side view using last-touch

---

[6]Campaigns targeting non-impacted operating systems remained unchanged, but, if an advertiser targeted iOS users, then the aggregated event measurement induced Meta to change the recommended setup and targeting for the overall campaign. See https://www.facebook.com/business/help/331612538028890?id=428636648170202 for the full details.

attribution. In contrast, Dataset B provides a platform-side view using information from the platform-specific pixel to assign credit for transactions to advertising sources. Furthermore, while Dataset A provides a view into revenues for a larger number of firms through a third-party analytics provider, Dataset B provides a direct view into the revenues accrued by a smaller number of firms and their full advertising spend. A key challenge with empirical privacy research is that privacy regulations influence both real and measured outcomes (Goldberg et al., 2024), potentially biasing estimates. These complementary data collection processes are valuable because they help mitigate these biases in the context of ATT.

In the next two subsections, we provide detailed information on the provenance of each dataset and what they contain. The high-level features of each dataset are summarized in Table 1. We specify which data are used in each analysis in relevant table or figure notes.

Table 1: Summary of Data

	Ads data	Revenue data
Dataset A (Grips) $N = 1,801$ firms	• Data by domain-date, containing device type, OS, and traffic source • Observe total revenue, # transactions, and # sessions	• Data by domain-date, containing device type, OS, and traffic source • Observe total revenue, # transactions, and # sessions
Dataset B1 $N = 2,220$ firms	• Data by domain-date from Meta, Google, and TikTok Platforms • Observe spending, impressions, clicks, and conversions • Additional granularity varies based on platform	
Dataset B2 $N = 222$ firms	Contains same ads data as Dataset B1	• Weekly aggregated Shopify sales data • Observe revenue, number of orders, and fraction of orders from repeat customers

2.2.1 Grips Data Overview (Dataset A)

Dataset A comprises an aggregation of first-party Google Analytics tracking data from direct data partnerships with domain operators. The panel consists mostly of classical online retailers in fashion, consumer electronics, beauty and cosmetics, and general e-commerce retail. Approximately $116 billion in spending is observed across all companies in Dataset A in 2022.

The primary determinant of domain selection within this panel is domains that use Google Analytics, which has a market share in the analytics sector of over 80%.[7] Geographically, the only country that is not covered by this panel is China, where most of Google's services are blocked.[8] Google Analytics is built without any personally identifiable information and uses first-party data to track the metrics that are used. Hence, the data is unaffected by the ATT event in so far as firms do not change which data they report through the service. We selected a subset of relevant variables from each domain: transactions, sessions, and revenue at the device-operating-system-traffic source-day level.

Table 2: Dataset Summary Statistics (Monthly Domain)

Dataset	Metric	Mean	Min	5th	50th	95th	Max
					Percentile		
Dataset A	Monthly Revenue ($1,000)	4,896.95	1.52	26.38	359.36	14,103.55	361,747.54
	iOS Share	0.25	0.00	0.03	0.24	0.51	0.75
	Android Share	0.21	0.00	0.02	0.18	0.49	0.73
	Mobile Share	0.44	0.00	0.05	0.45	0.75	0.92
	Meta Share	0.04	0.00	0.00	0.01	0.18	0.56
Dataset B2	Monthly Revenue ($000)	436.95	0.76	36.75	136.89	370.72	11,112.04

Notes: Revenue figures are reported in U.S. dollars. All values are computed over April 2020-April 2021.

We present a set of summary statistics on dataset A alongside dataset B2 in Table 2, which summarizes performance variables at the domain-month level from April 2020 until April 2021. The median monthly revenue is roughly $359,000, and the distribution of monthly revenue is right skewed, with a mean revenue of approximately $4.9 million. For the median domain, roughly 25% of revenue comes from iOS sessions versus 18% from Android. At 55%,

---

[7]A retailer that is notably absent from this panel is Amazon, which uses its own analytics system.

[8]https://w3techs.com/technologies/comparison/ta-adobeanalytics,ta-googleanalytics, January 1, 2024.

a slight majority of revenue comes from desktop sessions, with the remaining 45% coming from mobile devices. We also present the breakdown of transactions and sessions associated with different operating systems and device types.

2.2.2 Dataset B Overview

Dataset B contains weekly aggregates of advertiser performance for a separate set of firms. These advertisers, whose identities are anonymized, contract with the data provider and share their relevant performance data from various advertising and e-commerce platforms. For each of the advertising platforms, we observe the total amount of dollars (spend), the number of times the advertisements were seen (impressions) and clicked on (clicks) as well as the total number of conversions associated with the advertising campaign (conversions). The measurement of the first three (spend, impressions, clicks) is not affected by ATT; they are measured accurately and consistently before and after ATT. However, conversions are potentially affected by ATT as it is typically collected through a pixel that the advertiser embeds within its website or application that requires a consistent identifier across the platform of interest and the third-party website/app.[9] Within each advertising platform, we observe this data at different levels of granularity. For Meta, we observe performance broken down based on campaign objectives (e.g., off-platform conversions, on-platform clicks). For Google, we observe performance broken down based on Google product (e.g., Google Search, Display).

For a subset of these advertisers (dataset B2), we also observe weekly aggregated data from their Shopify accounts. Shopify is an online platform that provides sellers with a suite of software tools to bundle all their commerce activities – online and offline – in one dashboard.[10] Shopify handles the logistics involved in creating a website, payment processing, and many other aspects of selling online. Importantly for our purposes, this data provides us with

---

[9]See https://www.facebook.com/business/tools/meta-pixel for more information on the Meta pixel and https://ads.tiktok.com/help/article/tiktok-pixel?lang=en for more information on the TikTok pixel.

[10]For full details about Shopify, see https://www.shopify.com/blog/what-is-shopify.

a complete view of revenue for the advertisers both online and offline. From Shopify, we observe the total weekly revenue, the number of orders, and the fraction of orders that come from repeat customers. Unlike the data on conversions from the advertising platforms, the Shopify data has no measurement issues as a result of ATT. Notably, the measurement of repeat customers relies on data unaffected by the changes from ATT since they are typically user-provided email addresses or phone numbers. Summary statistics for the revenue data in dataset B2 are shown at the bottom of Table 2. The average monthly revenue in dataset B2 is $436,948, with a median revenue of $136,890.

3 First-Stage: Impact on Advertising Effectiveness

We start by investigating the direct effect of ATT on advertising performance. We exploit the fact that ATT only impacted certain types of advertising relying specifically on third-party data. First, we use dataset B1 to provide descriptive results suggesting that the effectiveness of conversion-optimized Meta ads was significantly degraded when ATT took effect. These results describe the effects as observed from Meta's vantage point. We see attributed conversions using the Meta pixel and can therefore measure how Meta-observed conversions shifted after ATT. Next, to isolate the causal effect of ATT on ad performance, we use within-advertiser variation in campaign objectives. Some campaigns are optimized for on-platform outcomes, which are not impacted by ATT, while others rely on third-party data and are therefore potentially degraded by ATT. Finally, we characterize advertiser substitution patterns after ATT. Overall, our results in this section document that there was a severe degradation of targeting for Meta advertisements and limited substitution from Meta to Google ads.

Descriptive Evidence on Meta Conversions: We first examine the extent to which ATT impacted the cost per pixel/conversion (CPP) and conversions from Meta advertising. We make use of dataset B1 and restrict attention to the advertiser campaigns on Meta that

13

Figure 3: Event Study on Conversions, Acquisition Costs, and CTR
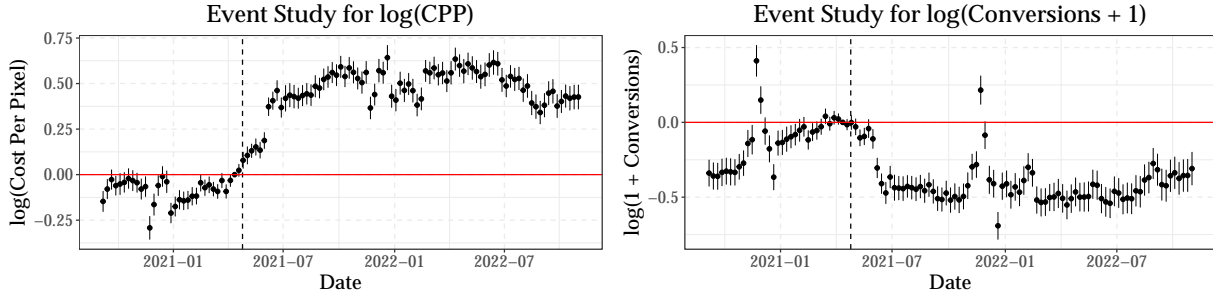
Notes: Results use Dataset B1. The figures plot the average log(Cost Per Pixel) on the left and log(Conversions + 1) on the right, after factoring out advertiser fixed effects.

are optimized explicitly for off-platform conversions. We restrict attention to a balanced panel of non-zero advertising spending across each of the considered periods.

We show weekly data on average cost per pixel and conversions in Figure 3 over the period of September 2020 until October 2022. Beyond the spikes around Black Friday and the holiday season, a clear pattern emerges. The rise (resp. fall) of CPP (resp. conversions) coincides with the adoption of ATT. In particular, CPP increased almost discontinuously by 25% beginning roughly six weeks after ATT took effect, consistent with the spike in adoption according to Figure 1.[11] Overall, this corresponds to a 50% increase in CPP post-ATT, which suggests a dramatic increase in acquisition costs.

While these results suggest that ATT had a dramatic effect on ad performance, it is important to note that these outcome variables are subject to measurement issues. The observed decrease in conversions is a mixture of both real reductions in conversions and the degraded ability to link advertisements to conversions. This highlights the challenge that both advertisers and Meta face after ATT, as accurately attributing conversions to advertisements plays a key role in learning effective targeting rules since it enables Meta to "close the loop." For us to determine whether there were real degradations in targeting, we next exploit the fact that ATT impacts only the ability to measure *off-platform* conversions.

---

[11]In the remainder of the paper, we show results at a monthly frequency, but prefer the weekly frequency for this plot to show how closely outcomes track the adoption of iOS 14.5.

Causal Effect on Meta targeting: We compare the relative performance of advertising campaigns that are optimized based on off-platform data (particularly sales) to campaigns that are optimized based on on-platform data (primarily link clicks). We use these to estimate a *within-advertiser* difference-in-differences analysis. This follows the same logic as Wernerfelt et al., 2022, in that ATT reduces the availability of third-party (off-platform) data for optimizing ad targeting but has no effect on first-party (on-platform) data. By focusing on within-advertiser performance, we are able to control for any possible differences across advertisers – for instance, their size or frequency of purchases – that are orthogonal to the treatment effect of interest as well as possible adjustments to the targeting algorithm by Meta over time. We consider the following specification for advertiser i, advertiser campaign objective j, and time t:

$$Y_{ijt} = \beta\Big(After_t \times T_j\Big) + \alpha_i + \kappa_t + \epsilon_{ijt} \tag{1}$$

where β is the coefficient of interest, T_j is whether the campaign j is in the treated group, $After_t$ indicates whether t is the past ATT implementation date, α_i denotes advertiser fixed effects, and κ_t denotes time fixed effects.

There are two possible concerns with this identification strategy. The first is the typical concern that the parallel trends assumption does not hold. To assess the reasonableness of this assumption we also consider a time-varying specification where we estimate a time-varying β and interact T_j with $date_t$ instead of $After_t$ (Freyaldenhoven et al., 2021). We find that there are no significant differences in trends between campaign effectiveness before ATT (see Figure 4). The second is that as off-platform optimized campaigns are more impacted relative to on-platform optimized campaigns, advertisers may reallocate their budgets towards on-platform campaigns such as link clicks. If there was substantial substitution towards on-platform objective optimized campaigns, it could create the spurious result of a relative change in campaign effectiveness. We show in Appendix Section A.1 that there is

a precise null effect on substitution towards on-platform objectives apart from an economically small degree of substitution in the extensive margin towards objectives optimizing for on-platform reach. As such, we conduct only our analyses using data from advertisers that used both objectives before ATT.

Figure 4: Difference-in-Differences for Relative Campaign Success and CTR
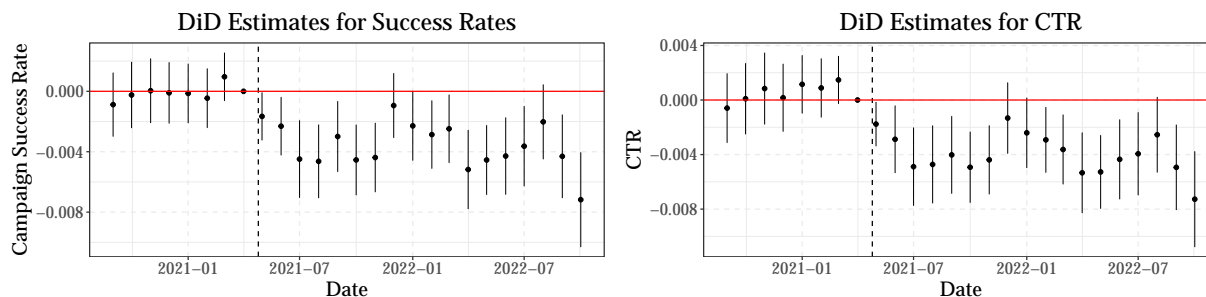
Notes: Results use dataset B1. The plots represent the time-varying treatment effects for the success rate on the left and the click-through rate on the right.

We now investigate the relative performances of the respective campaigns in completing their objectives. For conversion-optimized campaigns, an off-platform purchase is registered as a success, whereas for link click-optimized campaigns, an on-platform click is registered as a success. We then study the differential performance between two campaigns in terms of the measured success rates and the cost of achieving success.[12] Indeed, the left panel of Figure 4 shows that there is a clear and sudden drop in the relative success rate for the campaigns that are optimized for conversions, with strong evidence for parallel trends in the pre-ATT period across the two groups.

This result, however, could still be impacted by the aforementioned potential measurement issues. Thus, we also consider the success of both campaigns using only an *on-platform* measure, namely click-through rate. We show the results in the right panel of Figure 4. These estimates show a consistent pattern with again no evidence for pre-trends for either measure until the onset of ATT and then a sudden decline in click-through rate. The overall

---

[12]For this analysis, we drop two erroneous observations where the number of conversions is larger than the number of impressions.

estimated effect is a 0.0052 relative reduction in click-through rates for off-platform optimized campaigns, which represents a 37.1% reduction (in light of the baseline click-through rate of 0.014.) These estimates provide evidence that ATT caused a substantial reduction in the effectiveness of advertising targeted using off-platform data.

3.1 Budget Reallocation

Figure 5: Event Study of Relative Market Shares
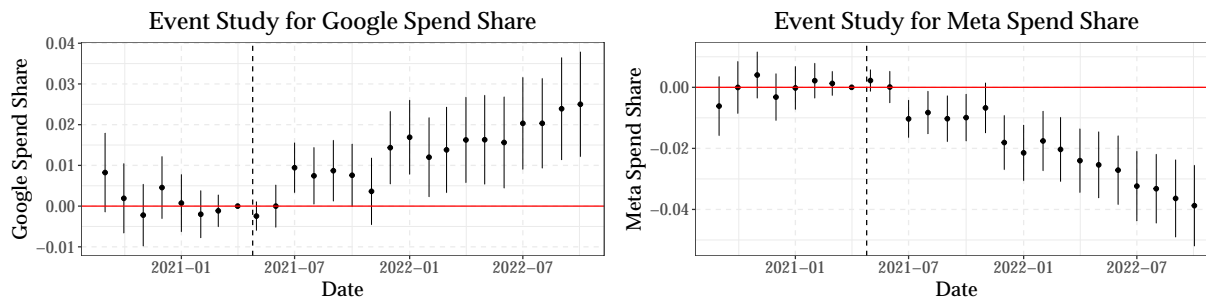
Notes: Results use dataset B1. The plots represent the event study estimates for Google spend share on the left and the Meta spend share on the right.

As discussed in the previous section, ATT significantly decreased the effectiveness of Meta advertising, which raises the question of how companies adapted to this change. If advertisers could not find substitutes for Meta ads and did not adjust their budgets accordingly, it would suffice to simply measure the decline in Meta's advertising effectiveness in a vacuum. Consequently, this section uses dataset B1 to provide descriptive evidence on how advertisers reallocated their online spending after ATT. Considering ATT as an exogenous negative shock to the efficacy of Meta advertising, this analysis sheds light on the substitutability of different advertising platforms, which is relevant to ongoing antitrust debates about Meta (Aridor, Forthcoming; Competition and Markets Authority, 2020; Morton & Dinielli, 2022).

As measuring the causal effect of substitution patterns across different advertising platforms is difficult due to seasonal trends in the usage of different modalities as well as the fact that ATT simultaneously impacts prices and quality, we focus on the effect of ATT on the relative market shares of advertising platforms within our data. Figure 5 presents the event

17

study estimates for the relative spend share of Meta and Google, respectively, in dataset B1. According to these estimates, there was little change in relative market share before the onset of ATT. After ATT, there was a gradual increase in the relative share of Google and a gradual decrease in the relative share of Meta. The mean market share for Meta ads was 73% in the baseline period but fell by approximately 6.8% post-ATT. In Appendix A.2, we use a difference-in-differences analysis to show that there was more reallocation for more Meta-dependent advertisers. In particular, they suggest that for the share of spend, clicks, and impressions, Google benefited at the expense of Meta, gaining 4.8 to 6.7 percentage points of market share, whereas there was no shift in market share to TikTok. Furthermore, these results indicate that most of the gain in share within Google was to Google Display ads.

These results indicate a meaningful reallocation of ad spending for the more Meta-intensive firms, suggesting that to measure ATT's full impact on these firms, it is not sufficient to study their advertising effectiveness directly—it is necessary to study their total revenue instead. We turn to this in the next section.

4   Impact on Firm Revenues

This section contains our main results, in which we estimate the impact of ATT on firm revenues. In an ideal world, there would be randomized variation in advertising firms' exposure to ATT. Unfortunately, ATT went into effect nearly simultaneously for all iOS users. Our approach to understanding its effects, therefore, relies on variation across firms in the extent to which they were vulnerable to being impacted by ATT based on their pre-ATT characteristics.

Empirical Strategy: To set ideas, consider two DTC apparel firms that rely on digital advertising to acquire and develop customers. Before ATT, firm A may rely primarily

on targeting specific customer segments on Instagram/Facebook to acquire consumers. In contrast, firm B may rely primarily on specific keywords in Google Search or targeting via Google Display advertisements. The results from Section 3 suggest that the ability to target advertisements via Meta was negatively impacted by ATT. Thus, we would expect that firm A would immediately experience a reduced effectiveness of their marketing strategy and a corresponding reduction in revenue.

In principle, the reduction in the effectiveness of Meta advertising need not directly impact firm A relative to firm B, as firm A could mitigate the negative shock by replicating the marketing strategy of firm B (i.e., substituting ad platforms) or making other changes to their marketing strategy. Indeed, the results from Section 3.1 indicate that the relative share of Google rose for more Meta-dependent firms – indicating some amount of budget reallocation away from Meta advertising for these firms. Nonetheless, many firms may be too dependent on the fine-grained targeting offered by Meta or may find Google advertising a poor substitute for customer acquisition. For our purposes, budget reallocation does not impact the interpretation of the estimated treatment effects as this is part of the effect of the ATT that we are measuring. Nevertheless, it would be valid to interpret our empirical specification as offering a lower bound for the implicit value of Meta advertising on the success of pre-ATT DTC firms.[13]

Our empirical strategy is, therefore, to perform an *across-advertiser* difference-in-differences specification that compares revenue before and after ATT for firms that are more vulnerable to ATT. Our main treatment indicator, capturing ATT vulnerability, is based on the pre-ATT reliance of each firm on Meta as a source of customer acquisition. For each domain, we take the one-year period prior to the implementation of ATT, which spans April 2020 to April 2021, and compute the average share of revenue that comes from sessions where the traffic source was Facebook/Instagram.[14] Next, we take the median split of this Meta

---

[13]In this case, budget reallocation matters for the interpretation of the treatment effect, and since there is reallocation, we are measuring the lower bound.

[14]We rely on Dataset A for this analysis as it provides us with the largest cross-section of firms.

revenue share and categorize each domain as having high or low exposure. We define our treatment variable based on the set of domains with a high Meta share of revenue in the pre-period and the control set as those with a low Meta share.

In addition, as ATT only applies to iOS users and not Android users, we also consider a specification defining ATT vulnerability based on a median split of firm revenue attributable to iOS users, following the same procedure. This captures a slightly different, yet complementary, dimension of dependence on pre-ATT targeted advertising relative to our baseline strategy.[15] While neither dependence on Meta users or iOS users is an exogenous firm characteristic, we again rely on the parallel trends assumption and test for the presence of different revenue trends between treated and control firms in the pre-period.

Formally, we estimate:

$$Y_{it} = \beta \Big(After_t \times T_i \Big) + \alpha_i + \kappa_t + \epsilon_{it}, \tag{2}$$

where T_i indicates whether they are more vulnerable to ATT, $After_t$ indicates whether time period t is after ATT, α_i denotes firm fixed effects and κ_t denotes time fixed effects. We also consider category-time fixed effects. Similarly to before, we cluster our standard errors at the domain level. Finally, in order to validate that there are no significant pre-trends – similar to before – we follow best practices as described in Freyaldenhoven et al., 2021 by plotting time-varying treatment effects, shown in Figures 6 and 7. We estimate the specification over the time period 12 months before and 18 months after the policy.

---

[15]While iOS dependence and Meta dependence are correlated, there is substantial variation in which firms are labeled treated under the two definitions. 29% of firms are considered treated under both definitions, 42% are considered treated under only one, and 29% are considered treated under neither.

Figure 6: Time-Varying Treatment Effects for Revenue (Meta Share Treatment)
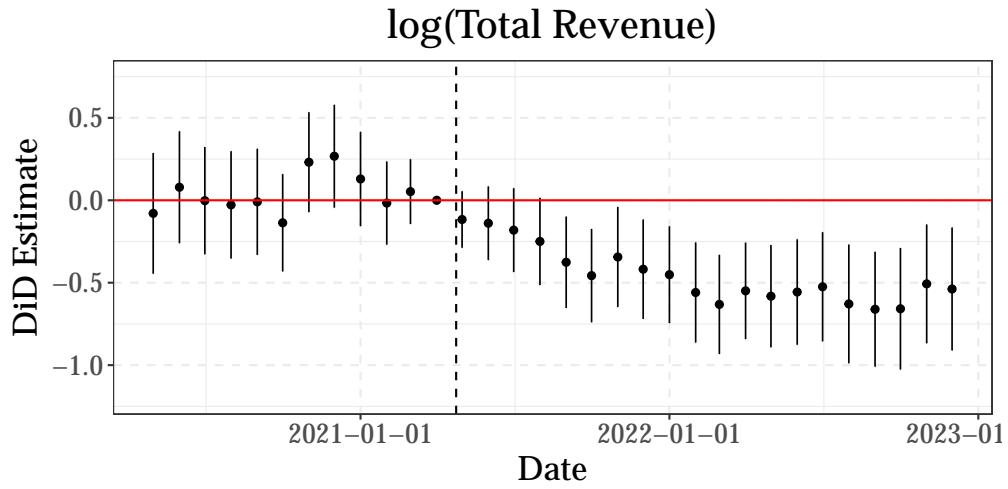
Notes: Results use dataset A. The estimates present the time-varying treatment effects for log(Total Revenue). The treatment indicator is based on a dummy indicating the domain-level pre-ATT share of revenue from Meta traffic is above the median.

Figure 7: Time-Varying Treatment Effects for Revenue (iOS Share Treatment)
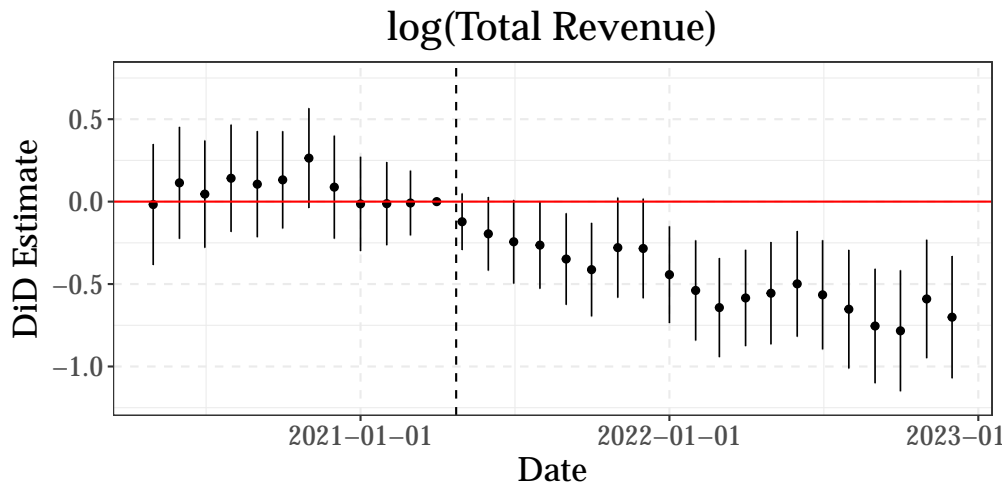
Notes: Results use dataset A. The estimates present the time-varying treatment effects for log(Total Revenue). The treatment indicator is based on a dummy indicating the domain-level pre-ATT share of revenue from iOS traffic is above the median.

Overall Revenue Changes: The main results are presented in Table 3, which shows that for domains with a high pre-ATT share of revenue from Meta advertising, there is a relative reduction in overall revenues that materializes over a three-month period after the implementation of ATT and then remains relatively flat thereafter. The measured effect decreases somewhat but is still significant when allowing for separate time trends for each e-commerce category. We include this as a specification check while noting that category and treatment may be correlated, as some categories are inherently more reliant on targeted digital advertising than others. The coefficients from columns (1) and (2) suggest a relative decrease in revenue of 39% and 35%, respectively.[16] Furthermore, columns (4) and (5) show that this effect is driven by small firms, which are defined as those with below-median pre-ATT average monthly revenue. Finally, in column (6) we study the impact on the log number of transactions and find that they fall by 18%.

In Table 4 we present results using the alternative treatment indicator based on the pre-ATT share of revenue from iOS users. All results are consistent with those in Table 3, suggesting that the estimated revenue effects are robust to how we categorize firms as more or less vulnerable to ATT.

We present the time-varying treatment effects in Figure 6 and Figure 7. In both cases, we find that in the pre-period, there are no significant differences between the treated and untreated domains in terms of log monthly revenue. In the post-period, beginning in month 1 we see a clear downward trend in revenue for the domains most exposed to ATT, with significant differences beginning within 4 months. This is again consistent with the gradual timing of adoption of iOS 14.5 amongst consumers documented in Figure 2. These results suggest that the rollout of ATT substantially lowered revenue of the DTC ecommerce firms most exposed to it.

---

[16]The effect size in percent is calculated as $exp(\beta) - 1$.

Table 3: Primary Revenue Estimates (Meta Treatment)

	(1) All	(2) All	(3) All	(4) Small Firms	(5) Large Firms	(6) log(Transactions)
After × Treated	-0.463***	-0.396**		-1.132***	0.158	-0.241**
	(0.165)	(0.180)		(0.302)	(0.121)	(0.100)
0 − 3 months × Treated			-0.138			
			(0.151)			
4+ months × Treated			-0.499***			
			(0.171)			
Domain FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Category × Month FE		Yes				
N	24868	24868	24868	12468	12400	24868
R^2	0.73	0.74	0.73	0.58	0.67	0.83

*p<0.1; **p<0.05; ***p<0.01

NOTES: Results use dataset A and the treatment indicator that relies on the share of revenue from Meta traffic. The first 5 columns use log(revenue) as the DV and column 6 uses log(# transactions). The rows present the estimated average treatment effect coefficient using the difference-in-differences specification. Standard errors are clustered at the domain level.

Table 4: Alternative Revenue Estimates (iOS Treatment)

	(1) All	(2) All	(3) All	(4) Small Firms	(5) Large Firms	(6) log(Transactions)
After × Treated	-0.522***	-0.352**		-0.999***	0.079	-0.237**
	(0.165)	(0.167)		(0.290)	(0.116)	(0.100)
0 − 3 months × Treated			0.040			
			(0.103)			
4+ months × Treated1			-0.483***			
			(0.159)			
Domain FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Category × Month FE		Yes				
N	24868	24868	24868	12468	12400	24868
R^2	0.73	0.74	0.73	0.58	0.67	0.83

*p<0.1; **p<0.05; ***p<0.01

NOTES: Results use dataset A and the treatment indicator that relies on the share of revenue from iOS traffic. The first 5 columns use log(revenue) as the DV and column 6 uses log(# transactions). The rows present the estimated average treatment effect coefficient using the difference-in-differences specification. Standard errors are clustered at the domain level.

Robustness Checks: We consider the Shopify data from dataset B2 as a robustness check. While we have visibility into a smaller number of firms via this data, there are several aspects to it that we can use to validate the robustness of the results from our primary analysis. The first benefit is that we can directly link the Shopify data users to both their Meta and Google advertising spend. As such, we can validate to what extent the revenue changes result from reduced advertising spend and consider an alternative measure of dependence based on advertising spending instead of on revenue sources.

We estimate the across-advertiser difference-in-differences specification (2) and rely on the same identification arguments. However, since in this dataset we directly observe advertising spend across Meta, Google, and TikTok, we rely on the spend share as the measure of Meta dependence and define treated firms as those with above the mean Meta spend.[17] We report the results for this analysis in Appendix B.1, which show the same quantitative result of revenue losses for more Meta-dependent firms. As such, despite dataset B2 skewing towards relatively smaller firms and dataset A skewing towards relatively larger firms, the consistency of our results across datasets indicates that our findings are robust to the specific dataset used to perform the analysis. Furthermore, given that we have a broader characterization of advertising spend relative to Dataset A, we additionally control for advertising spend and find qualitatively similar results in the main analysis, albeit with less precision as the primary analysis, as we have a smaller number of firms.

Possible Mechanisms: We next explore whether the mechanism for the revenue reductions is indeed due to the reduced effectiveness of targeted advertising, as suggested by the results from Section 3. To test this, we use the Shopify data in dataset B2 to characterize whether and to what extent there were changes in the number of new and repeat customer orders, respectively, as a result of ATT. Recall that the measure of consumer identity in this dataset is not impacted by ATT since it is directly collected at the point of sale. Beyond

---

[17]We use mean, as opposed to median dependence, since for dataset B2, the Meta usage is skewed, and the median is close to 1. As such, we use the mean of 0.641 as a more reasonable division point for this dataset.

hypothesizing that new customer orders should be reduced, we note that it is ambiguous whether the effect on repeat customer orders should be positive or negative. Faced with reduced advertising effectiveness, firms may reallocate marketing budgets to increase consumer retention.

Table 5: Estimates on New vs. Repeat Customers

	Dependent variable:		
	Repeated Order Ratio	log(New Customer Orders + 1)	log(Repeat Customer Orders + 1)
	(1)	(2)	(3)
After × Treated	3.994***	−0.337***	−0.154
	(1.415)	(0.128)	(0.141)
Domain FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Category × Month FE			
N	5,772	5,772	5,772
R^2	0.808	0.833	0.885

*p<0.1; **p<0.05; ***p<0.01

NOTES: Results use Dataset B2. The first column shows the estimated average treatment effect using the difference-in-differences specification for the repeated order ratio. The second and third columns are log(orders+1)coming from new and repeat customers respectively. Standard errors are clustered at the domain level.

We estimate the across-advertiser difference-in-differences specification (2) defining treatment as whether Meta advertising spend was above the mean within the set of advertisers and use the measure of the fraction of orders processed by a merchant that come from repeated customers according to dataset B2. The results are presented in Table 5. Column (1) indicates that the fraction of orders coming from repeat customers increased. Indeed, the pre-ATT baseline for the share of orders coming from repeat customers was 33.93, indicating that the estimates imply a 10.5% increase in the share of orders coming from repeat customers. To characterize the absolute impact on new and repeat orders respectively, we use this measure combined with the total number of orders to estimate the effects on the

number of orders coming from new and repeat customers respectively.

The results of estimating the same empirical specification using the log of new and repeat orders are presented in columns (2) and (3) of Table 5 and present a clear picture. Column (2) shows a statistically significant 28.5% decrease in orders coming from new customers, but column (3) shows a negative, but non-statistically significant, effect on repeat customer orders. We show in Appendix B.1 that this result is robust to controlling for total advertising spend across Meta, Google, and TikTok. In sum, this provides evidence that the revenue reductions are primarily due to weakened customer acquisition and that there does not appear to be a countervailing effect of increased customer retention. If anything, our results point to reductions in revenues among repeat customers as well.

5   Conclusion

As policymakers consider extending or implementing new privacy regulations that limit the ability of firms to target consumers online, it is important that they be fully informed about the economic costs to small businesses that may result from these regulations. In this paper, we have used several datasets on online advertising allocations, performance, and revenue to study the economic effects of Apple's App Tracking Transparency (ATT) policy. Our primary findings are that ATT significantly degraded the performance of Meta advertising and, subsequently, that more Meta-dependent advertisers had a 39.4% reduction in revenue that was primarily felt by small businesses and was driven by a reduction in orders from new customers. As such, while firms reallocated some of their advertising away from Meta towards the Google ecosystem to mitigate the impact of the decline in Meta advertising effectiveness, they still suffered a substantial decline in their revenues.

There are several policy and managerial takeaways from this paper. Our estimates suggest large economic costs of opt-in privacy regulation. While there are positive consumer welfare gains from the added privacy protections, the magnitude of the losses threatens the viability

26

of firms, such as direct-to-consumer firms, that rely on targeted social media advertising as their primary source of customer acquisition. As such, there could be a countervailing force on consumer welfare if the revenue losses are large enough to induce substantial exit and deter entry of these firms into product markets. Furthermore, this also implies that the pre-ATT value of targeted, social media advertising was similarly large and that the viability of direct-to-consumer business models is threatened following ATT.

These implications suggest several important directions for future work. First, our results suggest some equilibrium reallocation by advertisers in terms of marketing spend, though the magnitudes and impact on downstream revenue imply that other forms of online and offline advertising are weak substitutes for Meta advertising. Future work can better characterize the extent of substitutability and complementarity of various online advertising modalities and platforms. Second, while our results imply large economic costs and suggest they can lead to changes in the product market, future work can more fully characterize the effects on privacy regulation, as well as targeted advertising provided by social media platforms such as Meta, on consumer welfare in downstream product markets (Aridor et al., Forthcoming).

References

Acquisti, A., Taylor, C., & Wagman, L. (2016). The economics of privacy. *Journal of Economic Literature*, *54*(2), 442–492.

Aridor, G. (Forthcoming). Measuring substitution patterns in the attention economy: An experimental approach. *RAND Journal of Economics*.

Aridor, G., Che, Y.-K., & Salz, T. (2023). The effect of privacy regulation on the data industry: Empirical evidence from GDPR. *RAND Journal of Economics*, *54*(4), 695–730.

Aridor, G., Jiménez-Durán, R., Levy, R., & Song, L. (Forthcoming). The economics of social media. *Journal of Economic Literature*.

Bian, B., Ma, X., & Tang, H. (2021). The supply and demand for data privacy: Evidence from mobile apps. *Available at SSRN 3987541*.

Borkovsky, R., Goldfarb, A., Haviv, A., & Moorthy, S. (2017). Measuring and understanding brand value in a dynamic model of brand management. *Marketing Science*, *36*(4), 471–499.

Cecere, G., & Lemaire, S. (2023). Have I seen you before? Measuring the value of tracking for digital advertising. *Available at SSRN 4659963*.

Chen, B. (2021). The Battle for Digital Privacy Is Reshaping the Internet. *New York Times*. https://www.nytimes.com/2021/09/16/technology/digital-privacy.html

Cheyre, C., Leyden, B. T., Baviskar, S., & Acquisti, A. (2023). The impact of Apple's App Tracking Transparency framework on the app ecosystem. *Available at SSRN 4453463*.

Competition and Markets Authority. (2020). Online platforms and digital advertising: Market study final report. https://assets.publishing.service.gov.uk/media/5fa557668fa8f5788db46efc/Final_report_Digital_ALT_TEXT.pdf

Demirer, M., Hernández, D. J. J., Li, D., & Peng, S. (2024). Data, privacy laws and firm production: Evidence from the GDPR. *Available at NBER w32146*.

Freyaldenhoven, S., Hansen, C., Perez, J. E. P., & Shapiro, J. (2021, August). XTEVENT: Stata module to estimate and visualize linear panel event-study models. https://ideas.repec.org/c/boc/bocode/s458987.html

Ganglmair, B., Krämer, J., & Gambato, J. (2023). Regulatory compliance with limited enforceability: Evidence from privacy policies. *Available at SSRN*.

Goldberg, S. G., Johnson, G. A., & Shriver, S. K. (2024). Regulating privacy online: An economic evaluation of the GDPR. *American Economic Journal: Economic Policy*, *16*(1), 325–358.

Goldfarb, A., & Que, V. F. (2023). The economics of digital privacy. *Annual Review of Economics*, *15*, 267–286.

Goldfarb, A., & Tucker, C. E. (2011). Privacy regulation and online advertising. *Management Science*, *57*(1), 57–71.

Janssen, R., Kesler, R., Kummer, M. E., & Waldfogel, J. (2022). GDPR and the lost generation of innovative apps. *Available at NBER w32146*.

Jia, J., Jin, G. Z., & Wagman, L. (2021). The short-run effects of the general data protection regulation on technology venture investment. *Marketing Science*, *40*(4), 661–684.

Johnson, G. (2023). Economic Research on Privacy Regulation: Lessons From the GDPR and Beyond. *The Economics of Privacy*. http://dx.doi.org/10.2139/ssrn.4290849

Johnson, G., Runge, J., & Seufert, E. (2022). Privacy-centric digital advertising: Implications for research. *Customer Needs and Solutions*, *9*, 1–6.

Johnson, G. A., Shriver, S. K., & Du, S. (2020). Consumer privacy choice in online advertising: Who opts out and at what cost to industry? *Marketing Science*, *39*(1), 33–51.

Johnson, G. A., Shriver, S. K., & Goldberg, S. G. (2023). Privacy and market concentration: Intended and unintended consequences of the GDPR. *Management Science*, *69*(10), 5695–5721.

Kesler, R. (2022). The impact of Apple's App Tracking Transparency on app monetization. *Available at SSRN 4090786*.

Kollnig, K., Shuba, A., Van Kleek, M., Binns, R., & Shadbolt, N. (2022). Goodbye tracking? impact of iOS App Tracking Transparency and privacy labels. *Proceedings of the 2022 ACM Conference on Fairness, Accountability, and Transparency*, 508–520.

Laziuk, E. (2021). iOS 14.5 Opt-in Rate - Daily Updates Since Launch. *Flurry Analytics*. https://www.flurry.com/blog/ios-14-5-opt-in-rate-att-restricted-app-tracking-transparency-worldwide-us-daily-latest-update/

Li, D., & Tsai, H.-T. (2022). Mobile apps and targeted advertising: Competitive effects of data exchange. *Available at SSRN 4088166*.

Miklós-Thal, J., Goldfarb, A., Haviv, A., & Tucker, C. (2024). Frontiers: Digital hermits. *Marketing Science (Articles in Advance)*.

Miller, K. M., & Skiera, B. (2023). Economic consequences of online tracking restrictions: Evidence from cookies. *International Journal of Research in Marketing, 41*(2), 241–264.

Morton, F. M. S., & Dinielli, D. C. (2022). Roadmap for an antitrust case against facebook. *Stan. JL Bus. & Fin., 27*, 268.

Peukert, C., Bechtold, S., Batikas, M., & Kretschmer, T. (2022). Regulatory spillovers and data governance: Evidence from the GDPR. *Marketing Science, 41*(4), 746–768.

Runge, J., & Seufert, E. (2021). Apple is changing how digital ads work. Are advertisers prepared? *Harvard Business Review, digital article*.

Utz, C., Degeling, M., Fahl, S., Schaub, F., & Holz, T. (2019). (Un) informed consent: Studying GDPR consent notices in the field. *Proceedings of the 2019 ACM Sigsac Conference on Computer and Communications Security*, 973–990.

Wagner, K. (2021). Facebook users said no to tracking. Now advertisers are panicking. *Bloomberg*. https://www.bloomberg.com/news/articles/2021-07-14/facebook-fb-advertisers-impacted-by-apple-aapl-privacy-ios-14-changes

Wernerfelt, N., Tuchman, A., Shapiro, B., & Moakler, R. (2022). Estimating the Value of Offsite Data to Advertisers on Meta. http://dx.doi.org/10.2139/ssrn.4176208

Zhao, Y., Yildirim, P., & Chintagunta, P. K. (2021). Privacy regulations and online search friction: Evidence from GDPR. *Available at SSRN 3903599.*

Appendix A   Additional Advertiser Substitution Results

A.1   Advertiser On-Platform Objective Substitution

Table A.1: Meta Objective Substitution

	Dependent variable:		
	Spend Share	Impression Share	$\mathbb{1}(\text{Spend}_t > 0)$
	(1)	(2)	(3)
$After_t \times$ On-Platform Actions	0.004	0.015	0.028**
	(0.007)	(0.009)	(0.012)
$After_t \times$ On-Platform Reach	0.001	0.015*	0.026***
	(0.005)	(0.008)	(0.009)
On-Platform Actions	−0.885***	−0.834***	−0.697***
	(0.011)	(0.013)	(0.015)
On-Platform Reach	−0.921***	−0.868***	−0.844***
	(0.007)	(0.011)	(0.012)
Week FE	Yes	Yes	Yes
Advertiser FE	Yes	Yes	Yes
Observations	212,892	212,892	212,892
R^2	0.899	0.821	0.614
Adjusted R^2	0.899	0.820	0.613

*p<0.1; **p<0.05; ***p<0.01

NOTES: All results use dataset B1. The dependent variables are the share of spend (column 1), share of impressions (column 2), and whether there is non-zero spend (column 3). The left out category is off platform actions. Standard errors clustered at the domain level.

We study substitution within objectives on the Meta platform. One way advertisers might adapt is by shifting their marketing campaigns to optimize for actions that occur within the platform (on-platform), rather than actions that occur outside the platform (off-platform). This shift can help maintain the effectiveness of their targeting efforts, as on-platform actions can serve as good indicators for off-platform actions, and they still provide direct feedback for optimization. We characterize the different Meta objectives into three groups: Off-Platform Campaigns, On-Platform Actions, On-Platform Reach. For Off-Platform campaigns, we consider campaigns with one of the following objectives: conversions, sales outcomes, product catalog sales, app installs, app promotion. On-Platform actions consist of the following: link clicks, store visits, page likes, leads outcome, traffic outcomes, engagement outcomes, and

post engagement. On-Platform Reach consists of video views, brand awareness, reach, and awareness outcome.[18] We consider three outcome measures: the campaign objectives' share of spending, share of impressions, and an indicator for whether spend was non-zero. The former two metrics provide a measure of intensive margin substitution – to what extent do advertisers shift their share of spending more to on-platform actions – while the final metric provides a measure of extensive margin substitution – to what extent do advertisers start to run on-platform campaigns.

We consider a balanced panel of Meta advertisers that have positive spend on any campaign objective throughout the sample period and use the within-advertiser difference-in-difference specification from Equation 1. The results are reported in Table A.1, which leads to several observations. The baseline coefficient for on-platform actions and reach is substantially negative – indicating large baseline differences in off vs. on-platform spend. Indeed, before ATT nearly 93% of campaigns are optimized for off-platform conversions. Overall, the results show a precise null effect on substitution to on-platform objectives apart from a small amount of substitution in the extensive margin to objectives optimizing for on-platform reach.

A.2   Difference-in-Differences for Budget Allocations

In order to study whether more Meta-dependent advertisers were more likely to substitute away from Meta advertising we consider an across-advertiser difference-in-differences specification that defines the treated group as advertisers with above-average Meta advertising spend as a proportion of total advertising spend in Dataset B1.[19] We use a weekly balanced panel of advertisers who spend non-zero dollars on any advertising platform throughout the same sample period as before (September 2020 to October 2022) and our outcome variables are the relative market share of impressions, clicks, and total spending across the different

---

[18]This includes all campaign objectives except for messages and event responses since it is ambiguous how to categorize messages and event responses.

[19]We consider the mean for this analysis as the distribution of advertisers in the sample is skewed towards Meta. The cutoff is a market share of 73%, whereas the median is close to 100%.

platforms. The first three rows of Table A.2 show the results for the relative market shares of Google, Meta, and TikTok. They suggest that for each of the measures that we consider Google benefited at the expense of Meta, gaining 4.8 to 6.7 percentage points of market share, whereas there was no shift in market share to TikTok. Furthermore, rows (4) and (5) of Table A.2 show the change in market share across different Google products and there is a greater increase in the share of Google Display relative to Google Search.[20]

Table A.2: Advertising Platform Usage Post-ATT

|	Dependent variable:		
Platform	Spend Share	Impression Share	Click Share
	(1)	(2)	(3)
Google	0.048***	0.067***	0.057***
	(0.009)	(0.010)	(0.009)
Meta	−0.044***	−0.061***	−0.057***
	(0.009)	(0.010)	(0.009)
TikTok	−0.004	−0.005	−0.001
	(0.002)	(0.003)	(0.003)
Google Search	0.011	0.012	0.012
	(0.008)	(0.008)	(0.008)
Google Display	0.005***	0.028***	0.013***
	(0.002)	(0.006)	(0.004)

*p<0.1; **p<0.05; ***p<0.01

NOTES: Results use dataset B1. Each cell displays the estimated average treatment effect across each of the specifications. The first three rows present the results of the difference-in-differences specifications for share of Spend, Impressions, and Clicks on overall spending on Meta, TikTok, and Google. The final two rows present the same dependent variables for Google Search and Google Display products. Standard errors are clustered at the domain level.

[20]The results for rows (4) and (5) are estimated over January to October 2021 as Google launched its popular Performance Max product in November 2021, which led to substitution within the products in the Google ecosystem.

# Appendix B   Additional Analyses for Revenue

In Figure B.2 we plot the demeaned log monthly revenue for the treated and control set of domains, where treatment is defined using the Meta revenue share and the iOS revenue share, respectively. In both cases, in the pre-ATT period, we see nearly identical trends in revenue with modest monthly growth over time. Roughly when ATT takes effect, we see that this trend continues nearly linearly for the domains with a low iOS share of revenue, whereas the upward trend stops for the domains with a high iOS share of revenue, for which revenues over time flatten out.

Figure B.1: Log Revenue for Low and High Meta Shares
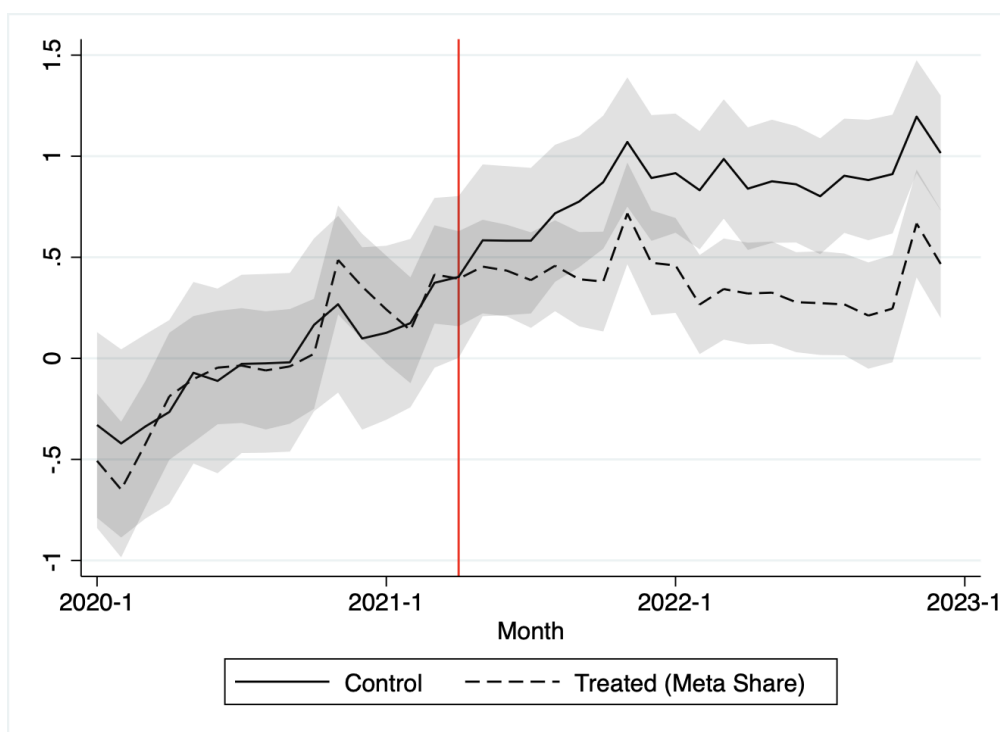
NOTES: Results using Dataset A. High and low Meta shares are calculated using a median split of pre-ATT revenue from Meta traffic. Plot shows log(revenue) demeaned using the pre-ATT mean along with 95% confidence intervals.

Figure B.2: Log Revenue for Low and High iOS Shares
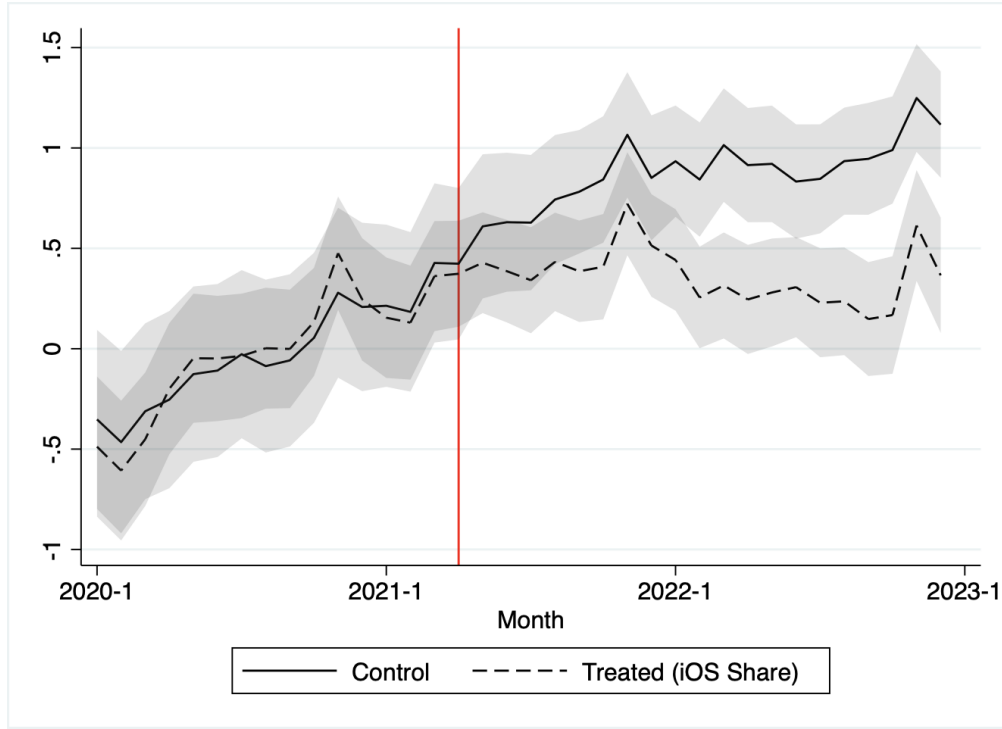
NOTES: Results using Dataset A. High and low iOS shares are calculated using a median split of pre-ATT revenue from iOS traffic. Plot shows log(revenue) demeaned using the pre-ATT mean along with 95% confidence intervals.

B.1 Shopify Analysis

Table B.1: Estimates on Sales

|	*Dependent variable:*		
Estimation Method	log(Orders)	log(Revenue)	Repeated Order Ratio
	(1)	(2)	(3)
DiD	−0.242*	−0.230*	3.994***
	(0.129)	(0.131)	(1.415)
DiD (w/ log(Ad Spending + 1) Control)	−0.171	−0.156	3.563**
	(0.118)	(0.119)	(1.419)
N	5,772	5,772	5,772

*p<0.1; **p<0.05; ***p<0.01

NOTES: All results use Dataset B2. The rows present the estimated average treatment effect coefficient using the difference-in-differences specification with and without controls for log(total advertising spending + 1). Standard errors are clustered at the domain level.

We utilize the same specification as in the main text – a difference-in-differences analyses across advertiser based on their reliance on Meta advertisers before the onset of ATT. We use a monthly aggregation of dataset B2 and compute each advertiser's pre-ATT market share of Meta advertising relative to Google and TikTok based on dataset B2.

Table B.1 presents the results for the outcome variables that we observe from Shopify: log(Revenue), log(Order Count), and Repeat Order Ratio. The first row presents the results. The takeaway across each of these is consistent: there is a reduction in orders and revenue of 20-22% and the fraction of total orders coming from repeat customers has increased. One important caveat is that the results are on revenues and don't control for possible shifts in costs, such as changing advertising spend. To understand whether this is simply a result of shifting advertising spend, we estimate the difference-in-differences specification controlling for the log of advertising spend. These results are presented in the second row of Table B.1. While this reports similar effect sizes for the ratio of repeat customers, we no longer find statistically significant reductions in revenues or orders though we still find sizeable and negative point estimates.

Finally, we show that the result on reduction in orders for new customers is robust to the inclusion of advertising spend and provide evidence for the reasonableness of parallel trends by measuring time-varying treatment effects. Table B.2 shows that the results on new vs. repeat customers is robust to controlling for ad spending. Figure B.3 considers the time-varying difference-in-differences specification with ad spending controls and provides some evidence that the parallel trends assumption seems to reasonably hold.

Figure B.3: Difference-in-Differences for Sales Outcomes
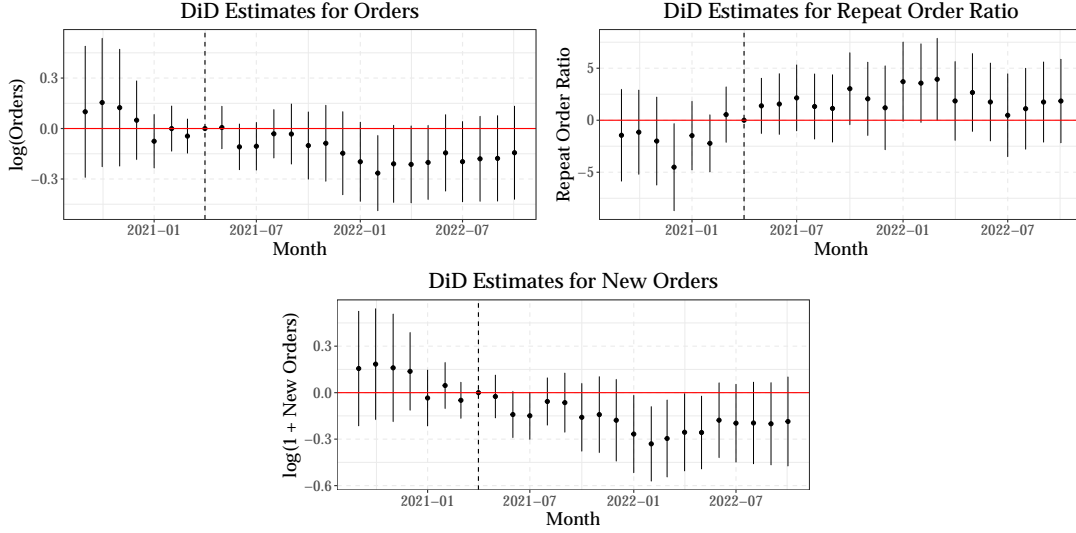
NOTES: All results use Dataset B2.The outcome variables on the top row from left to right: log(Orders), Repeated Customer Order Ratio. The outcome variable on the bottom row is log(New Customer Orders + 1). Plots are the difference-in-differences specification including controls for the log(Total Advertising Spend + 1).

Table B.2: Estimates on New vs. Repeat Customers

	Dependent variable:	
	log(New Customer Orders + 1)	log(Repeat Customer Orders + 1)
	(1)	(2)
DiD	−0.337***	−0.154
	(0.128)	(0.141)
DiD (w/ log(Ad Spend + 1) Control)	−0.257**	−0.097
	(0.116)	(0.132)
N	5,772	5,772
	*p<0.1; **p<0.05; ***p<0.01	

NOTES: The columns are log(1 + orders) coming from new and repeat customers. The rows present the estimated average treatment effect coefficient using the difference-in-differences and difference-in-differences specification controlling for log(1 + total advertising spending). The reported standard errors for these regressions are clustered standard errors at the domain level.