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EARLY EVIDENCE ON THE EFFECTS OF OPEN MOBILE IN-APP PAYMENT SYSTEMS FROM SOUTH KOREA

Boyoon Chang* & Keaton Miller**

ABSTRACT

Mobile app platforms are highly concentrated—Apple and Google each distribute over 75% of the total number of apps installed on the relevant devices. These platforms generally require developers to use a built-in system for processing payments with a commission rate of 30% for both the initial purchase of an app and any subsequent in-app purchases. In September 2021, South Korea became the first country to ban this lock-in; purchases made in the country may be conducted through any billing system developers wish. We analyze the short-term impact of this policy change (and Apple's subsequent adaptation) on demand for apps and revenue using difference-in-differences techniques and data on apps offered through Apple's App Store from a leading app analytics firm from January 2021 to December 2022. We find no evidence that the policy change generated substantive changes in South Korea's app marketplace.

JEL: K21, L12, L22, L41, L89

I. INTRODUCTION

The market for mobile phone operating systems is effectively a duopoly: together, Apple and Google provide the operating systems for over 99% of mobile phones sold globally. These firms each operate an app distribution platform: Apple's App Store for iOS, and Google's Play Store for Android. For app developers, interacting with these distribution platforms is effectively mandatory: there is no way for most mobile end-users to obtain apps outside of the distribution platform associated with the operating system for their device.

Apple and Google's control over the app distribution channels for their respective platforms has generated a significant level of concern from regulators and app developers alike (see for example, Allyn, 2021). Developers generally must sign contracts with platform owners with

* Postdoctoral Research Fellow, Arthur L. Irving Institute for Energy and Society, Dartmouth College, 33 Tuck Mall, Hanover, NH 03755, USA. E-mail: boyoonchang@gmail.com.

** Associate Professor of Economics, Department of Economics, University of Oregon, 435 PLC, 1285 University of Oregon, Eugene, OR 97403, USA. E-mail: keatonm@uoregon.edu. We thank the editor, two anonymous referees, David Evans, Grant McDermott, Jordi McKenzie, Z. Jay Wang, Eric Zou, and attendees at the Oregon Microeconomics Group, WEAI, and IIOC for helpful comments. We are grateful to Oregon Consumer Justice for research support under award 33136-4236A0. All errors are our own. We report no conflict of interest.

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terms that favor the platforms. Chief among these, and the focus of this study, are terms surrounding in-app purchases. Developers must use the payment systems provided by the platform owner, and must also remit standard commission rate of 30% of every in-app purchase to the platform owner as a commission.

In 2021, South Korea attempted to regulate these contracts by banning app store operators from enforcing exclusivity requirements for in-app payment processing. We study the effects of the law and the subsequent responses from Apple on its app market in South Korea. We develop hypotheses based on the claims of app developers: since the commission payment acts as a tax, a reduction in the tax may lead developers to lower prices of in-app purchases or improve the quality of their apps. This in turn may increase the number of users with the apps installed and increase the income of developers from in-app purchases. On the other hand, as argued by platform operators, users may be distrustful of alternative payment platforms and any benefit to developers may be more than offset by a shift in demand (Schreft, 2007; Eriksson *et al.*, 2021).

Two mitigating factors are important up front. First, the app market in South Korea is small relative to the rest of the world, which may reduce the policy effect for developers operating globally. Second, while Apple complied with the law by allowing developers to implement alternative in-app billing systems, its updated contracts still required a 26% commission on in-app revenue. Combined, these factors suggest a null hypothesis that, empirically, the law did not affect outcomes in South Korea's app market.

The appropriate question, therefore, is not "how much did the law affect apps?" but rather "*did* the law affect apps?" We thus focus our attention on apps with substantial pre-reform activity in South Korea. Our data consists of install and revenue data from Appfigures, a leading industry analytics provider. We test for changes in app-level outcomes using difference-in-differences and triple difference-in-differences techniques using app outcomes in other countries and time periods as controls. Ultimately, we find no evidence to overturn the null hypothesis: we conclude that the South Korean law largely did not affect Apple's app market in South Korea.

Our work contributes to the broader literature exploring the theoretical and empirical implications of tying strategies. Within this literature, most of the work has focused on examining the effects of tying (Carlton and Waldman, 2002; Amelio and Jullien, 2012; Derdenger, 2014; Choi and Jeon, 2021). In contrast, studies that analyze the effects of attempted remedies and/or regulations that seek to unbundle once-tied products are relatively scarce, in part because such regulations are themselves scarce. Gans (2011) shows that under certain conditions, mandatory untying policies may be effective at increasing social welfare. de Cornière *et al.* (2025a), in contrast, demonstrate that unbundling ancillary service activities from core platform operations restores sellers' market power, creating the potential for double marginalization that subsequently reduces both welfare and consumer surplus. Decarolis and Li (2023) examine the EU's attempts to untie mobile search in its jurisdiction and hypothesize that the success of any untying policy depends on the existence of "qualified challengers who can compete [on] quality." We contribute to this literature by providing an empirical analysis of the first app platform untying policy enacted globally.

Our work also contributes to the literature on mobile app platforms. Since there are multiple platforms available, a primary topic of interest has been multihoming, which has been shown to have ambiguous effects on prices and platform fees both in theory and in different empirical contexts (Landsman and Stremersch, 2011; Belleflamme and Peitz, 2019; Teh *et al.*, 2023). In the mobile app context, Hyrynsalmi *et al.* (2016) show that while multihoming is rare when considered against the broad population of apps, multihoming is much more common among the set of 'superstar' developers and apps; Ershov (2024) shows that platform congestion is a significant externality in this context. Gans (2012) provides a theoretical basis for the commission rates charged by platforms by demonstrating that charging for platform access (rather than taxing platform revenue) leads to an unravelling problem. Miao (2022) shows that the commission rates charged by platforms may *increase* social welfare by dampening the revenue-shifting incentive faced by firms selling ancillary goods (such as in-app purchases). Gans (2024) argues that app sales commissions in monopoly settings are set to maximize consumer surplus, not total welfare.

Quantitative empirical work on in-app payments is limited due to the limited set of reforms or instruments useful for identification, though some qualitative analyses exist (see for example, Ravoniarison and Benito, 2019; Hamari *et al.*, 2020; Salehudin and Alpert, 2022). One relevant qualitative analysis is that of Hwang and Kim (2022), who study the Korea reform and find that network effects and government guarantees against retaliation from Apple or Google drive decisions to switch to alternative billing systems or app distribution platforms. On the empirical side, Enache *et al.* (2023) use an exogenous adjustment to Apple's policies in 2021 along with data from five "freemium" mobile app games in European markets to estimate own-price elasticities of conversion (that is, the rate at which price changes generate changes in the propensity of users to convert from free to paid status) in the -1 to -4 range.

A growing body of literature provides a theoretical framework that examines the role of fees for platforms, although not many studies have directly and empirically focused on the app store market. For example, Gomes and Mantovani (2025) build a theoretical framework to derive the optimal cap on commissions, concluding that the cap should equal the amount of informational and convenience benefits provided by the platforms. de Cornière *et al.* (2025b) develop a model demonstrating that the third-degree price discrimination in two-sided markets can improve welfare compared to uniform pricing. Similarly, Wang and Wright (2018) compare two different fee structures, fixed per-transaction fees versus ad valorem fees, and find that ad valorem fees generally increases the welfare. We return to this literature below when motivating our hypotheses.

We proceed in Section 2 with a brief background on the mobile app market and the in-app billing policies of the major platforms. We discuss the theoretical literature in more detail and develop motivating hypotheses in Section 3. We introduce our data in Section 4 and provide some descriptive evidence. We detail our empirical approach in Section 5 and discuss our results in Section 6. We conclude in Section 7.

II. BACKGROUND

A. App Stores

App stores are digital platforms for selling and distributing mobile applications (apps). Developers publish their apps on these platforms while consumers use the platforms to discover, purchase, and access apps. Developers earn revenue through three methods. First, developers may charge an up-front price to download the app. Second, developers may embed advertisements into the app. Third, developers may lock certain features behind in-app purchases. These methods may be combined. For example, a strategy game app may be offered to download for free, but come with advertisements displayed between each round or before each turn. Users may pay a fee to remove such advertisements. The app may offer additional features in exchange for fees such as in-game performance bonuses (for example, increases to the rate at which the user progresses through the game) or changes to the appearance of game features (for example, 'cosmetic' changes to the characters in the game that do not affect game outcomes).

App stores offer users (among other features) limited security guarantees, a centralized record of purchases, and convenient re-installation of apps on new devices (for example, a user changing mobile devices which both use the Google Play Store may reinstall their applications on their new device with minimal intervention). These platforms exclusively offer developers access to large user bases (that is, developers who wish to offer their apps to Android users must do so through the Google Play Store) and baseline functionality related to online communication, payment systems, and cloud-based storage (of, for example, saved documents or play sessions). App stores generate revenue primarily by charging a commission on both purchases of apps with up-front prices and in-app purchases.[1] Historically, the 'tax rate' charged by both Google and Apple has been set at 30%. In other words, when a user spent $10 on either an up-front or in-app purchase, the developer received $7 and the platform received $3. In early 2021, both Apple and Google reduced the tax rate to 15% for the first $1 million of revenue earned by a developer each year.

Other app stores exist. For example, three major South Korean telecommunications companies maintain the ONE store platform which is installed on all mobile devices sold in South Korea that are serviced by the relevant wireless networks. ONE store offers developers a lower commission rate of 20% when using the ONE store payment platform and a 5% service fee when using other platforms.[2]

B. The South Korean Law and Responses

Responding to antitrust concerns, the South Korean National Assembly banned the operators of app store platforms from imposing payment methods on app developers starting September 14, 2021. This policy change made South Korea the first country to compel the opening of in-app billing systems by law.

The law required platform operators to submit detailed plans about how they would change their policies to comply. Google implemented changes in December 2021 that allowed developers to offer users in South Korea an alternative in-app billing system.[3],[4] To do so, developers must certify compliance with industry standards for security and fraud prevention, report all transactions from users in South Korea on a monthly basis, and pay a per-transaction service charge equal to the non-South-Korea market rate minus 4%. In other words, while developers may offer users an alternative payment system, most payments are still taxed at 26%. Apple followed suit in June 2022, mirroring Google's changes (including the 26% commission rate).[5]

Regulators in other countries are considering similar actions. The Netherlands' antitrust authority has ordered Apple to allow alternative payment methods for dating apps. In the United States, the Open App Markets Act is a bipartisan effort to untie payments and implement additional restrictions on app platform providers; the legislation was drafted in part in response to a lawsuit from a prominent app developer challenging Apple's in-app payment restrictions.

III. MOTIVATION AND PRIOR WORK

Our empirical analysis is motivated by the theory literature on two-sided platforms. In this section, we review and summarize some key findings in this space to explicate our hypotheses.

First, several authors have emphasized that price discrimination on the part of platform owners need not be simply extractive. Bhargava *et al.* (2022) focus on price discrimination (which they refer to as differential revenue sharing) based on the size of the suppliers using the platform and conclude that platform policies that favor smaller producers may also increase profits for larger producers through spillover effects. de Cornière *et al.* (2025b) examine a model

[1] Google also offers a monthly subscription service that includes access to a rotating library of paid apps.

[2] https://www.onestorecorp.com/en/sv/foreco/

[3] "Google's Latest App Store Payment Policy Raises Concerns in Korea," Pymnt, accessed Jan 7, 2025. https://www.pymnts.com/antitrust/2022/googles-latest-app-store-payment-policy-raises-concerns-in-korea/

[4] "Commission rates applied by the Google Play Store worldwide as of September 2024," Statistica, accessed Jan 7, 2025. https://www.statista.com/statistics/1497753/revenue-split-google-play-store/

[5] "Apple Enables Third-Party Payments in South Korea," Competition Policy International, accessed January 7, 2025. https://www.pymnts.com/cpi-posts/apple-enables-third-party-payments-in-south-korea/

of two types of sellers that differ according to their per-buyer revenue and similarly find that price discrimination can lead to more buyer participation, increasing profits even for those that face higher prices.

In a working paper, Anderson and Bedre Defolie (2025) develop a model of the app industry and explore the potential equilibrium impacts of third-party payment systems. They find that while such a reform certainly increases developer welfare, the effects on the apps and consumers depend on certain details of the environment. For example, if consumers and apps are homogeneous, the introduction of alternative payment systems simply shifts revenue from platforms to developers; app quality and prices are unaffected.

In their framework, platforms monetize consumers through device fees in addition to extracting revenue from consumer participation in the app store. They demonstrate that the introduction of third-party payment processors causes platform owners to reduce their commission to prevent all transactions from being diverted to third-party payment channels. The commission threshold is determined by consumers' transaction costs when using third-party payment systems. While this arrangement shifts app participation, thereby transferring rents from platforms to app developers, platforms re-optimize and increase device fees.

A substantial body of research explores the optimality of the commission fees and the effects of commission caps across various platform settings, including delivery platforms (Li and Wang, 2025; Sullivan, 2025; Gomes and Mantovani, 2025) and online booking platforms (Mantovani *et al.*, 2021).

From a theoretical perspective, Gomes and Mantovani (2025) show that platforms choose equilibrium commission fees to make suppliers indifferent between two alternatives: delisting from the platform (thereby reducing both costs and consumer demand) or remaining listed (gaining access to a larger consumer base but facing intensified competition). They further demonstrate that the socially optimal commission rates should reflect the informational externality generated by platforms. Similarly, Wang and Wright (2025) show that efficient platform fees should reflect the platforms' marginal costs adjusted for the markup differential between platform-mediated and direct sales channels.

Empirically, Li and Wang (2025) examine the implementation of commission caps on delivery platforms and find possible discriminatory platform responses: platforms may prioritize suppliers who pay higher commissions in their recommendation algorithms, effectively marginalizing those paying lower rates. Additionally, they observe platforms shifting costs to consumers through higher delivery fees to offset revenue losses from the capped commission.

In a working paper, Yu (2025) examines the U.S. app industry and explores the potential effects of several alterations to the fee structure both theoretically and through counterfactual exercises. Using data on music apps in the U.S., they demonstrate a positive correlation between developers' update decisions and app revenues and argue that capping platform commissions would change developers' incentives to innovate. Consistent with these findings, Lu *et al.* (2023) document an increased app update frequency in their counterfactual analysis of reduced commission rates.

Taken together, these results point to several hypotheses. First, the decoupling of in-app billing systems from the app distribution systems should lead to competition in the billing system space and lower commission rates. Assuming some degree of heterogeneity in both consumers and developers, these lower rates, in turn, should lead to higher app revenue. This higher revenue could come through either lower in-app prices or higher app quality, incentivizing greater usage by consumers. Changes in usage should occur on both the inter- and inframarginal bases, implying that the installed base of apps should increase (Ghose and Han, 2014).

Table 1. Summary statistics

Variable	Mean	Std. Dev.	1st %ile	25th %ile	Median	75th %ile	99th %ile
Downloads	16,122	48,569	6	468	2,261	11,214	253,390
Revenue	336,053	1,384,052	16	827	9,688	92,268	6,823,389
Observations				6,569			
Number of apps				87			
Number of developers				75			

IV. DATA AND DESCRIPTIVE EVIDENCE

To study the short-run effect of South Korea's legislation, and the corresponding policy responses of Google and Apple, we obtain panel data on app downloads and revenues from Appfigures. Appfigures is a firm that provides developers tracking and analytics services. Their data has been used by researchers in a number of fields to study app marketplaces (see for example, Coughlin *et al.*, 2016; Su *et al.*, 2021; Pybus and Coté, 2024).

We obtain Appfigures' dataset of app installations and revenue net-of-commission fees for the 174 apps that matched the following criteria: (a) available on Apple App Store and Google Play Store, (b) monetized solely through in-app purchases (that is, free to download), and (c) were "active" in the sense of positive downloads and revenue (with revenue of at least $1,000 in the 12-month period ending January 2024) in the South Korean market whenever they were available on the App Store. We obtain this data at the country-month level from January 2021 to December 2022 for South Korea, the United States, Japan, Germany, and France. We transform net revenues into gross revenues.

Table 1 reports summary statistics for our 6,569 app-country-month observations. There is substantial variation in the size of apps—the 25th percentile app is installed 468 times per country per month and earns $827 per country per month, while the 75th percentile app is installed over 11,000 times and earns over $90,000 over the same period. Most developers (86%) release only one app, though we note that the developer information released by Appfigures may obscure corporate or conglomerate ownership structures.

Our inclusion criteria results in a small number of apps relative to the number of "active" apps in the United States and in Europe. Our inclusion criteria are motivated by our research question (that is, we are aiming to test for the existence of an effect more than aiming to precisely measure its size) and our prior that the effect is likely to be small. These criteria, therefore, are designed to capture the set of apps that are the most likely to be impacted by the legislation, given their primary revenue source is in-app billing and their exposure to the South Korean market. Any effects of the policy change of Apple's App Store in response to the legislation should manifest within this group of apps that rely heavily on in-app purchase models and have a substantial presence in the regulated region. In other words, if there is any effect at all, it is most likely detectable in this set of apps.

It is important to note up front that these data consist of Appfigures' estimates of the relevant metrics. Appfigures forms these estimates by training prediction models on private data reported by developers who opt-in to its data collection program. In other words, Appfigures observes publicly-available data from Google and Apple for all apps, and private data from developers that opt-in. Appfigures trains prediction models on the set of apps for which private data are available, and produces estimates of the private data for all apps. It is these estimates that we use as the outcome measures in our analysis. Using the industry standard Mean Absolute Percentage Error (MAPE), their reported error rate falls in the range of 5% at best to 25% at worst.[6]

We proceed under the assumption that the Appfigures data are noisy measures of the true install and revenue data that have neutral bias relative to the reform. By 'neutral bias' we mean that to the extent that the Appfigures estimates are biased measures of the true data, that bias is not affected by the reform. Thus, comparisons of app performance pre- and post-reform can identify at least the direction of any effects. If, instead, the Appfigures data contain non-neutral bias, our results may be biased in either direction or attenuated from the true effect. This could occur if, for instance, the reform affected the way that Appfigures collected data for the relevant apps. We return to this possibility when we discuss our results below.

We chose the comparison countries as their per-capita spending on mobile apps is similar to that in South Korea. According to Statista, through the first three quarters of 2021, Japan had the highest mobile spending per capital with US$149. South Korea was the second with US$95, followed by the United States with US$90. Germany and France were 7th and 8th, respectively.[7] Although similar legal movements were put forth by developers against the two major app distribution platforms across several countries during the sample period, we observe no significant changes to the commission rate structure imposed by the Apple App Store until the end of 2022. This allows us to use the app performance data from these unaffected countries as a control group for our analysis.

We specifically analyze these apps' performance on the Apple App Store for two reasons. First, Apple's other in-app purchase policies remained stable over the period examined, unlike Google Play. This allows for a cleaner comparison of app performance pre- and post-treatment. Second, this analysis controls for substitution effects across app stores, as iOS users are inherently locked into the Apple App Store with no alternative store options on this operating system. This is less true for apps on Google Play, where developers have flexibility to distribute their apps through third-party app stores that offers different in-app purchase policies. Therefore, the estimated effects of legislation on app performance within the Apple App Store ecosystem can serve as an upper bound, as substitution effects are limited.

Finally, we are unable to directly observe developers' choice of in-app billing system in our data as the vendor does not collect this information. That said, conversations with developers in the space and examinations of apps reveal that at least some apps are willing to change payment systems, which confirms other qualitative investigations of app developers (Hwang and Kim, 2022).

Figure 1 and Figure 2 illustrate the distribution of app release years and primary app categories represented in the sample that are operating during 2021 and 2022, respectively. While we cannot analyze the performance of these apps across the policy change directly, we touch on this phenomenon in the conclusion. Within our data the top categories were Games (Role Playing, Strategy, and Action subcategories), followed by Social Networking apps.

V. EMPIRICAL STRATEGY

We estimate the effect of the policy change using a difference-in-differences model with fixed effects that compares the difference in outcomes for the treated group between the pre-treatment and post-treated periods to the difference in outcomes for the control group between the same

[6] See https://appfigures.com/support/kb/680/how-to-compare-your-downloads-and-revenue-wit h-estimates-in-compe titor-intelligence.

[7] https://www.statista.com/statistics/1275323/top-countries-mobile-per-capita-spending-w orldwide/

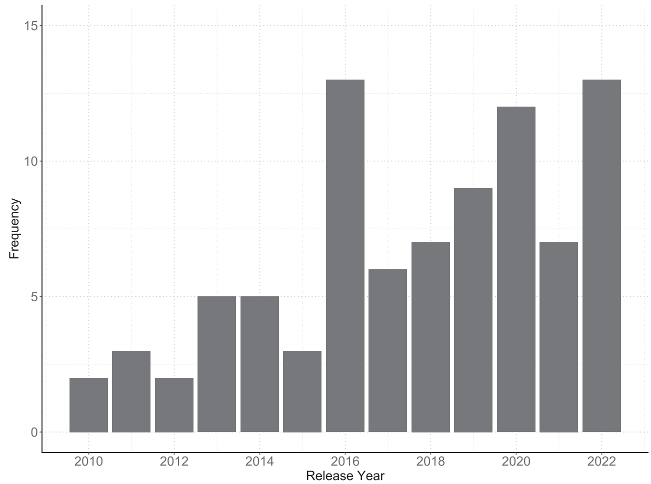

Figure 1. Distribution of App Release Years. *Notes:* This figure illustrates the number of in our study sample by year of release.

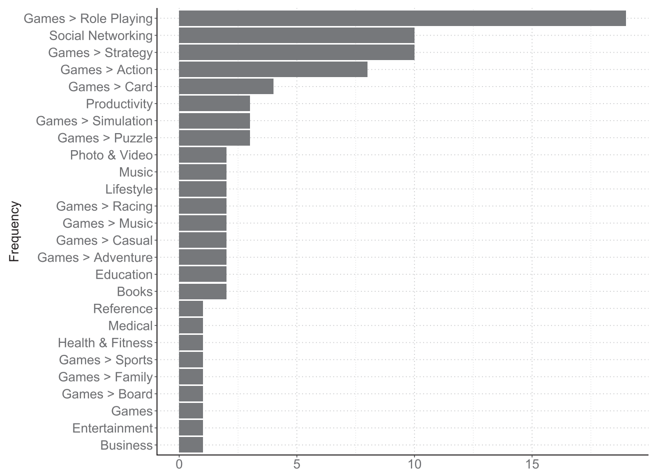

Figure 2. Distribution of App Categories. *Notes:* This figure illustrates the categorization of the apps in the study sample. The top six categories in the sample are General Games, Role Playing Games, Simulation Games, Strategy Games, Action Games, and Casual Games.

periods.[8] We estimate the parameters of

$$\log\left(Y_{ict}\right) = \beta_0 + \beta_1 D_{ict} + \alpha_i + \zeta_c + \gamma_t + u_{ict}, \tag{1}$$

where $\log(Y_{ict})$ is the logged outcome variable (either app installs or app revenue) for app i distributed in country c, at time t, D_{ict} is a treatment dummy that interacts PostReform_t, an indicator variable which is equal to 1 if t is June 2022 or later, and Treat_{ict}, an indicator variable

[8] Note that in this context, there is only one treated country, and so therefore many of the issues discussed by Callaway and Sant'Anna (2021) and Goodman-Bacon (2021) do not apply.

which is equal to 1 if app i for the users in country c is in the treated group. Fixed effects for cross sectional units are represented by α_i, ζ_c, which are app-level and country-level fixed effects, respectively. γ_t represents time fixed effects, and u_{ict} represents idiosyncratic app-country-time disturbances.

Although we have restricted our data to apps which are active (in the sense of strictly positive downloads and revenues) in the South Korean market, our data contain a few zeros. A visual inspection of the data reveals that many of these appear to be more accurately described as missing (for example, seeing a sudden drop in revenue to zero for one month and then a return to previous revenue levels), and so we drop those observations. We have also explored the possibility that these zeros are meaningful; there has been considerable debate about the appropriate estimation strategy to use in these cases (see, for example, Manning and Mullahy, 2001; Mullahy and Norton, 2024). As robustness checks, we use the Iterated Ordinary Least Squares method of Bellégo *et al.* (2022) and the two-part models of Belotti *et al.* (2015) to re-estimate our parameters and find qualitatively similar results.[9]

The parameter of interest is β_1, which represents the causal effect of Apple's changed policy on the outcomes (app installs and app revenue), provided that the identification assumption is satisfied.

As we do not know which apps changed payment methods, this parameter measures an "intent-to-treat" effect. For the difference-in-differences estimator to produce unbiased estimates, the parallel trend assumption must hold: the difference between the treated and the control remains constant throughout the analysis period in the absence of the treatment. Since dependent variable is logged, the estimate $\hat{\beta}_1$ can be interpreted as the percent change for treated apps during post-treatment period.

$\hat{\beta}_1$ isolates the change in app installs and app revenue that cannot be explained by what was observed as the difference between the treated and control group in the pre-treatment period.

That said, it is possible that any results (null or otherwise) stemming from that control group are driven by seasonal trends in download and in-app purchase behavior. To account for this, we explore specifications using two sets of control groups in a triple-difference-style approach. That is, we estimate the parameters of

$$\begin{aligned}\log(Y_{ict}) = \beta_0 &+ \beta_1 \text{PostYear}_t \times \text{PostMonth}_t \times \text{TreatCountry}_{ic} \\ &+ \beta_2 \text{PostYear}_t \times \text{TreatCountry}_{ic} + \beta_3 \text{PostMonth}_t \times \text{TreatCountry}_{ic} \\ &+ \beta_4 \text{PostYear}_t \times \text{PostMonth}_t \\ &+ \beta_5 \text{PostYear}_t + \beta_6 \text{PostMonth}_t + \beta_7 \text{TreatCountry}_{ic} \\ &+ \alpha_i + v_{ict},\end{aligned} \tag{2}$$

where $\log(Y_{ict})$ is the logged outcome variable (either app installs or app revenue) at time (year-month) t for app i that belongs in country c, PostYear_t, and PostMonth_t are an indicator variable which is equal to 1 if t is 2022 or later, and July or later, and TreatCountry_{ic} is an indicator variable which is equal to 1 if the app i country c pair is treated. Fixed effects for cross sectional units (apps) are represented by α_i, and idiosyncratic disturbances are represented by v_{ict}.

Equation (1) compares the outcomes between the treated and control groups and before and after the policy change. In essence, it captures the incremental changes of the outcome variables of the apps in South Korea relative to those in other countries post Apple's policy change.

However, if, for example, Apple's policy change in South Korea had any spillover effects on developer's decision in other countries (control group) or if unobservable confounder had led to abnormal changes in outcome variables only in 2022, estimates of the difference-in-differences

[9] Contact the corresponding author for details including regression tables.

(Equation (1)) may be biased. To address such concerns and to perform a robustness check of our difference-in-differences estimates, we implement a triple-difference estimator as shown in Equation (2) and use the relative difference between app outcomes of Korea and those of other countries in the absence of the policy change (year 2021) to measure the policy effect. Although both triple-difference and difference-in-differences estimators are prone to over-rejection or false positives, triple difference can be more effective in identifying the true effect by adding an additional layer of control (Olden and Møen, 2022).[10]

It is important to note that our analyses are subject to limitations, many of which we have discussed above. To summarize: first, our identification strategies assume that no event (other than the policy change) systematically affected outcomes for apps distributed on the Apple App Store in South Korea during the analysis period. Second, given that the installed base of smartphone users in the United States is approximately an order of magnitude higher than in South Korea (and the number of smartphone users worldwide is another order of magnitude higher), it is reasonable to believe that firms serving global users and facing high development costs would forgo implementing alter-native payment systems even in the context of South Korea's law. Lastly, due to data limitations, we cannot directly observe app-level switching of billing systems, which left us to analyze indirectly the legislation's effects through average app performance metrics, rather than directly counting affected apps and their individual performance changes caused by the legislation.

VI. RESULTS

A. Event Study

To ensure the ongoing consistency of app performance between Korea and other countries during the pre-treatment period, we conduct an event study. This methodology examines the parallel trend assumption in the pre-treatment period: accounting for app and country fixed effects, the average difference in app downloads during pre-treatment periods should not statistically differ from the period immediately preceding treatment onset.

Figure 3a illustrates the event study analysis of app downloads. We systematically compare app performance in the pre-treatment period against the period immediately preceding onset of treatment to identify any deviations in app behavior. The figure confirms no significant differences across time and countries before treatment, validating the parallel trend assumption. However, we do observe a potential drop from September 2021 through December 2021, though the confidence interval includes zero. This is a period of time of uncertainty in the market: the legislation had passed, but was not yet effective and neither Apple nor Google had not yet changed their policy.

Figure 3b presents the event study analysis of revenue. The estimates suggest there may be a consistent seasonal drop in revenue in the South Korean market (as compared to other markets) in November and December of each year. As with the revenue data, we see signs of a drop in revenue for the fourth month period between September and December 2021, though the estimates are noisy. This could represent anticipation effects, uncertainties about app store's compliance creating market fluctuations and technical disruptions, or simply systematic bias in the data during those months; for example, the legislation could have disrupted Appfigures' collection

[10] We could have used variation in the timing of the response across Google Play and Apple App Store as the third source of variation. However, using app performance on Google Play may not serve as a valid control, considering apps in our sample multi-home the two platforms, making Google Play performance a contaminated control. For example, they may wait until Apple's policy change, or have already responded when Google changed its policy or not respond at all altogether. As app performance may correlate across the two platforms, we use the alternative source of variation, the year and month treated, for the triple-difference estimation.

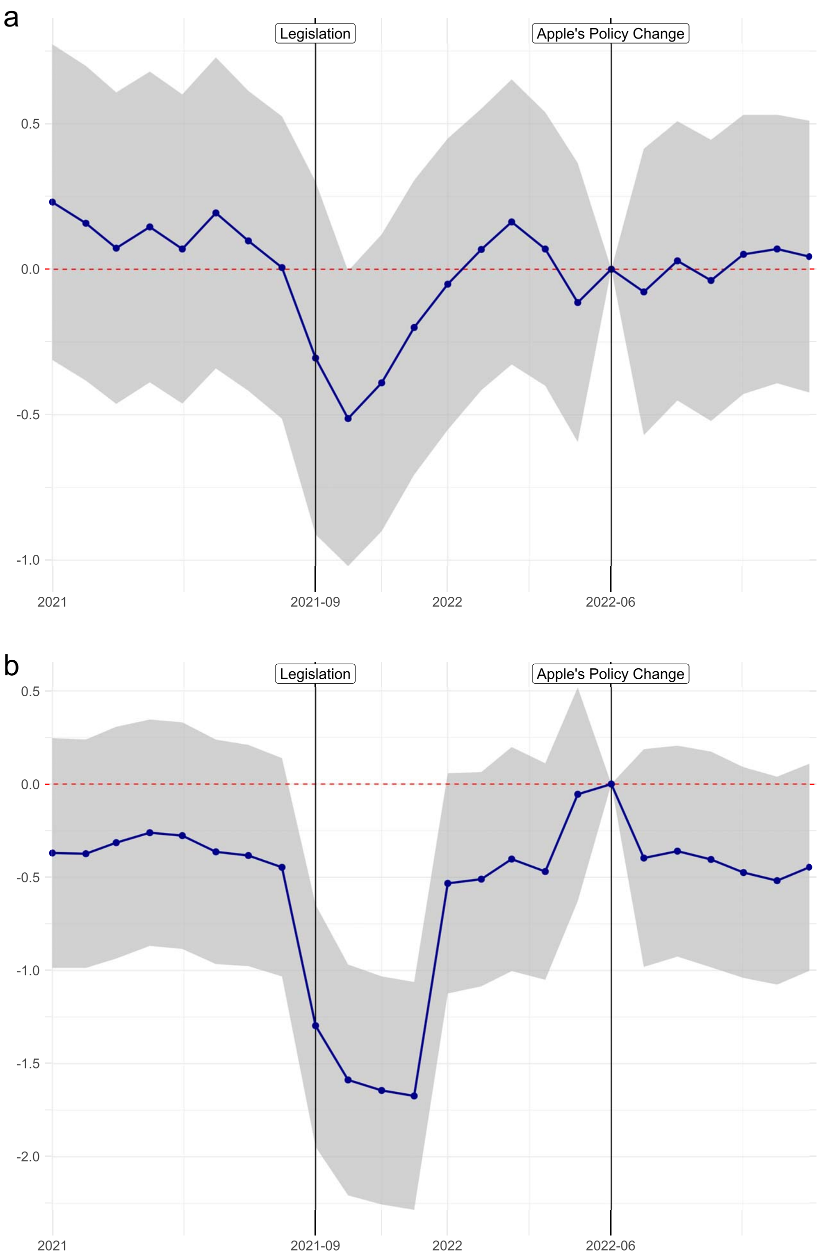

Figure 3. Event Study of App Reactions to Apple App Store Policy Change. (a) **App Installs**. (b) **App Revenue**. *Notes:* These figures illustrate the results of event study analyses of the Apple App Store policy change in South Korea. The dependent variable is the logged number of downloads in panel (a) and the logged app revenue in panel (b). The independent variables include app, country, and time fixed effects. The reference month is set on the month of June, 2022.

Table 2. DiD using other countries as controls

	Dependent variable	
	log(downloads) **(1)**	**log(revenue)** **(2)**
Post ∗ Treated	0.002	−0.093
	(0.064)	(0.079)
Observations	3302	3302
R^2	0.765	0.826
RMSE	1.0677	1.295
F Statistic	0.00	1.38

Notes: The reported coefficients are estimates of β_1 in Equation (1), the interaction term between indicators for the post treatment period and the treated country (Korea). The dependent variables are the logged number of installs in Column (1) and logged revenue in Column (2). The specification includes fixed effects for app, country, and time. The window of analysis is January 2022 to December 2022. Standard errors are clustered at the app level and are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 3. Robustness check of DiD models

	Dependent variable			
	log(downloads)		**log(revenue)**	
	(1)	**(2)**	**(3)**	**(4)**
Post ∗ Treated	0.001	−0.061	−0.102	−0.092
	(0.065)	(0.078)	(0.082)	(0.114)
FE: App	N	Y	N	Y
FE: Developer	Y	N	Y	N
Time frame	2022	2021–2022	2022	2021–2022
Observations	3302	5516	3302	5516
R^2	0.683	0.710	0.737	0.792
RSME	1.237	1.222	1.589	1.398
F Statistics	0.00	0.61	1.55	0.65

Notes: The reported coefficients are estimates of β_1 in Equation (1), the interaction term between indicators for the post treatment period and the treated country (Korea). The dependent variables are the logged number of installs in Columns (1)–(2) and logged revenue in Columns (3)–(4). In Columns (1) and (3), the specification includes fixed effects for developer, country, and time. In Columns (2) and (4), the specification includes fixed effects for app, country, time, and the window of analysis is January 2021 to December 2022. Standard errors are either clustered at the app level or developer level and are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

methodology for that period of time. To our knowledge, however, Appfigures' methodology is not dependent on the developer's choice of payment system.

B. Estimation Results

We estimate Equation (1) using the reghdfe package (Correia, 2014) and cluster standard errors by app.[11] Our estimation window consists of the six months prior to the reform through the six months after the reform (that is, January 2022 through December 2022). Table 2 summarizes our estimates of β_1. We find no significant evidence to refute the null hypothesis that the policy change did not affect the app market.

As a robustness check (Table 3), we also performed regression analysis with app developer fixed effects instead of app level fixed effect, and found no statistically significant app market

[11] Our primary concern is that there may be systematic variation in the accuracy of Appfigures' data across apps. We address this potential source of bias by including app fixed effects. Additionally, we cluster standard errors at the app level to account for within-app error correlation.

Table 4. Results using triple difference-in-differences

	Dependent variable	
	log(downloads) **(1)**	**log(revenue)** **(2)**
Treated Country ∗ Treated Year ∗ Treated Month	0.086	−0.027
	(0.104)	(0.098)
Treated Country ∗ Treated Year	−0.079	0.070
	(0.126)	(0.137)
Treated Country ∗ Treated Month	−0.102	−0.094
	(0.033)	(0.059)
Treated Year ∗ Treated Month	−0.118	-0.182^{**}
	(0.116)	(0.076)
Constant	7.815^{***} (0.103)	9.645^{***} (0.110)
Observations	5516	5516
R^2	0.581	0.673
RMSE	1.465	1.752
F Statistic	2.17	3.31

Notes: The reported coefficients are estimates of $\beta 1$ in Equation (2), the interaction term between indicators for the post treatment year, post treatment months, and the treated country (Korea). The dependent variables are the logged number of installs in Column (1) and logged revenue in Column (2). The specification includes app fixed effects. The window of analysis is January 2021 to December 2022. Standard errors are clustered at the app level and are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

effect of the policy. We also expand our estimation window to consider January 2021 through December 2022; and again, found no statistically significant effect of the policy on app installs or revenue.

We now turn to our estimates of Equation (2) shown in Table 4. We expand our analysis window to include January 2021 through December 2022 and once again cluster standard errors by app. Once again, we find no evidence to suggest that the policy affected the app market.

VII. DISCUSSION

An incredibly generous reading of these estimates would suggest that it is possible that the policy generated an increase in the number of installs and a decrease in the amount of revenue earned by Apple App Store apps in South Korea. However, the confidence intervals of all of our estimates include zero, and so it is difficult to come to any conclusion other than the null hypothesis: the policy change did not affect the South Korean app market.

That said, it is possible that the underlying behaviors do indeed move in the direction indicated by our hypotheses. The point estimates for installs suggest this. But, if so, is it reasonable to believe that this policy change could have generated a decrease in revenue (as the point estimates would suggest)?

One potential factor unexplored by our simple model lies in the trust (or lack thereof) that users have in third-party billing systems (Hillman and Neustaedter, 2017). Consumers may have less trust in third-party systems than in Apple's billing system integrated to the store (and indeed the operating system itself), and this lack of trust may lead consumers to reduce the frequency with which they buy in-app items. Indeed, a study of third-party payment systems in other contexts revealed several potential security flaws (Yang *et al.*, 2019). The simple need to choose a billing system may pose a sufficient friction to reduce the frequency of in-app purchases as well.

That said, however, our leading interpretation of these results is that the null hypothesis is correct. To summarize previous discussion, this conclusion is eminently plausible both because

Apple's policy change lowered the commission rate only marginally, and because developers are still operating under a "walled garden" where implicit or explicit measures, that is, developer's fear of possible retaliation or "scare screens"[12], can discourage developers and consumers from using third-party payment options. In fact, in the recent ruling of Epic Games, Inc. v. Apple, Inc. mandated the use of neutral notifications *and* the drop of commission fee for off-App Store purchases. This suggests that despite developers having the option to adopt other payment options, Apple's policy acted as a deterrent for consumers to opt for alternative payment options or generated sufficient friction to dissuade developers from integrating alternative billing systems.

VIII. CONCLUSION

The near-monopoly power of the two leading app distribution platforms have raised antitrust concerns that these platforms leverage their market power to tie in-app billing system with their platform operations and charge developers a dictated commission rate that is higher than would be present in a more competitive market (Whinston, 1990; Carlton and Waldman, 2002). To prevent leading platform operators from monopolizing in-app billing system, South Korea implemented an untying policy: app stores may not require developers to use any particular in-app billing system. This re-search examines the short-term effects of this legislation. We examine whether Apple App Store's response to the legislation has any significant impact on app demand as well as app revenue. While the previous literature has largely focused on negative market consequences of tying (reducing incentives to innovate, price that is charged higher than competitive rate, and so forth), the effects of unbundling are less well-studied. Our study is among the first to empirically investigate the market consequences of decoupling in-app billing system from operation of app distribution platforms.

Though we focus our analysis on apps which are active in the South Korean market (that is, that are most incentivized to respond to any policy change in that jurisdiction), we are unable to detect any substantial impact of the policy on observable outcomes including app installs and in-app gross revenue. We interpret this as informative: the impact of the policy change was *de minimis*. This is plausible in part because developers likely to be affected by the policy derive revenues from a global user base, making them less responsive to unilateral regulatory interventions, and in part because the development and promotion cycle of apps may delay any response.

Most importantly, however, Apple's response was designed to limit the ability of developers to capture additional revenue from in-app purchases. Apple still requires developers to continue to pay the platform a commission rate that is only marginally lower (3–4%points lower) than what was charged before for external purchases made outside the App Store.[13] Other efforts can also deter developers from adopting alternative payment systems. Peitz (2022) and Bar-Isaac and Shelegia (2025) explore the influence that platform owners have through app rankings, for example, platforms can downrank sellers to discourage off-platform price differentiations. Building on these views, if, for example, off-platform transactions are not accounted for in the sales-based app rankings, this can generate enough friction for developers such that they maintain their current payment systems despite the availability and legality of alternatives. On the other side of the market, platform owners may impose frictions on users seeking to use alternative billing systems such as sending potentially alarming or "scare" messages.

[12] The "scare screens" is a feature that warns consumers of potential risks associated with using other payment options.

[13] Apple charged 27% commission on external payments for dating apps in the Netherlands and also the same rate in the United States beginning January 16, 2024. Apple began to charge 26% in South Korea for purchases using external billing systems in June 2022.

We caution against over-extrapolation from these results to other markets. If, for example, the United States or the European Union were to implement more aggressive anti-tying and/or anti-competitive policies, one might reasonably expect the market to be more responsive. For example, the Digital Markets Act (DMA) imposes stricter and more comprehensive regulatory oversight on platform app stores. To restore market fairness and contestability in app store markets, Article 5(8) prevents tying arrangements across platform services, in addition to Article 5(7) that prohibits the tying between gatekeeper's app stores and payment services for in-app purchases that aligns closely with South Korea's legislation. The DMA further expands the regulatory scope through Article 5(4), which enables app developers to engage in platform disintermediation for in-app purchases, and Article 6(12), which mandates pricing regulations under Fair, Reasonable and Non-Discriminatory (FRAND) terms, requiring app store commission structures to comply with competitive pricing levels (Morton, 2024).

While these comprehensive regulations may achieve the intended welfare-maximizing and procompetitive outcomes, there remains the risk of regulatory arbitrage, where the platforms develop alternative strategies to circumvent compliance while encouraging or restoring exclusivity. For ex-ample, Crémer *et al.* (2024) discuss Apple's new tariff scheme (Core Technology Fee) in response to the DMA, which charges developers €0.50 per user per year if they do not distribute apps exclusively through Apple's platform. This fee effectively re-establishes exclusivity since it discourages the use of alternative app stores.

These factors (and our results) suggest that the market power of app platforms over developers can persist even in response to efforts by regulators to encourage a more competitive playing field. This suggests that policymakers should take a holistic view of the industry and anticipate potential responses from all market participants when considering alternative regulatory schemes for platforms. Franck and Peitz (2024) address these enforcement challenges by proposing a three-step methodology for identifying regulatory circumvention under the DMA's anti-circumvention provisions: (1) identifying the economic rationale of the conduct, (2) analyzing market effects, and (3) evaluating economic equivalence with practices targeted by relevant DMA obligations. That said, we expect the policy environment to continue to diversify, as different jurisdictions present varied political and legal contexts. For example, a lawsuit between Epic Games and Google is pending before the U.S. Supreme Court and will likely have substantial implications for platform policy in that country.

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