Cross-Platform Entry Effects of Commission Rates: Evidence from Mobile Applications in China*

Jiayi Hou[†] Xuan Teng[‡] Xuan Wang[§]

University of Hong Kong Ludwig-Maximilian University of Munich Peking University

September 25, 2026

Abstract

This paper studies how commission rates affect app entry and quality across platforms. We exploit a sharp increase in Android game commission from 30% to 50% in China and estimate its impact on Android and iOS app stores, separately, in a difference-in-differences framework. We document entry decline on both platforms, along with a mixed market-wide quality effect. Game entry falls by 47% on Android and 30% on iOS. Among unique apps across platforms, both high- and low-quality new games became fewer; in net, the average quality declined. These findings demonstrate two insights: there is supply-side platform complementarity and higher ad-valorem fees can discourage quality provision beyond the canonical screening effect.

Keywords: platform competition, commission rate, complementarity, entry, quality, app stores.
JEL Classification: K21, L11, L42, L86.

*We thank Alexandre de Cornière, Chiara Farronato, Doh-Shin Jeon, Fiona Scott-Morton, Martin Peitz, Ananya Sen, Hsin-Tien Tsai, Julian Wright, Daniel Xu and the participants at the VIDE Seminar, WEAI IOS Session, EARIE Mannheim, Munich Summer Institute, Economics of the Digital Markets Act (DMA) Workshop, Paris Digital Economics Conference, IIOC, University of Warwick, National University of Singapore, LMU Innovation Workshop, and Peking University for their valuable comments. All errors are ours.

[†]Faculty of Business and Economics, University of Hong Kong. Email: jiayihou@hku.hk.

[‡]Department of Economics, Ludwig-Maximilian University of Munich. Email: xuan.teng@econ.lmu.de.

[§]China Center for Economic Research and National School of Development, Peking University. Email: xuanwang@nsd.pku.edu.cn.

1 Introduction

Commission rates charged by app stores have raised worldwide antitrust disputes. Notable examples include *Apple v. Pepper* (2019), Spotify's complaint against Apple (2019), *Epic Games v. Apple* (2021), *State of Utah et al. v. Google LLC* (2021), and *Epic Games v. Google* (2023). Meanwhile, recent platform regulations prohibit anti-competitive practices that could lead to excessive commission rates. For example, the Telecommunications Business Act in South Korea (2021) prohibits app stores from penalizing apps for linking to an external payment webpage, and the Digital Markets Act (2022) in Europe requires gatekeepers to enable effective use of third-party app stores. Similar policies exisit in Japan's Mobile Software Competition Act (2024) and the proposed Open App Markets Act in the US.

The economic concerns of excessive commission rates are intertwined with app entry. App stores are often developers themselves; thus, a higher commission rate not only unilaterally increases the platform's revenue but also raises rivals' operating costs and potentially forecloses competition, raising the concern of excessive commission rate (Anderson and Bedre Defolie, 2024). Competing platforms may use lower commission rates to attract developers, potentially help curb excessive commissions. However, in a recent complaint against Apple, the U.S. Department of Justice (2024) notes that "most app developers do not view Android as a substitute for iOS or iOS as a substitute for Android". Formally, recent development in platform fee theories shows that when there is supply-side platform complementarity, platform competition may lead to (Jeon and Rey, 2025) or exacerbate (Teh and Wright, 2024) excessive commission rates. Whether there is supply-side platform complementarity is an empirical question, with a testable implication: when one platform increases its commission rate, are there fewer or more apps on rival platform(s)? One goal of this paper is to examine the entry effect of commissions across platforms.

A less-mentioned but welfare-relevant object is app quality, and it may change in an unexpected way with commission rates. Screening effect is a prevailing story in the literature: higher commission rates screen out low-quality products, leading to a higher average quality. However, as commission rates are ad-valorem, it may also generate a quality choice effect: higher commission rates flattens the quality-revenue curve, discouraging infra-marginal developers from quality investment, countervailing to the screening effect and may lead to a lower average quality. Thus the quality effect of commission rate is an

empirical question, amounting to the other goal of this paper.

Despite the policy and economic relevance of commission rate, there is little empirical evidence on its impacts. One main reason is the lack of variation. For example, Apple's App Store and Google Play Store, the two dominant app stores in most countries, both charge a default commission of 30% and introduced similar small-business programs only six months apart. To tackle this empirical challenge, this paper exploits a major change in Android game commission in China. In the absence of Google Play Store, multiple Android app stores form an oligopoly market in China. In August 2014, several leading Android app stores raised their commission rates on mobile games from 30% to 50%. This event is widely regarded as the pivotal change leading to the current 50% commission on games across most Android app stores in China. Meanwhile, Apple's App Store maintains its 30% commission rate across game and non-game categories. Therefore, this event allows us to study the entry and quality effects of a higher Android commission rate on Android and iOS, separately.

We compile a dataset on China's new apps released from August 2013 to January 2016. The data uniquely covers eight dominant Android app stores in China, four of which increased their commission rates in 2014. Furthermore, each app has a unified identifier across Android stores and Apple's App Store, enabling us to study both app development and platform choice. In sum, we obtain information on app category, release date, and store availability to study app entry, and on ratings, downloads, and security loopholes to study app quality. Defining a market as a category/quarter pair, we compare the number and quality of new apps in game and non-game markets before and after August 2014 in a difference-in-differences framework, separately on Android and iOS.

We begin with documenting a significant entry decline on Android, the fee-rising platform. In particular, we find a 47% decline in the number of new games on Android after its game commission increases from 30% to 50%, compared to the number of new apps in its non-game markets. We also find that the entry decline is particularly stronger among the Android stores that increased commission rates.

Examining entry on iOS, our findings support the complementarity between Android and iOS platforms in the eyes of app developers. In particular, we find a statistically significant 30% entry decline in game markets on iOS. This effect is driven by a market-wide decline in the number of unique games across iOS and Android, while the share of new games joining iOS significantly increased by 10 percents. In other words, although newly developed games become more likely to join iOS after the Android game commission

hike, there are fewer games developed in the first place, leading to a negative spillover effect on iOS entry.

Why didn't developers continue to produce games and only join iOS? We suspect that there must be some games that are only profitable to develop if joining both platforms. The micro-foundation can be supply-side, e.g., app development costs shared cross platforms, or demand-side, e.g., network effects in the utility from app usage, or both. We remain agnostic about the specific mechanisms and focus on a testable implication to confirm consistency, namely, a decline in new multi-homing games should account for the negative entry effect on iOS, rather than a change in the exclusive iOS games. Our empirical findings show exactly the expected pattern. In contrast, on Android, significant entry declines show up in both multi-homing and exclusive games.

In studying the quality effect of commission rates, we begin with documenting a market-wide average quality effect: the average ratings of unique new games across Android and iOS significantly decreased by 6%. The result is robust to alternative quality measures based on downloads and security loopholes. The finding indicates that the quality choice effect dominates the screening effect. Consistently, classifying apps into low-, medium-, and high-quality groups, we find that the number of new high-quality games significantly decreased by 48% and the number of new low-quality games significantly decreased by 24%.

To incorporate both screening and quality choice in one framework, we develop a simple two-stage quality choice model, where developers are heterogeneous in the efficiency to improve app quality. A higher commission rate generates different effects on different developers: entry selection on the efficiency screens out marginal developers who would choose low quality if entered, while optimal quality choice of infra-marginal developers becomes a lower quality at optimality given the lower marginal revenue with higher commission rate. We test the quality choice channel using an auxiliary data on incumbent version histories, as incumbent apps face quality choice but are immune to the screening effect. Consistently, we find that incumbent games postponed updates by 10% directly due to the commission rate increase.

Finally, zooming into each platform, we find quality effects in favor of iOS. In particular, the quality of new Android games declined while that iOS saw significantly fewer low-quality new games. These results reflect quality-specific app development and platform choice effects. For example, in the high-quality bin, Android suffer from both less development of high-quality new games and a lower share of these new high-quality

games joining Android. In contrast, the share of new high-quality games joining iOS significantly increased and offset the market-wide decline in high-quality game development.

Taken together, this paper conveys two insights and both are policy-relevant. First, there is supply-side platform complementarity among app stores. Combined with recent theoretical development in platform fees, it indicates that platform competition alone may not address excessive commissions and supports carefully designed regulations. It also has an implication for market definition: while gatekeepers are usually big platforms that invites focus on a platform at a time, platforms are not isolated and jointly define the relevant market. Second, entrant quality choice can co-exist or overturn screening effect in the context of ad-volarem fees. Thus, firm heterogeneity deserves attention in the design of tax, subsidy, or other ad-volarem policy instruments. Finally, as many markets are dominated by competing platforms that monetize on proportional commissions, such as e-commerce, food delivery, and ride hailing, these two insights likely apply to the other contexts of platform competition and regulation.

1.1 Related Literature

The main contribution of this paper is to provide the first empirical evidence on supply-side platform complementarity and to document the mixed quality effects of commission rate. The paper is related to three strands of literature: platform competition, multi-sided markets, and entrant quality.

Our empirical finding of platform complementarity directly speaks to the broad theoretical literature on platform competition. Rochet and Tirole (2003) and Caillaud and Jullien (2003) formalize two-sided markets, cross-group externalities, and platform pricing. Weyl (2010) provides a unifying welfare-based price-theoretic framework. Armstrong (2006) introduces the competitive bottleneck framework, which yields independent seller entry decisions across platforms in its canonical form. More recent theoretical work relaxes this independence assumption. Under different timing assumptions, Jeon and Rey (2025) and Teh and Wright (2024) show that when sellers' entry or profitability is correlated across platforms, platform fees can generate negative cross-platform spillovers, exacerbating the issue of excessive commission rates. Our paper directly relates to these theories by empirically studying how commission rates affect entry across platforms. To our best knowledge, our paper provides the first empirical evidence to answer this ques-

tion.[1]

Our paper is related to a growing empirical literature on multi-sided markets that examines how sellers respond to platform pricing and rules. This literature studies outcomes such as seller sales (Einav et al., 2014), prices (Farronato and Fradkin, 2022; Lam, 2023), product quality (Teng, 2025), and entry (Sullivan, 2023; Lee and Musolff, 2023; Conti and Santaló, 2023; Li and Tsai, 2026) in settings including e-commerce, peer-to-peer platforms, app stores, and food delivery markets. While these papers provide important evidence on seller-side responses to platform fees and design, many of them analyze a single platform in isolation and do not consider cross-platform spillovers or multi-homing decisions. Other papers that study multiple platforms assume independence of sellers' decisions across platforms (Rysman, 2004; Song, 2021), exogenous multi-homing (Kaiser and Wright, 2006), or platform choice conditional on development (Lee, 2013; Singh, Hosanagar and Nevo, 2021*a*,*b*).[2] Our paper complements these papers by studying the set of products available across platforms, which allows for platform complementarity arising at the product development stage, prior to or simultaneous with platform choice.

Our paper complements the literature on entrant quality by highlighting an underexplored mechanism—quality choice. The literature has largely studied the effects of different types of entry barriers, particularly fixed-cost type of entry barriers. For example, Ershov (2024) exploits a re-categorization of game apps on Google Play Store and finds that the resulting lower discovery costs lead to more entry and lower entrant quality, emphasizing the screening mechanism. Aguiar and Waldfogel (2018), Janssen et al. (2022), and Waldfogel (2025) highlight that product quality is imperfectly predictable prior to release and how this mechanism can lead to i) substantial welfare gains from new music, ii) fewer innovative apps after GDPR, and iii) welfare benefits from growth in female-authored books, respectively. In contrast, in our empirical setting, ad-valorem fees, rather than the fixed cost of entry, change. Accordingly, we document a signifi-

[1] Other theoretical studies on platform pricing have examined ad-valorem fees (Wang and Wright, 2017; Wang and Wright, 2018; Wang and Wright, 2025; Etro, 2023; Anderson and Bedre-Defolie, 2025), platform competition and multi-homing (Karle, Peitz and Reisinger, 2020; Tan and Zhou, 2021; Ronayne, 2021; Ronayne and Taylor, 2022; Teh et al., 2023; Llanes and Madio, 2025). Also see the survey by Jullien, Pavan and Rysman (2021).

[2] For example, Lee (2013) studies exclusivity contracts in video game consoles. In counterfactual simulations, he finds that when Sony PS2 loses its contractual exclusivity, Microsoft Xbox and Nintendo GameCube would gain game titles, indicating a positive spillover effect of contractual exclusivity, conditional on the set of available game titles. In contrast, our reduced-form analysis additionally considers the change in the set of available game titles when the commission rate changes.

cant effect on high-quality products, highlighting that higher ad-valorem fees reduce the marginal revenue of quality, thereby discouraging entrants to produce better products.

The rest of the paper is organized as follows. Section 2 explains the Android commission rate increase event in China's app industry in more detail. Section 3 describes our data and empirical strategy to study the cross-platform effects of commission rates. Section 4 and 5 presents our findings on the entry and quality effects, respectively. Section 6 provides a discussion and Section 7 concludes with policy implications.

2 Background

2.1 China's App Industry

We highlight three facts about China's app industry that are relevant for app store competition and app development. First, China's app industry is sizable, particularly in the gaming sector. In 2022, over two million mobile apps were available in China, generating over 110 billion new downloads, accounting for 43% of worldwide new downloads. In the mobile game sector, the worldwide revenue on iOS App Store and Google Play Store was $75 billion; while China's mobile games on iOS and Android app stores generated $27 billion. Back in the first half of 2013, China's mobile game sector was growing rapidly: sales revenue increased by 101%, and the number of mobile game players increased by 119%.[3] Thus, mobile games potentially generate sizable revenues for store owners.

Second, Android is the dominant ecosystem in China with the popularity of Android phones in China. According to the International Data Corporation (IDC), Android smartphones accounted for 79% of mobile phone shipments in China in 2014. For Android phone makers, the lower price of Android smartphones compared to iPhones brings popularity but also compresses profit margins. Thus, the sizable mobile game market provides an important additional revenue source for the Android phone makers in China. For the app developers, they would forgo many consumers if not joining any Android stores.

Third, Android app stores in China form an oligopoly, in the absence of Google Play Store. For example, there were 25 major Android app stores in 2022. These Android app stores have two main groups: i) first-party stores, owned by Android phone makers

[3]Sources of the statistics: China Ministry of Industry and Information Technology (MIIT), AppAnnie State of Mobile Report in 2023, Sensor Tower State of Mobile Gaming Report in 2023, Statista Mobile Games in China Statistics and Facts, and China Audio-Video and Digital Publishing Association (CADPA).

(e.g., Huawei), usually pre-installed on their devices, and ii) third-party stores, owned by tech companies who do not product smartphones (e.g., Tencent). In principle, Android phone users can multi-home between first-party and third-party Android stores on a single Android phone, and they can also download apps directly from browsers. However, consumers usually receive security warnings when attempting to bypass the first-party app stores and sometimes cannot find third-party app stores on the pre-installed first-party app store.[4] In the iOS ecosystem, Apple's App Store is the sole distribution channel in China, as in the other countries.

2.2 The Mobile Hardcore Alliance and Commission Rise

The Android commission rate shock happened with the formation of Mobile Hardcore Alliance (MHA) in August 2014. The Alliance has six initial members—Huawei, OPPO, Vivo, Coolpad, Lenovo, and Gionee, and was initiated and managed by an internet company, Wanka online. After several member changes since May 2016, there are eight member stores in the MHA nowadays.[5] The goal of MHA is to facilitate coordination among phone makers, game developers, and game players, by creating a joint marketplace across the member stores. These member stores play an important role in the Android ecosystem. According to Statista, as of May 2022, the app stores of Huawei, OPPO, and Vivo were the top three Android app stores in China, jointly accounting for 81% monthly active Android app store users.[6] Notably, MHA focuses exclusively on games and does not affect non-game apps.

The commission rise was an immediate policy of MHA after its formation. When confirming this event, we find that most Android app stores in China do not publicly disclose their commission rates, let alone their historical changes.[7] To address this information gap, we conducted interviews with multiple industry insiders, including nine app developers, three software development kit (SDK) providers, and four app-distribution service providers. Based on these interviews and contemporaneous news reports from

[4]The earliest news articles we found on these security warnings and disappearance of third-party stores are published in May 2015, nine months after the MHA was established.

[5]Specifically, since May 2016, Meizu, Nubia, and Honor have joined the MHA as new members, while Gionee has exited the MHA. Our sample period ends in January 2016. Before separating from Huawei in November 2020, Honor phones in China used the Huawei AppGallery as their pre-installed app store.

[6]Consumers using multiple stores are not deduplicated.

[7]The only exception we found is Huawei's App Gallery. Its official documentation shows that the store charges a 50% commission rate on games in China but 30% on games in other countries.

2014, we learned that before August 2014, the majority of Android app stores, including those owned by MHA members, charged a 30% commission rate.[8] Furthermore, anecdotal conflicts between game developers and MHA also supports the impacts of the higher commissions. For example, the blockbuster game, Genshin Impact, was not available on Huawei App Gallery at launch in 2020, and Tencent's games were briefly removed from the Huawei App Gallery on January 1, 2021. Nowadays, most Android app stores in China charge a 50% commission rate on games, and many online sources attribute this China-specific high commission rate to the formation of the MHA. Based on these multi-faceted facts, we believe that in August 2014 the MHA raised the commission rate on games from 30% to 50% in its members' app stores.

At least nine months later, MHA started enforcing some mitigating policies. For example, since May 2015, the MHA provides an SDK that allows developers to release their games simultaneously across all member stores, eliminating the need to prepare slightly different application packages that rely on each store's distinct Application Programming Interface (API). The MHA also offers monthly promotions for two selected games, as well as financial services to support game development, and organize annual meetings among the Android phone makers and game developers. It remains unclear whether MHA violates China's antitrust law, and we find no legal cases against the alliance per se.[9]

While the commission rates increased on MHA stores, we find that other Android app stores adopted the 50% game commission rate not until late 2017. Thus, third-party developers may switch from MHA stores to the other Android app stores.[10] Also, the existence of other Android app stores also gives large developers a better position in the

[8] We note that some online reports mention even higher commission rates before August 2014, such as 70%. However, according to our interviews, these extremely high rates happened in 2012 or earlier, and they reflected not only the ad-valorem platform fee on transactions but also additional charges for app marketing and operations.

[9] The most related cases we find are lawsuits against Vivo and OPPO regarding security warnings and their blocking of third-party app stores in 2017.

[10] Indeed, the formation of MHA reduces app store competition. In a counterfactual without MHA, the member stores and non-member stores may use other policies, such as promotions in the search results, to compete with each other. However, we believe that commission rate is a first-order factor in the supply-side competition, conditional on consumer base; thus we treat the MHA formation as a commission rate shock. For the demand-side competition, cross-platform (Android vs. iOS) variation in consumer base is more relevant for our research questions than within-Android cross-store competition. Different demand shocks across Android and iOS will not affect our estimates because our regressions are platform-specific. Within Android across stores, the most impactful change on the demand side is the first-party stores' restriction on consumers using third-party app stores, which happened not until May 2015. We expect it to offset the negative entry effect of a higher commission rates on MHA stores, which will attenuate our estimates toward zero, consistent with the patterns in our event studies

bargaining for a lower commission. Together, our context provides a lower bound on the effect of Google Play Store increasing commissions in other countries with equal importance of Android ecosystem.

3 Data and Empirical Strategy

We compile a dataset on the Android and iOS apps released in China during August 2013 to January 2016 and analyze it in a difference-in-differences framework comparing game markets to non-game markets, in order to study the effect of the game commission rise on the number and quality of new games cross Android and iOS.

3.1 Data

Sources and Samples. Our datasets are sourced from a commercial data company, Qimai, and an anonymous internet security company that conducts privacy and security inspections of Android apps for regulatory authorities in China. For some reasons, Android app stores in China do not record the initial release time. To obtain this information, we retrieve the complete version histories of Android apps from the anonymous internet security company. Then we take the release data of an Android app's first version as its initial release time. By merging these two datasets, we obtain the entry dates for 39,833 Android apps. The unmatched apps are due to missing bundle IDs in the version history dataset. Table E.1 compares observable characteristics between Android apps that are matched and unmatched across the two datasets; the results reveal no statistically significant differences. For iOS apps, Apple App Store provides the initial release time.

Our main sample contains top 50,000 apps on Android and iOS respectively from Qimai, ranked by cumulative downloads from the initial observed date up to 2022. Apps released around 2014 but delisted in 2022 will show up in the sample if its cumulative downloads reaches the bar. Given the downloads requirement, this sample is selective compared to a representative sample of apps released during our sample period. In Section 6, we obtain a representative sample of iOS apps from Sensor Tower.[11] We find that there is no significant difference between our entry estimates from the main sample and those from an economically-relevant sample.

[11] We thank Bruno Carballa-Smichowski for helping us obtain the representative sample on iOS.

Entry Variables. The release time of these apps ranges from 2008 to 2022, and we focus on those released during August 2013 to January 2016.[12] This sample period covers ten quarters relative to the formation of MHA in August 2014. We exclude apps released before August 2013 because there were very few entries before August 2013, and those released after January 2016 because there were confounding policy changes such as new regulations about mobile game content in May 2016 and thereafter.[13] In the end, we obtain 14,586 Android apps and 6,476 iOS apps released during the sample period.

Apart from release time, we obtain information on category, platform choice, store choice, ratings, downloads, and security. Note that during our sample period, all Android apps were free to download; and we do not observe prices of in-app purchase and subscriptions on either platform. The pass-through of commission rate on app prices remains an interesting question for future research.

There are 6,793 multi-homing apps across Android and iOS among the top 50,000 apps, identified from Qimai's internal unified app ID across platforms. Although the overall rate of multi-homing is lower than expected, Appendix Figure F.1 shows that the low multi-homing rate is driven by unpopular apps, while almost all popular apps multi-home. Furthermore, among apps released during our sample period, 44% iOS apps multi-home on Android, and 22% Android apps multi-home on iOS.

To measure platform choice between Android and iOS, we need to define joint markets across platforms and the choice timing. For the former, we construct joint markets consisting of unique apps across Android and iOS. For the latter, we clarify that apps may join a platform in the quarter of release or later, which we labeled as "native" and "ported" apps respectively. In Appendix Figure F.2, we find that native apps account for more than 88% of new apps on a platform at a given time. Thus, we focus on the share of new apps joining a given platform in the quarter of release.

Within android, the sample covers eight dominant Android app stores in China: i) MHA member stores: Huawei, Vivo, OPPO, and Meizu; ii) non-MHA stores: Xiaomi, Tencent, Baidu, and 360.[14] To capture Android apps' choice between MHA and non-

[12] Android apps' entry time starts from 2009, one year later than that of iOS, consistent with the first release of the iPhone in Hong Kong in 2008 and the first release of Android Smartphones (by HTC) in China in 2009.

[13] Examples of the new regulations on mobile games include the *Notice Regarding the Management of Mobile Game Publishing Services*, issued in May 2016, and the *Mobile Game Content Guidelines (2016 Edition)*, issued in June 2016.

[14] According to Statista, the eight Android app stores together account for 112% of Android app store monthly active users in China in 2022, without de-duplication of multi-homing consumers.

MHA Android app stores, we use approximations based on positive downloads.[15] Specifically, we assume that an Android app joins MHA (non-MHA) stores in its quarter of release if the app has positive downloads on MHA (non-MHA) stores in our data. Because our download data only begins in 2017, this proxy involves a temporal lag between the observed downloads and the initial store choice. We address this potential measurement error by verifying the robustness of our results across different observation years between 2017 and 2022. Since our primary focus is the cross-platform effect between Android and iOS rather than within-Android store choice, we provide a more detailed discussion of this approximation in Appendix B.2.

Quality Measures. To measure app quality, we mainly use the cumulative average rating five years after an app's release.[16] For multi-homing apps, we compute the average of quality across platforms, weighted by rating counts on each platform. A nontrivial share of apps lack ratings, representing 21% of the apps in our sample, reflecting that these apps did not receive ratings during the five years.[17] In Appendix B.3, we find that the share of no-rating apps is not significantly different across the game and non-game markets, before and after the commission rate increase, in the joint, Android, or iOS markets. To avoid arbitrarily assigning quality levels to no-rating apps, when studying quality effects, we focus on apps with ratings. Appendix Figure F.3 plots the distribution of app quality, which is highly left-skewed, similar to rating distributions observed on many other online platforms.

For robustness, we additionally consider alternative quality measures based on downloads and security. Specifically, our data contains iOS downloads since October 2019 and Android downloads since July 2016, reflecting when Qimai starts tracking app performance. To compare the change in the quality of Android and iOS apps, we compute the total incremental downloads in the last quarter of 2019 across the two platforms for apps released from August 2013 to January 2016.[18] For app security, our data contains

[15] We cannot observe the app-store-level entry time for Android apps, because for each version of each Android app, the internet security company conducts inspections for a random subsample of stores where the version of the app is available.

[16] We also examine robustness against different rating windows, i.e., one, two, three, or four years after release. To obtain most observations and the stabalized quality, we take the five-year window as the main result.

[17] The shares of apps lacking ratings are 19% and 18% on Android and iOS, respectively.

[18] Downloads after 2020 may reflect the pandemic effect, so we focus on pre-2020 downloads. We have also examined the downloads of Android apps on Android and the downloads of iOS apps on iOS. The results are similar.

two indexes for Android apps: the number of security loopholes and the security score—a normalized score that accounts for the different risk levels across different loopholes. We use these security indexes of the first version of the apps in our sample, capturing the security of these apps upon release. Appendix Figure F.4 shows that the average rating is positively correlated with each of these alternative quality measures.[19]

Finally, we aggregate the app-level data to market level so as to compare game and non-game markets. We define a market as a category-quarter pair. There are 24 categories on Android, 10 game, 14 non-game; and 35 categories on iOS, 14 game, 21 non-game. Here, game categories are subcategories within games, such as Action, Board and Card, and Role-Playing. When constructing the joint markets, we group the finer iOS categories into the broader Android categories and obtain 24 joint categories. Table E.2 list our cross-platform category mapping. Appendix B.1 provides further details in the category mapping procedure. In the end, we obtain 240 Android markets, 350 iOS markets, and 240 joint markets.

Table 1: Summary Statistics of New Apps at Market Level

	Android		iOS		Joint	
	Mean	SD	Mean	SD	Mean	SD
Num. of New Apps	60.78	[55.90]	18.50	[15.24]	77.30	[66.06]
% on MHA Stores	0.83	[0.20]	-	-	-	-
% on non-MHA Stores	0.99	[0.01]	-	-	-	-
% on Android	-	-	-	-	0.80	[0.17]
% on iOS	-	-	-	-	0.30	[0.24]
Avg. Quality of New Apps						
Cum. Avg. Rating	3.99	[0.35]	4.05	[0.43]	3.98	[0.31]
Incremental Downloads (million)	1.08	[1.07]	0.98	[1.43]	0.87	[0.88]
Security Score	58.06	[5.19]	-	-	-	-
Num. of Security Loopholes	19.96	[2.98]	-	-	-	-
Num of Markets	240		350		240	

Notes. The sample includes top apps released from August 2013 to January 2016, aggregated at market level. A market is a category-quarter pair. Joint markets consist of unique apps across Android and iOS. The % on MHA and non-MHA Stores are proxied by positive downloads in 2017, with similar results using different years from 2018 to 2022.

[19] About 12% sampled apps do not have downloads record in October 2019, likely due to inactive status. About 6% sampled Android apps do not have security indexes. In Appendix B.3, we find that these missing ratios are not significantly different across the game and non-game markets, before and after the commission rate increase, in the joint, Android, or iOS markets, separately.

Data Descriptives. Table 1 provides the summary statistics for new apps in our sample, aggregated at market level. We highlight three useful observations.

First, the average Android market had 61 new apps, more than three times the count in an average iOS market. The threefold gap reflects the larger total market share of Android smartphones and the resulting dominance of Android ecosystem in China.

Second, in the average joint market, about 77 new apps were released, of which 30% joined iOS and 80% joined Android in the quarter of release. These ratios reflect a multi-homing ratio of 10% across Android and iOS, lower than expected due to unpopular apps, as explained before. In contrast, multi-homing is prevalent within Android: 83% of new Android apps joined MHA stores and 99% joined non-MHA stores, approximated with 2017 downloads. This pattern reflects substantially higher multi-homing cost across platforms than within platforms, such as different programming language for developing Android vs. iOS apps.

Third, the average app quality in the average joint market is around 4.0 stars, with no notable difference between Android and iOS. Among the Android apps, the total incremental downloads across Android and iOS in 2019 Q4 is 1.11 million on average; among the iOS apps, the average level is 0.98 million; among all unique apps, the average level is 0.87 million. The slightly higher downloads among Android apps reflect the larger Android consumer base. We remain agnostic about the difference in app quality across platforms and study on the changes in app quality differentially for Android *versus* iOS after the Android commission rise. As a third measure of quality, Android apps obtain an average security score of 58 out of 100, with an average of 20 security loopholes.

For completeness, Appendix Figure F.5 divides the markets into game and non-game segments for Android and iOS and presents group-specific patterns in entry and quality. We observe a relative decline in game entry and quality on Android after the rise of Android game commission, with more subtle patterns on iOS. We formally analyze the data in a difference-in-differences framework.

3.2 Empirical Strategy

We empirically examine the effects of a higher Android game commission rate on the number and quality of new games, separately on Android and iOS. In doing so, we compare game markets to non-game markets. We consider 240 Android markets, 350 iOS markets, and 240 joint markets. Within each set of markets (Android, iOS, or joint), we

estimate a two-way fixed effects difference-in-differences model

$$y_{ct} = \gamma_0 + \beta\,(Game_c \times Post_t) + \gamma_c + \lambda_t + \varepsilon_{ct} \tag{1}$$

where c indexes categories and t indexes quarters, $Game_c$ indicates whether a category belongs to the game sector, and $Post_t$ indicates whether a quarter is after the commission rise. We include category fixed effects γ_c to absorb time-invariant unobservables at category level, and quarter fixed effects λ_t to absorb category-invariant unobservables that vary over time. A primary example of the unobservables absorbed by λ_t include market shares of iPhone versus Android phones. Even if the commission rate hike directly induced consumer switching between platforms, such a shift would be captured by our platform-time fixed effects.

To examine the evolution of game markets and non-game markets before the commission rise, we estimate the following event-study model

$$y_{ct} = \gamma_0 + \sum_{\tau=-4,\tau\neq-1}^{\tau=5} \beta_\tau(Game_c \times \mathbf{1}(t=\tau)) + \gamma_c + \lambda_t + \varepsilon_{ct} \tag{2}$$

where τ indexes quarters relative to the commission rise in August 2014. For instance, the three-month period from August to October 2014 is denoted as $\tau = 0$. We omit the quarter immediately preceding the commission rise as the base period. The difference between game markets and non-game markets relative to the base period ($\tau = -1$) is captured by the coefficient $\{\beta_\tau\}$.

To study entry effect, we set y_{ct} as log of the number of new apps. Thus the estimand is immune from proportional growth in entry. In cases where zero entry occurs, we apply Poisson regression following Chen and Roth (2024). Given the binary treatment, we translate the coefficient β into percentage changes using the formula $(e^{\beta} - 1) \times 100\%$.

For inference, we cluster standard errors at category level. Given the few clusters we have (24 or 35), following the literature (Cameron, Gelbach and Miller, 2008; Roodman et al., 2019), we use the wild cluster bootstrap-t method for inference and report the bootstrap p-values and 95% confidence intervals. In our empirical context, we found the results are similar to those using classical clustered standard errors. For exposition, we only report the results using the wild cluster bootstrap-t method.

Identification. Given the increase in commission rate on games, if developers substitute away from developing games toward developing non-game apps, our estimates would

exaggerate the magnitude of the direct entry effect and additionally capture a sector-switching effect. However, in our data we find that 98% developers on Android never switch between game and non-game apps, and the ratio is 99% on iOS. Furthermore, specialized assets in game *versus* non-game development also makes it hard for a game developer to become a non-game developer, or vice vera. For example, game developers need to work with game engines, design game loops and graphics, while non-game developers worry about managing proprietary and/or user-generated content, transactional databases, and business-domain knowledge (e.g., bank vs. health). Facing the gap in specialization, a non-game developer that transferred from the game sector is likely to produce a lower-quality app, leading to a relative increase in the quality of games, which is opposite to our empirical findings. Therefore, we believe that cross-category substitution, while possible, is not driving our results.

Our identification also relies on the assumption that developers did not systematically anticipate the commission increase prior to August 2014. Consistent with this assumption, contemporaneous reports describe the MHA formation as abrupt, and the event-study estimates in Sections 4 and 5 show no differential pre-trends in game versus non-game markets prior to the policy change.

4 Effect on the Number of New Games

In this section, we first show the main result: a higher Android game commission rate leads to fewer games on both Android and iOS. Then we investigate the mechanisms underlying the negative spillover effect on iOS entry.

4.1 The Direct and Spillover Effects on Entry

To study the entry effects across different platforms, we set the dependent variable y_{ct} as the log number of new apps in market ct on Android and iOS, separately. To verify that the effect is driven by the MHA's increase of the commission rate, we examine a third dependent variable: the log number of new apps in market ct on the MHA stores. All markets in our sample recorded positive entry during the sample period.

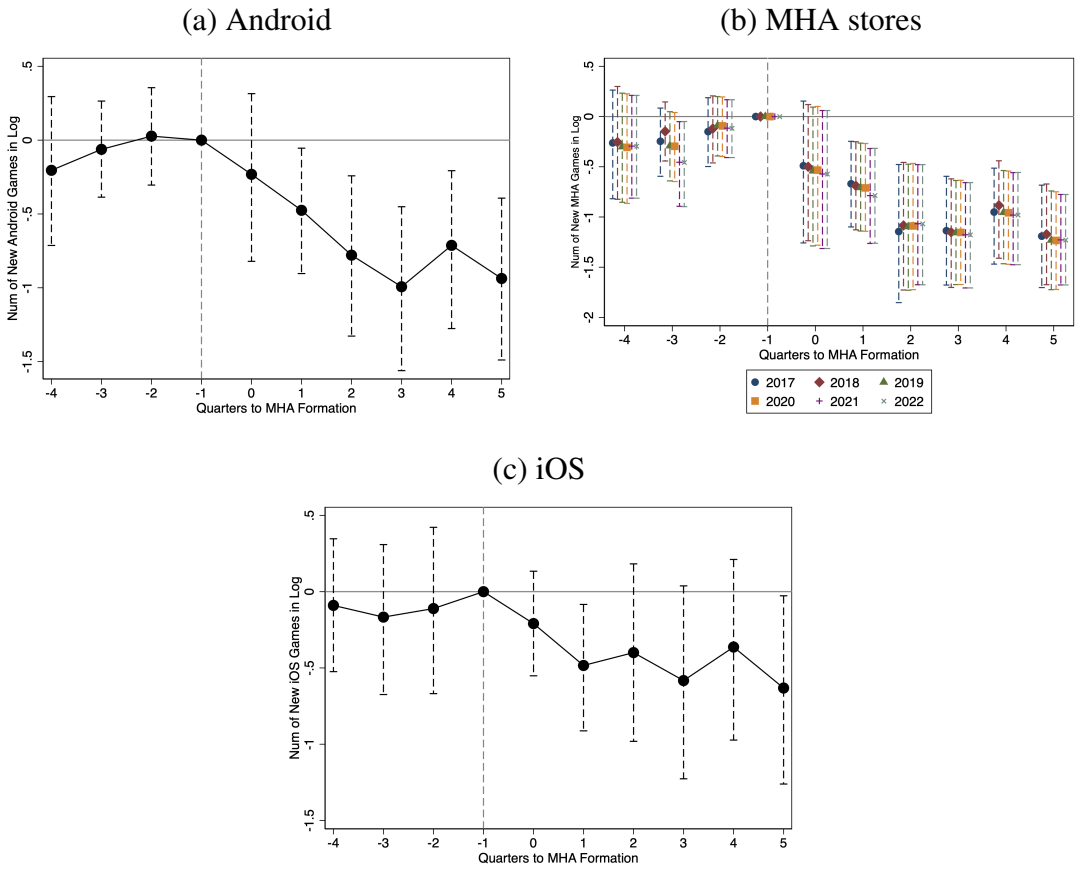


Figure 1: Android Game Commission Rise and Game Entry across Platforms

Notes. In plots (a) - (c), the dependent variable is the log of the number of new apps in a given category-quarter pair on Android, the MHA stores, and iOS respectively. Standard errors are clustered at the category level. The 95% confidence intervals are constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates. In Plot (c), Android apps' decision to join MHA stores is proxied by whether the app has any downloads on any MHA stores in the years shown in different colors. The choice of years is dictated by the availability of downloads data on MHA stores.

Figure 1 presents the event studies for the game entry on Android, iOS, and the MHA stores in Plots (a) - (c), respectively. Plot (a) shows a sharp decline in the number of new games in an average game market relative to the number of new apps in an average non-game market on Android after the Android game commission rise. Plot (b) displays a similar pattern on the MHA stores, robust to various approximations of Android apps' store choice. These results confirm the basic intuition: a higher Android game commission rate leads to fewer Android games.

Furthermore, the entry decline starts in the quarter of commission rise, deepens and

remains statistically significant afterwards. The decline slows in the third quarter after the commission rise, reflecting the attenuation effect of the other MHA policies such as the SDK, promotion, and financial services. Before the third quarter, only game commission changed on Android and particularly the MHA stores.

Importantly, Plot (c) documents the negative spillover effect on the rival platform. It shows that on iOS, the number of new games in an average game market *declined* significantly after the *increase* in the Android game commission rate, compared to the entry in non-game markets. We explore the underlying mechanisms in the next section.

Table 2: The Effects of a Higher Android Game Commission Rate on the Number of New Games across Different Platforms

	(1)	(2)	(3)
	ln(Number of New Apps)		
	Android	MHA Stores	iOS
Game × Post	-0.63***	-0.77***	-0.35***
bootstrap p-value	(0.007)	(0.002)	(0.001)
95% CI	[-1.04, -0.21]	[-1.21, -0.34]	[-0.58,-0.14]
Category FE	yes	yes	yes
Quarter FE	yes	yes	yes
Observations	240	240	350
Adj. R-sq.	0.89	0.90	0.82
Base Mean	32.58	23.48	7.34

Notes. Column (2) approximates an app's MHA store entry in the quarter of release with whether the app's downloads on MHA stores were positive in 2017. The approximation is dictated by unavailability of app-store level version history. The results are robust to alternative years of downloads data. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

Table 2 summarizes the magnitudes of these negative entry effects. Columns (1) - (3) show that, following the increase in the Android game commission rate, the number of new games in an average game market declined significantly by 47%, 54%, and 30% on Android, the MHA stores, and iOS, respectively, compared to the corresponding non-

game markets. In absolute terms, based on the pre-MHA baseline reported in the last row and the number of game categories on each platform, these estimates imply reductions of approximately 150, 120, and 28 games on Android, the MHA stores, and iOS in our top app sample, respectively.

4.2 Mechanisms of the Spillover Effect

We examine two related mechanisms underlying the spillover effects on iOS. We first show that the spillover is driven by a market-wide decline in game development, which dominates the increased likelihood for a new game joining iOS. Consistently, we also show that the spillover concentrates among multi-homing games, indicating the importance of multi-homing for game development.

4.2.1 Overall Profitability vs. Platform Choice

Conceptually, a higher Android commission generates two countervailing effects on iOS entry. On the one hand, the increase in Android commission weakly reduces the total expected revenue of a potential app across both platforms, making it less likely to cover the development costs. This *overall profitability* effect, or, app development effect, lowers the market-wide number of newly developed games and therefore reduces the number of new iOS games. On the other hand, a higher Android commission pushes some newly developed apps away from Android towards iOS. This *platform choice* effect increases the probability that a new game chooses to join iOS, and thus the number of new games on iOS.[20]

We empirically examine these two countervailing effects using the following dependent variables (y_{ct}) in the joint markets: (i) the log number of new apps, (ii) the share of new apps joining iOS, and (iii) the share of new apps joining Android.

[20]To clarify the platform choice effect, note that the likelihood of joining iOS is equal to the sum of the likelihood of only joining iOS and the likelihood of multi-homing. Since a newly developed app must join at least one platform, joining iOS only is mutually exclusive with joining Android, and its likelihood therefore increases when the Android commission rate rises. Moreover, the only way that a higher Android commission rate could reduce the likelihood of multi-homing is to shift apps that were previously indifferent between multi-homing and iOS-only towards iOS-only. Note that this shift reduces the likelihood of multi-homing, but the entire reduction is captured by the increase in the likelihood of joining iOS only. Thus, together, the likelihood of joining iOS increases with the Android commission rate. Indeed, empirically, we find that the share of iOS-only new games significantly increased by 13%, as the share of Android new games significantly decreased by 13% and they are mutually exclusive, as shown in Table 3. Meanwhile, the share of multi-homing new games significantly decreased by 3%.

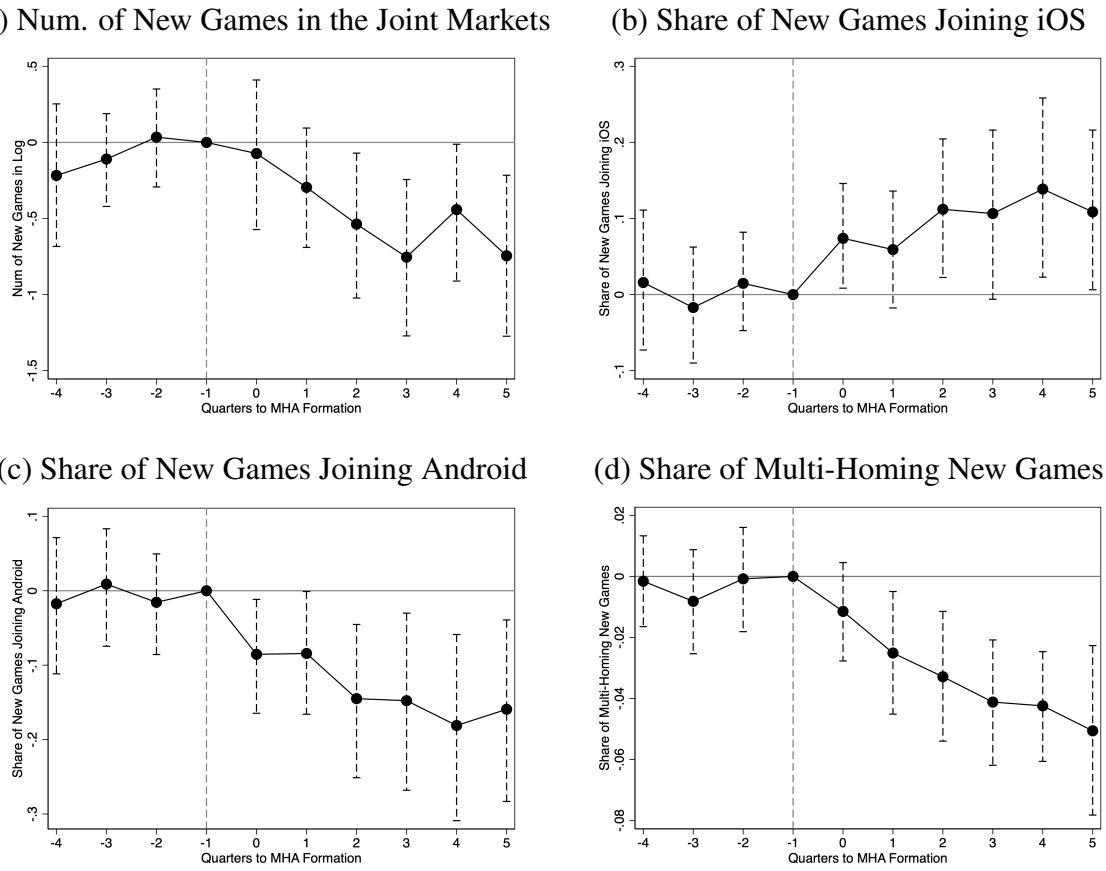


Figure 2: Overall Profitability Effect *versus* Platform Choice Effect

Notes. The dependent variable is the log number of new apps in the joint markets (plot a), the shares of new apps joining iOS and Android (plots b and c), and the share of multi-homing new games (plot d). Plot (a) shows the overall profitability effect of a higher Android commission. Plots (b) - (d) show the platform choice effects. Standard errors are clustered at the category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

Figure 2 presents empirical evidence for the two countervailing effects. Consistent with the overall profitability effect, Plot (a) shows that the number of new apps in the joint game markets declined sharply after the MHA's formation, compared to the joint non-game markets. Consistent with the platform choice effect, Plot (b) shows that the share of new apps choosing iOS increased significantly in the joint game markets after the MHA's formation. In contrast, Plots (c) - (d) show significant declines in the share of Android new games and the share of multi-homing new games. Thus, the game development declines more than the increase in the likelihood of new games joining iOS, leading to the negative

spillover effect on iOS entry.

Table 3: The Effects of a Higher Android Game Commission Rate on Game Development and Platform Choice

	(1)	(2)	(3)	(4)
	ln(Num. of	Platform Choice		
	New Apps)	% iOS	% Android	% Multi-Homing
Game × Post	-0.40**	0.10***	-0.13***	-0.03***
bootstrap p-value	(0.022)	(0.008)	(0.004)	(0.000)
95% CI	[-0.76, -0.05]	[0.02, 0.17]	[-0.21, -0.05]	[-0.05, -0.02]
Category FE	yes	yes	yes	yes
Quarter FE	yes	yes	yes	yes
Observations	240	240	240	240
Adj. R-sq.	0.93	0.88	0.85	0.61
Base Mean	41.63	0.17	0.83	0.01

Notes. The dependent variable is the number of new apps in the joint markets (in log), the shares of new apps joining iOS and Android, and the share of multi-homing apps in Columns (1) - (4), respectively. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

In terms of magnitude, Table 3 reports the average treatment effects on game development and platform choice. In the average game market, the number of new games declined significantly by 33% due to the higher Android game commission. Meanwhile, the share of new games joining iOS increased by 10 percentage points, and the share joining Android decreased by 13 percentage points, both statistically significant. Mechanically, the change in the multi-homing ratio is equal to the sum of the changes in the iOS ratio and the Android ratio, which move in opposite directions. Empirically, Column (4) shows that the share of multi-homing new games significantly decreased by 3 percentage points.

4.2.2 The Decline in Multi-Homing Apps

The overall profitability effect naturally raises a question: why didn't developers continue producing games and release them exclusively on iOS? A natural explanation is that some

games are only profitable if they join both platforms. Such cross-platform spillover (Teh and Wright, 2024) may root in the demand side: network effects in the utility of playing games (e.g., from interaction with other game players); or in the supply side: shared development cost across platforms, such as content, servers, and interface; or both. We remain agnostic about the specific micro-foundation but focus on a testable implication: the decline in entry on iOS should be driven by a reduction in the number of new multi-homing games, rather than changes in the number of new iOS-only games.

To that end, in each market on iOS, we decompose the new apps into those that also join Android (multi-homing) and those that are exclusively available on iOS. This decomposition generates zeros in many markets and restricts the variation in the data to construct wild-bootstrap confidence intervals of quarterly coefficients on the interaction terms. To deal with the variation issue, we modify the quarterly coefficients $\{\beta_\tau\}$ in Equation (2) to bi-quarter level: i.e., $\sum_{\tilde{\tau}\in\{-3,1,3,5\}} \beta_{\tilde{\tau}}(Game_c \times \mathbf{1}(t=\tau \vee t=\tau-1)$. Moreover, as taking logs is infeasible with zeros, we follow the literature (Chen and Roth, 2024) and change the linear regression to a Poisson regression. The dependent variable (y_{ct}) is the number of multi-homing (or iOS-only) apps. For comparison, we apply the same decomposition to Android markets and estimate analogous Poisson regressions.

Figure 3 presents the estimated bi-quarter coefficients $\{\beta_{\tilde{\tau}}\}$. The figure exhibits a striking contrast between iOS and Android. On iOS, the number of new multi-homing games declined significantly after the MHA's formation, while the number of new iOS-only games was not significantly affected. In contrast, as Android is directly affected by the shock, both the number of new multi-homing games and the number of Android-only games declined significantly after the MHA's formation.

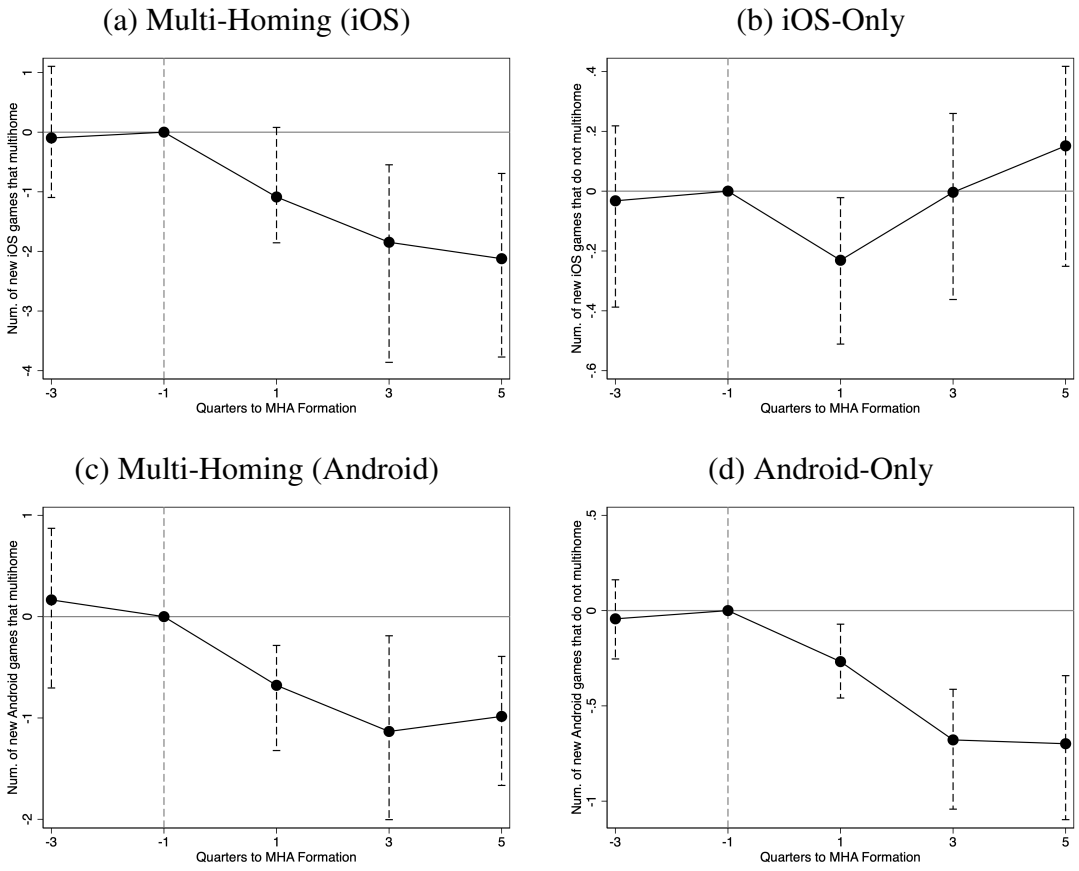


Figure 3: Decline in the Number of Multi-Homing New Games

Notes. This figure shows the estimated bi-quarter coefficients, grouping the quarter on the horizontal axis and its previous quarter. In panels (a) - (d), the dependent variable is the number of new multi-homing and exclusive apps in the iOS and Android markets, respectively. Zero entry occurs, so we use Poisson regressions in all panels. Standard errors are clustered at the category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

Table 4 reports the estimated average treatment effects on the number of exclusive and multi-homing new games. Columns (1) - (2) show that the number of multi-homing new games on iOS decreased significantly by 82% due to the higher Android game commission rate. Meanwhile, the number of iOS-only new games increased by 1% but the change is not statistically significant. Columns (3) - (4) show that the number of multi-homing and Android-only new games on Android decreased significantly by 66% and 42%, respectively. Together, these results suggest that multi-homing is not only profitable—and turns less profitable with higher Android commission, as shown previously by the decline

in the *share* of multi-homing new games—but also essential for some games.

Table 4: The Effects of a Higher Android Game Commission Rate on the Number of Multi-Homing and Exclusive Games across Platforms

	(1)	(2)	(3)	(4)
	Number of New Apps (iOS)		Number of New Apps (Android)	
	Multi-Homing	Exclusive	Multi-Homing	Exclusive
Game × Post	-1.71***	0.01	-1.02**	-0.54***
bootstrap p-value	(0.002)	(0.956)	(0.011)	(0.003)
95% CI	[-2.40, -0.97]	[-0.23, 0.20]	[-1.45, -0.69]	[-.85, -0.25]
Category FE	yes	yes	yes	yes
Quarter FE	yes	yes	yes	yes
Observations	350	350	240	240
Pseudo R-sq.	0.63	0.58	0.82	0.811
Base Mean	0.43	6.91	1.03	31.55

Notes. The table reports the estimated coefficient from Poisson Regression. In Columns (1) - (4), the dependent variable is the number of new multi-homing apps in the iOS markets, the number of new iOS-only apps in the iOS markets, the number of new multi-homing apps in the Android markets, and the number of new Android-only apps in the Android markets, respectively. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

Multi-Homing within Android. Is there similar spillover effect within Android across MHA and non-MHA stores? If so, how is it different from cross-ecosystem spillover effect? We can form some expectation from two institutional features, on the demand and supply sides, separately. First, during our sample period, an Android phone user could use multiple Android app stores on a single Android smartphone. In contrast, an Android phone user could not access the iOS App Store unless she also owns an iPhone. This generates a higher demand-side substitutability among Android app stores than across Android and iOS. Thus, within-Android, being available only on non-MHA stores would sacrifice less network effect than Android-iOS exclusivity. Second, porting across Android stores is also easier than across Android and iOS because the two

ecosystems use different codebases, among other frictions. Thus, within-Android exclusivity, being available only on non-MHA stores would save fewer porting costs than Android-iOS exclusivity. These two countervailing effects make it unclear whether the within-Android spillover effect would be stronger than Android-iOS spillover effect, or even exist at all.

In Appendix C, we examine within-Android cross-store spillover effect on entry. Similar to Android-iOS spillover, we find significantly fewer new Android games on non-MHA stores. Different from Android-iOS spillover, there is not only significantly fewer multi-storing new Android games on non-MHA stores, but also significantly *more* non-MHA-only new Android games. The latter is consistent with the higher demand-side substitutability among Android apps stores. Together, because most Android games multi-store, the former dominates the latter, leading to the net negative spillover effect within Android. These within-Android results should be interpretated with caution because apps' within-Android store availability are proxied with positive downloads.

5 Effect on the Quality of New Games

In this section, we study the market-wide and platform-specific quality effects of a higher Android commission. We begin with a market-wide analysis on unique apps across Android and iOS. We find that the average quality of new games significantly decreased, driven by a decline in the high-quality entry. We then propose a two-stage quality choice model that explains the quality effect and provide empirical support for the key mechanism, i.e., a higher ad-valorem fee discourages quality provision. Finally, we document platform-specific quality changes that turn out to be in favor of iOS.

5.1 Market-Wide Quality Effect

To study the market-wide quality effect, we examine the sample of joint markets consisting of unique apps across Android and iOS. We begin with the average quality of new apps, where quality is measured by the cumulative average rating 5 years after release.[21] Then we construct three quality bins—high, medium, and low—using the

[21] Using different rating windows—two, three, or four years after release—does not significantly change the results.

category-specific 33rd- and 67th- percentiles of app quality. This enables us to examine the quality distribution via three additional dependent variables: the number of high-, medium-, and low-quality new apps. Because zero entry occurs within quality bins and the variation to produce quarterly bootstrap confidence intervals is limited, we use Poisson regression and bi-quarter periods for bin-specific entry.

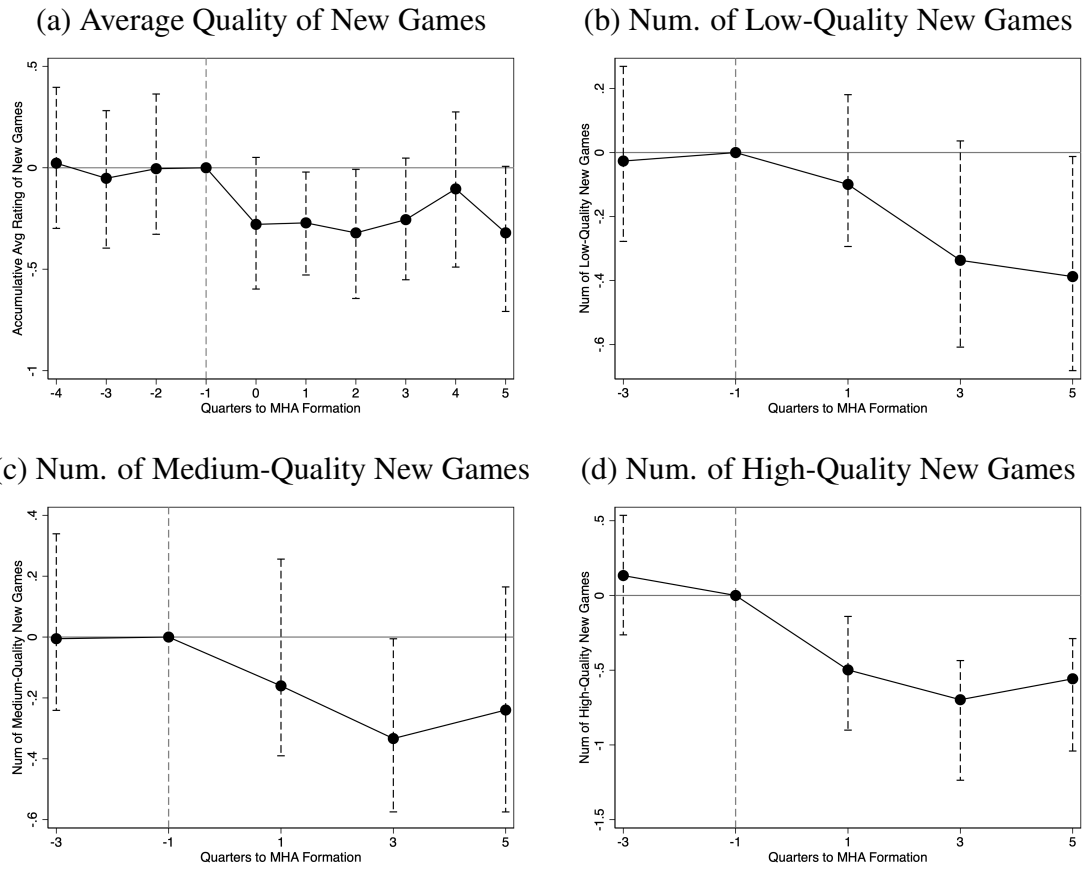


Figure 4: The Market-Wide Quality Effect of the Android Game Commission Rise

Notes. Standard errors are clustered at the category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

A standard screening theory would imply a higher average quality with higher commission, as low-quality entrants are screened out. However, Plot (a) of Figure 4 shows that the average quality of new games significantly declined after the increase in the Android game commission rate. This is not because screening did not happen. Plot (b) shows that

the number of low-quality new games significantly declined following the higher commission rate. Rather, it is due to a stronger decline in high-quality entry, as shown in Plot (d). Together, these quality-bin-specific entry effects lead to a leftward shift in the quality distribution: the share of high-quality new games significantly decreased, while the share of medium- and low-quality new games significantly increased, as shown in Appendix Figure F.7, and thus the decrease in average quality.

Table 5: The Market-Wide Quality Effect of a Higher Android Game Commission Rate

	(1)	(2)	(3)	(4)
	Avg. Quality	Num. of New Apps		
		High-Quality	Medium-Quality	Low-Quality
Game × Post	-0.25***	-0.65***	-0.24*	-0.27**
bootstrap p-value	(0.001)	(0.002)	(0.094)	(0.059)
95% CI	[-0.43, -0.08]	[-1.08, -0.39]	[-0.45, 0.05]	[-0.47, -0.02]
Category FE	yes	yes	yes	yes
Quarter FE	yes	yes	yes	yes
Observations	240	240	240	240
Adj. R-sq.	0.39	0.74	0.72	0.72
Base Mean	3.95	8.85	9.50	9.48

Notes. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. Appendix Table E.7 shows that the results are robust to an alternative downloads-based quality measure.

Table 5 summarizes the estimated market-wide effects on quality. Column (1) shows that the average quality of new apps in an average game market significantly decreased by 0.25 stars after the game commission increase, compared to that in an average non-game market. The baseline quality of new games was 3.95 stars before the commission increase, leaving the effect equivalent to a 6.3% decline in quality. Columns (2) - (4) show that the number of high-quality new games significantly decreased by 48%, dominating the 24%-decline in low-quality new games; whereas the medium-quality bin experienced the least entry decline. Appendix Table E.6 shows that the share of high-quality new games significantly decreased by 12 percentage points from a baseline of 26%; meanwhile, the share of medium- and low-quality new games significantly increased by 7 and 8 percentage points, respectively. Appendix Table E.7 further shows that these results—a stronger entry decline in high-quality bin and a leftward shift in quality distribution— are

robust to using downloads as an alternative quality measure.

5.2 Mechanisms of the Quality Effect

Entry decline in both the low- and high-quality bins indicate that a different mechanism is happening together with screening. We propose that the mechanism is entrants' quality choice: a higher commission reduces marginal revenue from quality. We develop a theoretical model that incorporates both mechanisms and provide empirical support for the quality choice channel.

A Quality Choice Model. We develop a simple two-stage quality choice model to explain our empirical findings on entrant quality. In the model, each monopolist developer is endowed with a quality-provision technology F_{0i} that governs her cost function to develop an app at different quality levels. In the first stage, developers choose whether to develop an app. In the second stage, developers choose the quality level of the potential app. As a result, developers with F_{0i} smaller than a threshold $\overline{F_{0i}}$ enter the market. The developers with $F_{0i} = \overline{F_{0i}}$ endogenously produce the lowest-quality apps. A higher commission rate pulls down the threshold and thus screens out the least efficient developers (marginal developers), leading to fewer low-quality new apps. Meanwhile, among the infra-marginal developers, the higher commission rate flattens the revenue curve with respect to quality, discouraging quality provision, leading to fewer high-quality new apps. Appendix A provides the formalization of the model and the proofs on the screening and quality choice effects.

To empirically examine the quality choice mechanism, we explore incumbents' versioning behavior, as incumbents still face quality choice but are immune from the screening mechanism.[22] We define incumbent apps as apps released before our sample period (August 2013) and construct the version age of each incumbent Android app in each quarter.[23] For detailed steps in the construction of the version age, please see Appendix B.4. We then estimate equations analogous to Equations (1) and (2), replacing category-fixed effects with app-fixed effects. Since fewer entrants can weaken competition and poten-

[22] As an alternative measure of incumbents' quality provision, we also examined the average rating of incumbent apps during the event-study sample period and find no significant effects, both in terms of magnitudes and statistical significance. It reflects that average ratings are not sensitive to versioning behaviors in this empirical context.

[23] Due to a lack of data on iOS app versions from August 2013 to January 2016, we cannot analyze the versioning behavior of incumbent iOS apps. It is an interesting research question for the future.

tially delay incumbents' versioning, we further estimate the effect of commission rates on versioning while controlling for the number of new Android and iOS apps.

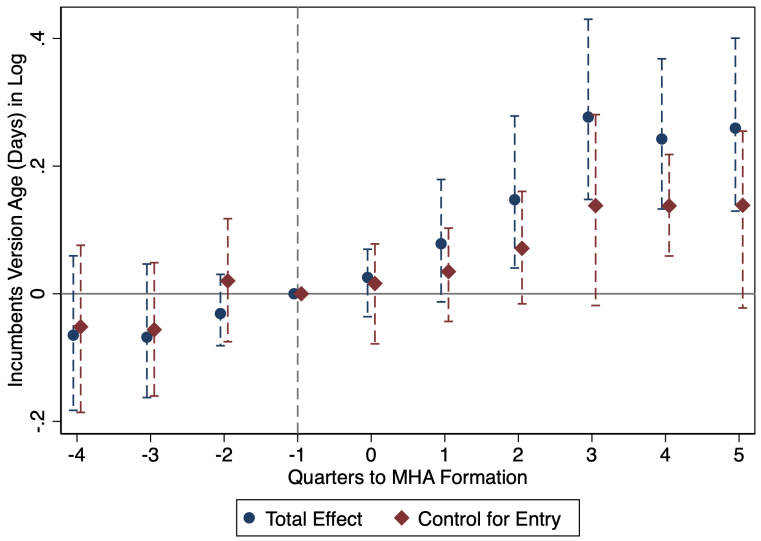


Figure 5: Effect on Version Age of Incumbent Android Apps

Notes. This figure compares the version age of Android incumbent games with Android incumbent non-game apps before and after the commission increase. The data is an app-quarter balanced panel of incumbent Android apps released before August 2013. We examine their versioning behavior during August 2013 to January 2016. The dependent variable is the log of version age in days. The blue dots are estimates without controls for the number of new apps on Android and iOS, while the red squares are estimates with such controls. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

Figure 5 presents the event-study estimates on incumbent version age, with and without controls for new app entry. It shows that after the commission increase, incumbent Android apps in the game markets significantly postponed versioning, compared to incumbent Android apps in the non-game markets. This negative quality provision effect is partially but not fully explained by the reduced entry after the commission rate increase.

Table 6 shows that compared to incumbent non-game Android apps, the version age of incumbent Android games significantly increased by 23% after the commission increase. Roughly half of the effect is explained by the weakened competition associated with fewer entrants on Android. The rest of the effect is directly due to the commission increase, reflecting the quality choice mechanism.

Therefore, we believe that higher ad-valorem fees reduce marginal revenue at all quality levels, thereby discouraging quality provision among infra-marginal entrants, domi-

nating the screening-out of marginal entrants who would choose low quality if entered and resulting in the lower average quality.

Table 6: The Effect of a Higher Android Game Commission Rate on Incumbent Game Version Age

	(1)	(2)
Variables	Log Version Age (in Days)	
Game × Post	0.21***	0.10*
bootstrap p-value	(0.007)	(0.064)
95% CI	[0.10, 0.33]	[-0.01, 0.21]
ln(Num. of New Android Apps)		-0.25***
bootstrap p-value		(0.000)
95% CI		[-0.39, -0.13]
ln(Num. of New iOS Apps)		-0.01
bootstrap p-value		(0.913)
95% CI		[-0.15, 0.12]
App FE	yes	yes
Quarter FE	yes	yes
Observations	34,200	34,200
Adj. R-sq.	0.62	0.63

Notes. The table reports the average effect of a higher Android game commission rate on incumbent Android games' version age. The sample is a balanced panel of app-quarter pairs, containing Android apps that are released before August 2013. We compute their version ages in each quarter from August 2013 to January 2016. Column (1) reports the total effect, while Column (2) separates the direct effect of commission rate from the indirect effect through reduced entry. We use the wild cluster bootstrap-t method at category level to conduct inference. Bootstrap p-values in parentheses. 95% confidence intervals in brackets. *** p<0.01, ** p<0.05, * p<0.1.

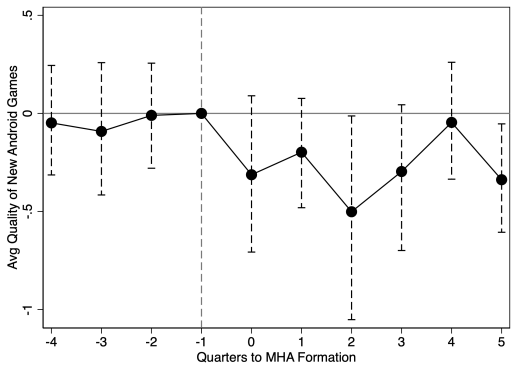
Quality Predictability. In Appendix D, we examine the predictability of app quality before release as an alternative explanation for the decline in high-quality entry, following the existing literature (Aguiar and Waldfogel, 2018; Janssen et al., 2022; Waldfogel, 2025). We find that app quality is not perfectly predictable: our set of predictors ex-

plains at most 13.3% of the variation in app quality. However, we further find that quality predictability significantly increases with predicted quality and is the highest in the high-quality bin, casting a puzzle that why, empirically, the high-quality bin experienced the strongest negative entry effect.

5.3 Platform-Specific Quality Effects

To examine how a higher Android game commission rate differentially affects the quality of new games on Android and iOS, we analyze three sets of dependent variables on each platform. First, the average quality of new apps on Android (iOS). Second, the number of high-, medium-, and low-quality new apps on Android (iOS). Third, the share of high-, medium-, and low-quality new apps joining Android (iOS). We take these dependent variables into Equations (1) and (2) to estimate the effect of the commission rate increase. As there are 6 joint categories without iOS apps, we focus on the 18 joint categories with both Android and iOS apps to compare their qualities.

(a) Avg. Quality of Android New Games



(b) Avg. Quality of iOS New Games

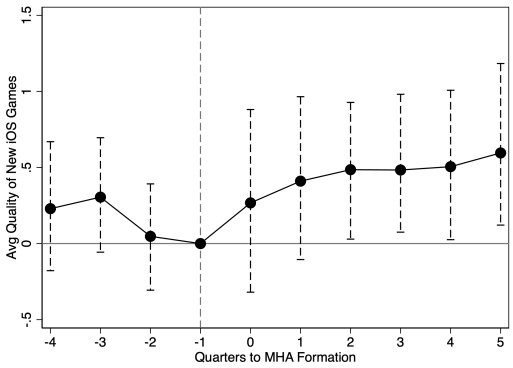


Figure 6: The Quality Effects of a Higher Android Game Commission Rate on Android and iOS

Notes. Standard errors are clustered at the category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

Figure 6 shows that before the commission increase, the average quality of new apps was similar across the game and non-game markets on Android and iOS separately. After the Android game commission rise, the average quality of new games declines on Android

and slightly increases on iOS. Consistently, Column (1) of Table 7 shows that the average quality of new games on Android significantly declined by 0.24 stars from a baseline of 3.89 stars after the Android game commission rise, while the average quality of new games on iOS insignificantly increased. Therefore, the quality effect favors iOS in the competition to attract mobile gamers.[24]

Furthermore, Columns (2) - (6) of Table 7 break down the average quality effects into the quality-bin-specific overall profitability effects and platform choice effects.[25] We find that, on Android, the quality decline is driven by significantly fewer high-quality new games on Android, which is not only due to the industry-wide decline in high-quality entry shown in Table 5, but also exacerbated by a significantly smaller share of high-quality new games choosing to join Android. On iOS, the platform choice effect fully offsets the overall profitability effect in the high-quality bin, leading to an insignificant effect on the number of high-quality iOS new games. In contrast, iOS mostly benefit from fewer low-quality new games, resulting from the market-wide entry decline in low-quality bin.

Appendix Table E.9 examines the quality distribution effects on Android and iOS separately and find consistent results. For example, on Android, the share of high-quality new games significantly decreased by 11%, explaining the decline in average quality. On iOS, we did not find significant changes in the quality distribution, consistent with the insignificant changes in the average quality.

Table 7 also shows that the platform choice effect is most pronounced in the medium- and high-quality bins. It likely reflects that higher quality apps lose more post-commission revenues when the ad-valorem fee increases. It may also reflect that Android users can bypass official app stores by downloading applications directly through mobile browsers. Thus, the opportunity cost of not joining any Android app store may be smaller for medium- and high-quality apps, enabling them to join only iOS when the Android commission rate increases. Consistent with this, Appendix Table E.10 shows that the share of iOS-only new games significantly increased in the medium- and high-quality bins, while the share of Android-only new games significantly decreased in these two bins.

[24] In Appendix Figure F.9, we analyze the full samples of Android markets and iOS markets separately, replacing the joint categorization with platform-specific categorizations. We find a significantly higher average quality for new iOS games.

[25] Appendix Figures F.10 and F.11 present the event study of the dependent variables in Columns (2) - (6) of Table 7. We find no significant difference across game and non-game markets on iOS or Android before the Android game commission rise.

Table 7: The Effects of a Higher Android Game Commission Rate on the Quality of New Games on Android and iOS

	(1) Avg Quality	(2)	(3) Num. New Apps	(4)	(5)	(6) %Joining the Platform	(7)
		Low	Medium	High	Low	Medium	High
	Panel A. Android						
Game × Post	-0.24***	-0.56**	-0.56**	-1.00***	-0.02	-0.21***	-0.29**
bootstrap p-value	(0.002)	(0.013)	(0.016)	(0.001)	(0.797)	(0.005)	(0.028)
95% CI	[-0.43, -0.08]	[-0.87, -0.17]	[-1.00, -0.16]	[-1.66, -0.57]	[-0.21, 0.15]	[-0.35, -0.07]	[-0.54, -0.04]
Observations	180	180	180	180	180	180	180
Adj. R-sq.	0.52	-	-	-	0.60	0.34	0.36
Pseudo R-sq.	-	0.61	0.63	0.70	-	-	-
Base Mean	3.89	10.93	9.86	7.64	0.81	0.70	0.63
	Panel B. iOS						
Game × Post	0.31	-0.64**	-0.01	0.02	-0.03	0.15**	0.25**
bootstrap p-value	(0.128)	(0.013)	(0.941)	(0.850)	(0.690)	(0.016)	(0.041)
95% CI	[-0.08, 0.75]	[-1.26, -0.23]	[-0.32, 0.51]	[-0.35, 0.24]	[-0.20, 0.15]	[0.03, 0.28]	[0.01, 0.49]
Observations	180	180	180	180	180	180	180
Adjusted R-squared	0.36	-	-	-	0.65	0.39	0.34
Pseudo R-squared	-	0.67	0.56	0.53	-	-	-
Base Mean	4.06	2.61	3.68	4.46	0.21	0.31	0.38

Notes. The table reports the quality effect of a higher Android commission rate on Android and iOS, separately in Panel (A) for Android and Panel (B) for iOS. Column (1) shows the average quality effect. Columns (2) - (4) show the underlying heterogeneous entry effects across high-, medium-, and low-quality bins, which are further explained by the heterogenous platform choice effects reported in Columns (5) - (7). For example, the industry-wide decline in low-quality entry, as reported in Table 5, and the significantly lower likelihood for low-quality new games choosing to join Android, as reported in Column (7) Panel (A), are consistent with the decline in low-quality entry on Android. We use the wild cluster bootstrap-t method at category level to conduct inference. Bootstrap p-values in parentheses. 95% confidence intervals in brackets. *** p<0.01, ** p<0.05, * p<0.1. All regressions include category-FE and quarter-FE.

These findings also shed light on how potential indirect consumer-side responses could affect our estimates. In particular, apart from the direct supply-side changes, our estimates may additionally contain be a feedback loop effect where changes in game entry influence mobile gamers' device choices, which in turn further impact game entry. Given that the net platform-specific quality effects favor iOS in the competition for mobile gamers, our estimated negative spillover effect on iOS entry likely represents a lower bound of the direct spillover effect on iOS.

5.4 Alternative Quality Measures

As a robustness check, Table 8 shows that our market-wide and platform-specific quality effects are robust to alternative quality measures based on downloads and security indexes. As explained in the data section, given the availability of downloads, we use incremental downloads in 2019 Q4 across Android and iOS stores as an alternative quality measure. For Android apps, we additionally observe the security score and the number of security loopholes. Similar to the results with average ratings, in Table 8, we find a market-wide decline in the average quality of new games and a platform-specific quality decline on Android but not on iOS. In particular, the downloads of new games released after the MHA's formation are significantly lower than those of new games released before the MHA's formation, by 40-44%, compared to the new apps in the non-game markets. For app security, new games released after the MHA's formation have 2.5 more security loopholes and 7% lower security scores than those released before the MHA's formation, compared to the new apps in the non-game markets. Appendix Figure F.12 displays the estimated $\{\beta_\tau\}$ from Equation (2) and shows no significant difference between new apps in the game and non-game markets before the MHA's formation in each of these quality measures.

Appendix Table E.7 augments Table 8 by showing market-wide quality distribution effects based on downloads. It shows that, using downloads as the quality measure, there is a significant entry decline in high-quality bin and a leftward shift in the quality distribution. For example, the share of high-quality new games significantly decreased by 9%, similar to those found with rating-based quality.[26]

[26]Theoretically, downloads may decrease or increase with higher commission rates, given the negative entry and quality effects. Specifically, fewer competitors may lead to more downloads of games released after the commission rise, while lower quality can result in fewer downloads. Thus, taking downloads as a quality measure when there is reduced entry provides a lower-bound estimate of the quality effect.

Table 8: Quality Effect Analysis with Alternative Quality Measures

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Downloads in 2019 Q4)				Security of the 1st Version	
	Joint Markets		Android	iOS	Security Score	Num. of Loopholes
Game × Post	-0.58***	-0.51**	-0.62**	-0.45	-4.06**	2.46**
bootstrap p-value	(0.003)	(0.027)	(0.038)	(0.108)	(0.016)	(0.020)
95% CI	[-0.97, -0.18]	[-0.99, -0.08]	[-1.24, -0.03]	[-1.02, 0.11]	[-7.40, -0.80]	[0.30, 4.69]
Observations	240	180	180	180	180	180
Adj. R-sq.	0.58	0.68	0.65	0.65	0.54	0.47
Base Mean	.48	.27	0.54	.18	56	20.45

Notes. The table reports the average quality effects of a higher Android commission rate, using downloads and app security as alternative quality measures. Columns (1) - (4) use the log downloads in 2019 Q4 as the quality measure. Column (5) uses the security score (1-100) as the quality measure. Column (6) uses the number of security loopholes as the quality measure. The security indexes are only available for Android apps. We first examine the industry-wide quality effects using downloads in Columns (1) - (2). Column (1) examines all joint markets, while Column (2) excludes those without iOS apps. Column (3) examines Android markets. Column (4) examines iOS markets. Overall, we find similar results relative to those using average ratings as the quality measure: an industry-wide decline in the average quality of new games, and a platform-specific quality decline on Android but not on iOS. We use the wild cluster bootstrap-t method at category level to conduct inference. Bootstrap p-values in parentheses. 95% confidence intervals in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table E.11 augments Table 8 by showing iOS-specific quality distribution effects based on downloads, which are similar to the results with average ratings. Together with the entry spillover effect and the rating-based quality effect, we find that the entry spillover effect is stronger among the low-quality (based on ratings) or the medium-quality (based on downloads) bins; however, the difference is not large enough to generate a significant change in the quality distribution. Therefore, the entry spillover effect on iOS is roughly evenly spread across quality bins.

Appendix Table E.12 augments Table 8 by showing Android-specific quality effects, using security indexes as the quality measures. It shows that there are significantly fewer new high-quality games on Android. The share of high-quality new games on Android significantly decreased by 16% and 19%, measured by security score and the number of security loopholes, respectively. These magnitudes are similar to those found with rating-based quality. Downloads-based quality measures show similar results on Android-specific quality effects, which we omit for exposition.

6 An Alternative Sample

In this section, we use an alternative sample on iOS to address the potential selection bias in the estimates from the main sample. The alternative iOS Sample is economically-relevant, capturing 99% all-time worldwide revenue and 95% all-time worldwide downloads generated by a 25% random sample of iOS apps released in China from February 2014 to January 2016. Using this sample, we find that there is no significant selection bias in our estimates from the main sample.

We obtain a 25% random sample of all iOS apps released in China from February 2014 to January 2016 from Sensor Tower.[27] We observe their release dates in China, category, along with a snapshot of their app performance cumulated to the data extraction date (August 31st, 2026) regardless of active status, among which we focus on global rating counts, all-time worldwide estimated downloads and revenues.

Many apps are developed as hobby projects and experimental products that are not actively engaging users. To isolate from such noise, we apply two steps to construct an economically-relevant sample that covers 99% total all-time worldwide revenues and

[27] That is 258,733 apps. Sensor Tower's current data extraction interface covers China's apps released since January 2014. For quarterly analysis around August 2014, we drop apps released in January 2014.

95% total all-time worldwide downloads across all apps in the 25% random sample.[28] In the first step, we select apps with positive global rating count, requiring the apps to have received at least one rating from any country it has been available during any time from January 2014 to August 2026.[29] In the second step, among these positive-rating-count apps, we focus on the those whose all-time worldwide downloads fall in the category-specific top 33rd percentile. Surprisingly or not, such an economically-sample contains 10% apps in the random sample, reflecting the power low in the app industry. At the end, the sample includes 27,271 iOS apps released in China from February 2014 to January 2016. These facts are summarized in Table 9.

Table 9: Apps in the 25% Random Sample: Economic Relevance

	(1)	(2)	(3)	(4)
Variable	All-Time Worldwide Downloads (Million)		All-Time Worldwide Revenue (Million $)	
In Subsample or Not	Out	In	Out	In
Num. of Apps	238,535	27,271	238,535	27,271
Mean	0.003	0.558	0.001	1.304
SD	0.155	7.858	0.215	90.299
Sum	803.165	15227.770	269.453	35562.950
Coverage	0.050	0.950	0.008	0.992

Notes. The table compares the apps in and out of the economically-relevant subsample in the 25% random sample of all iOS apps released in China from February 2014 to January 2016. Columns (1)-(2) examine all-time worldwide downloads from app release up to August 31st, 2026. Columns (3)-(4) examine revenues in the same regime. The table shows that our economically-relevant subsample covers 95% all-time worldwide downloads and 99% all-time worldwide revenue among the apps released during the sample period.

We estimate the entry and quality effects using this representative economically-relevant sample of iOS apps.[30] Tables 10 and 11 present the results for entry and quality,

[28] Using the full 25% random sample, we did not find significant effects on entry or quality on iOS.

[29] Rating count is more reliable than downloads or revenue, particularly for small values, because downloads and revenues are estimated based on top charts.

[30] To formally compare estimates from the two samples, we apply the same category adjustment two the main and the alternative samples, leaving 35 categories. Multiplied with 8 quarters (Feb 2014 - Jan 2016), we obtain 280 markets. Appendix Table E.13 documents the robust results with original categorization except adjustment for two extremely small categories.

where we always control for the differential effect of 4G-rollout on games. Appendix Figure F.13 and F.14 present the event studies.

Table 10: Alternative iOS Sample: Entry Effects

	(1)	(2)	(3)
Variables	ln(Num. New Apps)		ln(Selection Ratio)
Sample	Main	Alter	
Game × Post	-0.26**	-0.23***	-0.03
bootstrap p-value	(0.043)	(0.001)	(0.827)
95% CI	[-0.52,-0.01]	[-0.35,-0.12]	[-0.28, 0.24]
Game × Num. 4G Stations	-0.88	0.16	-1.04
	(0.73)	(0.24)	(0.69)
Observations	280	280	280
Adj. R-squared	0.82	0.95	0.76
Base Mean	7.54	77.71	0.11

Notes. The alternative iOS Sample is economically-relevant, capturing 99% all-time worldwide revenue and 95% all-time worldwide downloads generated by a 25% random sample of iOS apps released in China from February 2014 to January 2016. All regressions include category-FE and quarter-FE. Bootstrap p-values are labeled and reported in parentheses. Classical clustered standard errors are reported in parentheses without labels. Standard errors are clustered at category level. The p-value and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

We find that the entry estimates between this alternative sample and our main sample are insignificantly different from each other. In particular, Column (2) shows a point estimate of -0.23, qualitatively and quantitatively similar to the point estimate of -0.26 reported in Column (1), and both are statistically significant. Column (3) exploits the fact that the number of new apps in the main sample is equal to the number of new apps in the alternative sample multiplied with a selection ratio, i.e., $\ln(N_{ct}^{main}) = \ln(N_{ct}^{alter}) + \ln(Selection_{ct})$. Then, if the difference in $\ln(Selection_{ct})$ between game and non-game apps significantly changed after the commission rise, our main results would be subject to a selection bias. Column (3) shows that although that the selection bias is amplifies our estimated negative entry effect, the amplification is not statistically significant, and by a

small magnitude of 0.03 out of 0.26.

Table 11: Alternative iOS Sample: Quality Effects

Variables	(1) Avg. Downloads of New Apps	(2) Avg. Downloads of New Apps
Game × Post	0.51**	0.40**
bootstrap p-value	(0.044)	(0.045)
95% CI	[0.02,1.03]	[.01,0.78]
Game × Num. 4G Stations	-0.14	-0.06
	(0.96)	(1.01)
ln(Num. New Apps)		-0.47
		(0.65)
Observations	280	280
Adj. R-sq.	0.20	0.21
Base Mean	0.50	0.50

Notes. "Downloads" means all-time worldwide from app release to August 31, 2026, regardless of active status. All regressions include category-FE and quarter-FE. Bootstrap p-values are labeled and reported in parentheses. Classical clustered standard errors are reported in parentheses without labels. Standard errors are clustered at category level. The p-value and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

Lacking average ratings in China Apple App Store, we use all-time worldwide downloads to measure app quality. Table 11 shows that the average quality of iOS games increased by 0.5 million downloads from a pre-shock base mean of 0.5 million after the Android game commission rise, strongly supporting our conclusion that the quality effects across platforms are in favor of iOS in attracting mobile game players. This download increase is large, likely reflecting an increase in the app quality or a demand redistribution with fewer products. Given that entry and quality can be joint decision, we parsimoniously account for the correlation between entry and average downloads by adding the number of new apps as a control variable in Column (2). The quality effect slightly attenuates to 0.4 million downloads, remaining statistically significant, and the entry-downloads correlation is negative, as expected, though statistically insignificant. Appendix Table E.14 decomposes the average quality effect into quality-bin-specific entry effects, showing that entry decline happens across all quality bins.

7 Conclusion

This paper studies how platform commission rates affect entry and quality in digital markets. Exploiting a large increase in Android game commission rates in China, we show that higher platform fees substantially reduce entry on the platform that raises its fee and also generate a negative spillover on the entry on a competing platform. This spillover is driven by a decline in multi-homing, reflecting the importance of multi-homing for app profitability. We further document a significant decline in the quality of new products, driven by a sharp reduction in high-quality entry, reflecting that higher commission rates discourage quality provision. Together, these findings show that higher platform fees can distort both entry and innovation, with effects that extend beyond the focal platform.

From a policy perspective, our results suggest that platform competition alone may be insufficient to discipline commission rates. When platforms are complements from the perspective of developers, higher fees on one platform can reduce participation and innovation across platforms. As a result, the welfare consequences of platform fees extend beyond consumer prices or usage on a single platform and include reduced variety and lower-quality innovation. These findings highlight the importance of accounting for developer-side incentives and cross-platform interactions when designing and evaluating platform regulation in digital markets.

Finally, due to a lack of data on the download price, in-app purchase prices, and app business models during 2013-2016, we cannot analyze the effects of commission rates on these margins.[31] A systematic analysis of changes in porting, app prices, and business models in response to platform fees remains an interesting direction for future research.

References

Aguiar, Luis, and Joel Waldfogel. 2018. "Quality predictability and the welfare benefits from new products: Evidence from the digitization of recorded music." *Journal of Political Economy*, 126(2): 492–524.

[31] However, note that Android apps were free to download in China during 2013-2016. Thus, there was no pass-through to download price on Android. Obtaining prices on in-app purchases and subscriptions is generally hard because of many different plans provided by a given app.

Anderson, Simon, and Özlem Bedre Defolie. 2024. "Hybrid platform model: Monopolistic competition and a dominant firm." *The RAND journal of economics*, 55(4): 684–718.

Anderson, Simon, and Ozlem Bedre-Defolie. 2025. "Two-Sided Market Pricing with Heterogeneous Users: Theory and Application." CEPR Discussion Paper DP20700.

Armstrong, Mark. 2006. "Competition in two-sided markets." *The RAND journal of economics*, 37(3): 668–691.

Caillaud, Bernard, and Bruno Jullien. 2003. "Chicken & egg: Competition among intermediation service providers." *RAND journal of Economics*, 309–328.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The review of economics and statistics*, 90(3): 414–427.

Chen, Jiafeng, and Jonathan Roth. 2024. "Logs with zeros? Some problems and solutions." *The Quarterly Journal of Economics*, 139(2): 891–936.

Conti, Annamaria, and Juan Santaló. 2023. "The Hidden Costs of Fairness in Platform Markets." *Available at SSRN 4403845*.

Einav, Liran, Dan Knoepfle, Jonathan Levin, and Neel Sundaresan. 2014. "Sales taxes and internet commerce." *American Economic Review*, 104(1): 1–26.

Ershov, Daniel. 2024. "Variety-based congestion in online markets: Evidence from mobile apps." *American Economic Journal: Microeconomics*, 16(2): 180–203.

Etro, Federico. 2023. "Platform competition with free entry of sellers." *International Journal of Industrial Organization*, 89: 102903.

Farronato, Chiara, and Andrey Fradkin. 2022. "The welfare effects of peer entry: the case of Airbnb and the accommodation industry." *American Economic Review*, 112(6): 1782–1817.

Janssen, Rebecca, Reinhold Kesler, Michael E Kummer, and Joel Waldfogel. 2022. "GDPR and the lost generation of innovative apps." National Bureau of Economic Research.

Jeon, Doh-Shin, and Patrick Rey. 2025. "Platform Competition and App Development." *Working Paper*.

Jullien, Bruno, Alessandro Pavan, and Marc Rysman. 2021. "Two-sided markets, pricing, and network effects." In *Handbook of industrial organization*. Vol. 4, 485–592. Elsevier.

Kaiser, Ulrich, and Julian Wright. 2006. "Price structure in two-sided markets: Evidence from the magazine industry." *International Journal of Industrial Organization*, 24(1): 1–28.

Karle, Heiko, Martin Peitz, and Markus Reisinger. 2020. "Segmentation versus agglomeration: Competition between platforms with competitive sellers." *Journal of Political Economy*, 128(6): 2329–2374.

Lam, H Tai. 2023. "Platform Search Design and Market Power." *Working Paper*.

Lee, Kwok Hao, and Leon Musolff. 2023. "Entry Into Two-Sided Markets Shaped By Platform-Guided Search." *Working Paper*.

Lee, Robin S. 2013. "Vertical integration and exclusivity in platform and two-sided markets." *American Economic Review*, 103(7): 2960–3000.

Li, Ding, and Hsin-Tien Tsai. 2026. "Mobile apps and targeted advertising: Competitive effects of data sharing." *Journal of the European Economic Association*, jvaf025.

Llanes, Gastón, and Leonardo Madio. 2025. "App Store Competition." *Available at SSRN*.

Rochet, Jean-Charles, and Jean Tirole. 2003. "Platform competition in two-sided markets." *Journal of the European Economic Association*, 1(4): 990–1029.

Ronayne, David. 2021. "Price comparison websites." *International Economic Review*, 62(3): 1081–1110.

Ronayne, David, and Greg Taylor. 2022. "Competing sales channels with captive consumers." *The Economic Journal*, 132(642): 741–766.

Roodman, David, Morten Ørregaard Nielsen, James G MacKinnon, and Matthew D. Webb. 2019. "Fast and wild: Bootstrap inference in Stata using boottest." *The Stata Journal*, 19(1): 4–60.

Rysman, Marc. 2004. "Competition between networks: A study of the market for yellow pages." *The Review of Economic Studies*, 71(2): 483–512.

Singh, Amandeep, Kartik Hosanagar, and Aviv Nevo. 2021*a*. "Network Externalities

and App Store Fees in Mobile Platforms." *Available at SSRN 3911638*.

Singh, Amandeep, Kartik Hosanagar, and Aviv Nevo. 2021*b*. "Network Externalities and Cross-Platform App Development in Mobile Platforms." *Working Paper*.

Song, Minjae. 2021. "Estimating platform market power in two-sided markets with an application to magazine advertising." *American Economic Journal: Microeconomics*, 13(2): 35–67.

Sullivan, Michael. 2023. "Price Controls in a Multi-Sided Market." *Working Paper*.

Sutton, John. 2007. "Market structure: theory and evidence." *Handbook of Industrial Organization*, 3: 2301–2368.

Tan, Guofu, and Junjie Zhou. 2021. "The effects of competition and entry in multi-sided markets." *The Review of Economic Studies*, 88(2): 1002–1030.

Teh, Tat-How, and Julian Wright. 2024. "Competitive bottlenecks and platform spillovers." *Available at SSRN 4741349*.

Teh, Tat-How, Chunchun Liu, Julian Wright, and Junjie Zhou. 2023. "Multihoming and oligopolistic platform competition." *American Economic Journal: Microeconomics*, 15(4): 68–113.

Teng, Xuan. 2025. "Self-Preferencing, Quality Competition, and Welfare in Mobile Application Markets." CESifo Working Paper.

Waldfogel, Joel. 2025. "The Welfare Effects of Gender-Inclusive Intellectual Property Creation: Evidence from Books." *Journal of Political Economy*, 133(7): 000–000.

Wang, Chengsi, and Julian Wright. 2025. "Regulating platform fees." *Journal of the European Economic Association*, 23(2): 746–783.

Wang, Zhu, and Julian Wright. 2017. "Ad valorem platform fees, indirect taxes, and efficient price discrimination." *The RAND Journal of Economics*, 48(2): 467–484.

Wang, Zhu, and Julian Wright. 2018. "Should platforms be allowed to charge ad valorem fees?" *The Journal of Industrial Economics*, 66(3): 739–760.

Weyl, E Glen. 2010. "A price theory of multi-sided platforms." *American economic review*, 100(4): 1642–1672.

Appendix A A Quality Choice Model

We develop a simple two-stage quality choice model to explain our empirical findings on the quality effect of a higher commission rate. The model carries the following insights: a higher commission rate screens out developers that are inefficient at producing good apps; meanwhile, it discourages remaining developers from providing high-quality apps.

In the first stage, single-app monopolist developers incur an entry cost C, and every potential entrant i is endowed with a quality provision technology $F_i(\cdot)$ that governs their cost of quality provision. In the second stage, each entered developer i chooses quality level $u_i \in [1,\infty)$ to maximize their variable profits. The net profit of developer i is the post-commission-rate revenue subtracting from the quality provision cost and the entry cost:

$$\pi_i = (1-r)R(u_i) - F_i(u_i) - C \tag{A.1}$$

where r denotes the commission rate and $R(u_i)$ denotes expected revenue as a function of quality. For regularity, we assume that $C > 0$ and that $R(u) > 0$, $R'(u) > 0$, $F_i(u) > 0$, and $F_i'(u) > 0$ for all $u \geq 1$ and all i. We also assume that each developer's second-stage problem has a unique interior solution satisfying the strict second-order condition.

To facilitate analysis, we specify that $F_i(u_i) = F_{0i}F(u)$, adapted from the quality choice model in Sutton (2007). Then, the heterogeneity in quality provision technology is fully characterized by a scalar F_{0i}. Moreover, F_{0i} carries a simple interpretation: the cost to provide the minimum quality 1. Following the above regularity conditions, we assume that $F_{0i} > 0, \forall i$ and $F(u) > 0, \forall u \geq 1$.

Solving the game from the second stage, the following first-order condition defines the optimal quality choice for each entered developer i, denoted as $u_i^*(r, F_{0i})$.

$$(1-r)R'(u_i^*(r,F_{0i})) - F_{0i}F'(u_i^*(r,F_{0i})) = 0 \tag{A.2}$$

which defines the following optimal profit that a potential entrant i could obtain, denoted as $\pi(r, F_{0i}, C)$.

$$\pi(r,F_{0i},C) \equiv (1-r)R(u_i^*(r,F_{0i})) - F_{0i}F(u_i^*(r,F_{0i})) - C \tag{A.3}$$

In the first stage, developers enter if and only if $\pi(r, F_{0i}, C) \geq 0$. Applying the Envelope Theorem, we observe that $\frac{\partial \pi(r,F_{0i},C)}{\partial F_{0i}} = -F(u_i^*(r,F_{0i})) < 0$. We assume that the

support of F_{0i} contains a unique break-even type $\overline{F_{0i}}$ such that $\pi(r,\overline{F_{0i}},C)=0$. Monotonicity then implies that $\pi(r,F_{0i},C)\geq 0$ for all $F_{0i}\leq\overline{F_{0i}}$, so precisely these potential entrants enter the market. Note that $\overline{F_{0i}}$ is a function of (r,C). Thus, we can denote the optimal quality of the marginal entrant as $L(r,C)\equiv u_i^*(r,\overline{F_{0i}})$. Then, the following lemma establishes that all entrants have higher quality than $L(r,C)$.

Lemma A.1. $L(r,C)\leq u_i^*(r,F_{0i}),\forall F_{0i}\leq\overline{F_{0i}}$.

Proof. Since $F_{0i}\leq\overline{F_{0i}}$, all we need to show is that optimal quality decreases with F_{0i}. In other words, $\frac{\partial u_i^*(r,F_{0i})}{\partial F_{0i}}<0$. To show this, we apply the Implicit Function Theorem to Equation (A.2) to obtain

$$\frac{\partial u_i^*(r,F_{0i})}{\partial F_{0i}}=\frac{F'(u_i^*(r,F_{0i}))}{(1-r)R''(u_i^*(r,F_{0i}))-F_{0i}F''(u_i^*(r,F_{0i}))}<0.$$

The inequality follows because the numerator is positive and the denominator is the second-order derivative of the profit function evaluated at the optimal quality, and thus negative. QED

Then, the following proposition establishes that, a higher commission rate can lead to both fewer low-quality apps and fewer high-quality apps.

Proposition A.1. *A higher commission rate r causes:*

- *A lower optimal quality for each entrant:* $\frac{\partial u_i^*(r,F_{0i})}{\partial r}<0,\forall F_{0i}\leq\overline{F_{0i}}$.
- *A higher quality threshold to enter:* $\frac{\partial L(r,C)}{\partial r}>0$.

Proof. To prove the first result, applying the Implicit Function Theorem to the first-order condition in Equation (A.2), we obtain

$$\frac{\partial u_i^*(r,F_{0i})}{\partial r}=\frac{R'(u_i^*(r,F_{0i}))}{(1-r)R''(u_i^*(r,F_{0i}))-F_{0i}F''(u_i^*(r,F_{0i}))}<0$$

where the inequality follows from observing that the denominator is the second-order derivative of the profit function evaluated at the optimal quality and thus negative.

To prove the second result, we derive $\frac{\partial L(r,C)}{\partial r}$ from its definition $L(r,C)=u_i^*(r,T(r,C))$, where $T(r,C)$ denotes the threshold technology $\overline{F_{0i}}$. In particular,

$$\frac{\partial L(r,C)}{\partial r}=\frac{\partial u_i^*(r,\overline{F_{0i}})}{\partial r}+\frac{\partial u_i^*(r,\overline{F_{0i}})}{\partial F_{0i}}\cdot\frac{\partial T(r,C)}{\partial r} \tag{A.4}$$

Only $\frac{\partial T(r,C)}{\partial r}$ is new. To derive it, we apply the Implicit Function Theorem to the definition equation of $\overline{F_{0i}}$: $\pi(r,\overline{F_{0i}},C)=0$. Thus,

$$\frac{\partial T(r,C)}{\partial r}=-\frac{\partial \pi(r,T(r,C),C)/\partial r}{\partial \pi(r,T(r,C),C)/\partial F_{0i}}$$

Then, applying the Envelope Theorem to the value function defined in Equation (A.3), we obtain

$$\frac{\partial \pi(r,T(r,C),C)}{\partial r}=-R(L(r,C)),\quad \frac{\partial \pi(r,T(r,C),C)}{\partial F_{0i}}=-F(L(r,C)),\quad \frac{\partial T(r,C)}{\partial r}=-\frac{R(L(r,C))}{F(L(r,C))}.$$

Collecting all the results, we have

$$\begin{aligned}
\frac{\partial L(r,C)}{\partial r} &= \frac{R'(u_i^*(r,\overline{F_{0i}}))}{(1-r)R''(u_i^*(r,\overline{F_{0i}}))-\overline{F_{0i}}F''(u_i^*(r,\overline{F_{0i}}))} \\
&\quad + \frac{F'(u_i^*(r,\overline{F_{0i}}))}{(1-r)R''(u_i^*(r,\overline{F_{0i}}))-\overline{F_{0i}}F''(u_i^*(r,\overline{F_{0i}}))}\left(-\frac{R(L(r,C))}{F(L(r,C))}\right) \\
&= \frac{R'(u_i^*(r,\overline{F_{0i}}))}{(1-r)R''(u_i^*(r,\overline{F_{0i}}))-\overline{F_{0i}}F''(u_i^*(r,\overline{F_{0i}}))} \\
&\quad \cdot\left(1-\frac{F'(u_i^*(r,\overline{F_{0i}}))}{R'(u_i^*(r,\overline{F_{0i}}))}\frac{R(L(r,C))}{F(L(r,C))}\right) \\
&= \frac{R'(L(r,C))}{(1-r)R''(L(r,C))-T(r,C)F''(L(r,C))}\left(1-\frac{F'(L(r,C))}{R'(L(r,C))}\frac{R(L(r,C))}{F(L(r,C))}\right) \\
&= \frac{R'(L(r,C))}{(1-r)R''(L(r,C))-T(r,C)F''(L(r,C))}\left(1-\frac{1-r}{T(r,C)}\frac{R(L(r,C))}{F(L(r,C))}\right) \\
&= \frac{R'(L(r,C))}{(1-r)R''(L(r,C))-T(r,C)F''(L(r,C))}\left(-\frac{C}{T(r,C)F(L(r,C))}\right)>0.
\end{aligned}$$

where the second equality follows from rearranging terms, the third equality follows from the definition $L(r,C)\equiv u_i^*(r,\overline{F_{0i}})$ and $T(r,C)=\overline{F_{0i}}$, the fourth equality follows from evaluating the first-order condition in Equation (A.2) at $L(r,C)$ to obtain $F'(L(r,C))/R'(L(r,C))=(1-r)/T(r,C)$, and the last equality follows from evaluating the zero-profit condition in Equation (A.3) at $F_{0i}=T(r,C)$. The final inequality uses $C>0$, $T(r,C)>0$, $F(L(r,C))>0$, and the negative second-order condition. QED

Intuitively, the first result follows from the feature that a higher commission rate flattens the revenue curve of quality provision. The second result follows from the feature

that a higher commission rate screens out the developers with the least efficient quality provision technology and thus the low-quality apps.

Appendix B Details on Data

B.1 Category Adjustment and Joint Categories

In the original data, there are 25 Android categories and 41 iOS categories. We drop and merge categories with extremely few entrants to obtain 24 Android categories and 35 iOS categories. This is because, following the literature, we aim to quantify the entry effect in percentage whenever possible; on the other hand, logs with zeros cause issues when interpreting the results (Chen and Roth, 2024). Faced with the trade-off, we choose to adjust categorizations of small categories with fewer than 15 new apps during our event-study sample period.

On Android, we drop the "Utility Software" category, which only accounts for 0.09% of new Android apps in our event-study sample while the next small category accounts for 0.43% entry. This gives us 24 Android categories.

On iOS, we drop the "Other Games" category, which only accounts for 0.05% of new iOS apps in our event-study sample. We also merged the next five extremely small categories to their larger counterparts. Specifically, we merge "Developer Tools" (0.05% new apps) into "Utilities" (3.31% new apps), "Magazines and Newspapers" (0.08% new apps) into "News" (1.23% new apps), "Graphics and Design" (0.24% new apps) into "Photo and Video" (3.43% new apps), "Games - Casino" (0.16% new apps) into "Games - Strategy" (1.00% new apps), "Games - Word" (0.21% new apps) into "Games - Trivia" (0.66% new apps). At the end, we obtain 35 iOS categories.

Given the 24 Android categories and 35 iOS categories, we then construct joint categories to study the platform choice of apps in the same joint category. To that end, we map iOS categories to Android categories using multi-homing apps: for each iOS category, we identify the Android category with the largest overlap in app identities and then assign all apps in that iOS category to the matched Android category. Appendix Table E.2 documents the mapping.

B.2 Approximate Store Choices within Android

We cannot observe the app-store-level entry time for Android apps, because for each version of each Android app, the anonymous internet security company conducts inspections for a random subsample of stores where the version of the app is available.

To capture Android apps' choice between MHA and non-MHA Android app stores, we use approximations based on positive downloads. Specifically, we assume that an Android app joins MHA (non-MHA) stores in its quarter of release if the app has positive downloads on MHA (non-MHA) stores in our data. Because our download data only begins in 2017, this proxy involves a temporal lag between the observed downloads and the initial store choice. We address this potential measurement error by verifying the robustness of our results across different observation years between 2017 and 2022. The choice of years is dictated by the availability of downloads data.

Plausibly, the year closest to the quarter of release, which is 2017, provides the best approximation. That said, some Android apps may change their store choices between 2013 - 2017. By constructing the approximated store choice based on downloads in each of the years from 2017-2022, we can then examine how much store choices have changed during 2017-2022, and shed light on how much store choices would have changed during 2013 - 2017.

Overall, we did not find much difference in the estimated treatment effects using approximations based on different years between 2017 -2022, as shown in Plot (c) in Figure 1 and Appendix Figure F.6. Note that there was a nine-month suspension of new video game approvals by regulators from March to December 2018 in China, which plausibly caused larger effects than the commission increase. Thus, these results strengthens our confidence in the approximation-based results on the within-Android entry effects.

B.3 Missings in Quality Measures

In this section, we systematically examine missings in quality measures based on ratings, downloads, and security. When constructing each quality measure, we first construct a stable app-level quality from app-platform-level quality. If a multi-homing app lacks a quality measure on one platform, then its quality falls back to the platform where it has a quality measure. These dimensions generate five dependent variables given a quality measure: three missing rates of the app-level quality in the joint, Android, and iOS markets, and one missing rate of app-platform quality in the Android and iOS markets separately.

We find that, in all these five dimensions, for all the quality measures, the missing ratios are not significantly different across the game and non-game markets, before and after the commission rate increase.

Appendix Table E.3 reports these missing rates at app level. For example, for average ratings, 21% unique apps across Android and iOS, 19% Android apps, and 18% iOS apps lack the aggregated app-level average ratings. Meanwhile, 22% Android apps lack Android-specific average ratings, thus 3% Android apps fall back to their iOS ratings to obtain its app-level average rating. Finally, 32% iOS apps lack iOS-specific average ratings, thus 14% iOS apps fall back to their Android ratings to obtain its app-level average ratings. Note that 39% iOS apps multi-home on Android, and 19% Android apps multi-home on iOS in our sample of unique apps released during the sample period.

Appendix Table E.4 reports the estimation results, showing that the increase in Android game commission rates did not significantly affect the appearance of no-rating new apps, no-downloads new apps, or no-security new apps. This holds true for the unique apps, Android apps, and iOS apps, using aggregated measures across Android and iOS or platform-specific measures. The quality analyses in the main text focus on the aggregated app-level quality measures whenever possible. For the ease of exposition, we leave the event-study figures upon request.

B.4 Construction of Version Age

Given a quarter τ spanning from date $T0_\tau$ to date $T1_\tau$, we calculate the version age of app j as the average age of existing versions in the quarter, weighted by the days that the version exists. Specifically, we first order the versions of an app j by the ascending order of release time. Then, for each existing version k in the quarter, we calculate the days that the version exists in the quarter, denoted as $Days^{\tau}_{jk}$, and the age of the version in the quarter, denoted as Age^{τ}_{jk}, using the following formulas, respectively.

$$\begin{aligned} Days_{jk\tau} &= \min\{T1_\tau, ReleaseDate_{j,k+1}\} - \max\{T0_\tau, ReleaseDate_{j,k}\} + 1 \\ Age_{jk\tau} &= \min\{T1_\tau, ReleaseDate_{j,k+1}\} - ReleaseDate_{j,k} + 1 \end{aligned} \tag{B.1}$$

where $ReleaseDate_{jk}$ is the release date of the k-th version of app j. In the case that the version k is the last version, the minimum function returns $T1_\tau$. Then, the version age of

app j in quarter τ is given by

$$VersionAge_{j\tau} = \sum_{k:Days_{jk\tau}>0} \frac{Days_{jk\tau}}{T1_\tau - T0_\tau + 1} Age_{jk\tau} \tag{B.2}$$

We denote K_τ as the number of versions existing in the quarter, which is the cardinality of the set $\{k : Days_{jk\tau} > 0\}$. In the case that multiple versions are released on the same day, we treat these released versions as one version.

Appendix C Spillover Effect within Android Ecosystem

In this section, we focus on Android apps and stores. We examine whether there is a within-Android cross-store entry effect; in particular, a negative spillover effect on the number of new games on the non-MHA stores, analogous to the effect observed on iOS; and, if so, whether this effect is also driven by a decline in the multi-homing Android games that join both MHA and non-MHA stores. The answer to the first question is predictable. Since the descriptive statistics in Section 3 show that almost all Android apps join non-MHA stores, we expect an estimated negative entry effect on the non-MHA stores that is close in magnitude to the effect on Android (Col 1 in Table 2). The second question is more interesting, and we address it by decomposing the apps on the non-MHA stores into non-MHA-only apps and multi-homing apps (within Android). Appendix Figure F.6 plots the estimated coefficients $\{\beta_\tau\}$ in Equation (2). We find no evidence of differential pre-trends between game and non-game markets on the non-MHA stores prior to the MHA's formation.

Appendix Table E.5 reports the estimated effects on the entry into the non-MHA stores. As expected, Column (1) shows a negative within-Android spillover effect: the number of new games in the average game market on the non-MHA stores decreased significantly by 47% due to the increase in MHA's game commission rate, a magnitude similar to the decline observed for Android overall. Column (2) presents a result that differs from the Android-iOS story: the number of new non-MHA-only games increased significantly by 151%. Recall that the number of iOS-only games was insignificantly affected. This contrast reflects the higher substitutability among Android stores on the demand side. However, because most Android apps multi-home between MHA and non-MHA stores, the substantial decline in the number of new multi-homing games (by 49%,

shown in Col 3) fully offset the rise in new exclusive games, resulting in the overall negative spillover effects in Column (1).

Appendix D Predictability of App Quality

In this section, we examine an alternative explanation for the decline in high-quality entry from the existing literature (Aguiar and Waldfogel, 2018; Janssen et al., 2022; Waldfogel, 2025). Specifically, developers cannot perfectly predict the quality (or success) of their apps; when the commission rate increases, developers stop developing some ex-ante mediocre apps that are ex-post high-quality.[32] To that end, we follow Waldfogel (2025) and regress the app quality on a set of quality predictors known before release, including the developer's previous ratings, employee size upon business registration, and the app's team size before release.[33]

Columns (1) and (3) in Appendix Table E.8 show that app quality is indeed not perfectly predictable before release. In particular, our quality predictors can only explain 5.4% of the variation in app quality, which can only increase to 13.3% after including category-quarter fixed effects and developer fixed effects. Moreover, as expected, more employees and larger teams predict higher quality. Across developers, higher ratings of previously developed apps predict higher quality. Within developers, however, a higher-rated app is typically followed by a lower-rated one, consistent with mechanical regression to the mean.

However, the pattern of quality predictability across quality bins hardly reconciles with the pattern of entry effect. In particular, given that the negative entry effect is statistically significant in the high-quality bin but not in the medium-quality bin, if imperfect predictability drives the quality effect, then quality predictability should be lower in the high-quality bin than in the medium-quality bin. However, Columns (2) and (4) in Appendix Table E.8 show that the absolute value of residual quality, the opposite of quality predictability, decreases significantly with predicted quality, implying that quality pre-

[32] To be clear, the imperfect predictability of quality generates uncertainty in post-commission revenue, which can lead to fewer ex-post high-quality new products with a higher commission rate. Note that in this simple theory, we follow the literature to assume a constant entry cost across quality levels.

[33] We collect the employee size data from the Business Registry Database, which is maintained by the State Administration for Industry and Commerce (SAIC). We merge the database to our app data via developer names. We collect the app team size data via the internal portal of a major app developer in our sample.

dictability increases with predicted quality. Similarly, the last three rows in Columns (1) and (3) show that the standard deviation of residual quality is the largest in the low-quality bin, followed by the medium-quality bin, and the smallest in the high-quality bin. Appendix Figure F.8 plots the overall distribution of residual app quality, and its distribution by quality bins, showing a similar pattern. Therefore, while imperfect quality predictability exists in our empirical context, it may not fully explain the quality effect of commission rates.

Appendix E Additional Tables

Table E.1: Summary Statistics of Android Apps by Matching Status between Qimai Data and Anonymous Data

	Matched (1)	Unmatched (2)	p-value (3)
Game (0/1)	0.216	0.218	0.749
	(0.412)	(0.413)	
Average Ratings	3.088	3.072	0.440
	(1.592)	(1.547)	
File Size	40.374	41.413	0.318
	(88.765)	(110.281)	
Description Length	1232.141	1233.434	0.902
	(886.848)	(1159.913)	
MHA Store (0/1)	0.908	0.907	0.434
	(0.289)	(0.292)	
Non-MHA Store (0/1)	0.998	0.997	0.409
	(0.050)	(0.054)	
Obs.	39,833	10,167	

Notes. This table presents the means of observable characteristics and their differences by matching status. Game is a binary indicator for gaming apps. Average Ratings captures the mean user rating score for each app by the end of 2022. File Size measures the app installation package size in MB. Description Length quantifies the word count of the app's store listing description. MHA Store and Non-MHA Store are binary indicators representing app availability on MHA and non-MHA stores, respectively. Column (1) shows the means and standard deviations for the matched apps. Column (2) displays the means and standard deviations for the unmatched apps. For each characteristic, we test for differences across groups, with the corresponding p-values shown in Column (3).

Table E.2: Cross-Platform Category Mapping

Panel A: Game Categories	
Android Category	**iOS Categories**
Action and Shooting	Action, Adventure
Role Playing	Role Playing
Management and Simulation	Simulation, Strategy
Board and Card	Board, Card
Racing	Racing
Sports	Sports
Casual and Puzzle	Casual, Music, Puzzle, Trivia
Panel B: Non-Game Categories	
Android Category	**iOS Categories**
Office and Study	Business, Reference, Productivity, Education
Health and Sports	Health and Fitness, Medical
Finance	Finance
Travel	Travel Navigation
Photo	Photo and Video
Social Networking	Social Networking
Lifestyle	Weather, Food and Drink
Shopping	Shopping
Systematic Tools	Utilities
Music and Video	Music, Entertainment
News and Reading	News, Books

Notes. We map iOS categories to Android categories using multi-homing apps. For each iOS category, we identify the Android category with the largest overlap in app identities and then assign all apps in that iOS category to the matched Android category. There are six Android categories without multi-homing apps: Games-Simulation, Games-Online, Games-Other, Parenting, Wallpaper, and Other Apps.

Table E.3: Share (%) of Apps Missing Quality Measures

	(1)	(2)	(3)
	Rating	Downloads	Security
Unique Apps (Joint)	21	12	
Android Apps			
Aggregated	19	15	
Android	22	19	6
iOS Apps			
Aggregated	18	4	
iOS	32	16	

Notes. This table reports the share of apps missing quality measures in five cases: (1) among the unique apps across Android and iOS using the aggregated value across Android and iOS; (2) among the Android apps using the aggregated value across Android and iOS; (3) among the Android apps using the Android value; (4) among the iOS apps using the aggregated value across Android and iOS; (5) among the iOS apps using the iOS value. For example, among the unique apps across Android and iOS, 21% apps lack the aggregated average ratings across Android and iOS; among the Android apps, 19% apps lack the aggregated average ratings and 22% apps lack the Android average ratings, and . The gap reflects that 3% Android apps lack Android ratings but have iOS ratings.

Table E.4: Effect on Missing Ratios of Quality Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Markets and Apps	Joint		iOS		Android		
Measure Construction	Aggregated		Aggregated	iOS	Aggregated	Android	
Variables	Share of No-Rating New Apps						
Game × Post	-0.03	-0.02	-0.05	-0.06	-0.03	-0.03	
bootstrap p-value	(0.528)	(0.476)	(0.242)	(0.136)	(0.521)	(0.562)	
95% CI	[-0.11, 0.05]	[-0.09,0.04]	[-0.13,0.03]	[-0.14,0.02]	[-0.11,0.04]	[-0.11,0.04]	
Observations	240	180	180	180	180	180	
Adjusted R-squared	0.55	0.70	0.65	0.65	0.74	0.74	
Base Mean	.31	.31	.17	.17	.34	.34	
Variables	Share of No-Downloads New Apps						Share of No-Security
Game × Post	-0.06*	-0.03	0.02	0.01	0.04	0.03	0.02
bootstrap p-value	(0.073)	(0.198)	(0.246)	(0.301)	(0.113)	(0.208)	(0.162)
95% CI	[-0.13,0.01]	[-0.08,0.01]	[-0.01,0.04]	[-0.01,0.04]	[-0.01,0.09]	[-0.01,0.07]	[-0.01,0.05]
Observations	240	180	180	180	180	180	180
Adjusted R-squared	0.45	0.69	-0.04	-0.05	0.60	0.54	0.47
Base Mean	.27	.23	.01	.01	.29	.3	.15

Notes. This table shows that the occurrence of missing a given quality measure is not significantly different across the game and non-game markets, before and after the MHA formation. The first column considers all broad categories defined on Android. The other columns considers the categories with overlapping between Android and iOS. We consider two dimensions in which the market average quality can differ by construction: (i) the considered apps—-unique apps (joint markets), Android apps, and iOS apps; and (ii) whether the quality measure is platform-specific or aggregated across platforms. In the join markets, aggregated measure is natural. Thus, in total, there are five cases to consider given the categories. Finally, we consider all three quality measures used in the paper: the main quality measure based on cumulative average ratings, the alternative quality measures based on incremental downloads in 2019 Q4 and security indexes. The security indexes are only available for Android apps. We cluster standard errors at category level. Due to few clusters, we report the p-value (in parentheses) and 95% confidence interval (in brackets) from the wild cluster bootstrap-t method. Event-study figures are available upon request.

Table E.5: Within-Android Spillover Effects on the Non-MHA Android Stores

	(1)	(2)	(3)
	Num of New Android Apps on the Non-MHA Stores		
	in log	Exclusive	Multi-Homing
Game × Post	-0.64***	0.92***	-0.68***
bootstrap p-value	(0.006)	(0.002)	(0.004)
95% CI	[-1.05,-0.23]	[0.53,1.38]	[-1.08,-0.40]
Category FE	yes	yes	yes
Quarter FE	yes	yes	yes
Observations	240	240	240
Adj. R-sq.	0.89		
Pseudo R-sq.		0.67	0.85
Base Mean	32.00	9.08	22.93

Notes. The table reports the estimated coefficient β from Equation (1) using the sample of non-MHA Android markets. Column (1) uses linear regression. Columns (2) - (3) use Poisson Regression. In Columns (1) - (3), the dependent variables are the number of new apps, the number of new exclusive apps, the number of new multi-homing apps that also join the MHA stores, respectively. Standard errors are clustered at category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

Table E.6: The Market-Wide Effect of a Higher Android Game Commission Rate on Quality Distribution

	(1)	(2)	(3)
	Share of New Apps		
	High-Quality	Medium-Quality	Low-Quality
Game × Post	-0.12***	0.07***	0.08**
bootstrap p-value	(0.000)	(0.007)	(0.021)
95% CI	[-0.17, -0.06]	[0.02,0.11]	[0.01,0.16]
Category FE	yes	yes	yes
Quarter FE	yes	yes	yes
Observations	240	240	240
Adj. R-sq.	0.20	0.16	0.10
Base Mean	0.26	0.21	0.22

Notes. The table reports the estimated coefficient β from Equation (1), using the sample of joint markets. In Columns (1) - (3), the dependent variables are the share of high-, medium-, and low-quality new apps, respectively. We measure app quality with the cumulative average rating five years after release, in order to capture the stabilized lifetime quality. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1.

Table E.7: Market-Wide Entry Effects by Quality Bin based on Downloads

	(1)	(2)	(3)	(4)	(5)	(6)
		Num of New Apps			Share of New Apps	
Variables	Low	Medium	High	Low	Medium	High
			Panel (A) All Categories			
Game × Post	-0.16*	-0.45*	-0.54***	0.07*	0.02	-0.09***
bootstrap p-value	(0.090)	(0.053)	(0.001)	(0.059)	(0.622)	(0.007)
95% CI	[-0.41,0.04]	[-0.67,0.01]	[-0.87,-0.29]	[-0.00,0.14]	[-0.04,0.08]	[-0.14,-0.03]
Observations	240	240	240	240	240	240
Base Mean	11.2	11.03	9.85	0.37	0.30	0.33
Pseudo R-sq.	0.73	0.74	0.76			
Adjusted R-sq.				0.14	0.02	0.01
			Panel (B) Overlapping Categories			
Game × Post	-0.18*	-0.54**	-0.54***	0.07*	0.01	-0.08*
bootstrap p-value	(0.067)	(0.019)	(0.001)	(0.085)	(0.798)	(0.054)
95% CI	[-0.48,0.02]	[-0.76,-0.13]	[-0.91,-0.29]	[-0.01,0.15]	[-0.05,0.06]	[-0.15,0.00]
Observations	180	180	180	180	180	180
Base Mean	15.18	15.29	13.36	0.35	0.33	0.32
Pseudo R-sq.	0.66	0.69	0.72			
Adjusted R-sq.				0.21	-0.01	0.12

Notes. This table shows robustness of market-wide quality effects reported in Table 5 and Appendix Table E.6 with respect to an alternative quality measure based on incremental downloads in 2019 Q4. All regressions examine the joint markets. Panel (A) consider all joint categories, Panel (B) considers the 18 categories with both Android and iOS apps. Columns (1) - (3) examine the entry effects by quality bin. Columns (4) - (6) examine the effect on quality distribution. We find that entry declines not only in the low-quality bin but also particularly in the high-quality bin, similar to the findings in Table 5 and explaining the decline in average quality reported in Table 8. Consistently, the share of high-quality new games significantly decreased, similar to those estimated from rating-based quality measure as reported in Appendix Table E.6. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using the wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. All regressions include category-FE and quarter-FE.

Table E.8: Imperfect Quality Predictability and the Variation of Predictability across Quality Bins

	(1) Quality	(2) σ	(3) Quality	(4) σ
Log employee size upon registration	0.071***		0.085***	
	(0.005)		(0.007)	
Log team size before release	0.617***		0.542***	
	(0.167)		(0.196)	
Developer previous ratings	0.340***		-0.705***	
	(0.023)		(0.064)	
Missing employee size	0.219***		0.284***	
	(0.027)		(0.035)	
Missing team size	3.014***		2.176***	
	(0.672)		(0.788)	
Missing previous ratings	1.577***		-2.843***	
	(0.098)		(0.284)	
Predicted Quality		-0.242***		-0.243***
		(0.025)		(0.013)
Constant	-0.616	1.688***	4.516***	1.612***
	(0.677)	(0.103)	(0.835)	(0.053)
Category-Quarter FE	no		yes	
Multi-App Developer FE	no		yes	
Observations	11,197	11,197	10,302	10,302
Adj. R-sq.	0.054	0.009	0.133	0.037
SD of Resid. in:				
low-quality bin	1.056		0.996	
medium-quality bin	0.574		0.521	
high-quality bin	0.238		0.320	

Notes. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Columns (2) and (4) take the absolute value of residual quality ($\sigma_j = |\widehat{quality}_j - quality_j|$) from the regression in Columns (1) and (3) as the dependent variable, respectively. The sample includes all apps released during our event-study sample period. We include developer fixed effects for developers with multiple apps. Single-app developers are pooled into a common baseline group. Missing employee size is due to an imperfect match between the Business Registry Database and our app data. We collect the team size data via the internal portal of a major app developer in our sample, and therefore the team size for the other app developers' apps remains unobserved. Missing previous ratings are due to the app being the first (or only) app developed by the developer. The adjusted R-sqs in Columns (1) and (3) show that quality is imperfectly predictable. The standard deviations of residuals and Columns (2) and (4) show that the quality predictability increases with quality.

Table E.9: Effect on Quality Distribution on Android and iOS

	(1)	(2)	(3)
Dep. Var.	Share of New Apps in a Given Quality Bin		
Quality Bin	Low	Medium	High
	Panel (A). Android Markets		
Game × Post	0.08**	0.06**	-0.11***
bootstrap p-value	(0.019)	(0.019)	(0.009)
95% CI	[0.01, 0.14]	[0.01, 0.11]	[-0.20, -0.03]
Observations	180	180	180
Adj. R-sq.	0.09	0.37	0.46
Base Mean	0.25	0.21	0.2
	Panel (B). iOS Markets		
Game × Post	-0.09	0.08*	0.05
bootstrap p-value	(0.288)	(0.099)	(0.599)
95% CI	[-0.26, 0.08]	[-0.01, 0.21]	[-0.20, 0.28]
Observations	180	180	180
Adj. R-sq.	0.31	0.09	0.35
Base Mean	0.21	0.26	0.36

Notes. The table reports the estimated coefficient β from Equation (1). To compare the two platforms, we use the 18 joint categories that contain both Android and iOS apps. With 10 quarters, this gives us 180 markets. Columns (1) - (3) use the sample of Android markets in these joint categories. Columns (4) - (6) use the sample of iOS markets in these joint categories. In Columns (1) - (6), the dependent variables are the share of low-, medium-, and high-quality new apps on Android and iOS, respectively. Standard errors are clustered at category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using the wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. We have examined and found that the game and non-game markets do not present significantly different pre-trends in these dependent variables. The table shows that the quality distribution of new games on Android shifts left toward lower levels, while the quality distribution of new games on iOS does not change significantly. All regressions include category-FE and quarter-FE.

Table E.10: Effect on Android-Only and iOS-Only Ratios by Quality Bin

	(1)	(2)	(3)
Dep. Var.	Share of Exclusive New Apps in a Given Quality Bin		
Quality Bin	Low	Medium	High
	Panel (A) Android Markets		
Game × Post	0.03	-0.15**	-0.25**
bootstrap p-value	(0.690)	(0.016)	(0.041)
95% CI	[-0.15, 0.20]	[-0.28, -0.03]	[-0.49, -0.01]
Observations	180	180	180
Adj. R-sq.	0.65	0.39	0.34
Base Mean	0.79	0.69	0.62
	Panel (B) iOS Markets		
Game × Post	0.02	0.21***	0.29**
bootstrap p-value	(0.797)	(0.005)	(0.028)
95% CI	[-0.15, 0.21]	[0.07, 0.35]	[0.04, 0.54]
Observations	180	180	180
Adj. R-sq.	0.60	0.34	0.36
Base Mean	0.19	0.30	0.37

Notes. The table reports the estimated coefficient β from Equation (1), using the sample of joint markets that contain both Android and iOS apps. In Columns (1) - (6), the dependent variables are the share of Android-Only and iOS-Only new apps in the low-, medium-, and high-quality bins, respectively. We have examined and found that the game and non-game markets do not present significantly different pre-trends in these dependent variables. The table shows that the share of iOS-Only new games increased in high- and medium-quality bins, while the share of Android-Only new games decreased in these quality bins. These effects drive the platform choice effects by quality bin in Table 7. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using the wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. All regressions include category-FE and quarter-FE.

Table E.11: iOS-Specific Quality Effects based on Downloads

	(1)	(2)	(3)
Quality Measure	Incremental Downloads in 2019Q4		
Quality Bin	Low	Medium	High
	Panel(A) Num. of New Apps in a Given Bin on iOS		
Game × Post	-0.05	-0.32**	-0.31*
bootstrap p-value	(0.652)	(0.018)	(0.088)
95% CI	[-0.49,0.26]	[-0.50,-0.04]	[-0.94,0.07]
Observations	180	180	180
Base Mean	3.54	8.25	2.61
Pseudo R-squared	0.71	0.60	0.62
	Panel(B) Share of New Apps in a Given Bin on iOS		
Game × Post	0.04	0.02	-0.06
bootstrap p-value	(0.475)	(0.544)	(0.249)
95% CI	[-0.08,0.16]	[-0.06,0.12]	[-0.18,0.06]
Observations	180	180	180
Base Mean	.27	.51	.22
Adjusted R-squared	0.23	0.41	0.28

Notes. The table shows that the iOS-specific quality effects reported in Table 5 based on ratings are robust to an alternative quality measure based on downloads. The sample uses the same iOS markets as in Table 5. While the quality-bin specific entry effects differ a bit: the number of medium-quality new iOS games significantly declined here but not with rating-based measures; the quality distribution effect remains robust: there is no significant changes in the quality distribution across new games on iOS. This result also explains the null effect on the average quality of new iOS games based on downloads, which is reported in Table 8. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using the wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. All regressions include category-FE and quarter-FE.

Table E.12: Effect on Quality Distribution on Android: Quality Measured by Security

	(1)	(2)	(3)	(4)	(5)	(6)
Quality Measure		Security Score			Num of Security Loopholes	
Quality Bin	Low	Medium	High	Low	Medium	High
			Panel (A) Num. of New Apps			
Game × Post	-0.39*	-0.52**	-1.06***	-1.05***	-0.53**	-0.35
bootstrap p-value	(0.067)	(0.030)	(0.000)	(0.000)	(0.029)	(0.146)
95% CI	[-0.72,0.03]	[-0.88,-0.09]	[-1.58,-0.64]	[-1.51,-0.67]	[-0.90,-0.09]	[-0.69,0.14]
Observations	240	240	240	240	240	240
Base Mean	7.75	10.73	9.32	10.45	10.7	6.65
Pseudo R-sq.	0.722	0.672	0.730	0.722	0.693	0.714
			Panel (B) Share of New Apps			
Game × Post	0.13***	0.04	-0.16***	-0.19***	0.06	0.13***
bootstrap p-value	(0.006)	(0.314)	(0.000)	(0.001)	(0.138)	(0.003)
95% CI	[0.04,0.21]	[-0.03,0.11]	[-0.26,-0.06]	[-0.31,-0.07]	[-0.02,0.15]	[0.04,0.21]
Observations	240	240	240	240	240	240
Base Mean	.26	.38	.37	.43	.37	.2
Adjusted R-sq.	0.08	0.11	0.05	0.08	0.07	0.14

Notes. Quality increases with security score and decreases with the number of loopholes. Columns (1)-(3) report the results with security score, and Columns (4)-(6) report those with the number of security loopholes. Panel (A) studies the number of new apps, i.e., quality-specific entry. Panel (B) studies the share of new apps falling in each quality bin, i.e., quality distribution effect. Together, they show the robustness of Android-specific quality effects (reported in Table 5) with respect to alternative quality measures. They also explain the decline in average security of new Android games, reported in Table 8, by showing the leftward shift of quality distribution. Event study figures are available upon request. Standard errors are clustered at the category level. The p-value (in parentheses) and 95% CI (in brackets) are constructed using the wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. All regressions include category-FE and quarter-FE.

Appendix F Additional Figures

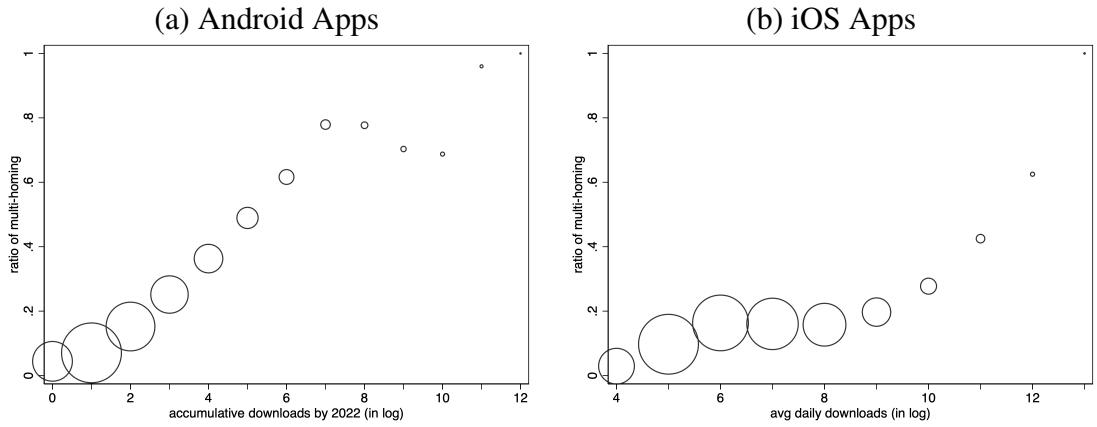


Figure F.1: Ratio of Multi-Homing by Log-Downloads Bins

Notes. This binned scatter plot presents the correlation between multi-homing and downloads among Android apps and iOS apps separately. Given the data we observe, we use the cumulative downloads by 2022 of Android apps and the average daily downloads between 2017 to 2022 of iOS apps. Each download bin is an integer of the log-downloads. The ratio of multi-homing is the number of multi-homing apps divided by the number of apps in that bin. The figure shows that the overall low ratio of multi-homing is driven by unpopular apps, while the most popular apps almost all multi-home.

Table E.13: Alternative iOS Sample with Original Categorization: Entry and Quality Effects

Variables	(1) ln(Num. of New Apps)	(2) Avg. Downloads	(3) Avg. Downloads
Game × Post	-0.15**	0.46**	0.44**
bootstrap p-value	(0.040)	(0.035)	(0.035)
95% CI	[-0.29,-0.01]	[0.02,0.92]	[0.02,0.85]
Game × Num. 4G Stations	-0.11	-0.16	-0.18
	(0.31)	(0.77)	(0.76)
Observations	336	336	336
Adj. R-sq.	0.94	0.23	0.23
Base Mean	59.11	0.42	0.42
	(4)	(5)	(6)
	Num. of New Apps in a Given Downloads Bin		
	Low	Medium	High
Game × Post	-0.156**	-0.236***	-0.253***
bootstrap p-value	(0.046)	(0.001)	(0.021)
95% CI	[-0.34,-0.01]	[-0.38,-0.09]	[-0.43,-0.05]
Game × Num. 4G Stations	-0.667*	0.243	0.403
	(0.367)	(0.385)	(0.332)
Observations	336	336	336
Pseudo R-sq.	0.74	0.75	0.75
Base Mean	19.84	19.89	19.37

Notes. The alternative iOS Sample is economically-relevant, capturing 99% all-time worldwide revenue and 95% all-time worldwide downloads generated by a 25% random sample of iOS apps released in China from February 2014 to January 2016. The categorization is almost original, except that there are two categories with very few entry: Developer Tools and Graphics & Design, each representing 0.01% and 0.02% of all new apps, respectively. We merge them into Utilities and Photo & Video respectively. Similar issue shows up in the main sample, which we address in the same way—see details in Appendix B.1. This adjustment leaves us 42 categories, multiplied with 8 quarters around August 2014 (the month of Android game commission rise), amounting to 336 markets for analysis. "Downloads" in the table means all-time worldwide downloads from app release to August 31, 2026, regardless of active status.. Low-, medium-, and high-downloads bins are constructed with 33rd and 67th percentiles. Poisson regressions are applied to the bin-specific entry analysis due to appearance of zero entries. Bootstrap p-values are labeled and reported in parenthesis. Clustered standard errors are reported in parenthesis without labels. Standard errors are clustered at category level. The p-value and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. Overall, the results are similar to those with aligned categorization between the main and alternative samples, which are reported in Table 10. For the ease of exposition, we omit the event study figures, which are available upon request.

Table E.14: Alternative iOS Sample: Avg Quality Effect Decomposition

Downloads Bin	(1) Low	(2) Medium	(3) High
	Panel (A). Num of New Apps in a Given Bin		
Game × Post	-0.151*	-0.266***	-0.258**
bootstrap p-value	(0.061)	(0.000)	(0.013)
95% CI	[-0.34,0.01]	[-0.42,-0.13]	[-0.44,-0.06]
Game × Num. 4G Stations	-0.560	0.184	0.398
	(0.399)	(0.373)	(0.327)
Observations	280	280	280
Pseudo R-sq.	0.68	0.69	0.68
Base Mean	25.61	26.68	25.43
	Panel (B). Share of New Apps in a Given Bin		
Game × Post	-0.00	-0.01	0.02
bootstrap p-value	(0.976)	(0.470)	(0.501)
95% CI	[-0.06,0.06]	[-0.05,0.03]	[-0.03,0.06]
Game × Num. 4G Stations	-0.14	0.10	0.04
	(0.10)	(0.10)	(0.09)
Observations	280	280	280
Adj. R-sq.	-0.13	-0.08	-0.09
Base Mean	0.34	0.34	0.32

Notes. The alternative iOS Sample is economically-relevant, capturing 99% all-time worldwide revenue and 95% all-time worldwide downloads generated by a 25% random sample of iOS apps released in China from February 2014 to January 2016. "Downloads" in the table means all-time worldwide downloads from app release to August 31, 2026, regardless of active status. Lack of average rating data, we use downloads as a quality measure. Low-, medium-, and high-downloads bins are constructed with 33rd and 67th percentiles. Poisson regressions are applied to the bin-specific entry analysis due to appearance of zero entries. Bootstrap p-values are labeled and reported in parenthesis. Clustered standard errors are reported in parenthesis without labels. Standard errors are clustered at category level. The p-value and 95% CI (in brackets) are constructed using wild-cluster bootstrap-t method. *** p<0.01, ** p<0.05, * p<0.1. Overall, we find that entry decline happens across all quality bins. Appendix Figure F.13 presents the event studies.

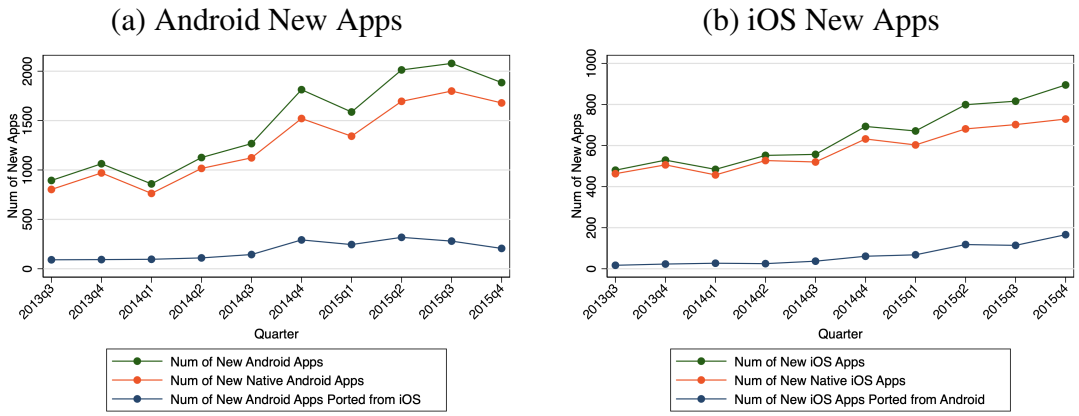


Figure F.2: Number of New Apps Decomposed into Native and Ported Apps on Android and iOS

Notes. The figure decomposes the number of new apps on a platform into the number of new native apps and the number of new apps ported from the other platform. Native apps on a platform are apps that join the platform in the quarter of release. The rest of the apps newly joining a platform in a given quarter are those that were released on the other platform in previous quarters. The figure shows that native apps account for the majority of the entry: on iOS and Android, native apps in a quarter account for 88% and 91% of new apps in the quarter, respectively.

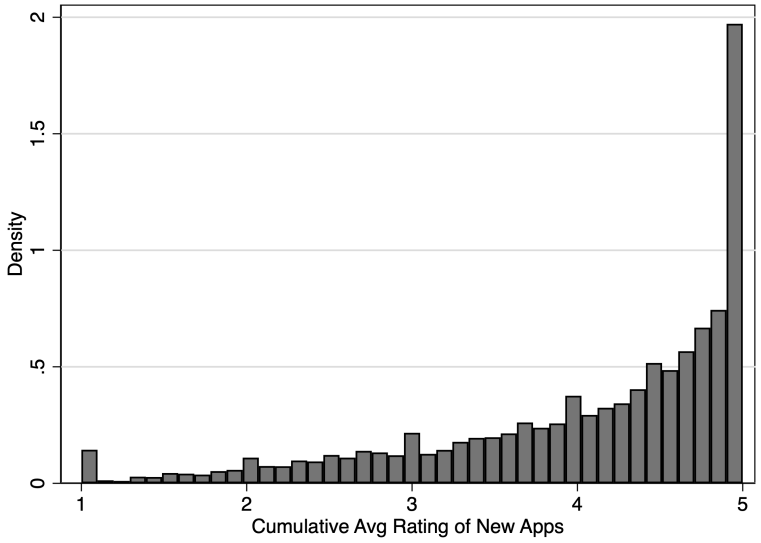
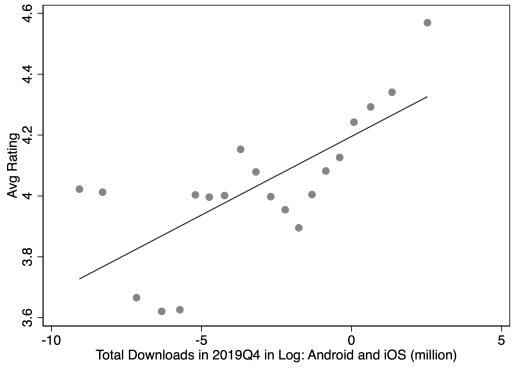


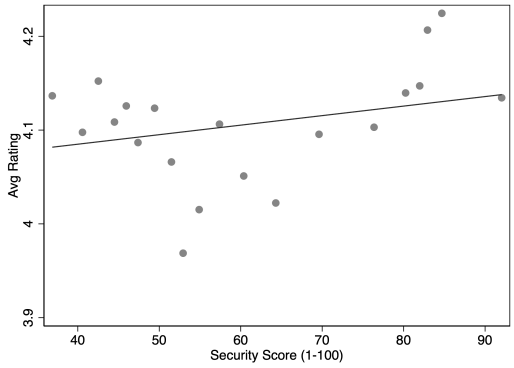
Figure F.3: Histogram of App Quality, Measured by Cumulative Average Ratings 5 Years after Release

Notes. The figure plots the histogram of app quality among new apps released from August 2013 to January 2016 on either Android or iOS or both. For multi-homing apps, we take the average quality weighted by the number of ratings on each platform.

(a) Log Total Downloads in 2019 Q4 (million)



(b) Security Score (1-100)



(c) Num. of Security Loopholes

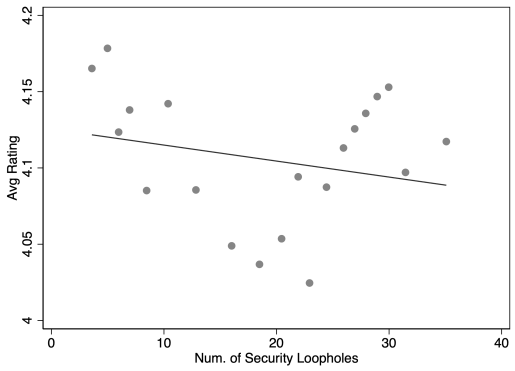


Figure F.4: Bin Scatter Plot: Average Rating and Alternative Quality Measures— Downloads, Security Score, and Security Loopholes

Notes. These bin scatter plots display the correlation between average rating and our three alternative quality measures: the downloads across Android and iOS in 2019 Q4, the security score upon release, and the number of security loopholes upon release. As expected, we find the average rating and downloads are strongly positively correlated. Moreover, average rating is positively correlated with security scores and negatively correlated with the number of security loopholes.

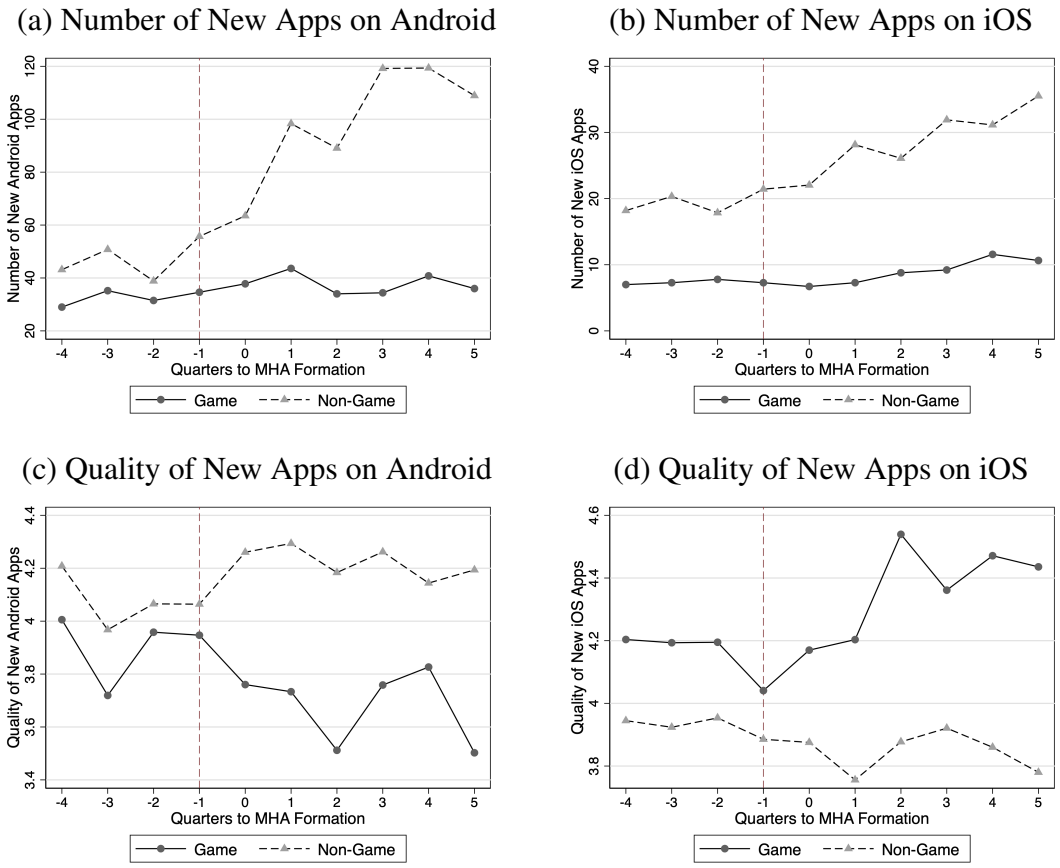
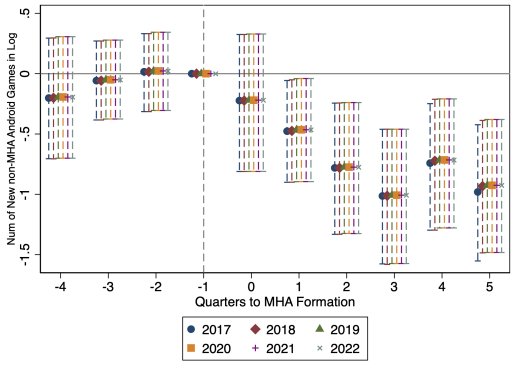


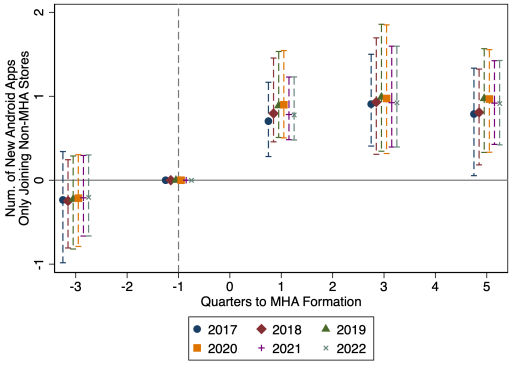
Figure F.5: Game *versus* Non-Game Markets: Number and Quality of New Apps

Notes. This figure plots the number and average quality of new apps in an average game market compared to an average non-game market, on Android and iOS, respectively. A market is defined as a category-quarter pair. The solid line denotes game markets, and the dashed line denotes non-game markets. In addition, we verified the following change in correlation patterns. The correlation between the number and quality of apps are negative before the game commission rise in both game and non-game markets. After the game commission rise, the correlation remains negative in non-game markets (though weaker), but turns positive and stronger in game markets. This pattern holds true for both Android and iOS, indicative of spillovers.

(a) Num. of New Apps in the non-MHA Android Markets (in log)



(b) Exclusive on non-MHA Stores



(c) Multi-Home on non-MHA and MHA

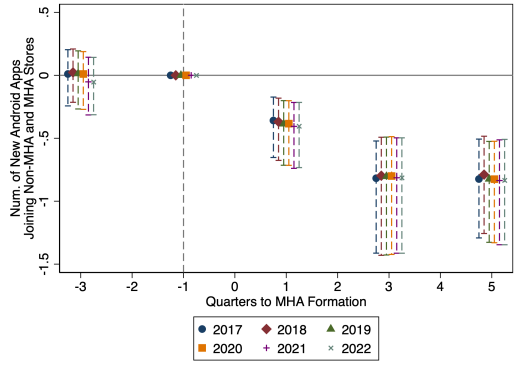
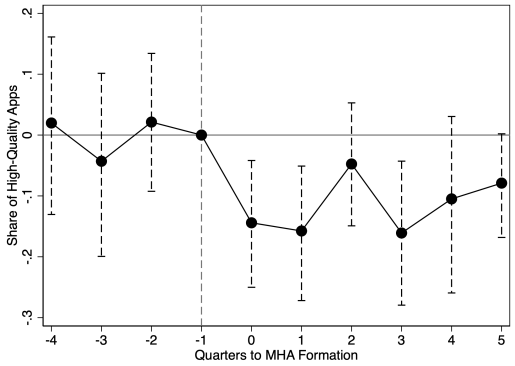


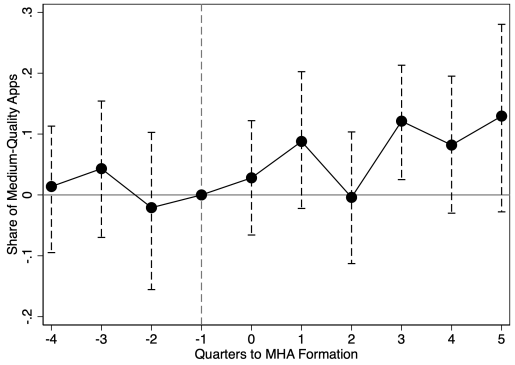
Figure F.6: Within-Android Spillover Effects on the Non-MHA Android Stores

Notes. This figure shows the estimated coefficients $\{\beta_\tau\}$ from Equation (2) using the sample of non-MHA Android markets. Panel (a) uses the standard specification. Panels (b) - (c) have two modifications: the coefficients are bi-quarter, grouping the quarter and the previous quarter; and the regression is Poisson. This bi-quarter level choice is dictated by the many zeros after the decomposition of new apps into exclusive and multi-homing ones in each market. β_{-1} is normalized to zero. The dependent variable is the number of new apps in the non-MHA Android markets (in log), the number of these apps that are exclusively on non-MHA stores, and the number of these apps that are also available on the MHA stores, respectively. Standard errors are clustered at category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates. Note that we approximate an Android app's decision to join the non-MHA stores in the quarter of release by checking whether the app has any downloads on the non-MHA stores in the years shown in different colors. The choice of years is dictated by the availability of downloads data.

(a) Share of High-Quality New Apps



(b) Share of Medium-Quality New Apps



(c) Shareof Low-Quality New Apps

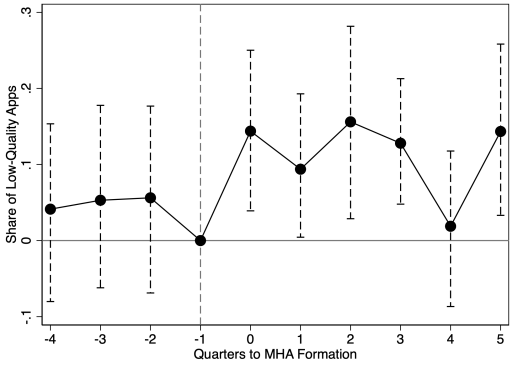


Figure F.7: The Effect of a Higher Android Game Commission Rate on Quality Distribution

Notes. The figure shows the estimated coefficients $\{\beta_\tau\}$ from Equation (2) in joint markets consisting of unique new apps across Android and iOS. The dependent variables are the share of high-, medium-, and low-quality new apps, where quality is measured by aggregated cumulative average ratings across Android and iOS, weighted by the number of ratings on each platform. The results show a leftward shift in market-wide quality distribution of new games after Android game commission rise: the share of high-quality new games significantly decreased, while the share of medium- and low-quality new games significantly increased. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

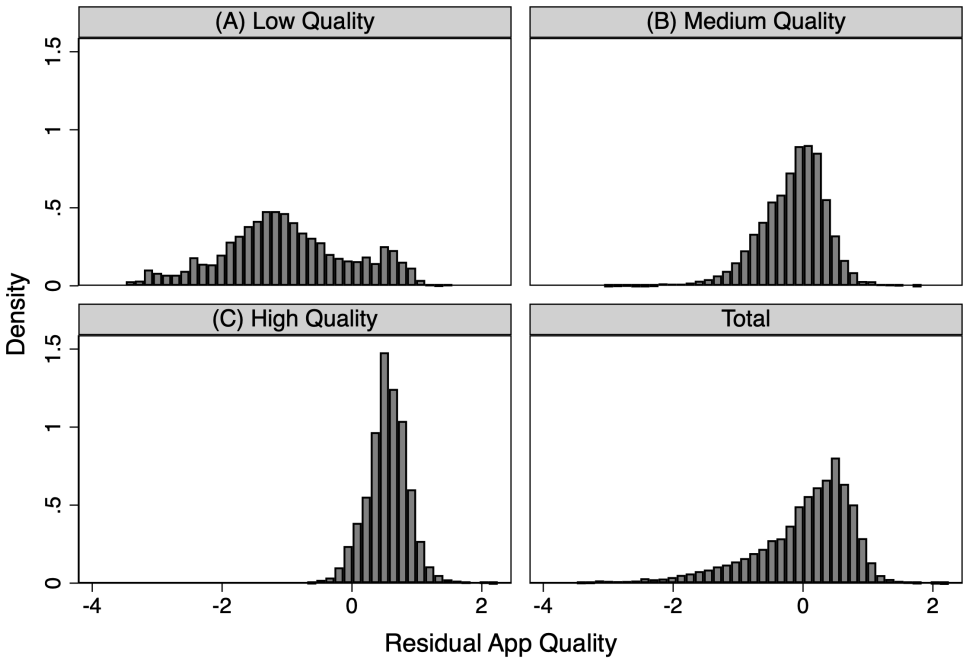
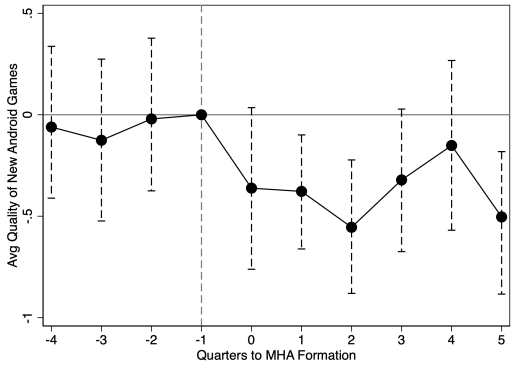


Figure F.8: Overall Distribution of Residual App Quality and by Quality Bins

Notes. The figure plots the histograms of residual app quality constructed from the regression in Column (4) of Table E.8. The sample includes apps released during our event-study sample period, i.e., from August 2013 to January 2016. It shows that the variation of quality predictability decreases in the order of low-, medium-, and high-quality bins.

(a) Avg. Quality of Android New Games



(b) Avg. Quality of iOS New Games

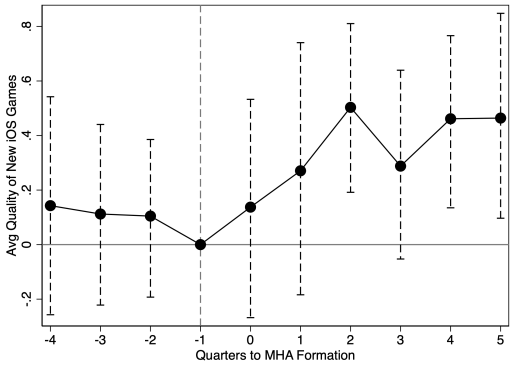


Figure F.9: The Quality Effects of a Higher Android Game Commission Rate on Android and iOS with Alternative Categorizations

Notes. This figure presents the estimated coefficients $\{\beta_\tau\}$ from Equation (2). β_{-1} is normalized to zero. In panels (a) - (b), the dependent variable is the average quality of new apps in Android and iOS markets, respectively. In Panel (a), the sample includes the 240 Android markets from the 24 Android categories. The corresponding estimated β from Equation (1) is -0.33 with a bootstrap p-value of 0.000. In Panel (b), the sample includes the 350 iOS markets from the 35 iOS categories. The corresponding estimated β from Equation (1) is 0.26 with a bootstrap p-value of 0.034. Standard errors are clustered at category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

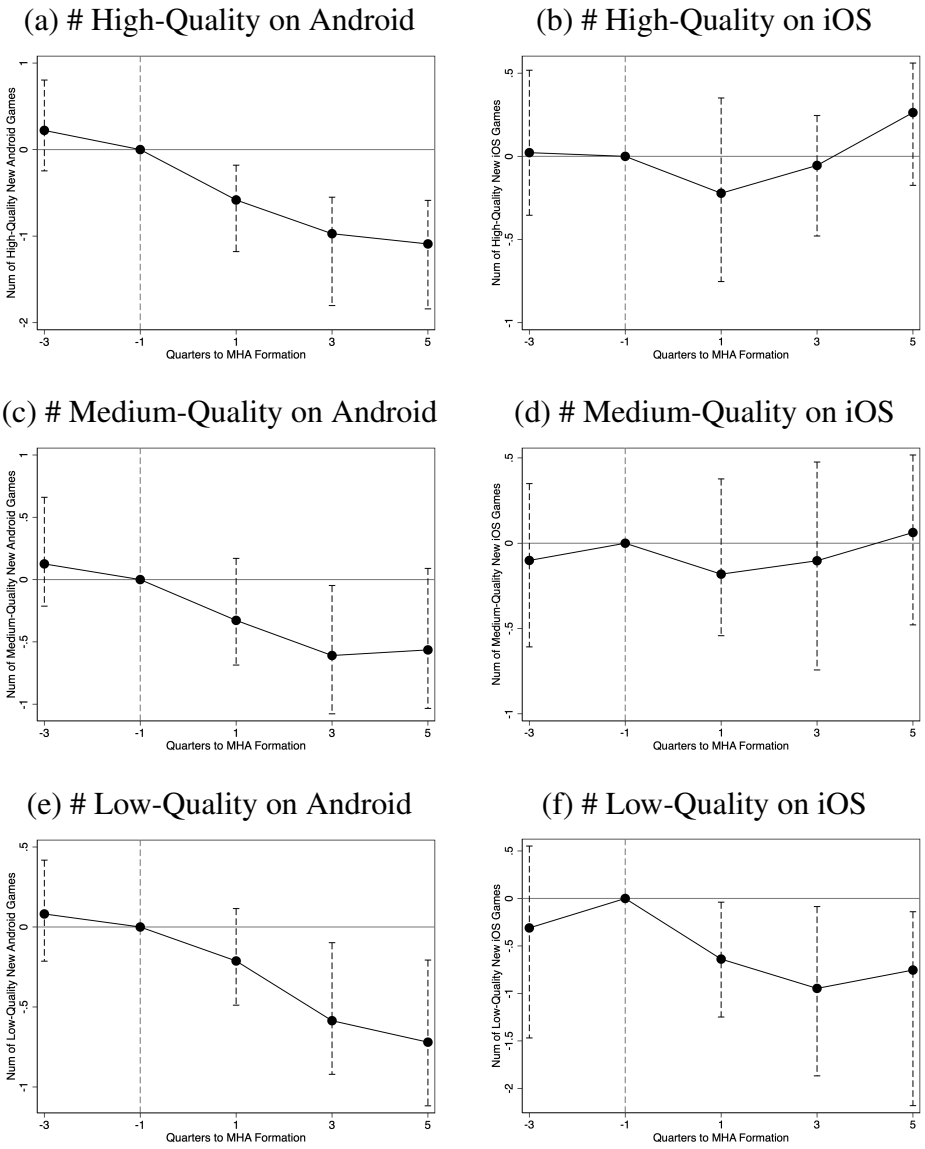


Figure F.10: Effect on the Game Entry on Android and iOS by Quality Bin

Notes. This figure shows the estimated coefficients $\{\tilde{\beta}_\tau\}$ from Equation (2) with bi-quarter coefficients, grouping the quarter on the horizontal axis and its previous quarter. $\tilde{\beta}_{-1}$ is normalized to zero. This bi-quarter level choice is dictated by the many zeros after the decomposition of new apps into each quality bin on each platform. Given the zeros, we use Poisson regressions in all panels. In panels (a) - (f), the dependent variables are the number of new high-, medium-, and low-quality apps in the Android and iOS markets, respectively. Standard errors are clustered at the category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates. The figure shows parallel pre-trends results for the DiD estimates reported in Columns (2) - (4) in Table 7. It also shows that the number of high-quality new games decreased on Android, while the number of low-quality new games decreased on both platforms.

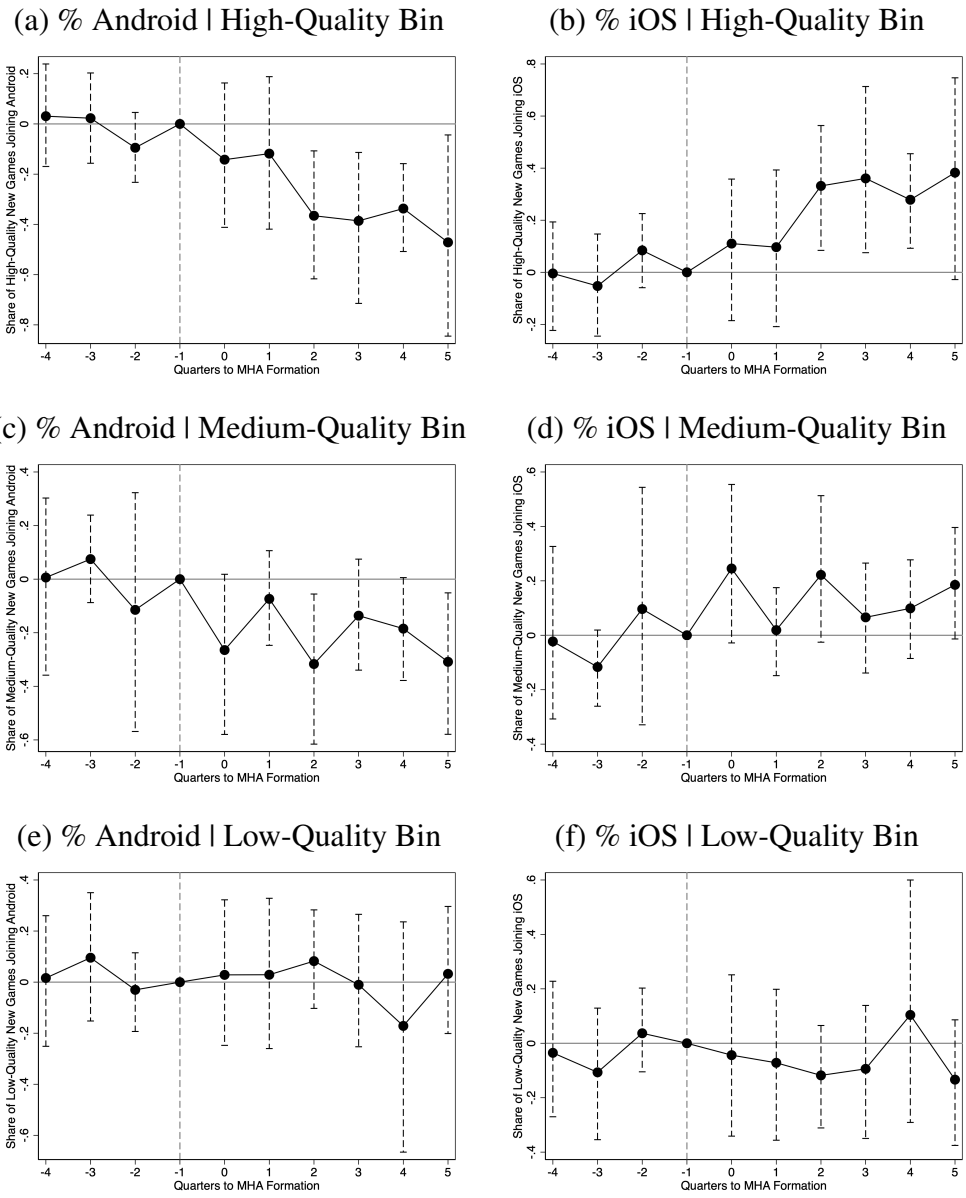
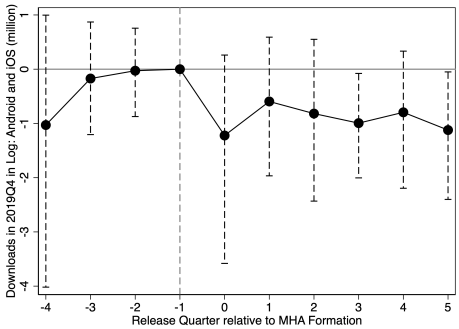


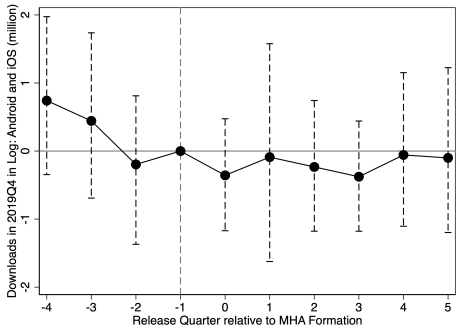
Figure F.11: Effect on the Platform Choice by Quality Bin

Notes. This figure shows the estimated coefficients $\{\beta_\tau\}$ from Equation (2). β_{-1} is normalized to zero. In panels (a) - (f), the dependent variables are the share of high-, medium-, and low-quality apps choosing to join Android and iOS, respectively. Standard errors are clustered at category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates. The figure shows parallel pre-trends results for the DiD estimates reported in Columns (5) - (7) in Table 7. It also shows that the platform choice effect is the most pronounced in the medium- and high-quality bins.

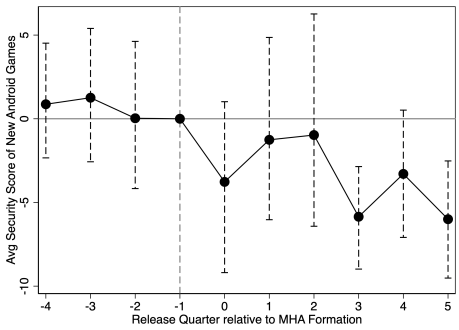
(a) Log Downloads in 2019 Q4 (Android)



(b) Log Downloads in 2019 Q4 (iOS)



(c) Security Score (Android)



(d) # Security Loopholes (Android)

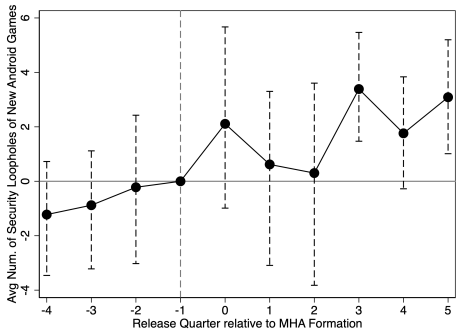


Figure F.12: Quality Effect Analysis with Alternative Quality Measures

Notes. This figure examines the average quality effect of a higher Android commission rate using alternative quality measures. It shows the estimated coefficients $\{\beta_\tau\}$ from Equation (2). β_{-1} is normalized to zero. Standard errors are clustered at category level. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates. In Panels (a) - (b), we take the total downloads across Android and iOS in 2019 Q4 as the measure of app quality. Then we compute the average quality of apps released in the quarter on the horizontal axis in a category. We consider Android apps and iOS apps in Panels (a) and (b), respectively. In Panel (c), we take the security score of the first version of an app as the measure of the quality of an app upon release. In Panel (d), we measure app quality with the number of security loopholes. Both security indexes are only available for Android apps. We find no significant difference between game and non-game markets in these quality measures before the MHA's formation.

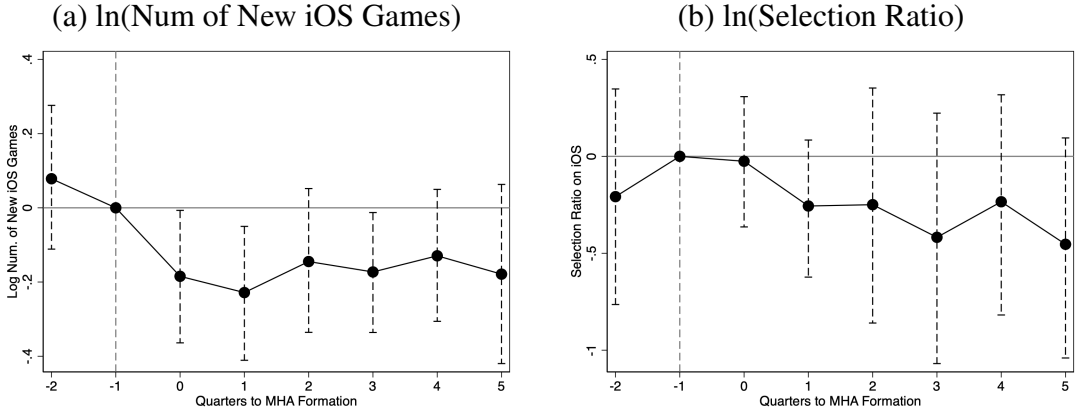


Figure F.13: Event Study on iOS Game Entry from an Economically-Relevant Sample

Notes. The alternative iOS Sample is economically-relevant, capturing 99% all-time worldwide revenue and 95% all-time worldwide downloads generated by a 25% random sample of iOS apps released in China from February 2014 to January 2016. These 10% apps capture 99% all-time worldwide revenue and 95% all-time worldwide downloads. Plot (a) replicates the entry event study applied to the main sample, showing a significant decline in iOS game entry after Android game commission rise, with no significant difference in the pre-trends. Plot (b) studies the selection bias—the number of new apps in the main sample divided by the number of new apps in the alternative sample, formally showing that the entry estimates from the two samples are not statistically significant different from each other. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates.

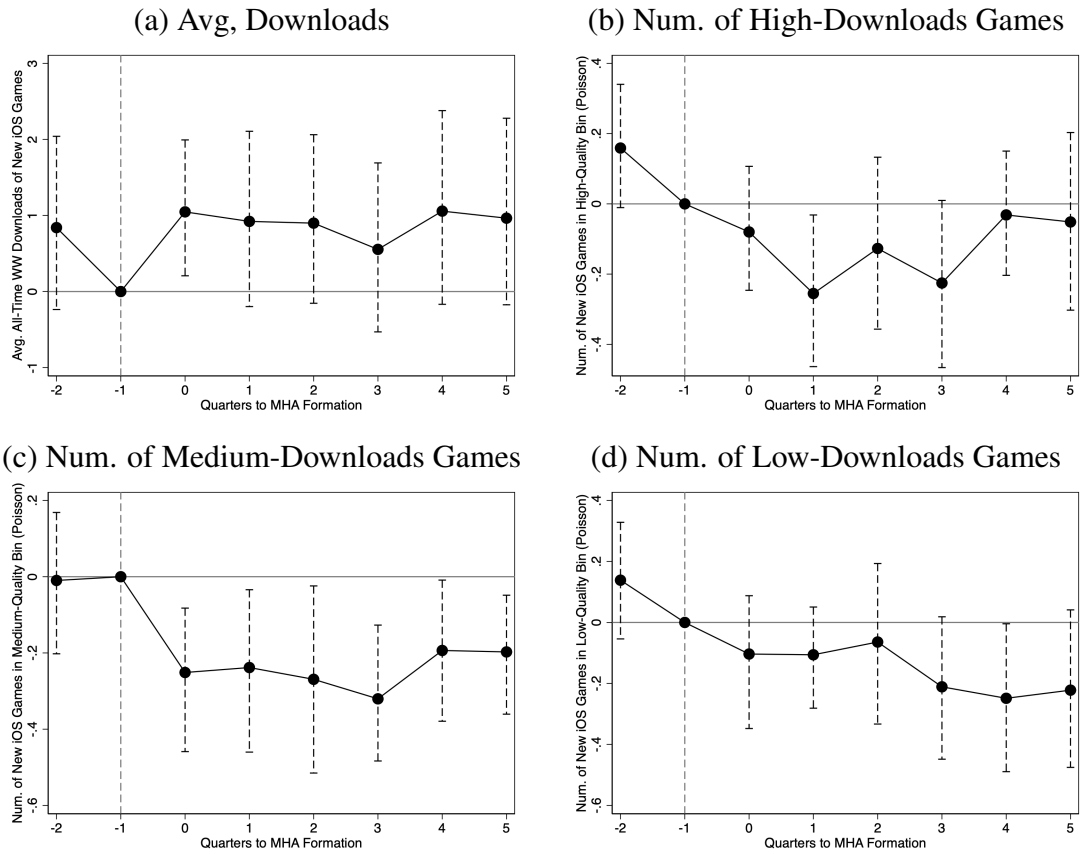


Figure F.14: Event Study on iOS Game Quality from an Economically-Relevant Sample

Notes. The alternative iOS Sample is economically-relevant, capturing 99% all-time worldwide revenue and 95% all-time worldwide downloads generated by a 25% random sample of iOS apps released in China from February 2014 to January 2016. These 10% apps capture 99% all-time worldwide revenue and 95% all-time worldwide downloads. The 95% confidence intervals are shown as dashed lines, constructed using the wild cluster bootstrap-t method and therefore may not be symmetric around the estimates. We do not find significant difference in pre-trends across the four variables. We find entry decline across all quality bins. In net, the average quality, measured by all-time worldwide downloads, increased after the Android game commission rise.