The Social Network Mixtape: Essays on the Economics of the Digital World

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Abstract

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This dissertation studies economic issues in the digital economy with a specific focus on the economic aspects of how firms acquire and use consumer data.

Chapter 1 empirically studies the drivers of digital attention in the space of social media applications. In order to do so I conduct an experiment where I comprehensively monitor how participants spend their time on digital services and use parental control software to shut off access to either their Instagram or YouTube. I characterize how participants substitute their time during and after the restrictions. I provide an interpretation of the substitution during the restriction period that allows me to conclude that relevant market definitions may be broader than those currently considered by regulatory authorities, but that the substantial diversion towards non-digital activities indicates significant market power from the perspective of consumers for Instagram and YouTube. I then use the results on substitution after the restriction period to motivate a discrete choice model of time usage with inertia and, using the estimates from this model, conduct merger assessments between social media applications. I find that the inertia channel is important for justifying blocking mergers, which I use to argue that currently debated policies aimed at curbing digital addiction are important not only just in their own right but also from an antitrust perspective and, in particular, as a potential policy tool for promoting competition in these markets. More broadly, my paper highlights the utility of product unavailability experiments for demand and merger analysis of digital goods. I thank Maayan

Malter for working together with me on collecting the data for this paper.

Chapter 2 then studies the next step in consumer data collection process – the extent to which a firm can collect a consumer's data depends on privacy preferences and the set of available privacy tools. This chapter studies the impact of the General Data Protection Regulation on the ability of a data-intensive intermediary to collect and use consumer data. We find that the opt-in requirement of GDPR resulted in 12.5% drop in the intermediary-observed consumers, but the remaining consumers are trackable for a longer period of time. These findings are consistent with privacy-conscious consumers substituting away from less efficient privacy protection (e.g, cookie deletion) to explicit opt out—a process that would make opt-in consumers more predictable. Consistent with this hypothesis, the average value of the remaining consumers to advertisers has increased, offsetting some of the losses from consumer opt-outs. This chapter is jointly authored with Yeon-Koo Che and Tobias Salz.

Chapter 3 and Chapter 4 make up the third portion of the dissertation that studies one of the most prominent uses of consumer data in the digital economy – recommendation systems. This chapter is a combination of several papers studying the economic impact of these systems.

The first paper is a joint paper with Duarte Gonçalves which studies a model of strategic interaction between producers and a monopolist platform that employs a recommendation system. We characterize the consumer welfare implications of the platform's entry into the production market. The platform's entry induces the platform to bias recommendations to steer consumers towards its own goods, which leads to equilibrium investment adjustments by the producers and lower consumer welfare. Further, we find that a policy separating recommendation and production is not always welfare improving. Our results highlight the ability of integrated recommender systems to foreclose competition on online platforms.

The second paper turns towards understanding how such systems impact consumer choices and is joint with Duarte Gonçalves and Shan Sikdar. In this paper we study a model of user decision-making in the context of recommender systems via numerical simulation. Our model provides an explanation for the findings of [1], where, in environments where recommender systems are typically deployed, users consume increasingly similar items over time even without recommendation. We find that recommendation alleviates these natural filter-bubble effects, but that it also leads to an increase in homogeneity across users, resulting in a trade-off between homogenizing across-user consumption and diversifying within-user consumption. Finally, we discuss how our model highlights the importance of collecting data on user beliefs and their evolution over time both to design better recommendations and to further understand their impact.

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Dedication

To all the people who I've crossed paths with who, directly or indirectly, shaped the way I see the world.

Chapter 1: Drivers of Digital Attention: Evidence from a Social Media Experiment

1.1 Introduction

In the past two decades social media has evolved from a niche online tool for connecting with friends to an essential aspect of people's lives. Indeed, the most prominent social media applications are now used by a majority of individuals around the world and these same applications are some of the most valuable companies in the modern day.[1] Due to the sheer amount of time spent on these applications and concentration of this usage on only a few large applications, there has been a global push towards understanding whether and how to regulate these markets [2, 3].[2] At the heart of the issue is that consumers pay no monetary price to use these applications, which renders the standard antitrust toolkit difficult to apply as the lack of prices complicates the measurement of demand and identification of plausible substitutes for these applications.[3] The demand measurement problem is further compounded by the fact that some fraction of usage may be driven by addiction to the applications or, more broadly, inertia [5, 6]. This facet of demand inflates the market share of these applications and makes it difficult to disentangle whether substitution between prominent applications is due to habitual usage or direct substitutability. This decomposition is further infor-

[1]For instance, Facebook, which owns several prominent social media and messaging applications, is the 6th most valuable company in the world with over a trillion dollars in market capitalization. Additionally, Twitter has a market capitalization of over 50 billion dollars and is in the top 500 highest valued companies in the world according to https://companiesmarketcap.com/ on August 30th, 2021.

[2]As pointed out by [4], the increased concentration of consumer attention can have ramifications far beyond this market alone since increased concentration in this market influences the ability for firms to enter into product markets that rely on advertising for product discovery.

[3]This issue was at the heart of the Facebook-Instagram and Facebook-WhatsApp mergers. Without prices, regulatory authorities resorted to market definitions that only focused on product characteristics, as opposed to substitution patterns of usage. For instance, Instagram's relevant market was only photo-sharing applications and WhatsApp's relevant market was only messaging applications. This issue continues to play a role in the ongoing FTC lawsuit against Facebook where a similar debate is ongoing.

mative about whether policies aimed at curbing digital addiction are important from an antitrust perspective. These two complications together have led to substantial difficulties in understanding the core aspects of consumer demand that are crucial for market evaluation and merger analysis.

In this paper I empirically study demand for these applications and illustrate how these findings can be used for conducting merger evaluation in such markets. I conduct a field experiment where, using parental control software installed on their phone and a Chrome Extension installed on their computer, I continuously track how participants spend time on digital services for a period of 5 weeks.[4] I use the parental control software to shut off access to YouTube or Instagram on their phones for a period ranging from one to two weeks. I explicitly design the experiment so that there is variation in the length of the restriction period and continue to track how participants allocate their time for two to three weeks following the restrictions. The time usage substitution patterns observed during the restriction period allow me to determine plausible substitutes, despite the lack of prices. The extent to which there are persistent effects of the restrictions in the post-restriction period allows me to uncover the role that inertia plays in driving demand for these applications.

I exploit the rich data and variation generated by the experiment to investigate aspects of demand related to competition policy and merger analysis. I provide an interpretation of the time substitution observed when the applications are restricted; this interpretation sheds light on relevant market definitions for the restricted applications – the set of applications that are considered substitutable relative to the application of interest – which have played a prominent role in antitrust policy debates. I further use the experimental substitution patterns to determine whether there is evidence of “dynamic" elements of demand as well as important dimensions of preference heterogeneity. Guided by these results, I estimate a discrete choice model of time usage with inertia to produce an important measure of substitution that is crucial for merger analysis: diversion ratios. I provide estimates of diversion ratios both with and without inertia, disentangling the extent of diversion due to inertia versus inherent substitutability of the applications. In order to understand how important inertia is for merger analysis, I apply the two sets of diversion ratio estimates to

[4] This ensures that I have objective measures of time usage which is crucial for my study as subjective measures of time spent on social media applications are known to be noisy and inaccurate [7].

evaluate mergers between social media applications. One important policy interpretation of the no inertia counterfactual is to provide insight into how and whether policies aimed at curbing digital addiction are important not just in their own right, but also in influencing usage and diversion between the applications to the extent that they would influence merger assessments.

Broader antitrust concerns motivate the following two questions about substitution patterns: what types of activities do participants substitute to and is this substitution concentrated on prominent applications or dispersed among the long tail? The most directly relevant question is whether or not there is evidence that they substitute across application categories. This has featured prominently in debates between these applications and regulators since the degree to which applications such as YouTube and Instagram are substitutable is important for monopolization claims about Facebook and mergers between different types of applications. Even if there is cross-category substitution, then it is also important to understand to what extent this is concentrated towards popular applications such as YouTube, within the vast Facebook ecosystem which spans application categories, or dispersed towards smaller applications competing with them. I argue that the set of applications that consumers substitute to during the restriction period serves as the broadest market definition since it measures consumer substitution at the “choke" price – the price which is sufficiently high so that no one would use the application at all.[5] Thus, even with zero consumer prices, the product unavailability variation alone allows me to assess the plausibility of claims that applications such as YouTube and Instagram directly compete against each other for consumer time.

In order to assess the extent of cross application category substitution, I manually pair each observed application in the data with the category it is assigned to on the Google Play Store. For the Instagram restriction group, I find a 22.4% increase in time spent on other social applications, but also a marginally significant 10.2% increase in time spent on communication applications. For the YouTube restriction group, I find that there is a null effect of substitution towards other

[5] This is similar to the interpretation given to such experiments in [8]. Note that the variation does not isolate the observed substitution to be about price exclusively. Indeed, one can broadly interpret this as the substitution at the choke advertising load or application quality as well. This will lead to some nuance in the value of this variation in the demand model, but is not first-order for the relevant market definition exercise.

entertainment applications, but also find a 15.2% increase in time spent on social applications. While this provides evidence for cross-category substitution, there is a notable asymmetry where blocking Instagram, a social media application, does not lead to substitution towards entertainment applications such as YouTube, whereas blocking YouTube, an entertainment application, leads to substitution towards social applications such as Instagram and Facebook.[6] Pairing these results with the conservative relevant market definition test implies that market definitions ought to span across the application categories between which I observe substitution. I show that, under this market definition, concentration is meaningfully lower relative to only using application categories as the relevant market definition.

There are several nuances to the implications of the application category substitution on the degree of market concentration. First, for both the YouTube and Instagram restriction, there is considerable substitution towards the outside option – off the phone.[7] This indicates that, even if I consider substitutes across all the categories on the phone, participants were not able to find a viable substitute in any other application. The framing of the debate in terms of within versus across category substitution therefore is potentially misleading as this shift towards the outside option implies that both YouTube and Instagram have considerable market power. Second, a large part of this market concentration is due to Facebook's joint ownership of Facebook, Instagram, Messenger, and WhatsApp; considering these as being independently owned applications substantially reduces the degree of market concentration even more so than multi-category market definitions. Indeed, non-Instagram Facebook owned applications have a 17.9% increase in time spent for the Instagram restriction group. Thus, some of the observed cross-category substitution is substitution within the Facebook ecosystem. Third, I elicit a subjective measure of how each participant uses the set of prominent social media, entertainment, and communication applications and find that, especially for social media applications, participants use the applications for different reasons ranging from

[6]This casts some subtlety to a debate in [3] between Facebook and regulators where Facebook uses outages of YouTube to claim that they compete with them. My experimental results point to a similar result in response to the YouTube restriction, but notably I observe an asymmetry where the reverse is not true during the Instagram restriction. Indeed, for Instagram, there is more within category substitution relative to cross category substitution.

[7]Using the data from the weekly surveys and the Chrome Extension, I am able to conclude that only a small fraction of this time is due to substitution to the restricted application on other devices.

social connection to pure entertainment. This points to the application categories not necessarily capturing the different uses of these applications and partially explaining some of the observed cross-category substitution.

The experimental design further allows me to understand whether there are potentially dynamic elements associated with demand by assessing whether the restrictions modify post-restriction time allocations on the restricted application as well as those substituted to during the restriction period. There are two possible channels through which the restrictions could impact post-restriction usage. The first possible channel is that the restriction could serve as a shock to participants' habits and depress usage after the restriction period. While I remain agnostic about the mechanism through which this change would occur, one important descriptive statistic is that up to 51% of the participants in the study are psychologically addicted to social media according to the scale by [9] that participants complete in the baseline survey. Thus, the experiment could serve as a shock to the addictive habits of the participants in the experiment. The second possible channel is intertemporal substitution whereby the restrictions lead participants to defer consumption until the restriction period is lifted. These two channels are not mutually exclusive and the aim is to assess which of these is first-order in modeling demand. This motivates why I design the experiment so that the restriction lengths are relatively long and also varied in length as one might expect that these effects are more apparent the longer the restriction length is.

I explicitly test whether there is a spike in usage of the restricted application on the day that it is no longer blocked for the participants and find no evidence of this for either the one or two week restriction group. I use this as evidence that the intertemporal substitution channel is not prominent as one would expect the built up usage during the restriction period to lead to a spike in usage when the application was returned. I find a consistent body of evidence that there is a persistent reduction in time spent on the restricted applications and that this is primarily driven by the participants that had the two week restriction, *but not* those for the one week restriction. For the Instagram restriction, the two week restriction group reduced average daily usage relative to the control group by 5 minutes and had a similar reduction relative to the one week restriction group. Estimating quantile treatment effects indicates that this is mainly driven by the heaviest users of the applications. A survey sent after the study indicates that this reduction in time spent persists even a month following the conclusion of the study. For the YouTube restriction, there is suggestive evidence of a similar difference between the one and two week restriction group, but the resulting difference in average daily usage is not statistically significant. However, I find that participants in the YouTube restriction spent more time on applications installed during the restriction period relative to the control group and persisted to use these applications even in the post-restriction period. I use both the persistent reduction in usage of Instagram and the increased usage of applications installed during the restriction period of YouTube as evidence that inertia plays a role in demand for these applications.

The experimental results shed light on aspects of demand required to understand the usage of these applications. However, in order to conduct merger analyses, an important output of a demand study is estimates of *diversion ratios*. The diversion ratio from application i to application j is defined as the fraction of sales / consumption that gets diverted from application i to application j as a result of a change in price / quality / availability of application i. Diversion ratios provide a quantitative magnitude of substitution between two applications and are especially important for merger analysis as they play a prominent role in the current US horizontal merger guidelines for measuring possible unilateral effects. I estimate a discrete choice model of time usage between prominent social media and entertainment applications and use the estimates to compute second-choice diversion ratios – diversion with respect to a change in availability. I incorporate the insights from the experimental results directly into the demand model. I incorporate inertia by including past usage into consumer utility similar to state-dependent demand estimation models [10, 11]. Furthermore, I directly incorporate the heterogeneity in subjective usage of the applications into the utility function in order to capture the preference heterogeneity indicated by the experimental results and exploit the granular time usage data that I collect in order to have a flexible outside option that varies across time.

The main counterfactual that I consider is to shut down the inertia channel and compute how this impacts overall usage of the set of considered applications as well as the estimated diversion ratios. I find that longer term inertia drives nearly 40% of overall usage of the considered applications. I provide two interpretations of this counterfactual – an upper bound of how policies aimed at curbing digital addiction impact diversion and a more direct measure of substitutability between applications. For the first interpretation, while I remain agnostic to the behavioral mechanism behind the estimated inertia, a large portion of it is likely from addictive usage as indicated by the qualitative evidence accumulated throughout the study.[8] Since my experiment does not precisely isolate the extent to which usage is driven by addiction, I consider the results as an upper bound on how the addiction channel influences usage and diversion. Regulators around the world are actively debating about how to deal with these digital addiction issues, whether through directly regulating the time usage on these applications or indirectly regulating the curation algorithms and feed designs used on them.[9] Thus, my counterfactual sheds light on whether these policies would influence usage of these applications sufficiently much in order to meaningfully change merger assessments. The second interpretation is that this allows me to disentangle the extent to which the diversion between two applications is due to inherent substitutability or inertia. For instance, there is large observed diversion from Snapchat to Instagram, which could be due to Snapchat and Instagram being inherently substitutable applications or it could be due to the fact that people are more likely to have built up habit stock of Instagram that induces them to be more likely to use it in the absence of Snapchat. However, it could also increase the converse diversion from Instagram to Snapchat since, for smaller applications, they are less likely to have built up habit stock and may

[8]Indeed, contemporaneous work by [12] similarly shows that 31% of usage of these applications is driven by behavior consistent with rational addiction.

[9]There are bills proposed in the US Congress, such as the Kids Internet Design and Safety Act, `https://www.congress.gov/116/bills/s3411/BILLS-116s3411is.pdf`, aimed at regulating certain design features that encourage excess usage and the Social Media Addiction Reduction Technology (SMART) Act, `https://www.congress.gov/bill/116th-congress/senate-bill/2314/text`, directly aiming to limit time spent on these applications. In the European Union the currently debated Digital Services Act has several stipulations on regulating curation algorithms, `https://digital-strategy.ec.europa.eu/en/policies/digital-services-act-package`. In China, the government has explicitly set a time limit of 40 minutes on children's usage of the popular social media application TikTok, `https://www.bbc.com/news/technology-58625934`. Furthermore, there is a constant stream of popular press articles focusing on additional proposals to limit the addictive nature of these applications (e.g. see `https://www.wsj.com/articles/how-to-fix-facebook-instagram-and-social-media-change-the-defaults-11634475600`).

actually benefit from the lack of built up habit stock on larger applications such as YouTube. The diversion estimates without inertia thus filter out the second channel and provide a more natural measure of substitution between these applications. I argue that this measure of diversion is useful for common merger assessments in these markets between prominent and nascent applications by parsing out the fact that the nascent application may not have built up substantial habit stock of consumers in aggregate, resulting in low diversion in the baseline even if the applications are highly substitutable.

I conclude the paper by applying the diversion ratios to hypothetical merger evaluations between prominent social media and entertainment applications. I develop a version of the Upward Pricing Pressure test for attention markets where applications set advertising loads (i.e. number of advertisements per unit time) and advertisers' willingness to pay depends on the time allocations of consumers. As is standard, I use the estimates of consumer diversion from my model, set a threshold on the efficiency gains in application quality arising from a merger, and determine whether a merger induces upward pressure on advertising loads. My formulation captures a unique aspect of online "attention" markets where additional consumer time on an application induces greater ability to target consumers and increases advertiser willingness to pay.

I find that, depending on how sensitive advertising prices are to time allocations, many mergers between prominent social media applications should be blocked with inertia, but many do not without inertia. The main intuition behind this is that with inertia the mergers that get blocked, such as Snapchat-YouTube, are due to the merged firm's incentive to increase advertising loads on the smaller application (Snapchat) in order to divert consumption towards the larger application (YouTube). When there is no inertia in usage, the diversion from the smaller to the larger application is lower since YouTube does not get the benefit of already being a popular application with a large amount of consumer habit stock built up. Thus, my results indicate that the role of inertia in inflating market shares and diversion ratios towards the largest applications is important for justifying blocking mergers between the smaller and larger applications. This highlights how digital addiction issues are directly relevant to antitrust policy as they inflate the time usage and diversion between applications by a sufficient amount to lead to meaningfully different conclusions about mergers between these applications.

More broadly, this paper highlights the usefulness of product unavailability experiments for demand and merger studies between digital goods. I exploit the insight that digital goods enable individual level, randomized controlled experiments of product unavailability that are difficult to conduct with other types of goods and in other markets. These experiments enable causal estimates of substitution patterns and identify plausible substitutes even when consumers pay no prices. Furthermore, they can be used to estimate the relevant portions of consumer demand that are difficult to estimate using only observational data and are required for relevant market definition and merger assessment. As a result, they serve as a practical and powerful tool for antitrust regulators in conducting merger assessments in digital markets.

The paper proceeds as follows. Section 2.1 surveys papers related to this work. Section 1.3 provides a full description of the experiment and the resulting data that I collect during it. Section 1.4 describes pertinent descriptive statistics of the data that are useful for understanding how participants spend their time and use the social media applications of interest. Section 1.5 documents the experimental results with respect to time substitution both during and after the restriction period. Section 1.6 develops and estimate the discrete choice time usage model with inertia. Section 1.7 posits the Upward Pricing Pressure test that I use for hypothetical merger evaluation and applies it to mergers between prominent social media applications. Section 2.7 concludes the paper with some final remarks and summary of the results.

1.2 Related Work

This paper contributes to four separate strands of literature, which I detail below.

Economics of Social Media: The first is the literature that studies the economic impact of social media. Methodologically my paper is closest to [13, 14, 15] who measure the psychological and economic welfare effects of social media usage through restricting access to services. [13, 15] restrict access to Facebook and measure the causal impact of this restriction on a battery of psychological and political economy measures. [14] measures the consumer surplus gains from free digital services by asking participants how much they would have to be paid in order to give up such services for a period of time. This paper utilizes a similar product unavailability experiment, but uses the product unavailability experiment in order to measure substitution patterns as opposed to quantifying welfare effects.

A concurrent paper that is also methodologically related is [12]. They utilize similar tools to do automated and continuous data collection of phone usage.[10] They focus on identifying and quantifying the extent of digital addiction by having separate treatments to test for self-control issues and habit formation. I argue that my experimental design also enables me to understand the persistent effects of the restriction, which I use to identify a demand model of time usage with inertia. While my experiment does not allow me to identify the precise mechanism behind this inertia effect, I rely on [12] to argue that the most likely possible mechanism is tied to digital addiction. Thus, I view [12] as being complementary to my work as I focus on the competition aspect between these applications, but also find patterns consistent with their results.[11]

Finally, there is a burgeoning literature on the broader economic and social ramifications of the rise of social media applications. [18] study the impact of limiting social media usage to ten minutes a day on academic performance, well-being, and activities and observes similar substitution between social media and communication applications. The broader literature has focused on political economy issues associated with social media [19, 20, 21, 22] as well as its psychological impact [23, 24, 25, 26, 27, 28].

Product Unavailability and State-Dependent Demand Estimation: The second is the literature

[10] An important antecedent of this type of automated data collection is the “reality mining" concept of [16] who first used mobile phones to comprehensively digitize activities done by experimental participants and, at least for the author, served as an important point of inspiration. One further point worth noting is that the study done by [12] relies on a custom-made application, whereas the primary data collection done in my paper relies on a (relatively) cheap, publicly available, parental control application and an open source Chrome extension which is more accessible to other researchers. Furthermore, [12] are only able to comprehensively track participants on smartphones, whereas I can additionally comprehensively track substitution towards other devices without having to rely on self-reported data.

[11] In the theory literature, [17] study competition between addictive platforms where platforms trade off application quality for increased addictiveness, whereas in this paper I study the role of addiction in diversion estimates between prominent applications.

in marketing that studies brand loyalty and, more broadly, state-dependent demand estimation. The discrete choice model of time usage that I consider closely follows the formulation in this literature where past consumption directly enters into the consumer utility function and the empirical challenge is to disentangle the inertia portion of utility from preference heterogeneity [29, 10, 30]. I consider that consumers have a habit stock that enters directly into the utility function, which I interpret as inertia that drives usage of the applications and is similar to the formulation in [11].

Relative to this literature, I exploit the fact that I conduct an experiment and induce product unavailability variation as a shock to consumer habits in order to identify this portion of consumer utility. [31, 8, 32] explore the value of product unavailability in identifying components of consumer demand. In this paper my focus is on using this variation to understand the impact of inertia, though in section A.5 I directly use the results of [8, 32] who utilize the treatment effect interpretation of the product unavailability experiment as an alternative approach to estimate diversion ratios. Finally, [33] studies a natural experiment of product unavailability due to website outages in order to understand the medium term effects of inertia on overall usage.

Attention Markets: The third is the literature that studies “attention markets" (see [34], Section 4 for an overview). An important modeling approach taken in the theoretical literature, starting from [35] and continuing in [36, 37, 38] is modeling the “price" faced by consumers in these markets as the advertising load that the application sets for consumers. In the legal literature a similar notion has emerged in [39, 40] who propose replacing consumer prices in the antitrust diagnostic tests with “attention costs." Relative to the theoretical literature in economics, [39, 40] interpret these “attention costs" as being broader than just advertising quantity and including, for instance, reductions in application quality. I use this notion to interpret product unavailability as being informative about the relevant market definition exercise through observing substitution at the choke value of attention costs. I develop an Upward Pricing Pressure (UPP) test, following [41], for this setting where I model the market in a similar manner and treat the advertising load experienced by consumers as implicit prices on their time. In the UPP exercise, similar to [4], applications can provide hyper-targeted advertisements based on the amount of “attention" of consumers that they capture. This formulation differs from existing UPP tests that have been developed for two-sided markets, such as [42], by explicitly relying on the notion of advertising load as the price faced by consumers.

Mobile Phone Applications: The fourth is the literature that studies the demand for mobile applications, which typically focuses on aggregate data and a broad set of applications. This paper, on the other hand, utilizes granular individual level data to conduct a micro-level study of the most popular applications. [43] study competition between mobile phone applications utilizing aggregate market data and focus on download counts and the prices charged in the application stores, as opposed to focusing on time usage. [44, 45] study the demand for time usage of applications in Korea and China respectively building off the multiple discrete-continuous model of [46]. [44] extends [46] to allow for correlation in preferences for applications and applies this to a panel of Korean consumers mobile phone usage. [45] further extended [44] and explicitly models and separately identifies the correlation in preferences and substitutability / complementarity between applications. [45] considers the impact of pairwise mergers between applications, but mainly focuses on the pricing implications of the applications (i.e. how much they could charge for the application or for usage of the application). Relative to these papers there are two important differences. First, I exploit the granularity of the data to model time allocation as a panel of discrete choices instead of a continuous time allocation problem. Second, I exploit my experimental variation to study the role of inertia in usage of these applications as opposed to complementarity / substitutability.

This paper also contributes to a broader literature that studies other aspects of competition in the mobile phone application market. This literature focuses on the impact that "superstar" applications have on firm entry and the overall quality of applications in the market [47, 48, 49]. One interpretation of my study is that I shut off a "superstar" application, such as Instagram or YouTube, and characterize the consumer response. One key variable that I study is the extent to which participants downloaded and spent time on new applications during the period when these "superstar" applications were temporarily "removed" from the market. I find that the restriction induces participants to download and spend time on new applications, highlighting that the inertia from the usage of these applications may impede consumers from actively seeking out new applications and serve as a barrier to entry.

1.3 Experiment Description and Data

1.3.1 Recruitment

I recruit participants from a number of university lab pools, including the University of Chicago Booth Center for Decision Research, Columbia Experimental Laboratory for Social Sciences, New York University Center for Experimental Social Science, and Hong Kong University of Science and Technology Behavioral Research Laboratory. A handful of participants came from emails sent to courses at the University of Turin in Italy and the University of St. Gallen in Switzerland. Furthermore, only four participants were recruited from a Facebook advertising campaign.[12] The experimental recruitment materials and the Facebook advertisements can be found in subsection A.1.1. Participants earned $50 for completing the study, including both keeping the software installed for the duration of the study as well as completing the surveys. Participants had an opportunity to earn additional money according to their survey responses if they were randomly selected for the additional restriction.

Preliminary data indicated that there was a clear partition in whether participants utilized social media applications such as Facebook, Instagram, Snapchat, and WhatsApp as opposed to applications of less interest to me such as WeChat, Weibo, QQ, and KakaoTalk.[13] As a result, the initial recruitment survey (see Figure A2) ensured that participants had Android phones as well as used applications such as Facebook/Instagram/WhatsApp more than applications such as WeChat/Weibo/QQ/KakaoTalk. I had 553 eligible participants that filled out the interest survey.

[12]While these participants only ended up making up a small fraction of overall participants, in order to ensure that the nature of selection was consistent across the different recruiting venues the Facebook advertisements were geographically targeted towards 18-26 year olds that lived in prominent college towns (e.g. Ann Arbor in Michigan, Ames in Iowa, Norman in Oklahoma, etc.). This was to ensure that there was similar demographic selection as those implicitly induced by recruitment via university lab pools.

[13]This was from another experiment that collected mobile phone data from the same participant pool.

The resulting 553 eligible participants were then emailed to set up a calendar appointment to go over the study details and install the necessary software. This occurred over the period of a week from March 19th until March 26th. At the end, 410 participants had agreed to be in the study, completed the survey, and installed the necessary software.

There are two points of concern that are worth addressing regarding recruitment. The first is whether there is any selection into the experiment due to participants seeking limits on their use of social media applications. In the initial recruitment it was emphasized that the purpose of the study was to understand how people spend their time with a particular focus on the time spent in their digital lives, in order to dissuade such selection into the experiment. Once the participants had already registered, they were informed about the full extent of the study. However, they were still broadly instructed that the primary purpose of the study was to understand how people spend their time and that they may face a restriction of a non-essential phone application. The precise application that would be restricted was not specified in order to further ensure there were no anticipatory effects that would bias baseline usage. The second is that I do not exclusively recruit from Facebook or Instagram advertisements as is done in several other studies (e.g. [13, 22, 12]), but instead rely on university lab pools. This leads to an implicit selection in the type of participants I get relative to a representative sample of the United States (e.g. younger, more educated), however it does not induce as much selection in the intensity of usage of such applications that naturally comes from recruiting directly from these applications. For a study such as this some degree of selection is inevitable, but in this case I opted for selection in terms of demographics instead of selection on intensity of application usage as for a study on competition this was more preferable.

1.3.2 Automated Data Collection

The study involved an Android mobile phone application and a Chrome Extension. Participants were required to have the Android mobile phone application installed for the duration of the study and were recommended to install the Chrome Extension. Despite being optional, 349 of the participants installed the Chrome Extension. It is important that I collect objective measures of time allocations for the study as subjective measurements of time on social media are known to be noisy and inaccurate [7].

The Android mobile phone application is the ScreenTime parental control application from ScreenTime Labs.[14] This application allows me to track the amount of time that participants spend on all applications on their phone as well as the exact times they're on the applications. For instance, it tells me that a participant has spent 30 minutes on Instagram today as well as the time periods when they were on the application and the duration of each of these sessions. Furthermore, it allows me to restrict both applications and websites so that I can completely restrict usage of a service on the phone.[15] This application is only able to collect time usage data on Android, which is why I only recruit Android users.

For the purposes of the study, I create 83 parental control accounts with each account having up to 5 participants. The parental control account retains data for the previous five days. The data from the parental control application was extracted by a script that would run every night. The script pulls the current set of installed applications on the participant's Android device, the data on time usage for the previous day, the most up to date web history (if available) and ensures the restrictions are still in place.[16] It also collects a list of participants whose devices may have issues with the software.[17] I pair the data with manually collected data on the category of each application pulled from the Google Play Store.

[14]For complete information on the application see `https://screentimelabs.com`.

[15]For instance, if I want to restrict access to Instagram then it's necessary to restrict the Instagram application as well as `www.instagram.com`. It does this by blocking any HTTP requests to the Instagram domain, so that the restriction works across different possible browsers the participant could be using.

[16]Note that the only usage of the web history would be to convert browser time to time on the applications of interest.

[17]The script flags if a participant had no usage or abnormally low usage (~10% usage relative to the running average). The next morning I reach out to the participants who are flagged and ask them to restart their device or, in extreme cases, reinstall the software. I keep a list of participants who were contacted this way and confirmed there may be an issue with the software and drop the day from the data when the software is not working properly. The primary reason for the instability is usually based on the device type. Huawei devices have specific settings that need to be turned off in order for the software to run properly. The vast majority of issues with Huawei devices were resolved in the setup period of the study. OnePlus and Redmi devices, however, have a tendency to kill the usage tracking background process unless the application is re-opened every once in a while. As a result, participants with these phones were instructed to do so when possible. This is the most common reason a phone goes offline. Figure A10 plots a histogram of the number of active days with the software working across participants and shows that this issue only impacts a small fraction of participants.

The Chrome Extension collects information on time usage on the Chrome web browser of the desktop/laptop of participants.[18] All the restrictions for the study are only implemented on the mobile phone so that participants have no incentive to deviate to different web browsers on their computers at any point during the study.[19,20] Participants can optionally allow time tracking on all websites and can view how much time the application has logged to them in the Chrome Extension itself (see Figure A7).[21] The final data that I make use of from the extension are time data aggregated at the daily level as well as time period data (e.g. 9:50 - 9:55, 10:30-10:35 on Facebook).

1.3.3 Survey Data

In order to supplement the automated time usage data, I elicit additional information via surveys. The surveys allow me to validate the software recorded data, to get information about how participants spend time on non-digital devices, and to elicit qualitative information about how participants use the set of prominent social media and entertainment applications. There are three types of surveys throughout the study.

Baseline Survey: The first is the baseline survey that participants complete at the beginning of the study. This survey is intended to elicit participants' perceived value and use of social media applications as well as basic demographic information. The full set of questions is provided

[18]The source code for the Chrome Extension is available here: `https://github.com/rawls238/time_use_study_chrome_extension`. The extension is modified and extended based off David Jacobowitz's original code. Some participants had multiple computers (e.g. lab and personal computers) and installed the extension on multiple devices.

[19]By default the Chrome Extension only collects time spent on entertainment and social media domains with the rest of the websites logged under other. In particular, it only logs time spent on the following domains: instagram.com, messenger.com, google.com, facebook.com, youtube.com, tiktok.com, reddit.com, pinterest.com, tumblr.com, amazon.com, twitter.com, pandora.com, spotify.com, netflix.com, hulu.com, disneyplus.com, twitch.tv, hbomax.com.

[20]The software is setup with the participants over Zoom where they were instructed that the restriction was only on the phone and they should feel free to use the same service on the computer if they wished to do so. Thus, it was important that participants did not feel as though they should substitute between web browsers on the computer as this would lead me to not observe their true computer usage.

[21]The time tracking done by the Chrome Extension is crude due to limitations on how Chrome Extensions can interact with the browser. The Chrome Extension script continually runs in the background and wakes up every minute, the lowest possible time interval, observes what page it is on, and then ascribes a minute spent to this page. This process induces some measurement error in recorded time, but gives me a rough approximation of time spent on each domain. The recorded data is continually persisted to my server, which allows me to see what the recorded website was for every minute as well as aggregates by day.

in subsection A.1.2.

There are two questions which require additional explanation. The first is that I elicit the monetary value that participants assign to each application using a switching multiple price list [50]. I provide them with a list of offers ranging from \$0 - \$500 and ask them if they would be willing to accept this monetary offer in exchange for having this application restricted on their phone for a week. I ask them to select the cut-off offer, which represents the minimum amount they would be willing to accept to have the application restricted. This elicitation is incentive-compatible since the participants are made aware that, at the end of the study period, two participants will have one application and one offer randomly selected to be fulfilled and thus have an additional restriction beyond the one in the main portion of the study.

The second is a hypothetical consumer switching question, a commonly used question in antitrust cases where regulatory authorities ask consumers how they think that they would substitute if a store was shut down or prices were raised [51]. In this scenario, the question asks how participants think they would substitute if the application was made unavailable. I ask which general category they think they would substitute their time to, instead of particular applications. For instance, I ask whether losing their Instagram would lead to no change or an increase in social media, entertainment, news, off phone activities, or in-person socializing. I ask participants to choose only one category so that they are forced to think about what the biggest change in their behavior would be.

Weekly Surveys: Every week throughout the study there are two weekly surveys that participants complete. The first is sent on Thursdays, which contains a battery of psychology questions and was part of the partnership for this data collection and not reported on in this paper.[22] The second is sent on Saturday mornings and asks participants to provide their best guess at how much time they are spending on activities off their phones. It is broken down into three parts: time spent on applications of interest on other devices, time spent on necessities off the phone, and time spent on leisure activities off the phone.

[22]However the questions that participants answered are presented with the survey instruments in subsection A.1.2.

Endline Survey: The endline survey contains the following questions geared towards understanding participants' response to the restrictions. The goal is to try to disentangle the mechanisms at play in potential dynamic effects of the restrictions. The questions are all multiple choice questions that ask how participants think they reallocated their time during the week of the restrictions and how they think their time spent after the restrictions changed relative to before the restrictions. The full details of the questions and possible responses can be found in subsection A.1.3.

One Month Post-Experiment Survey: I send the participants a survey one month following the conclusion of the main study period. They are told that if they fill out the survey they will have an opportunity to receive a $100 Amazon Gift Card, but it is separate from the experimental payment. The survey asks if they think they are spending a lot less, somewhat less, similar, somewhat more, or a lot more time compared to the pre-experiment levels of usage on their phone, social media in general, and each of the applications of interest. It also asks them to expand on why they think their behavior has changed, if they claim that it has. There are also a number of psychology questions asked in the survey, which I do not report here.

1.3.4 Experiment Timeline

The experiment timeline is as follows. There is an initial week where the software is set up on the devices and I remove participants where the software does not work at all with their phone. After all of the participants have the software set up on their devices, there is a week where I collect baseline, pre-restriction, time usage data. Following this, there is a two week restriction period, but some participants have no restrictions at all or restrictions that last only a week. Participants do not know whether they will have a restriction at all or which applications I target for the restrictions beyond the fact that it will be a non-essential social media or entertainment application. They are only informed of the restriction and its duration two hours before the restriction went into effect at 11:59 PM on Friday night so that they have limited time to anticipate the restriction. After the restrictions, there are two weeks where I collect time allocations when there are no restrictions, so that I can measure any persistent effects on behavior for the participants. Finally, the participants complete the endline survey and then, to ensure a degree of incentive compatibility for the WTA elicitations, two participants are randomly selected and potentially have an additional week of restriction depending on their survey responses and the randomly selected offer. The following summarizes the timeline:

- March 19th - March 26th: Participants complete the baseline survey and install required software
- March 27th- April 2nd: Baseline Usage period
- April 3rd - April 17th: Restriction period
- April 18th - May 2nd: Post-Restriction period
- May 3rd - May 10th: Additional Restriction for two participants

1.3.5 Experimental Restrictions

For the main experimental intervention, I restrict to participants that make use of either YouTube or Instagram. From the original 410 participants, 21 had phones that were incompatible with the parental control software and so were dropped from the study. There were 15 participants that did not use either YouTube or Instagram and so were given idiosyncratic applications restrictions.[23] The remaining 374 of the participants are the primary focus – 127 of which have YouTube restricted, 124 of which have Instagram restricted, and 123 which serve as a control group.[24] Within the set of participants that have Instagram blocked, 65 have it restricted for two weeks and 59 have it restricted for one week. Within the set of participants that have YouTube blocked, 64 have it restricted for two weeks and 63 have it restricted for one week. There was minimal attrition from the experiment with only 2 participants from the control group, 2 participants from the YouTube

[23] For most participants in this group this restriction comprised of Facebook or WhatsApp, but for some subset of participants this restriction was Twitch, Twitter, or Facebook Messenger.

[24] The remaining participants who did not use Instagram or YouTube were idiosyncratically restricted from a single application for one week. For most participants this was Facebook or WhatsApp, but it also included Messenger and Twitter as well.

restriction group, and 4 participants from the Instagram restriction group dropping from the experiment – in most cases due to reasons orthogonal to treatment (e.g. getting a new phone, tired of surveys). The experimental timeline, treatment assignments, and participant attrition are summarized in Figure A9.

In order to ensure that the experimental groups are balanced on usage of the applications of interest, I employ block randomization utilizing the baseline usage data from March 27th until April 1st. I categorize the quartile of usage for Instagram and YouTube for each participant and assign each participant into a block defined as the following tuple: (Instagram quartile, YouTube quartile). Within each block, I determine the treatment group uniformly at random (Instagram, YouTube, Control) and then again to determine whether the restriction is one or two weeks. The resulting distribution of usage across the treatment groups for the applications of interest can be found in Figure A11. It shows that the resulting randomization leads to balanced baseline usage between the groups both on the restricted applications as well as other social media applications.

1.3.6 Pilot Experiment

In order to get additional power for my experimental estimates, I will sometimes pool data with the pilot experiment that I ran between 9/29/2020 and 12/4/2020. The phone data collection software is the same as the main experiment, but there was no Chrome Extension for this version of the study. The primary differences between the two experiments are that the pilot experiment included several restrictions for each participant and the sample size was substantially smaller. The study consisted of 123 participants recruited from the Columbia Business School Behavioral Research Lab. Participants were similarly paid $50 for completing the study.[25]

The timeline for the study was as follows. Participants had a virtual meeting to set up the software from 9/29 - 10/10. The vast majority of participants were set up before 10/3, but a handful were set up between 10/3-10/10. There are two experimental blocks. The first block runs

[25] In order to ensure that there was little cross-contamination of participants from the pilot study in the larger study, different lab pools were utilized for the pilot vs. main study. However, to my knowledge, there were only 3 participants who overlapped between the two different experiments.

from 10/3 until 11/7. The period between 10/3 and 10/10 serves as the baseline usage for this block. Participants were randomized into group A and B on 10/10. Group A had a restriction on Facebook and Messenger together from 10/10-10/17, followed by a week of no restrictions, a week of YouTube restriction, and finally a week of no restrictions. Group B had no restrictions for 10/10-10/17, followed by week of Instagram restriction, a week of no restrictions, and finally a week of Snapchat and TikTok restricted together. In the second experimental block that runs from 11/7 - 12/4, participants were randomly assigned each week to either have a restriction or be in the control group. The period from 11/7-11/14 serves as a second week of baseline usage and the order of the restrictions across the weeks is as follows: Facebook/Messenger, YouTube, Instagram.

1.4 Descriptive Statistics

In this section I provide a basic overview of the data. I describe the demographics of the participants and how they spend their time, which mobile applications they use, how much they value the different applications, and how they use each of the applications of interest.

Participant Demographics: I report the gender and age of the participants in the study in Table A1 and Table A2 respectively. Given that the participants were recruited primarily through university lab pools, they are younger relative to the national average with an average age of 26 years old and a median age of 23 years old.[26] The participants, especially due to the fact that this study was conducted during the COVID-19 pandemic, were geographically distributed not just around the United States, but also the world.

Time Allocations: Figure A12 plots the distribution of daily phone and computer usage across participants during the baseline period. For both devices, the distribution is right-skewed and usage is quite substantial with participants averaging 3-4 hours of usage on each device per day. When considering the aggregate time spent across the devices, participants spend around 6 hours on average per day across their phone and computer. Figure A13 displays phone usage across the

[26]There were some exceptions to this, primarily from participants drawn from the Chicago Booth lab pool which attracts a more representative sample of the population relative to other lab pools. Thus, from this lab pool several older participants were recruited.

week, indicating that there isn't substantial variation in usage patterns across days. However, there is variation in usage patterns within the day with peak usage around lunch and in the later evening hours. Finally, Figure A14 displays self-reported time allocations throughout the experiment on other forms of media and life activities and shows that they are fairly constant over the course of the experiment. For the rest of the paper, I largely focus on the phone data, using the computer usage and the self-reported time allocations for robustness checks.

Table 1.1: Summary Statistics on Usage and WTA

Application	Mean Weekly Time	Median Weekly Time	Mean WTA	Median WTA	Mean WTA per Minute	Total Users
WhatsApp	173.81	92.17	$138.83	$50.00	$0.80	300
YouTube	297.46	90.50	$95.59	$40.00	$0.32	387
Instagram	201.02	125.00	$65.91	$35.00	$0.33	313
Facebook	132.21	30.50	$56.58	$25.00	$0.43	275
Messenger	58.43	5.50	$73.68	$25.00	$1.26	262
Reddit	131.83	25.75	$60.50	$25.00	$0.46	160
Snapchat	55.16	17.50	$64.23	$25.00	$1.16	181
TikTok	289.95	109.58	$59.70	$25.00	$0.21	84
Twitter	75.74	11.00	$48.53	$20.00	$0.64	170

Notes: Each row reports the statistics for the specified application. Usage and WTA is conditioned on participants with recorded phone data who use the application. Columns 1 and 2 report the mean and median weekly time of participants who report using the application. Columns 3 and 4 report the mean and median WTA value of the participants who report using the application. Column 5 reports the mean WTA value divided by the mean weekly usage. Column 6 reports the total number of participants who report using the application.

Applications Used: Next, I turn to understanding what applications participants spend their time on. Figure A15 plots both the distribution of the number of applications participants use as well as how many participants use each application. This reveals two distinct patterns. First, most participants use a large number of applications and there is a clear "long tail" of applications that are only used by a handful of participants. Second, Table A3 displays the summary statistics of the different phone categories and shows that most of the time on the phone is spent on communication, entertainment, or social media applications. For the rest of the paper, I aggregate across the long tail of applications and focus on the most prominent social media and entertainment applications.

Usage of Social Media and Entertainment Applications: I restrict attention to the most popular social media and entertainment applications. Despite the long tail observation, there is extensive multi-homing across these applications as observed in Figure A16, which shows that most participants use between 4 and 7 of the applications of interest. Table A4 displays the complete multi-homing matrix which computes the fraction of users of application X that also use application Y and finds no obvious clusters of usage patterns.

Table 1.1 provides summary statistics for the applications of interest on the reported value of each application as well as the amount of time spent on the different applications.[27,28] I report only participants that either stated in the activity question on the initial survey that they use this application or if there is recorded time on the application on their phone. Since these were elicited at the beginning of the study period, I compute summary statistics for the observed phone time during the baseline week. There are several takeaways from the summary statistics. First, the most used and valued applications among participants are Instagram, YouTube, and WhatsApp. There is a stark drop-off between these applications and the rest both in terms of value and time spent. Indeed, not only do more participants make use of and value these applications more, but, even conditional on usage, participants spend more time on them. This motivates the applications that I choose to restrict from participants. Second, distributions of value and time usage are both right skewed, especially for applications such as TikTok and YouTube, which motivates estimating treatment effects across the distribution and not just average treatment effects.[29] Furthermore, it means that there will be meaningful differences in interpreting the results of specifications using logs versus levels. The correlation between the average time spent and average value of the applications is confirmed by a more detailed analysis in section A.3 that finds that an additional minute of daily usage corresponds to a 5.8 cents increase in value.

[27]In the results reported here I drop participants that filled in the maximum monetary amount for each application.

[28] Table A5 reports the time allocations on the computer as well as the phone. It shows that for the applications of interest most of the time is spent on the phone with the exception of YouTube where participants spend a significant amount of time on the application on both the computer and the phone.

[29]It is important to further point out that my participants are for the most part consumers of content on these applications and do not post content that often. Table A6 shows that most participants are mainly consumers of content on applications such as YouTube, Reddit, and TikTok, while they most often post content on Instagram and Snapchat. However, even on these applications, there are not many participants who post at a relatively high frequency.

Table 1.2: Stated Activities

Application	Entertainment	Keep up with Friends	Communication	Get Information	Shopping	Total Users
Facebook	0.26	0.36	0.14	0.20	0.04	322
Messenger	0.01	0.08	0.88	0.02	0.02	287
Instagram	0.37	0.47	0.08	0.07	0.01	349
YouTube	0.78	0.002	0.002	0.22	0.002	403
TikTok	0.92	0.02	0.05	0.02	0.0	111
WhatsApp	0.01	0.06	0.92	0.02	0.0	320
Twitter	0.22	0.03	0.06	0.67	0.01	229
Snapchat	0.09	0.31	0.58	0.02	0.0	199
Reddit	0.38	0.0	0.02	0.60	0.01	240
Netflix	0.97	0.004	0.01	0.02	0.004	271

Notes: Each row reports the stated activities for the specified application. The final column displays the total number of participants who report using the application. The other cells report the proportion of participants who use the application and report using the application for the column purpose.

Qualitative Aspects of Usage: Finally, I explore some qualitative aspects of the applications of interest from the surveys. First, participants have heterogeneous usage of the same applications as observed by Table 1.2. This is important for the claim of cross category competition as it shows that applications with different application categories, such as Instagram and WhatsApp or Facebook and YouTube, have overlap in terms of their perceived usage by participants. This fact is important for rationalizing participants' substitution in response to the restrictions. Second, a significant fraction of the participants are psychologically addicted to social media. Figure A17 displays the number of addiction categories that participants exhibit according to their survey responses. This shows that 17% of the participants are addicted to social media under the most conservative definition and 51% under the less stringent definition.[30] This is important for understanding the possible behavioral mechanisms behind some of the experimental results as the restrictions may have persistent effects on the participants by breaking their addictive habits.

[30] According to [9], a conservative measure of addiction is when a participant marks 3 or higher on all categories. However, a less stringent definition of addiction is if a participant marks 3 or higher on at least four of the categories.

1.5 Experimental Results

In this section I analyze the substitution patterns of time allocations throughout the study period. I characterize what applications and activities are considered substitutes for the restricted applications by measuring participant substitution during the restriction period. I relate these substitution patterns to issues of relevant market definition. I then explore the extent to which there were persistent effects of the restriction by investigating how time allocations differ after the treatment period relative to before it. The insights from this section will be used to guide the demand model estimated in Section 1.6.

1.5.1 Time Substitution During the Restriction Period

I focus on understanding what applications participants substitute to during the restriction period.

Conceptual Framework

There are a wide range of possible activities that participants could substitute towards and it is challenging to define the precise substitution patterns that are most relevant to the question of consumer demand and merger analysis. There are two broad questions of interest that guide the analysis. The first is what *types of activities* do participants substitute to and the second is *how dispersed* across different applications are the substitution patterns. These questions are at the heart of the debate about monopolization arguments surrounding Facebook and, more generally, in merger evaluation between applications in this market.

Substitutable Activites: A directly relevant question to the ongoing debate between Facebook and regulators is which types of applications are most substitutable for the restricted applications. For instance, in [3] Facebook contends that it competes with a broad range of applications that compete for consumer time such as YouTube, which is not traditionally considered a social media application, whereas regulators contend that the most relevant competitors are other social media applications such as Snapchat. One of the challenges underlying this debate has been the lack of prices in these markets as standard market definition tests rely on understanding substitution with respect to price. Despite the lack of prices, the theoretical literature on two-sided media markets (starting from [35]) and the legal literature [39, 40] have noted that in these markets consumers face implicit costs on their time and attention that are direct choice variables for the application. This indicates that one alternative harm in lieu of higher prices is an increased cost on consumer attention, which can take the form of increased advertising load or decreased quality.[31]

Under this interpretation, the substitution observed during the restriction period is a limit case of taking "attention costs" to their choke values where no one would consume the application. Thus, it can serve as a conservative test of substitutability and, in particular, can function as the most conservative possible market definition – only including the applications and activities that are at all substitutable. This has appeal as a tool for practitioners as well since, in practice, variation in "attention costs" is substantially more ambiguous and difficult to come by relative to price variation in other markets. Furthermore, experiments such as the one analyzed in this paper are feasible due to the nature of digital goods.[32] Since the default approach taken by regulators has been to consider only applications within the same application category as relevant substitutes, a direct empirical question is whether there is only substitution within application category or across application categories as well. In order to study this in a disciplined manner, I use the categories assigned to the applications in the Google Play Store and characterize substitution across these different application categories. If I observe no cross-category substitution at this point, then the implication is that smaller increases in "attention costs" would similarly not lead to considerable substitution between these categories. If I do observe cross-category substitution, then it only says that such a market definition is not entirely unreasonable.

[31] [39, 40] propose modifications of the standard Small but significant and non-transitory increase in price (SSNIP) test explicitly considering this harm in lieu of the standard price test. This test was used in the FTC's lawsuit against Facebook by arguing that the Cambridge Analytica scandal was an exogenous decrease in quality through privacy harms and measured substitution in monthly active users to do the market definition exercise.

[32] Even without directly implemented experiments, natural experiments caused by product outages would induce similar variation and enable similar estimates. For example, extended outages such as the Facebook, WhatsApp, Messenger, and Instagram outage on 10/4/2021 could be utilized to a similar extent, `https://www.nytimes.com/2021/10/04/technology/facebook-down.html`.

Substitution Dispersion: Another important question is the extent to which substitution is concentrated towards a small number of prominent applications or dispersed among the long tail of applications. This captures a different dimension of competition relative to category substitution. This is because it focuses on understanding whether the set of substitutable applications are prominent applications that are likely more attractive to advertisers relative to smaller applications in the long tail. Furthermore, with the data collected during the study, I am able to observe whether participants actively seek out new applications in the long tail, indicating that the presence of these applications prevents this search process and that participants are unsure about appropriate substitutes. For instance, a participant that uses YouTube to keep up with the news or to get trading advice may not have a readily available substitute on their phone and go search in the Google Play Store for a new application if they are restricted from YouTube.

Empirical Specification

The primary empirical specification that I utilize to estimate the average treatment effect of the experimental interventions is as follows, with i representing a participant and j representing an application / category:

$$Y_{ijk} = \beta T_i + \kappa X_i + \gamma Y_{ij,-1} + \alpha_t + \epsilon_{ijk} \tag{1.1}$$

where Y_{ijk} represents the outcome variable of interest k weeks after their restriction, $Y_{ij,-1}$ represents the outcome variable of interest during the baseline period (i.e. the first week), T_i represents a treatment dummy, X_i represents a dummy variable for the block participant i was assigned to, and α_t denotes week fixed effects. The main parameter of interest is β; $Y_{ij,-1}$ controls for baseline differences in the primary outcome variable and X_i controls for the block assigned to the participant in the block randomization, which is standard for measuring average treatment effects of block randomized experiments [52].

For analyzing substitution patterns during restriction period, I consider Y_{ijk} as the average daily time spent on applications / categories during the days when the participant's software was active and logging data. When analyzing the substitution during the restriction period, I focus on the outcome variables only during the first week of the restriction. Due to this, I omit the week fixed effects and report heteroskedasticity-robust standard errors. When I consider multiple weeks of usage, as in subsection 1.5.2, I include this term and cluster standard errors at the participant level. I also consider Y_{ijk} as the number of newly installed applications, but for this outcome variable, I do not have any baseline data and so estimate the specification omitting the baseline usage term.

I am interested in not just the average treatment effects, but also effects across the distribution since, for instance, one might imagine that heavy users of an application or category would respond differently than infrequent users of an application or category at the baseline. As a result, I also estimate quantile treatment effects using the same specification. I estimate these effects using a standard quantile regression since the fact that treatment status is exogenous allows for identification of the conditional QTE with a quantile regression [53]. Finally, since the distribution of usage is skewed and, occasionally, sparse I consider the specifications in both logs and levels. In order to accomodate the zeros in my data, I use the inverse hyperbolic sine transform in lieu of logs, which leads to a similar interpretation of coefficient estimates [54].

Category Market Definition and Cross-Category Substitution

Cross-Category Substitution: I test the extent of cross-category substitution by measuring the average treatment effect of time substitution towards other categories as a result of the restriction. Table 1.3 displays the results for the Instagram restriction. Each cell in the table reports the estimated average treatment effect in order to make the results digestible. I consider the effects of each restriction on category usage separately. I report the results both from this experiment as well as pooled with the pilot study that included two separate restriction periods for different subsets of participants. For these results, I additionally control for the experimental period as well as cluster standard errors at the participant level. I report the results of each restriction on category time in levels, logs, and share of phone usage (i.e. not including time off phone). However, due to the skewed distribution of usage, I primarily focus on the log specification as it captures the response of the average participant and is not driven by the most intense users of the applications.

The overall amount of time spent on all social applications drops across all specifications (column 1), but the time spent on non-Instagram social applications increases by 22.4% (column 2). This means that there was considerable substitution towards other social applications, but not enough to entirely counteract the loss of Instagram. Column (3) indicates that there is some cross-category substitution to communication applications with the logs specification pointing to a marginally significant 10-12% increase in time spent on such applications. This is consistent with the qualitative evidence from the participants in section A.8. For instance, one participant stated *Instagram was restricted for me and because I mainly use it as a communication app, I was not significantly affected. I just used regular text, video call, and Snapchat to keep up socially.* I observe fairly precise null results for substitution from Instagram to entertainment or other applications.

Table 1.4 displays the results for the YouTube restriction. Similar to the results for the Instagram restriction, there is a sharp decrease in own-category time during the restriction period (see column 1). However, unlike the results of the Instagram restriction, there is a precise null of substitution towards other applications within the same category (see column 4). Column (1) points to an increase in time spent on social applications with a roughly 15% increase in time spent on these applications, while columns (3) and (5) suggest little increase in time spent on communication and other applications. Finally, Figure A18 displays the effects of the restriction along the entire distribution and shows that the own-category substitution for both applications is upward sloping across deciles, indicating that more intensive overall users of social media and entertainment applications respectively were more likely to look for close substitutes.

Table 1.3: Instagram Category Substitution

	Dependent variable:					
	Social	Social (No IG)	Communication	Entertainment	Other	Overall Phone Time
	(1)	(2)	(3)	(4)	(5)	(6)
Category Time	-18.922^{***}	4.129	3.618	-7.337	-6.760	-28.023^{**}
	(4.361)	(3.498)	(3.737)	(5.226)	(5.649)	(12.438)
Category Time - Pooled	-18.718^{***}	4.216^{*}	3.152	-0.569	-2.100	-15.199^{*}
	(3.117)	(2.476)	(2.776)	(3.894)	(4.153)	(9.191)
asinh(Category Time)	-0.461^{***}	0.220^{**}	0.127^{*}	-0.030	-0.095	-0.054
	(0.100)	(0.092)	(0.073)	(0.135)	(0.083)	(0.051)
asinh(Category Time) - Pooled	-0.595^{***}	0.224^{***}	0.102^{*}	0.075	-0.012	-0.044
	(0.101)	(0.078)	(0.057)	(0.098)	(0.065)	(0.048)
Category Share	-0.059^{***}	0.048^{***}	0.051^{***}	0.008	-0.001	-
	(0.014)	(0.013)	(0.013)	(0.016)	(0.015)	
Category Share - Pooled	-0.068^{***}	0.042^{***}	0.052^{***}	0.003	0.013	-
	(0.013)	(0.011)	(0.011)	(0.012)	(0.012)	

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

Notes: This regression reports the average treatment effect of average daily time spent on applications in different categories during the Instagram restriction. I only consider participants with software active at least 3 days in the baseline and treatment group. The columns show time spent on social, social (without Instagram), communication, entertainment, other categories, and overall phone time respectively. The entertainment category includes applications marked as entertainment, video players/editors, and comics. The column with social (without Instagram) aggregates social time across all groups excluding time spent on Instagram, both in the baseline and treatment periods. The first, third, and fifth rows display the primary specification estimated on data from the current experiment. The reported standard errors for these regressions are heteroskedasticity-robust standard errors. The second, fourth, and sixth rows display the primary specification estimated on data pooled from the current experiment and the pilot experiment. The reported standard errors for these regressions are clustered standard errors at the individual level, to accommodate the multiple treatments during the pilot study. The category share row measures the on phone share of time spent on the category.

Table 1.4: YouTube Category Substitution

	Dependent variable:					
	Social	Communication	Entertainment	Entertainment (No YT)	Other	Overall Phone Time
	(1)	(2)	(3)	(4)	(5)	(6)
Category Time	2.757	-0.615	-43.433^{***}	2.076	-4.538	-44.688^{***}
	(4.487)	(3.675)	(6.799)	(4.022)	(6.780)	(14.474)
Category Time - Pooled	3.985	-2.833	-46.415^{***}	-3.344	-3.666	-50.778^{***}
	(2.936)	(3.348)	(5.694)	(2.934)	(4.630)	(11.317)
asinh(Category Time)	0.159^{*}	0.013	-1.527^{***}	0.183	-0.045	-0.154^{***}
	(0.085)	(0.069)	(0.157)	(0.138)	(0.075)	(0.051)
asinh(Category Time) - Pooled	0.152^{**}	-0.044	-1.404^{***}	0.066	-0.060	-0.150^{***}
	(0.068)	(0.051)	(0.121)	(0.109)	(0.065)	(0.045)
Category Share	0.056^{***}	0.042^{***}	-0.136^{***}	0.013	0.035^{**}	-
	(0.014)	(0.011)	(0.016)	(0.009)	(0.015)	
Category Share - Pooled	0.056^{***}	0.025^{***}	-0.124^{***}	0.011	0.042^{***}	-
	(0.012)	(0.009)	(0.014)	(0.009)	(0.012)	

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

Notes: This regression reports the average treatment effect of average daily time spent on applications in different categories during the YouTube restriction. I only consider participants with software active at least 3 days in the baseline and treatment group. The columns show time spent on social, communication, entertainment, entertainment (without YouTube), other categories, and overall phone time respectively. The entertainment category includes applications marked as entertainment, video players/editors, and comics. The column with entertainment (without YouTube) aggregates entertainment time across all groups excluding time spent on YouTube, both in the baseline and treatment periods. The first, third, and fifth rows display the primary specification estimated on data from the current experiment. The reported standard errors for these regressions are heteroskedasticity-robust standard errors. The second, fourth, and sixth rows display the primary specification estimated on data pooled from the current experiment and the pilot experiment. The reported standard errors for these regressions are clustered standard errors at the individual level, to accommodate the multiple treatments during the pilot study. The category share row measures the on phone share of time spent on the category.

Survey Evidence of Cross-Category Substitution: In order to provide further evidence for cross-category substitution, I utilize the results from the hypothetical switching survey question asked at the beginning of the experiment. In this question, participants are asked to broadly assign which category of activities and applications they would substitute to if they lost access to the application. The results are reported in Table A10, which show that only 46% of participants stated they would switch to other entertainment applications in lieu of YouTube and only 23% stated they would switch to other social media applications in lieu of Instagram.

Indeed, for Instagram around the same percentage stated they would substitute to other hobbies compared to other social media applications. The large drop in own-category time and general time on digital devices paired with cross-category substitution would be consistent with these results. Furthermore, they are consistent with the heterogeneity in stated activities reported in Table 1.2. The fact that the uses of the applications are heterogeneous and intersect with applications that are not in the same formal application category helps to understand why I observe cross-category substitution. It further suggests a broader issue with using the functional product categories for applications whose content and use mostly come from user-generated content.

Table 1.5: Herfindahl–Hirschman Index Across Market Definitions

	Social	Entert.	Comm.	Social + Entert.	Social + Comm.	Social + Entert. + Comm.
Current Ownership	0.344	0.572	0.232	0.222	0.271	0.184
Independent Ownership	0.203	0.572	0.163	0.181	0.094	0.101

Notes: This table displays the Herfindahl–Hirschman Index (HHI) based on different application category market definitions using the baseline period data. I take the category(s) in each column as the definition of the market and compute the HHI of this market. The first row displays the HHI under the current ownership structure (i.e. Facebook owns Facebook, Instagram, Messenger, and WhatsApp). The second row displays the HHI if each of these applications was independently owned.

Implications for Market Concentration: A natural question is whether different market definitions would result in qualitatively different assessments of the degree of concentration in the market. I focus on the categories between which substitution was observed and compute the most common market concentration index, the Herfindahl–Hirschman Index (HHI), using the different combinations of application categories as market definitions.[33] Table 1.5 displays the results, separating out the measures by applications individually and then by incorporating Facebook ownership into the computation. Table 1.5 displays the results, separating out the measures by applications individually and then by incorporating Facebook ownership into the computation. An HHI above 0.25 generally indicates excessively high concentration. There are two main observations. First, multi-category market definitions leads to substantially lower estimated concentration than the application category market definitions alone. Second, despite this, market concentration would be substantially lower if each of the Facebook-owned applications was independently owned, regardless of whether the market definition was single or multiple categories.

Newly Installed Applications and Long-Tail Substitution

In this section, I analyze whether the restrictions induce the participants to substitute towards prominent applications or explore new applications and substitute towards the long tail of applications available in the Google Play Store. I use the fact that I observe the set of installed applications on the phone every day to construct a measure of the number of newly installed applications and the corresponding time spent on them. Furthermore, I characterize whether participants substitute towards applications in the Facebook ecosystem – Facebook, Messenger, WhatsApp, Instagram –, "major" applications, or "long tail" applications as a proxy to understand whether substitution is directed towards larger applications or scattered across the long tail of applications. I define "major" applications as those that are not in the Facebook ecosystem or core phone applications, but are in the top 25 applications in terms of average time usage in the baseline period.[34]

[33] HHI is defined as follows: $HHI = \sum_j s_j^2$.

[34] The set of major applications comprises of the applications: Reddit, YouTube, TikTok, Netflix, Twitter, Discord, Snapchat, Twitch, LinkedIn, Spotify, Zoom, Telegram, Hulu, Prime Video, Signal, Google, Amazon Shopping. I exclude time spent on Messages, Phone, Gmail, Clock, Gallery, Google Play Store, Camera, Browser, Chrome Beta, Drive.

Table 1.6: Newly Installed Applications During the Restriction Period

	Dependent variable:				
	Number of Applications Installed	asinh(Number of Applications Installed)	% change in Applications Installed	Time on New Applications	asinh(Time on New Applications)
	(1)	(2)	(3)	(4)	(5)
Instagram Treatment	0.239	0.022	0.003	1.436	0.083
	(0.644)	(0.107)	(0.004)	(1.471)	(0.164)
YouTube Treatment	0.900	0.174	0.005	3.584**	0.392**
	(0.641)	(0.106)	(0.004)	(1.462)	(0.163)
Block Control	Yes	Yes	Yes	Yes	Yes
Observations	364	364	364	364	364
R^2	0.052	0.050	0.035	0.042	0.031
Adjusted R^2	0.008	0.007	−0.009	−0.002	−0.013
Residual Std. Error (df = 347)	4.985	0.828	0.031	11.379	1.271

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

Notes: Reported standard errors are heteroskedasticity-robust standard errors. Columns (1) and (2) report the regression with the dependent variable as the total number of newly installed applications in levels and logs respectively. Column (3) reports the regression with the dependent variable as the % increase in new applications. Columns (4) and (5) report the regression with the dependent variable as the average daily time spent on these new applications in levels and logs respectively.

Newly Installed Applications: I construct a measure of the number of newly installed applications as follows. For each week, I collect the set of applications that had been detected to be installed on the phone at any point during the week.[35] Then, for each week following the baseline week, I compute the number of applications that were present on the participant's phones this week that were not present in the previous week, the time spent on these new applications during the week, and the percentage increase in total applications between the weeks.

I estimate specification (1.1) with the dependent variables as the number of newly installed applications and the amount of time spent on them. Similar to before, I focus on the first week of the restriction period with the results are reported in Table 1.6. I find that there is an imprecise

[35]Recall that the set of installed applications is pulled at the same time that the data is pulled from the parental control application and so occurs late at night.

increase in the number of newly installed applications for YouTube, but that there is a statistically and economically significant increase of 3.5 minutes per day in time spent on these applications. For Instagram, there does not appear to be an increase in the number of installed applications or a difference in the time spent on them. One interpretation of this result is that for Instagram the substitutes are more apparent to participants (e.g. Facebook), which leads to less need to install new applications. For YouTube, the substitutes are less apparent so participants are less likely to have readily available substitutes and thus spend more time off the phone as well as be more likely to explore new alternatives.

Table A18 further shows that a substantial proportion of participants not only believe they substituted towards other applications during the restriction, but also actively "invested" in them so that they could source better content from them. For instance, one participant wrote *"I had to figure out what I want from other applications I didn't know offered similar content before time, after the restriction elapsed, I had adjusted to sourcing for such content on both apps"*. This suggests that not only was there active adjustment in the extensive margin of installing new applications, but also adjustments to the extent to which participants more fully explored the capabilities of other applications.

Substitution to the Long-Tail: I now study whether participants are substituting to a few prominent applications or dispersed amongst the long tail of applications. To investigate this question, I use the same empirical specification as the cross-category substitution regressions, but consider the categories as overall time on the Facebook ecosystem, major applications, and long tail applications. Table A11 displays the results for Instagram. Indeed, while there is little observed substitution to "long tail" applications or other major applications, there is a clear pattern of substitution towards other Facebook-owned applications with a 17.9% increase in non-Instagram Facebook-owned applications. Table A12 displays the results for YouTube. The effects in this case are more muted with a clear drop in "major applications" due to the drop in YouTube time, but only a small amount of substitution towards the other categories. Once I condition on phone usage, I find that the largest share gain is to the Facebook ecosystem and the long tail applications. Thus, substitution for Instagram is more concentrated, in particular concentrated within the Facebook ecosystem, compared to the more dispersed substitution patterns observed for YouTube.

Off Phone Substitution

One possible concern is that since the restriction is only on the phone, participants may substitute to the restricted application on other devices, which would bias the previous estimates. This would understate substitution towards other applications since cross-device substitution would potentially replace time spent on other phone applications. However, given that I find cross-category substitution, this would mean that if there is cross-device substitution, then the estimates provided here are a lower bound and I am underestimating the extent of this substitution.

In order to assess the extent of cross-device substitution, I rely on the weekly subjective time use surveys and the data from the Chrome Extension. In the weekly surveys, the participants self-report how much time they spent on several applications off their phone. Table A13 displays the results on non-phone Instagram and YouTube time, which show negative point estimates on the time spent on both of the applications. Indeed, the estimates point to a statistically significant *reduction* in time spent on YouTube off the phone.[36]

The result that time on the restricted applications potentially *decreases* on non-phone devices seems implausible and possibly driven by biases in self-reported time usage data. The biases in such data has been pointed out by [7] in the context of social media usage. I use the data from the Chrome Extension in order to get an objective measure of how participants substituted, which allows me to validate whether the self-reported data is indeed biased or if it was the case that participants did not substitute at all across devices. Table A14 considers the same specification for the subset of participants that have installed the Chrome Extension. I estimate whether there was a change in overall computer time, Instagram time on the computer, and YouTube time on

[36]One possible worry is that participants are misinterpreting the survey and reporting aggregate time spent on the application across all devices. However, the survey was explicitly designed to include a grayed out column for phone time saying that it was automatically collected and then next to it including a time for other device time in order to minimize the likelihood of this occurring. Furthermore, I obtained the same result in the pilot experiment and this was the main reason I added the Chrome Extension in order to have a non-self reported measure of this quantity.

the computer. Table A14 finds little evidence that overall computer time changed as a result of the treatment. However, there is a marginally significant increase of 9.3 minutes of computer time on YouTube during the YouTube treatment and a statistically significant increase of 1.58 minutes of computer time on Instagram during the Instagram treatment. These point estimates indicate that there was a small amount of cross-device substitution due to the restrictions only being on the phone. In order to interpret the magnitude of the cross-device substitution, it is important to recall from Table A5 that the baseline usage of Instagram computer usage is only 1 minute a day on average. Furthermore, Figure A21 shows the time series of usage of the restricted applications across both devices and indicates that the aggregate usage of the applications drops dramatically during the treatment week.

Thus, the objective data provided by the Chrome Extension allows me to conclude that there was a small amount of cross-device substitution. This means that I am likely underestimating the degree of substitution towards other applications on the phone, but not too substantially. Furthermore, the discrepancy in the sign of the effect between the survey-based measures and the objective measure from the Chrome Extension highlights the importance of collecting non-self reported data for time allocations.

Beyond the extent of cross-device substitution towards the restricted application, there is a broader question of whether there are non-digital substitutes to the restricted applications. Column (6) of Table 1.3 and Table 1.4 displays the estimated average treatment effects for overall phone usage during the Instagram and YouTube treatments respectively. It shows that there is a reduction of 29 minutes and 44 minutes per day of phone time as a result of the Instagram and YouTube treatments respectively. The logs specification shows a lesser effect with a statistically significant and meaningful drop in phone time for YouTube, but an imprecise, negative point estimate for Instagram. Consistent with this, I find that this is primarily driven by reductions in phone usage of participants in the upper deciles of phone usage.[37] Figure A19 shows that while the YouTube

[37] Figure A20 plots the quantile treatment effects for each decile for logs of overall phone time. It shows that the QTE of the Instagram treatment is quite similar across deciles, whereas for YouTube it is more likely to be driven by reductions in the lower deciles.

restriction leads to fairly depressed phone usage throughout the entirety of the day, the reduction in phone usage for the Instagram treatment is largely in the afternoon and evening hours. Thus, it is plausible that, especially for Instagram, participants are substituting to non-digital substitutes during these hours. These results indicate that the restrictions led to substantial diversion towards the outside option, suggesting that many participants are unable to find viable substitutes and thus that the restricted applications have large market power over phone usage. It is unclear what activities off the phone participants are substituting to as Table A15 displays the estimated average treatment effect on the most natural off-phone substitutes, such as cable television, video games and streaming services, and finds no effect on time spent on these services.

1.5.2 Time Substitution After Restriction Period

In this section I explore the extent to which there are persistent effects as a result of the restrictions. This is important for understanding whether there are potentially dynamic elements of demand for such services and will be used to guide the demand model in Section 1.6.

Conceptual Framework and Empirical Specification

In order to build intuition for the mechanisms that may lead to dynamic considerations, consider the following informal dynamic model of demand for social media. There are $t = 1, ..., T$ time periods throughout the day. At each time period t, there is a focal activity that participants are engaged in. This could be being in class, hanging out with friends at a bar, doing homework at the library, cleaning their place of residence, going to sleep, etc. This focal activity is assumed not to include consumption of content on the applications, meaning that consumers never explicitly schedule to spend time on these applications. Activity on the applications of interest comes about for two possible reasons. First, at some time period t, with some probability they seek out specific information. For instance, this could be that they want to search for specific content – profiles of people they went to high school with, content that their friends sent, pictures of a party their friends went to from the previous weekend, etc. Second, at time period t, with some probability they want to “kill time" – for instance taking a break from studying/work, waiting for a friend to arrive at a restaurant, finding some entertainment when the focal task is boring (e.g. taking the subway).[38] This is consistent with the survey responses of some of the participants in the experiment as they said they were habituated to open up the application to take a break from their main tasks and sometimes attempted to do so though even though they knew the application was restricted.[39]

Let's consider the effect of the restriction on the first possible reason for using such applications – seeking out specific content. Suppose a participant who has their Instagram restricted wants to look up a specific profile on Wednesday during the restriction week. They have the following alternatives. They can either substitute to another application that may be able to retrieve similar content (e.g. if such information is available on Facebook instead of only just Instagram), they may substitute to the computer if there is some urgency, or they may intertemporally defer accessing this information until the restriction is over on Saturday, leading to a temporary spike in usage.[40,41] Given this, I formulate the following hypothesis:

Hypothesis 1. *There is a spike of usage on the day the participants get the restricted application back. Furthermore, this spike is larger for the two week restriction group compared to the one week group.*

[38] In the mid-2000's these perpetual short periods of downtime were termed “micro boredom" by Motorola, indicating that the mobile phone could fill up these brief lulls in life.

[39] As some examples from survey responses from participants on how they dealt with the restrictions:

- *At first restricting instagram was frustrating as I had the application on my homescreen and built muscle memory for the past 4 years to press that part of the screen where the instagram shortcut is. I removed instagram from my home screen and after 5 days of the restriction i completely realized instagram was nor important at all for me and only time i open it is when i receive a direct message.*
- *I kept opening instagram time after time forgetting that is was blocked*
- *It's strange, because I didn't feel like I needed YouTube, I just knew I had spent a lot of time on it. However, when it became restricted, I noticed how much time I had spent simply laying about and watching YouTube. It felt weird knowing that my instinct was to immediately press the YouTube button when I got bored, and I realized I perhaps need/use it more than I think.*

[40] Indeed, one participant noted that *It was a little annoying especially whenever my friend shared something that can only open on that platform. But after a couple of days I was able to make my peace with it.*

[41] While the possibility of computer substitution would dampen this effect, especially if the most important content is the one that consumers would be less likely to intertemporally substitute and more likely to substitute immediately to a different device, one would expect that if it is present at all there should still be a spike in activity the day that participants get the application back on their phones.

The effect of the second possible reason for using such applications – “killing time" – does not imply such intertemporal substitution. The fact that I want to view Instagram to kill time on the subway on Wednesday does not mean that I will delay this until I get Instagram back on Saturday since the main utility from this is to make my subway experience less boring – not anything to do with particular content on Instagram. However, it does imply that there is a *habitual* aspect of this behavior that could play an important role in determining demand. If I am habituated to open up Instagram or YouTube to entertain myself, then this will naturally increase the amount of time spent on these applications and their resultant market share. The experiment could reveal the extent to which this is present as the restriction serves as a shock to the habits of the participants and, if such an effect is present, may lead to changes in how participants spend their time after the restrictions. The underlying assumption driving this is that the restriction will not change how the participant's spend their day (i.e. which focal activities they engage in), but could change their habits which would change the activity that they decide to engage in to fill up the void of time. This leads to the following two hypotheses:

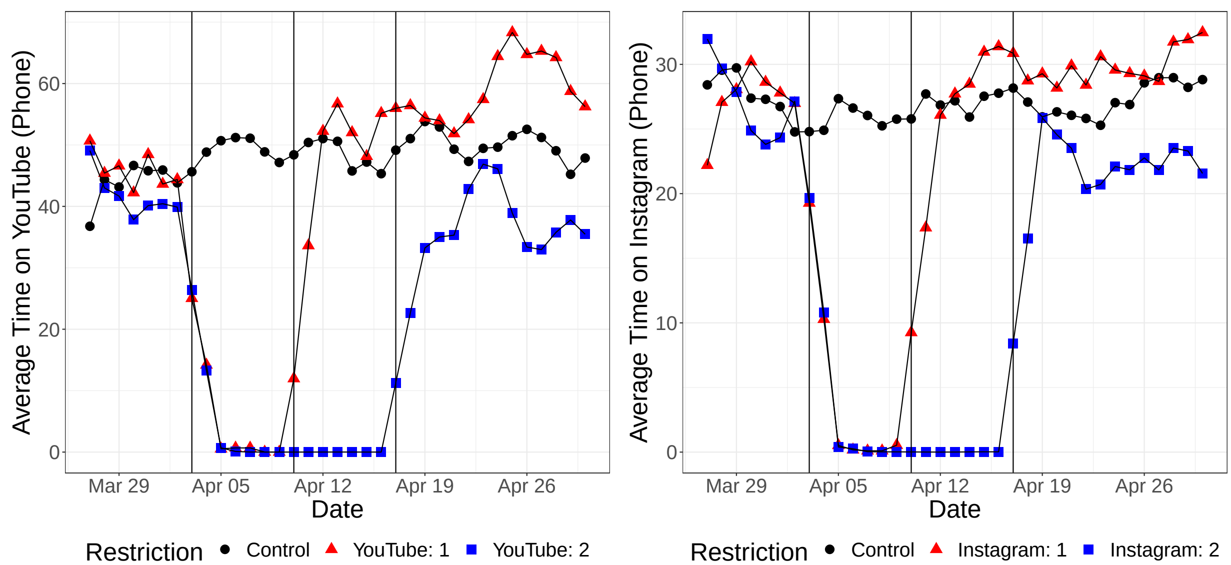
Hypothesis 2. *There is a persistent change in usage of the restricted applications once the restrictions are lifted. Furthermore, this persistent change is larger for the two week restriction group compared to the one week group.*

Hypothesis 3. *There is a persistent change in usage of applications that participants substituted to during the study period. Furthermore, this persistent change is larger for the two week restriction group compared to the one week group.*

In order to empirically test these hypotheses, I consider the same specification as in Section 1.5.1 with (1.1) as the primary specification that I estimate. I consider the two weeks after the restrictions for the participants, $k \in \{1, 2\}$, in order to test Hypothesis 2 and Hypothesis 3. Since, for these hypotheses, there are potentially important differences between restriction lengths, I will also estimate heterogeneous treatment effects across the restriction lengths. For all of these regressions, I cluster standard errors at the participant level. Finally, I directly test Hypothesis 1 by comparing the differences in the mean usage across the different treatment groups on the day when the restricted application is unblocked.

Experimental Test of Dynamic Effects

Figure 1.1: Time on Restricted Applications



Notes: This figure plots the smoothed average daily usage on Instagram (left) and YouTube (right). Each point is the average of the last 3 days of usage (including the current date). Each figure plots the usage of the control group, one week and two week restriction group for the application.

Intertemporal Substitution after Restriction: I plot the time series of the application usage across the different treatment arms. Figure 1.1 plots the time series of the moving average of time spent on the restricted application for the control group, the one week restriction group, and the two week restriction group. There are two striking patterns. First, in both treatments, the one week restriction group appears to jump back to the pre-experiment levels almost immediately after the restriction is lifted. Second, in both treatments, the two week restriction group does not appear to return to the pre-restriction levels and there is no evidence of a spike in usage on the day the restriction is lifted. Figure A21 shows that the same trend appears to hold when I plot the raw time series and if I include time logged from the Chrome extension. Thus, I reject Hypothesis 1 based on this, but note that I cannot rule out that part of the reason that I do not see an effect on this dimension is because the treatment allows for substitution to the computer.[42]

Persistent Effects on Restricted Applications: Figure 1.1 points to the fact that there may be a persistent reduction in usage after the restriction, in particular for the 2 week treatment group. I estimate specification (1.1) with heterogeneous effects across restriction lengths and for the 2 week group alone with the results reported in Table A16 and Table A17. Columns (1) and (2) of Table A16 and Table A17 show the change in restricted application time in levels and logs for Instagram and YouTube respectively. For Instagram, there is a statistically significant difference in post-restriction time between these two for the levels specification and the 2 week restriction group. Furthermore, columns (3) and (4) drops the 1 week group entirely and estimates the treatment effect for only the 2 week group. This further confirms that there is a drop of approximately 5 minutes of time on Instagram on average for the Instagram restriction group. For YouTube, there is a negative, but imprecise point, estimate for both specifications. Given the skewed usage distribution and the discrepancy between logs and levels, one might expect that the changes in post-restriction usage are driven by those at the high end of the usage distribution. Figure A22 estimates the QTE of post-restriction effects and confirms this intuition.

A natural question is, if such post-restriction effects exist, how persistent are they? It is plausible that these effects dissipate very quickly, but I only observe participants for 2-3 weeks following the restriction. In order to understand how much longer the effects last, I rely on an optional survey that was sent one month following the conclusion of the study asking how they had been spending their time relative to before the experiment.[43] Participants could mark whether they were spending a lot less time (1), somewhat less time (2), the same amount of time (3), somewhat more time (4), or a lot more time (5). They could also mark if they did not use the application or had started to use it during the study period. I estimate the impact of the restrictions on overall phone, overall social media, Instagram, and YouTube usage. Table A19 displays the estimated average treatment effect, which shows that there is still a large drop in the Instagram treatment group's overall social media

[42]Even though I observe that this substitution is minimal it's possible that this usage was more "directed" for specific usage which dampens the possible intertemporal effect.

[43]Participants were incentivized by being able to enter the chance to win a $100 Amazon Gift Card by completing the survey. However, they had already been paid their experimental payment after the conclusion of the study period.

and Instagram usage. This result must be caveated for the following two reasons. First, there is potential for selection bias since participants with stronger responses to the treatment may be more willing to respond. However, roughly an equal number of participants from both the treatment and control group responded, indicating this may not be a large concern.[44] Second, these are unverifiable survey responses, so it is possible that some of the results are driven by experimenter demand. Subject to these caveats, these results show that a one or two week restriction led to a reduction in usage *nearly two months* later. Combined with the other results, this provides evidence that there were persistent effects of the restrictions and thus support for Hypothesis 2.

Persistent Effects on Non-Restricted Applications: Table A20 and Table A21 provide estimates for persistent changes on usage of non-restricted applications as a result of the Instagram and YouTube treatments. I focus on applications / categories where I observed substitution towards during the restriction period and the applications installed during the restriction period.[45] I find little evidence of persistent changes in usage along these dimensions. The only notable persistent increase is in the amount of time spent on applications that were installed during the restriction period for YouTube. There is a marginally significant increase in time spent on Instagram for participants in the YouTube treatment.[46] However, beyond this, there are minimal persistent changes on other applications. It must be noted that these are average treatment effects and I observed heterogeneous substitution during the restriction period itself, so it does not rule out that there were persistent changes in time usage but that these are so heterogeneous that they would not be picked up by this specification. Indeed, Table A18 indicates that participants self-report having persistent effects on other applications, but the effect sizes may be too small for them to be detectable given the power of the experiment. However, given this evidence, I reject Hypothesis 3.

[44] Note that since there is only partial response I do not include the controls for randomization block.

[45] In order to economize on space I do not include in the interaction term in the reported estimates. Instead, I estimate the ATE of the persistent for both restriction lengths (without an interaction) and then report point estimates for the 14-day treatment group alone.

[46] A similar effect was observed during the pilot study.

1.6 Model of Time Usage with Inertia

Motivated by the experimental results, I estimate an individual level discrete choice model of time usage with inertia. There are two experimental results in particular that point to the importance of inertia. First, participants spent time on newly installed applications and persisted to use these applications, even once the restriction period was over. Second, there is a reduction in usage of the restricted application in the post-restriction period, especially for the heaviest participants. One interpretation of this result is that the restriction serves as a shock to participants' habits, which induces them to reduce their usage of the application. In the remainder of the section, I will detail and provide estimates of the model. The main outputs of interest from the model are to quantify the role of inertia in usage and produce diversion ratios between the considered applications. These will provide a more refined view of substitution between the applications of interest.

1.6.1 Model and Identification

I model participant's choices as a panel of discrete choices. This is a formalization of the informal model in Section 1.5.2 where the participant chooses a single application at each time period to use and this choice is partially determined by the habits of the participant.[47,48] There is a set of applications $\mathcal{J} = \{1, ...J\}$, indexed by j, and a set of participants $\mathcal{I} = \{1, ..., I\}$, indexed by i. I consider each application restriction as its own separate "market", indexed by k, which dictates the choice set available to consumers.[49] I use the disaggregated time period data, denoted by t, at the time interval of 15 minutes.[50] Participant i receives the following utility from application j in

[47] Other models of time demand for applications such as [44] and [45] consider a multiple discrete-continuous framework. [55] takes a similar approach as mine when considering time allocation demand for channels on cable television. One benefit to the discrete choice approach is that it enables me to flexibly control for variation in usage throughout the day and week, which is apparent in Figure A13, as well as directly incorporate past usage into the utility function.

[48] Recall that in the experimental section there was little evidence of intertemporal substitution, evidenced by the lack of a spike of usage on the day the restriction was returned. Thus, the formulation assumes that participants are myopic in their choices so that their usage does not directly take into account how their choice this period will impact future period usage.

[49] Beyond the experimental restrictions to Instagram and YouTube, some participants were restricted from Twitter, WhatsApp, Facebook, or Messenger since they did not use the main experimental applications of interest.

[50] In order to construct this I compute the time allocations allotted to each application in each interval, including off the phone time, and assign the chosen application as the maximum of these quantities. I aggregate the minute by

market k and time period t:

$$u_{ijkt} = \beta^{q(i)} \cdot h_{ijt} + \zeta^{q(i)} \cdot r_{ijt} + \omega^{q(i)} \cdot r_{ijt}^2 + \gamma_j^{q(i)} + \kappa^{q(i)} \cdot ac_{ij} + \epsilon_{ijkt} \tag{1.2}$$

$\gamma_j^{q(i)}$ denotes application fixed effects, ac_{ij} incorporates the subjective usage of application j, which comes from Table 1.2, for participant i, and ϵ_{ijkt} is the Type-1 Extreme Value error. $q(i)$ denotes the type of participant i that is determined by running k-means on the aggregated baseline data of the considered applications in order to group participants into different types. Thus, the specification accommodates preference heterogeneity across participants both by having type-specific estimates of the coefficients and by incorporating the subjective uses of the applications directly into the utility function.[51]

The main parameters of interest are those that relate to consumer inertia. There are broadly two types of inertia effects that are present – short-term and long-term inertia. The primary interest is in understanding long-term inertia, but it is important to account for short-term inertia. Short-term inertia accounts for the fact that a participant is more likely to choose application j in period t if they used the application in period $t-1$. I include a term, r_{ijt}, which is defined as the number of directly consecutive periods which participant i has used application j. Since this short-term component potentially has satiation effects, it enters both linearly and quadratically into the utility function. It is important to emphasize that the short-term inertia is largely a nuisance term that allows me to better estimate the more important longer term aspect of inertia.[52]

The second type of inertia, which is my primary interest, is the longer-term "habit" portion of usage. In order to capture this, I formulate this in a similar manner as [11] with a continuous stock of usage that the participant accumulates. Motivated by the apparent difference in long-run behavior between the one and two week restriction groups, I define the habit stock, h_{ijt}, as the total

minute data to a 15 minute interval so that the computations are not too cumbersome, but the estimates from the model are nearly identical if I use smaller time intervals.

[51] Incorporating some degree of preference heterogeneity is key in order to ensure that the estimates on the inertia terms are not positive due to misspecification [56, 10].

[52] Without properly controlling for short-term inertia, it is likely that the ϵ_{ijkt} would be serially correlated which would bias the estimates of $\beta^{q(i)}$.

amount of time participant i has spent on application j in the past two weeks.[53] Thus, under this formulation, the experiment induces a shock to the longer-term habit portion of usage, but, beyond the initial period when the restrictions are lifted, not the short-run inertia portion.

The granularity of the data allows me to vary the outside option flexibly across time. For any time index t, I allow the outside option to vary across the week of the experiment $w(t)$, day of the week $d(t)$, and hour of the day $o(t)$. I collapse the hours of the day into morning (7 AM - 12 PM), afternoon (12 PM - 6 PM), evening (6 PM - 1 AM), and late night (1 AM - 7 AM). I normalize the outside option to zero at afternoons, Fridays, and the final week of the experiment. Thus, the utility for the outside option is denoted as follows where $\alpha_{o(t)}$ denotes hour of day fixed effects, $\iota_{d(t)}$ denotes day of week fixed effects, and $\mu_{w(t)}$ denotes week fixed effects:

$$u_{i0tk} = \alpha_{o(t)} + \iota_{d(t)} + \mu_{w(t)} + \epsilon_{i0tk}$$

The assumption that ϵ_{ijkt} are independent and identically distributed according to a Type-1 extreme value distribution induces the following probability that application j will be chosen by participant i:

$$l(h_{ijt}, a_{ij}, r_{ijt}; \theta) = \frac{\exp(\beta^{q(i)} \cdot h_{ijt} + \zeta^{q(i)} \cdot r_{ijt} + \omega^{q(i)} \cdot r_{ijt}^2 + \gamma_j^{q(i)} + \kappa^{q(i)} \cdot ac_{ij})}{\exp(\alpha_{o(t)} + \iota_{d(t)} + \mu_{w(t)}) + \sum_{j'} \exp(\beta^{q(i)} \cdot h_{ij't} + \zeta^{q(i)} \cdot r_{ij't} + \omega^{q(i)} \cdot r_{ij't}^2 + \gamma_{j'}^{q(i)} + \kappa^{q(i)} \cdot ac_{ij'})} \tag{1.3}$$

Identification: The primary parameter of interest is $\beta^{q(i)}$. The identification argument for this parameter is as follows. First, following the literature on state-dependent demand estimation (e.g. [10]), it is important that the specification sufficiently captures individual preference heterogeneity. This is captured by incorporating the subjective usage of each of the considered applications as well as having type-specific estimates.[54] Second, the experiment induces exogenous variation in the

[53] Note that there is an initial conditions problem at the beginning of the experiment since there is no previous data to use to define this. Because of this I drop the first two days of data entirely from the estimation and, for any date in the first two weeks, I multiply the accumulated "stock" by the inverse of the fraction of the current time period by the time period exactly 2 weeks from the start of the experiment. I chose two days since the descriptive statistics point to usage not varying drastically across the days of the week and preliminary experiments showed that after two days the habit stock variable is fairly constant in the baseline period.

[54] The biggest worry about unobserved heterogeneity in usage comes from the extreme users of specific applications

habit stock of the restricted applications as well as the other applications (via substitution during the restriction period). Thus, the core identification assumption is that the experiment only serves as a shock to the habits of the participants.

Estimation: I restrict myself to the most prominent social media and entertainment applications – Facebook, TikTok, Twitter, Reddit, YouTube, Instagram, and Snapchat – and denote every other application or off phone activity as the outside option. For these applications, I collect the average daily usage in the baseline period for each participant and cluster the participants according to k-means. I then estimate the model separately for each type. Since my model is likelihood-based, I estimate the parameters using maximum likelihood estimation.

1.6.2 Model Estimates and Validation

The first step of estimation requires classifying the participants into different types using k-means. There is a large literature in data mining and statistics about choosing the "optimal" k that trades off the parsimony of having fewer clusters against the reduction in within-cluster variance that arises from additional clusters. In this case an additional consideration is that it is important to ensure that the clusters have sufficiently many individuals to allow for estimation of the parameters of interest for this group, but also having sufficiently many clusters to capture the unobserved preference heterogeneity. I consider an index of these measures for choosing the "optimal" k which reports $k = 3$ and $k = 6$. In order to accommodate additional heterogeneity in consumer preferences, I utilize $k = 6$.[55] Figure A23 displays the resulting clusters and the time allocations within each of them. The resulting clustering of participants identifies sets of power users. Cluster 1 captures the more typical users of these applications who have moderate usage of each of the applications. Clusters 2 and 3 capture the YouTube intensive participants. Cluster 4 captures the

or bundles of applications. The clustering formulation is able to capture the differences in preference intensity for these participants and consider separate estimates for them. The approach of discretizing a potentially continuous distribution of unobserved heterogeneity through k-means has precedent in [57].

[55] I additionally consider density-based spatial clustering of applications with noise (DBSCAN) [58] and spectral clustering [59] which are clustering algorithms that do not restrict themselves to convex regions. Following best practices for the methods, I find that they do not result in substantially different clusterings with DBSCAN leading to 3 clusters and spectral clustering leading to 7. Both pick out similar clusterings as k-means with $k = 3$ and $k = 6$, so I opt for using k-means.

power users of Reddit. Cluster 5 identifies participants who are power users of TikTok, but also use the other social media applications extensively. Cluster 6 identifies participants who are power users of Facebook and Instagram.

The estimates from the model are presented in Table A24. I report the estimates of each type separately. The first observation is that the coefficient on h_{ijt} is fairly consistent across the different types as well as the estimate for the influence of short-term inertia, r_{ijt} and r^2_{ijt}. Both of these terms are statistically different from 0, indicating that both the short-term and long-term inertia channels play a role. The coefficient on r^2_{ijt} is negative, indicating satiation effects. The differences in the natural usage of each of the applications across the different types, which is reflected in Figure A23, naturally translates to differences in the estimated application fixed effects. The estimated time fixed effects that vary the outside option are similar across the different types and follow the variation in phone usage across the week depicted in Figure A13. The coefficients on the different subjective uses of the applications varies across the types in accordance with the most used applications by participants classified as that type.

I validate the in-sample fit of the model by comparing how well the model is able to match the actual market shares throughout the study period. Recall that the model is estimated including the restriction period. Thus, I compare how well the model is able to predict the average market shares in the non-restriction period in addition to how well it is able to predict the substitution during the restriction period. In particular, I compare how well the model is able to capture substitution towards other applications and the outside option in the Instagram and YouTube restriction periods respectively. Table A25 shows that the model fits the data reasonably well as it matches the non-restriction period market shares and predicts the extent of substitution towards other applications and the outside option as a result of the experimental restrictions.[56]

[56]The estimates of the main parameters of interest are robust to model specification. Preliminary specifications did not include application fixed effects, but rather only included application characteristics and were estimated over the entire sample. Over these different specifications, the model had a poorer fit of the overall data, but had a similar estimate of roughly 0.01 for h_{ijt} across the different types. Furthermore, omitting either the short-term inertia or subjective application usage from the utility function leads to an increase in the coefficient of h_{ijt} but does not change the resulting estimates or main outputs from the model dramatically.

Table 1.7: Second-Choice Diversion Ratios

	Instagram	Twitter	YouTube	TikTok	Reddit	Snapchat	Facebook	Outside Option
Instagram	-	0.0047	0.023	0.0063	0.0029	0.0072	0.013	0.94
Twitter	0.027	-	0.025	0.013	0.0041	0.0059	0.012	0.91
YouTube	0.024	0.0044	-	0.0071	0.0081	0.0055	0.011	0.94
TikTok	0.028	0.0092	0.03	-	0.0029	0.019	0.012	0.9
Reddit	0.014	0.003	0.034	0.0034	-	0.0052	0.0079	0.93
Snapchat	0.026	0.0036	0.022	0.018	0.0047	-	0.011	0.91
Facebook	0.026	0.0042	0.023	0.0061	0.0037	0.0064	-	0.93

Notes: This table displays the estimated second-choice diversion ratios that come from the estimated model. The cell in each row k and column j is computed by $D_{kj} = \frac{s_j(\mathcal{J}\setminus\{k\}) - s_j(\mathcal{J})}{s_k(\mathcal{J})}$.

The primary output of the estimated model is the second-choice diversion ratio. The second-choice diversion ratio between application j and k provides an estimate of what fraction of consumption of application k would shift from application k to application j when application k was removed from the choice set. Typically, regulatory authorities use second-choice diversion ratios coming from switching surveys as a critical input to merger evaluation [51, 8] and this input will be crucial for the merger exercise conducted later in the paper. In order for the model to provide reasonable estimates for this quantity it is important that it is able to predict how participants would substitute towards the other applications if the application was not available. The model validation exercises showed that the model is able to do this for the Instagram and YouTube restrictions and thus ought to provide a reasonable estimate of this quantity. Table 1.7 displays the estimated diversion ratios, which are given by $D_{jk} = \frac{s_j(\mathcal{J}\setminus\{k\}) - s_j(\mathcal{J})}{s_k(\mathcal{J})}$. Each of the predicted shares is computed as before, by a weighted average over the different types according to the fraction of participants assigned to a type. The diversion ratios across each of the different applications predict a large amount of diversion to the outside option, with Instagram and YouTube having the highest diversion towards the outside option.

An alternative approach to compute the second-choice diversion ratios is to directly utilize the estimated average treatment effects of substitution during the product unavailability period. [8] provide a treatment effects interpretation of second-choice diversion ratios that does this and, in [32], provide a method to compute the rest of the matrix of diversion ratios for applications without experimental variation. In section A.5, I apply their methods to get an alternative estimate of the diversion ratios, shown in Table A22. The estimated diversion ratios are similar to those estimated using the model. The main noticeable difference is that the diversion between, for instance, Instagram-Facebook, YouTube-Instagram, and Instagram-TikTok is noticeably larger than the estimates produced from the model. This is due to the fact that these are nonparametric and driven by the difference in point estimates of the treatment effects. The nonparametric diversion ratio estimates are displayed in Table A23, which shows the degree of imprecision in their estimation. This is since the power requirements to get precisely estimated diversion ratios are not just being able to distinguish the estimated substitution towards one application as being greater than zero, but rather being able to distinguish estimated substitution towards one application as being greater than other applications with positive substitution. Thus, my experiment is not sufficiently powered to determine whether these differences in the estimated diversion ratios come from imprecision or actual differences. As a result, I rely on the estimated diversion ratios from the model.[57]

1.6.3 Counterfactual: No Long-Term Inertia

The counterfactual I consider is to understand the role of the long-term inertia channel in driving the usage of the social media and entertainment applications. Concretely, I impose $\beta^{q(i)} = 0$ and characterize the change in the resulting market shares and diversion ratios, which will allow me to consider how this channel influences merger assessments in Section 1.7. It is important to understand the interpretation of this counterfactual since it is not a direct policy counterfactual. This inertia channel comprises a number of different aspects of usage – ranging from addictive impulses to more natural mechanisms such as switching costs — and I provide several interpretations of the counterfactual that are directly motivated by regulatory and antitrust concerns.

[57] Another important consideration is that this method does not allow me to consider how the diversion ratio changes with and without inertia.

First, while some aspects of inertia are natural components of application choice, there are addictive elements of these applications and a number of policy instruments have been proposed for alleviating this issue. Indeed, the evidence from the baseline survey indicates that, for my sample, one of the behavioral mechanisms at play is addictive concerns as a substantial fraction of the participants are psychologically addicted to social media services. There has been a contention that some of this addiction may arise from design choices made directly by these applications. For instance, the objective function of the content curation algorithms and design patterns such as infinite scroll news feeds encourage excessive usage of these applications [60]. As a result, a number of policy proposals have been raised aimed at regulating these aspects of the applications in order to indirectly impact the extent of addiction to these applications, while other policy proposals aim to directly limit the time spent on these applications in order to directly address the problem. One interpretation of the counterfactual is to understand the effect of these proposed regulations by shutting down the role of these addictive concerns in usage. However, since the long-term inertia channel in my model comprises a number of different behavioral mechanisms beyond the addictive concerns, the resulting market shares and diversion ratios can be viewed as a limit case that characterizes an upper bound for how such policies would impact usage and diversion.

Second, the diversion ratios produced under the no inertia counterfactual are more natural measures of substitution between the applications than in the baseline. This is since the baseline diversion ratios include the extent of diversion due to both direct substitutability and habitual usage. This leads to naturally higher diversion for applications with higher habitual usage (e.g. Instagram, YouTube). Thus, the diversion ratios produced by the no inertia counterfactual are plausibly more policy-invariant measures of diversion that focus only on the direct substitutability of any given two applications. This measurement of diversion is useful for evaluating a common type of merger in this market where a prominent application acquires a nascent application (e.g. see [61] more generally and [62] for mobile applications specifically).[58] In this case, the prominent application

[58]This was one purported motivation for instance behind the Facebook and Instagram merger, see `https://www.theverge.com/2020/7/29/21345723/facebook-instagram-documents-emails-mark-zuckerberg-kevin-systrom-hearing`.

acquires the nascent application before it is able to build up the habit stock of consumers and so the estimated measures of diversion in the baseline may be low mainly due to the fact that the nascent application has not been around sufficiently long to build up the habit stock of consumers. Thus, the more direct measures of diversion produced under this counterfactual could be useful for evaluating these types of mergers.

Table 1.8: Market Shares (No Inertia)

Application	No Inertia: Weeks 1,4,5	Baseline: Weeks 1,4,5	No Inertia: Weeks 4,5	Baseline: Weeks 4,5	No Inertia: Week 1	Baseline: Week 1
Instagram	0.0148	0.0276	0.0149	0.0276	0.0145	0.0275
Outside Option	0.941	0.901	0.94	0.899	0.943	0.904
Twitter	0.00364	0.0044	0.00371	0.0045	0.00345	0.00411
YouTube	0.0191	0.0339	0.0195	0.0347	0.0179	0.0318
TikTok	0.00388	0.00797	0.00392	0.00817	0.00376	0.00743
Reddit	0.00471	0.00644	0.00479	0.00673	0.00447	0.00563
Snapchat	0.00456	0.00679	0.00456	0.00682	0.00456	0.00672
Facebook	0.00852	0.0122	0.00858	0.0121	0.00833	0.0124

Notes: Columns 1 and 2 display the predictions of the model over week 1, 4, and 5 including the long-term inertia term and without. Columns 3 and 4 display the prediction of the model only over weeks 4 and 5. Columns 5 and 6 display the prediction of the model only over week 1. Each cell displays the market share of the row application under the specification designated by the column.

Table 1.8 compares the average market shares with and without the inertia term across different weeks of the experiment when participants had the full set of applications available to them. Since the results across the different subsets of weeks are quantitatively very similar, I restrict focus to the first two columns which compare the differences across all weeks in the experiment. The first observation is that the overall market share of the set of inside applications drops by nearly

40% when this channel is shut down.[59] Table A28 displays the reduction of usage in percentages, showing that YouTube, Instagram, and TikTok have the largest percentage reduction in average usage when this channel is shut down. Recall that TikTok in particular has a smaller number of users in my sample relative to the other applications, but, conditional on using the application, has one of the highest average time allocations. As a result, it is not too unsurprising that the model predicts that inertia is a large driver of usage for this application.

I further compute the estimated second-choice diversion ratios when the inertia channel is shut down. The estimates are displayed in Table A26 with the percentage differences between the baseline and no inertia case presented in Table A27. There is a drop in the diversion ratios from other applications towards the most prominent applications such as Instagram or YouTube, but there is not a reduction across the entire matrix of diversion ratios. For instance, there is an increase in diversion from Instagram to Reddit as well as from Snapchat to Twitter, which indicates that the smaller applications in my sample can actually benefit from the lack of inertia for the larger applications such as Instagram or YouTube.

1.7 Merger Analysis

In this section I conduct hypothetical merger analysis between the prominent social media and entertainment applications. I propose a variant of the standard Upward Pricing Pressure (UPP) test for attention markets and then use the estimated diversion ratios from Section 1.6 to implement this test. I implement the test using the estimated diversion ratios with and without inertia and characterize which merger decisions would change due to the influence of inertia.

[59] This observed decrease is qualitatively similar across other model specifications. Allowing application-specific coefficients on h_{ijt} leads to a quantitatively identical change in the overall usage of the applications. A separate model model specification that did not include application fixed effects and instead used application characteristics reported a range of 32-40% reduction in overall usage. Furthermore, concurrent work by [12] estimates a similar quantity using a substantially different model specification, a slightly different set of applications, and experimental design and find a 31% reduction.

1.7.1 Upward Pricing Pressure Test

In order to evaluate a merger between two applications, I need to specify the profit function of the applications and, importantly, their choice variables. I retain the same notion discussed in Section 1.5.1 where the primary choice variable of the applications is to set the attention costs faced by consumers. I follow the literature on two-sided media markets by supposing that the primary manifestation of this is through choosing the advertising load of the application – the number of advertisements per unit time experienced by the consumer. The model is necessarily stylized, but captures the fundamental elements of the market.

Formally, I consider that the applications are playing a Bertrand-Nash game where each application j sets its advertising load in order to maximize profits. The application revenue per unit time depends on the quantity of advertisements per unit time, a_j, and the price that advertisers are willing to pay for advertising on this application, $P_j(t_j(\boldsymbol{a}))$, which depends on the time allocations of consumers, $t_j(\boldsymbol{a})$. The marginal cost of the application, denoted c_j, is the marginal cost of serving these advertisements to consumers. Thus, the profit maximization problem for application j is given by:

$$\arg\max_{a_j} \pi_j(\boldsymbol{a}) = (a_j \cdot P_j(t_j(\boldsymbol{a})) - c_j) \cdot t_j(\boldsymbol{a}) \tag{1.4}$$

Consumers face differentiated goods where the main “price" that they face is the advertising quantity set by the application, which induces a disutility that depresses their time allocations. Advertising is modeled as a homogenous good that is differentiated only through the time that consumers spend on the application. This is the main channel through which network effects show up in this formulation as it captures that advertisers have a willingness to pay that varies with the ability to target consumers and depends on the time allocations of consumers. The prices are set through second-price auctions so that they reflect the willingness to pay of advertisers to acquire the attention of the consumers on that application at a given point in time. Consequently, the application does not directly set the advertising prices.

Given this formulation, I propose an Upward Pricing Pressure (UPP) test to conduct merger analysis [41]. The goal of the test is to determine whether a merger would lead to "upward pressure" on prices and, if so, the merger is blocked. The effect of a merger is ambiguous since it exerts upward pressure on prices by enabling the firm to internalize the diversion between the merged firms' applications, but also puts downward pressure on prices through the efficiency gains resulting from the merger. In this setup, the relevant quantity is not whether the merger induces higher prices, but whether it induces higher advertising loads.

In order to formulate the test, I need to specify the joint profit function of the merged firm. The main change is that the merger induces efficiency gains that arise through an increase in the quality of the applications. Following [63], I incorporate this by supposing that consumers face a quality-adjusted advertising load. One possible interpretation of this is that the increased ability to target advertising leads to more useful advertisements for consumers, though the quality gains are broader than just the impact on advertising. Thus, the merged profit function between applications 1 and 2 is as follows:

$$\pi_1 + \pi_2 = \Big(P_1(t_1(\boldsymbol{a})) \cdot (a_1 + v_1) - c_1\Big) \cdot t_1(\boldsymbol{a}) + \Big(P_2(t_2(\boldsymbol{a})) \cdot (a_2 + v_2) - c_2\Big) \cdot t_2(\boldsymbol{a})$$

where v_1, v_2 denote the quality gains for application 1 and 2 respectively as a result of the merger. The UPP evaluates whether, for each application individually, the first-order condition of the merged profit function is positive (i.e. whether $\frac{\partial(\pi_1+\pi_2)}{\partial a_1} > 0$ or $\frac{\partial(\pi_1+\pi_2)}{\partial a_2} > 0$) when evaluated at the current equilibrium advertising loads and prices (e.g. the pre-merger advertising loads and prices). The derivation of the UPP test is provided in section A.7 and is given by:

$$UPP_1 = D_{12} \cdot \Big(\underbrace{P_2 \cdot (a_2 + v_2) - c_2}_{\text{Revenue from Diversion}} + \underbrace{\frac{dP_2}{dt_2} \cdot (a_2 + v_2) \cdot t_2(\boldsymbol{a})}_{\text{Price Change from Diversion}} \Big) - \underbrace{\Big(v_1 \cdot \Big(\frac{dP_1}{dt_1} \cdot t_1(\boldsymbol{a}) + P_1 \Big) \Big)}_{\text{Quality Efficiency Gains}}$$

D_{12} denotes the diversion ratio from application 1 to 2. There are two separate channels that put upwards pressure on the quality-adjusted advertising load faced by consumers. The first is that the diversion towards application 2 from application 1's increase in advertising load enables the application to gain additional revenues due to the additional time spent on the application. The second captures the impact of the diversion towards application 2 on the advertising prices for application 2. There is one channel that puts downward pressure on the quality-adjusted advertising load, which is that the merger induces quality improvements. This quality gain could come through a number of channels, such as improved curation algorithms, better product infrastructure, or improvements in advertising targeting.

1.7.2 Data and Additional Discussion

In order to be able to conduct merger evaluation using the UPP test, I need to collect or estimate the following quantities. I require data on the advertising quantities, advertising prices, time allocations of consumers, and marginal costs. Further, the test necessitates an estimate of consumer diversion between the two applications being merged as well as an estimate for how responsive advertising prices are to additional time spent on an application.

Advertising Load and Prices Data: The data that I use is as follows. The time allocation data comes from the baseline period during the experiment. The advertising load data comes from self-reported data from experimental participants where they were asked to report how many advertisements they experience per minute of usage on each of the applications. I take the average across all the participants to get a measure of the advertising load for each application. The advertising price data comes from the average price per impression on each application in 2021 as reported in trade blogs.[60] Finally, I suppose that the marginal cost of serving advertisements is zero.[61]

Measure of Consumer Diversion: The main variable in the UPP formulation that comes from consumer demand is the estimated diversion ratio. In the UPP formulation, it enters as $-\frac{\partial t_2}{\partial a_1} / \frac{\partial t_1}{\partial a_1}$, which is the diversion ratio with respect to advertising load. Since the advertising load is constant

[60] Note that these prices are relatively low. For instance the average price per impression on Instagram is \$0.00791, which is contrast to the larger value of additional time on the application documented in section A.3.

[61] This is a reasonable approximation since the marginal cost of serving an individual advertisement involves the computational cost of coordinating a (generalized) second-price auction and serving an HTTP request. This is common in technology infrastructure where fixed costs are relatively high, but the marginal cost of network requests is negligible.

over the period of the study, I do not have the variation to identify the advertising load coefficient, and consequently the diversion ratio with respect to advertising load, in a demand model. However, the product unavailability variation and estimated model in Section 1.6 provides me with an estimate of the second-choice diversion ratio. Second-choice diversion ratios are commonly used instead of price-based diversion ratios in merger evaluation cases [51, 8]. Furthermore, [8] show that the difference between these estimates depends on the curvature of demand with respect to the variable of interest. This means that whether or not second-choice diversion ratios are reasonable proxies for advertising load diversion ratios depends on the curvature of demand with respect to advertising load. The main empirical evidence from the literature on the relationship between advertising loads and time allocations comes from [64] who show a strikingly linear relationship between time allocations and advertising load in a 21 month experiment on Pandora. Following the results of [8], this linearity indicates that second-choice diversion ratios should provide a reasonable approximation to diversion with respect to advertising load.

Advertising Price Setting: The modeling of the advertising market requires some additional discussion as it is stylized on several dimensions. The assumption that advertising, holding constant consumer time allocations, is a homogeneous good is consistent with qualitative evidence in [3] and discussions with industry professionals who state that the primary determining factor of where to spend their advertising budget is the reach and ability to target on the application.[62,63] The assumption that the applications do not explicitly set prices is consistent with practice where advertising prices are typically set via generalized second-price auctions.

The formulation utilizes the following assumptions on how components of the model impact advertising prices. The first assumption is that advertising is a homogenous good whose main differentiation is through the reach and targeting that depends on consumer time spent on the

[62]The main differentiation factor beyond this that is within control of the application is the advertising form – for instance the distinction between video and non-video advertising is considered important. However, this is generally considered secondary to the targeting aspect and thus I omit it from modeling considerations.

[63]In this formulation I am only focusing on the price of an impression. In reality advertisers pay a price based on the number of impressions (denoted CPM) and the number of clicks (denoted CPC). If I make the assumption that the click-through rate is identical across applications then this can alternatively interpreted as CPC instead of CPM. However, I do not explicitly model these two channels separately.

application. The second is that the price does not change based on the advertising load.[64] Thus the only channel through which an application is disincentivized from increasing its advertising load is through the disutility incurred by consumers and the resulting impact on advertising price. For the UPP exercise, I only consider that the elasticity of advertising price with respect to time is non-negative. This is since the advertisements in the applications of interest are predominantly behaviorally targeted display advertising, which crucially is reliant on consumer histories in order to target advertising effectively. The literature in marketing that quantifies the value of consumer-specific information on the willingness to pay of advertisers in these markets finds a substantial increase in advertiser willingness to pay as a result of having this information [65, 66, 67].[65] Thus, due to these assumptions, I naturally consider evaluating mergers on a grid of parameters where $\frac{dP_1}{dt_1} = \frac{dP_2}{dt_2}$.

Quality Efficiency Gains: I suppose that the merger induces efficiency gains through improved application quality. Predicting the extent of these efficiency gains at the time of a merger is a challenging problem and the typical approach is to assign a fixed efficiency credit as a result of the merger [41]. The efficiency credit assignment is typically done by setting the value of this parameter to a fraction of the pre-merger quantity. In this case, when considering whether there is upward pressure for application i when evaluating a merger between applications i and j, I set $\nu_i = 0.1 \cdot a_i^*$ and $\nu_j = 0$ so that the efficiency credits for application i are set to be 10% of the pre-merger advertising levels.

[64] These two aspects implicitly rely on the following two assumptions. The first is that there are no explicit crowding out effects between advertisers when the quantity of advertisements on a single application is increased. The second is that I have omitted directly specifying advertiser payoffs, but one possible (strong) assumption would be if advertisers are homogeneous in the value of a conversion. In this case the price increase from additional time allocations is that additional precision in targeting increases the probability of conversion, thus increasing the price they are willing to pay for the unit of attention.

[65] An additional piece of evidence for this is in [68] who study the value of consumers to advertisers after the EU's General Data Protection Regulation and argue that more persistently trackable consumers result in an increase in the valuation of an average consumer.

1.7.3 Merger Evaluation

All of the terms in the UPP formulation are specified besides $\frac{dP_1}{dt_1}$. In order to provide sharper intuition about the different mechanisms at play, I consider two separate cases to provide a cleaner characterization of the results: zero advertising price elasticity, $\frac{dP_1}{dt_1} = 0$, and non-zero advertising price elasticity, $\frac{dP_1}{dt_1} > 0$. I consider these cases using the diversion ratios with inertia and then without inertia with the results summarized in Table 1.9.

Merger Evaluation With Inertia

For each case, I compute the UPP for the merger between each pair of applications and report which gets blocked. Recall that a merger is blocked if $UPP_i > 0$ is larger than zero for at least one of the applications i involved in the merger.[66]

Table 1.9: Summary of UPP Merger Analysis

	$\frac{dP_1}{dt_1} = 0$	$\frac{dP_1}{dt_1} > 0$
With Inertia	Snapchat-Instagram, Reddit-Instagram, Snapchat-YouTube, Reddit-YouTube, Reddit-Facebook	Snapchat-Instagram, Reddit-Instagram, Snapchat-YouTube, Reddit-YouTube, Twitter-YouTube, TikTok-Instagram, TikTok-YouTube, Twitter-Instagram
Without Inertia	Reddit-YouTube, Reddit-Instagram, Snapchat-YouTube	Reddit-YouTube, Reddit-Instagram, Snapchat-YouTube

Notes: This table summarizes the results of the UPP merger analysis exercise. I consider mergers between each pair of applications in the list: YouTube, Instagram, Snapchat, Reddit, Instagram, TikTok, Twitter, Facebook. I do not consider the fact that Facebook and Instagram are jointly owned and, consequently, do not consider a hypothetical merger between the two. For the case when $\frac{dP_1}{dt_1} > 0$, I report the results when $\frac{dP_1}{dt_1} > 0.004$.

[66]Note that I do not include Facebook-Instagram since these applications are already merged in reality and, for mergers involving these applications, I do not explicitly take into account the joint effect of these.

Zero Advertising Price Elasticity: This assumption isolates the tradeoff between the *(Revenue from Diversion)* and *(Quality Efficiency Gains)* terms. In this case, the following mergers get blocked: Snapchat-Instagram, Reddit-Instagram, Snapchat-YouTube, Reddit-YouTube, Reddit-Facebook. These are predominantly mergers between smaller applications with relatively low advertising loads and prices (i.e. Reddit, Snapchat) and larger applications (i.e. Facebook, Instagram, YouTube). This is primarily caused by upward pressure on advertising load for the smaller application. There are two underlying reasons behind this. First, there is relatively high upward pressure due to diversion from the smaller to the larger applications. By increasing advertising load and inducing time substitution towards the larger applications, the joint firm earns additional revenues since a large share of time gets diverted towards the larger application and a single unit of time on e.g. Instagram is more valuable than a single unit of time on e.g. Reddit. Second, there is relatively lower downward pressure since these smaller applications have lower advertising loads inducing smaller values for the efficiency gain threshold.

Non-Zero Advertising Price Elasticity: Now I consider the case when the own advertising price elasticity is non-zero. This introduces additional upward pressure relative to the previous case since now the *(Price Change from Diversion)* term is non-zero, which depends on the own-price elasticity with respect to time as well as the pre-merger advertising load and average time allocations of application 2. However, it also induces additional downward pressure that depends on the own-price elasticity with respect to time and the pre-merger average time allocations of application 1. I consider the grid of $\frac{dP_1}{dt_1} = \frac{dP_2}{dt_2} \in \{0.0001, 0.0002, ..., 0.009\}$, which leads to the same set of mergers to be blocked as in the zero advertising price elasticity case, with the one exception of Reddit-Facebook. Once $\frac{dP_1}{dt_1} > 0.0004$, then the following mergers also get blocked: Twitter-YouTube, TikTok-Instagram, TikTok-YouTube, Twitter-Instagram. In this case, the same mechanism as before is at play – there is an incentive for the merged firm to increase advertising loads from the smaller application (e.g. TikTok) to divert additional time towards the larger applications. Since the average time allocations on the larger application are higher than on the smaller application, the upward pressure wins out relative to the downward pressure and induces additional upward pressure relative to the previous case. This is not always the case, however, as evidenced by the fact that the Reddit-Facebook merger is no longer blocked.

Merger Evaluation Without Inertia

I consider the same merger evaluation exercises under the counterfactual that the inertia channel is shut down. This leads to two changes in the UPP evaluation relative to Section 1.7.3: the time allocations across applications are lower, given by Table 1.8, and the subsequent diversion between the applications is lower for most of the applications, given by Table A26. Note that the percentage changes in the diversion ratios and market shares relative to the baseline inertia case are provided in Table A27 and Table A28.

Zero Advertising Price Elasticity: In this case, the only term which changes relative to the inertia baseline is the diversion weights on *(Revenue from Diversion)*. This means that the downward pressure remains the same as before, but the upward pressure is potentially softened due to the decrease in diversion between these applications. Due to this, the following mergers are no longer blocked: Snapchat-YouTube, Reddit-Facebook. However, Reddit-YouTube, Reddit-Instagram, and Snapchat-Instagram remain blocked indicating that the softening of diversion between these applications due to the lack of inertia is not sufficiently strong to induce all mergers to not be blocked.

Non-Zero Advertising Price Elasticity: I consider the same range for $\frac{dP_1}{dt_1}$ as before. In this case, both the additional upward and downward pressure are suppressed due to the lack of inertia. The upward pressure from *(Price Change from Diversion)* is suppressed due to the decrease in diversion as well as the decrease in the average time allocations on application 2. The downward pressure from *(Quality Efficiency Gains)* is suppressed due to the decrease in average time allocations on application 1. In the cases when a merger was previously blocked, the upward pressure is softened more relative to the downward pressure, which leads to the following mergers no longer being blocked across all values in the set of considered parameters: TikTok-Instagram, TikTok-YouTube, Twitter-Instagram, Snapchat-YouTube, and Twitter-YouTube. Indeed, not only are no additional mergers blocked beyond those that were present before including this channel, but, for $\frac{dP_1}{dt_1} > 0.0002$, even the merger between Reddit and Instagram is no longer blocked. Thus, in this case the role of inertia is so strong that it is the primary reason for blocking the mergers between these applications.

1.8 Conclusion

In this paper I report the results of an experiment where I continuously monitor how participants spend time on digital services and shut off their access to Instagram or YouTube on their phones for one or two weeks. I use the resulting data on how participants substitute their time during and after the restrictions in order to uncover a rich picture of the demand for social media and entertainment applications. I illustrate how the estimated substitution patterns can be used to guide questions of market definition and merger evaluation that have troubled regulators.

I find that participants with the YouTube restriction spend time on applications installed during the restriction period and that participants with the two week Instagram restriction reduce their time spent on Instagram even after the restrictions are lifted. Motivated by this, I estimate a discrete choice model of time usage with inertia and find that inertia accounts for a substantial fraction of usage. Finally, I develop an Upward Pricing Pressure Test for attention markets and show that the presence of inertia is crucial for justifying blocking several mergers between social media applications. Overall, my results emphasize the usefulness of product unavailability experiments for demand and merger analysis in attention markets. These experiments provide a clean way of measuring substitution patterns in these markets as well as identifying addiction/inertia effects, which allow for a comprehensive picture of demand for these applications that are relevant to antitrust issues. These experiments are feasible to conduct for regulatory authorities since the nature of digital goods enables individual level, randomized controlled experiments of product unavailability.

My results point to a broad competition for time between social media applications, but also emphasize that inertia drives a substantial fraction of their usage and diversion towards larger applications such as Facebook and YouTube. There are two broader policy takeaways from these results. The first is that, due to the personalized nature and importance of user-generated content on these applications, determining plausible substitutes according to similarities in product characteristics alone – as has been done in several prominent merger cases – is likely to be insufficient. The second is that due to the role of consumer inertia in driving usage and its importance in determining merger assessments, policies aimed at curbing aspects of these applications that are conducive to addictive usage are an important policy tool at the disposal of regulators aiming to promote competition in these markets. I believe that the insights from this paper can help push forward the regulatory debate and lead to a better understanding of these zero price attention markets. Going forward, one important direction for future work is to further incorporate the networked aspect of these applications into the demand analysis in order to provide a richer picture of the longer run substitution patterns.

Chapter 2: The Effect of Privacy Regulation on the Data Industry: Empirical Evidence from GDPR

2.1 Introduction

Technological advances in the past several decades have led to enormous growth in the scale and precision of consumer data that firms collect. These advances have been followed by progress in machine learning and other data processing technologies that have allowed firms to turn data into successful products and services and earn vast economic returns along the way.[1] However, at the same time, there has been an increasing number of high profile data breaches and a growing feeling of despondency amongst consumers who lack control over this process.[2,3] Beyond the immediate economic harm resulting from such data breaches consumers might also value privacy for its own sake.[4] Against this backdrop, government regulators have proposed and enacted data privacy regulation that empowers consumers to have more control over the data that they generate. The European Union was the first to enact such legislation, the General Data Protection Regulation, which has served as a blueprint for privacy legislation in California, Vermont, Brazil, India, Chile, and New Zealand.[5] However, we lack empirical evidence on the effectiveness and broader impact

[1]Several popular press outlets have gone as far as stating that "data is the new oil" meaning that the world's most valuable resource is now data, not oil (e.g. The world's most valuable resource is no longer oil, but data `https://www.economist.com/leaders/2017/05/06/the-worlds-most-valuable-resource-is-no-longer-oil-but-data`. Retrieved on January 9th, 2020.).

[2]There have been many but among the most prominent are the Cambridge Analytica and Equifax data breaches. Cambridge Analytica harvested the personal data of millions of people's Facebook profiles without their consent and used it for political advertising purposes. The Equifax data breach exposed the names, dates of birth, and social security numbers of 147 million individuals.

[3]We Hate Data Collection. That Doesn't Mean We Can Stop it. `https://www.nytimes.com/2019/11/15/opinion/privacy-facebook-pew-survey.html`. Retrieved on January 3rd, 2020.

[4]For the different motivations for privacy, see, for instance: [69]. As noted by [70], consumer privacy preferences contain both an instrumental and non-instrumental component.

[5]While such regulation is not entirely novel, the scope and robustness of previous regulation pales in comparison to that of GDPR. Several states in the United States and countries around the world are debating and implementing their own privacy regulations with similar scope and stipulations as GDPR. For more information on the specifics of

of such regulation. Such evidence is critical not only for guiding the design of upcoming regulation, but also to understand fundamental questions in the economics of privacy.

This paper empirically studies the effects of the EU's General Data Protection Regulation (GDPR), in particular, its requirement that consumers be allowed to make an informed, specific, and unambiguous consent to the processing of their data. The consent requirement provides a frontline defense of privacy for consumers: by denying consent, a consumer can block a website from collecting personal data and sharing it with third-party affiliates. At the same time, consent denial inhibits firms from tracking consumers across time and across websites, thereby building historical profiles of consumers. Without them, these firms may not be able to learn and predict consumer behavior and target their services and advertising accordingly.

Our investigation focuses on three broad questions. First, to what extent do consumers exercise the consent right enabled by GDPR? Anecdotal and survey evidence suggests that consumers value their privacy. Yet, as commentators argue, consumers may not be willing to act on their privacy concerns even at little cost or inconvenience.[6] We do not yet have clear empirical answers on this question, and consumers' decisions in their GDPR opt-out could shed light on their "revealed" value of privacy.

Second, how does GDPR change the composition of consumers observed by firms? Even prior to GDPR, consumers were able to protect their privacy by utilizing browser-based privacy protection means. However, utilizing these privacy means does not eliminate their footprints altogether but rather simply generate "spurious" identifiers that are difficult for firms to distinguish from genuine footprints left by consumers who do not adopt them. This process creates noise in the data observed by firms that could make it difficult for them to track consumers and predict their behavior. Under the GDPR regime, however, the same consumers may simply opt out, in which case they do not leave any footprints, and this could in principle make the remaining consumers more

the various laws and how they relate to GDPR: 6 New Privacy Laws Around The Globe You Should Pay Attention To. https://piwik.pro/blog/privacy-laws-around-globe/. Retrieved on March 10th, 2020.

[6] A prevalent theme in the economics of privacy literature consistently finds a privacy paradox - the apparent inconsistency between individual's strong stated preferences for privacy and their willingness to give away personal information at little cost [69]. This directly implies that a natural hypothesis is that consumers may ask legislators for such privacy means but, ultimately, make little use of them.

easily trackable and identifiable. This raises an interesting question of externalities created by privacy tools on the other consumers and for the firms. To the best of our knowledge, these forms of privacy externalities not only differ from those recognized in the theoretical literature [71, 72, 73] but more importantly have never been empirically identified.

Third, how does the GDPR privacy protection impact firms that rely crucially on consumer data? Specifically, how does consumer opt-out affect firms' abilities to learn and predict consumer behavior and to provide targeted advertising? And how do advertisers react to such a change? These questions are particularly important for the competitive landscape of the digital economy. While big technology firms such as Google or Facebook enjoy virtually unlimited access to consumer data based on their extraordinary reach and presence, many third-party companies can only access the data shared by first-party affiliates. A concern is that the playing field of these firms, already threatened by the big tech companies, may be further weakened by the increased consent requirement of data regulation.[7] How such a third-party firm copes with GDPR could provide a valuable clue on how data regulation may influence the competitive playing field of the digital economy.

To answer these questions, we use the data provided by an anonymous intermediary that contracts with many of the largest online travel agencies and travel meta-search engines around the world. The dataset is uniquely suited for the current inquiries in several respects. An integral part of the intermediary's business is to predict consumer behavior. Upon each visit by a consumer at an online travel agency (its first-party affiliate), this firm predicts the likelihood of the consumer buying from the website and places advertisements from alternative travel agencies to consumers it deems unlikely to purchase from the original website.

The data links consumers' behavior across time and across websites using cookies (set by the

[7] This concern has been raised in the recent literature [74, 75] and the popular press (GDPR Has Been a Boon for Google and Facebook. https://www.wsj.com/articles/gdpr-has-been-a-boon-for-google-and-facebook-11560789219. Accessed on June 2nd, 2020) which show that GDPR led to an increase in market concentration of web trackers, favoring those from Google and Facebook. This concern is exacerbated by the scope of these companies that allows them to collect data across many different devices and domains that are not feasible for smaller third-party vendors (All the Ways Google Tracks You—And How to Stop It. https://www.wired.com/story/google-tracks-you-privacy/. Accessed on June 2nd, 2020).

intermediary)—small files stored attached to a consumer's web browser that allow the intermediary to identify consumers. We observe (in anonymized and aggregated form) the same rich consumer information as the intermediary and link them just as the intermediary can. If a consumer does not consent to data storage using GDPR opt-out, then his/her cookies cannot be stored, so the consumer is no longer observed by the intermediary. We can directly infer consumer privacy choices from the number of consumer visits as seen by this (third-party) intermediary and the change in composition, necessary to answer the first two questions. We also observe revenues from keyword-based online advertising, and observe the output of a proprietary machine learning algorithm that predicts the purchase likelihood, which will help us to address the third question.

Our empirical design exploits the fact that the intermediary contracts with many different platforms all around the world who were differentially impacted by the introduction of GDPR. Furthermore, the machine learning algorithm is trained and deployed separately for each online travel website. This means that changes in data on one website, due to GDPR or other factors, do not impact the performance of the algorithm on other websites. We exploit these features of our data and the geographic reach of GDPR to utilize a difference-in-differences design for several outcome variables across major European countries and other countries where GDPR was not implemented.

We find that GDPR resulted in approximately a 12.5% reduction in total cookies, which provides evidence that consumers are making use of the increased opt-out capabilities mandated by GDPR. However, we find that the remaining set of consumers who do not opt out are more persistently trackable. We define trackability as the fraction of consumers whose identifier a website repeatedly observes in its data over some time period. We find that trackability has increased by 8% under GDPR.

We explore the mechanisms behind the increased trackability and argue that the most plausible explanation is that the individuals who make use of GDPR opt-out are primarily substituting away from other browser-based privacy means, such as cookie blockers, cookie deletion, and private browsing. While the latter generates many "bogus" short-lived consumers (as a new ID is assigned to a consumer, thus making her appear as a new user, each time she visits the site), the former—the

GDPR opt-out—simply removes these individuals from the data. As a result, those consumers that remain in the data after the implementation of GDPR are more persistently identifiable.

Given this change in consumer composition, we explore the extent to which this affects advertising revenues. In our setting the revenues that we observe come from keyword-based advertising and, further, when consumers opt out they are no longer exposed to advertisements from the third party intermediary. We find that there is an immediate drop in the total number of advertisements clicked and a corresponding immediate decline in revenue. Over time, though, advertisers on average increase their bids for the remaining consumers, leading to a smaller overall decline in revenue. This indicates that the remaining set of consumers are higher value consumers compared to the pre-GDPR set of consumers. One possible mechanism for this is that the increased identifiability of consumers allows for advertisers to better attribute purchases to advertisements than before. This increased attribution ability leads to an increase in perceived overall value of consumers by advertisers.

Finally, we study the effect that GDPR had on the intermediary's ability to predict consumer behavior. In particular, we study the performance of the classifier used by the intermediary, which is a crucial element of its business. The classifier provides a prediction of the probability that a consumer will purchase on the website where she is currently searching. We find that there is evidence that the classifier did not immediately adjust to the post-GDPR distribution. However, despite this, we still find that the ability of the classifier to separate between purchasers and non-purchasers did not significantly worsen after GDPR and that, if anything, the changes to the data observed by the intermediary should lead to improvement in its ability to separate between purchasers and non-purchasers.

Our results suggest a novel form of externalities that privacy-conscious consumers exert on the rest of economy—including other consumers and the firms and advertisers relying on consumer data. Their switching away from inefficient browser-based means of privacy protection to an explicit opt-out (enabled by data privacy regulation) could expose the digital footprints of those who choose not to protect their privacy and make them more predictable. These externalities have potentially important implications. First, third-party firms will suffer from loss of consumers who opt out, but this loss will be mitigated by the increased trackability of those consumers who remain. Indeed, our analysis suggests that the mitigating effect could be important; while we find a negative point estimate on overall advertising revenue, this decrease is not statistically significant. Meanwhile, the welfare effect on the remaining consumers depends on how their data is used by the firms. If their data is used to target advertising and services to their needs, as appears to be so far the case, the externality is largely positive and they will be also better off. However, if the data is used to extract their surplus—a possibility in the future—, they could be harmed by the increased trackability.

Related Work

The protection of consumer privacy and its consequences has been studied by economists, legal scholars, and computer scientists for several decades. We contribute to three strands of literature in the economics of privacy.

Consequences of Data Privacy Regulation: A closely related study that also explores the short run effect of GDPR is [76]. We see these two studies as complementary in terms of the data scenario and findings. Our study utilizes data at a more dis-aggregate level but is confined to one industry whereas [76] have a broad cross-section of different websites and are able to investigate to what extent the effect of the GDPR works through a user acquisition channel. Instead, we are able to look in more detail at cookie lifetime and how GDPR has affected advertising revenues of third party firms.

Several other papers have studied the impact of the GDPR in other domains [77, 78, 79, 80, 81]. [75, 74] show that GDPR increased market concentration amongst web technology services. [82, 67] study the effectiveness of previous data privacy regulations on online advertising. [83] conduct an experiment with a European telecommunications provider to test how consumers respond to the more stringent opt-in requirements that are mandated by GDPR. Finally, [84] estimates a structural model of advertising auctions and shows through counterfactual calculations that advertisement revenue drops substantially more under an opt-in rather than an opt-out policy. We complement these papers by utilizing the scope of our setting to tie each of these pieces together and characterize how they interact with each other and are impacted by data privacy regulation.

Information Externalities: An important consequence of a consumer's privacy decision is the informational externality generated by that decision, as information revealed by one consumer can be used to predict the behavior of another consumer.[8,9] Several recent theoretical studies argue how such externalities can lead to the underpricing of data, and results in socially excessive data collection [94, 71, 72, 73, 95]. [96] theoretically studies how privacy choices by consumers can have pecuniary externalities on other consumers by affecting firms' incentives for price discrimination. The current paper identifies a novel form of informational externalities. While the existing research focuses on how a consumer's decision to reveal her private data can predict the behavior of, and thus can inflict externalities on, those who do not reveal their data, we recognize externalites that run in the opposite direction. Namely, we show that the decision by a privacy-concerned consumer to switch from obfuscation to a more effective GDPR-enabled opt-out may increase the trackability of, and thus exert externalities on, the opt-in consumers who choose to reveal their data. More importantly, to the best of our knowledge, this is the first paper that identifies privacy externalities empirically.[10]

Preferences for Privacy: The broader literature on the economics of privacy, recently surveyed in [69], has studied the privacy preferences of individuals. One prevalent research strand is understanding the privacy paradox, which is the apparent disparity between stated and revealed prefer-

[8]There is also an emerging, broadly related, literature that studies implications of a more data-driven economy [85, 86, 87, 88, 89, 90]

[9]Implicit in the study of the effect on consumer predictability is the notion that privacy is not simply about the revelation of a consumer's information but also the ability of a firm to predict the behavior of a consumer. The idea that privacy is additionally a statistical notion is a common thread in the literature on differential privacy [91, 92]. Differential privacy studies the marginal value of an individual's data for the accuracy of a statistical query and gives a mathematical framework for trading off the privacy loss of an individual revealing her information and the marginal change in the accuracy of a statistical query. For a discussion of the economic mechanisms at play in differential privacy based methods, see [93]. While the intuition behind differential privacy is similar to what we study, we do not explore the design of algorithmic privacy tools. Rather, we empirically document the statistical consequences of privacy choices made by individuals on the predictability of others.

[10]Our explanation for these externalities is consistent with work which shows that the inability to link consumers over time may lead to difficulties in measuring experimental interventions [97, 98].

ence for privacy. In particular, consumers state a strong preference for privacy, but are willing to give up their personal information for small incentives [99, 100, 101]. [102] use a field experiment to evaluate individual preferences for privacy and find evidence of context-dependence in how individuals value privacy. Using stated preferences via a survey, [103] show that consumer's privacy concerns have been increasing over time. [70] shows via a lab experiment that consumer privacy preferences can be broken down into instrumental and non-instrumental components. Our study contributes to this literature by analyzing consumer privacy choices made in a consequential setting, instead of only looking at stated preferences. We find that a significant fraction of consumers utilize the privacy means provided by GDPR, giving suggestive evidence that consumers do value their privacy in consequential settings and not only say that they do.

The paper is structured as follows. Section 2.2 overviews the relevant details from European privacy law and consumer tracking technology. Section 2.3 describes the data and empirical strategy that is used for this study. Section 2.4 provides evidence on the degree to which consumers make use of the privacy tools provided by GDPR. Sections 2.5 and 2.6 analyze the extent to which this affects online advertising revenues and prediction, respectively. Section 2.7 concludes.

2.2 Institutional Details

In this section we discuss European privacy laws and the relevant details of the General Data Protection Regulation. We will then describe how websites track consumers online and how GDPR can affect such tracking.

2.2.1 European Data Privacy Regulation

GDPR was adopted by the European Parliament in April 2016. Companies were expected to comply with the new regulations by May 25th, 2018.[11] It required substantial changes in how firms

[11] GDPR was intended to overhaul and replace the Data Protection Directive which was enacted in 1995. GDPR further complements the other major European Privacy Regulation, The Privacy and Electronic Communications Directive, also known as the "Cookie Law". Relative to this law, GDPR strengthened the territorial scope to include data generated by EU consumers, no matter the location of the firm processing the data, and strengthened the degree of firm transparency and stipulations on consumer consent.

store and process consumer data. Firms are required to be more explicit about their data retention policy, obligating them to justify the length of time that they retain information on consumers and delete any data that is no longer used for its original purposes. Furthermore, it required firms to increase the transparency around consumer data collection and to provide consumers with additional means to control the storage of personal data.

The primary component of GDPR that we focus on is the new data processing consent requirement. Under the regulation firms need *informed, specific, and unambiguous* consent from consumers in order to process their personal data, which requires consumers to explicitly opt into data collection. Recital 32 of the regulation spells out what consent means:

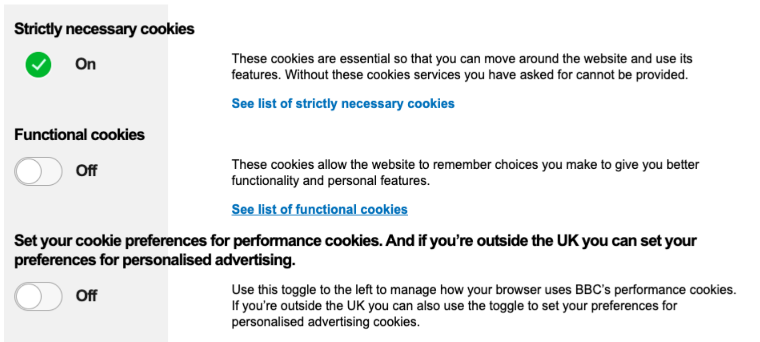
> *Consent should be given by a clear affirmative act establishing a freely given, specific, informed and unambiguous indication of the data subject's agreement to the processing of personal data relating to him or her, such as by a written statement, including by electronic means, or an oral statement. This could include ticking a box when visiting an internet website, choosing technical settings for information society services or another statement or conduct which clearly indicates in this context the data subject's acceptance of the proposed processing of his or her personal data. Silence, pre-ticked boxes or inactivity should not therefore constitute consent.*

Panel (a) of Figure 2.1 shows an example of a post-GDPR cookie policy from the BBC, a news organization based in the United Kingdom, and panel (b) of Figure 2.1 shows a cookie policy of a firm in the United States. The former highlights the specifications of the law, specifying what type of cookies are stored for what purposes and giving consumers the opportunity to opt out from them individually. The latter has no explicit option for the consumers to opt out of data collection. Instead, it directs consumers to use browser-based privacy means, which allow to control the website's cookies.

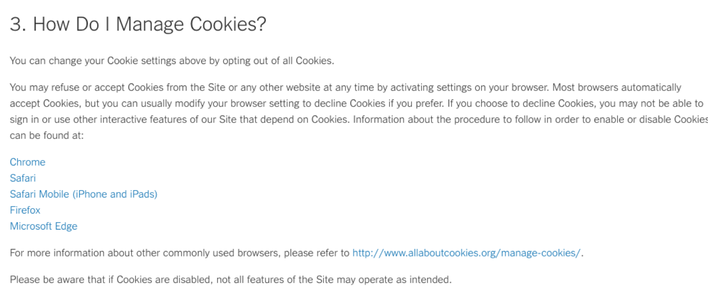
The consent requirement is an important component of the law, though there were many other stipulations of the law that enhanced consumer privacy protection and required substantial changes by firms in order to be in compliance. The fines for non-compliance with the legislation are large

Figure 2.1: Example Consent Notifications

(a) Post-GDPR consent dialog



(b) Standard opt-out on US websites



Notes: The top panel shows a GDPR opt in consent dialog for the BBC. The dialog is explicit about the data that the website collects and requires the consumer to opt into all non-essential data collection. Each separate purpose of data processing is consented to individually. The bottom panel shows an "opt out" dialog for a website in the US that is not required to be GDPR compliant. The website directs consumers to manage their browser cookies and does not have any direct options for the consumer to opt out of data collection.

- the maximum of €20 million, or 4% of total global annual sales for the preceding financial year - giving strong incentives for firms to comply with the regulation. According to Pricewaterhouse-Coopers, many firms are spending millions of dollars in order to comply with the regulation.[12] However, despite this observation, there was still considerable non-compliance around the onset of the law and in the next section we will discuss how this non-compliance affects the interpretation of our estimates.

[12]Pulse Survey: GDPR budgets top $10 million for 40% of surveyed companies. https://www.pwc.com/us/en/services/consulting/library/general-data-protection-regulation-gdpr-budgets.html. Retrieved on December 15th, 2019.

2.2.2 Consumer-Tracking Technology

The primary consumer tracking method that we focus on in this study are web cookies.[13] Cookies are small text files that are placed on consumer's computers or mobile phones. The attachment of a cookie gives websites, in principle, a persistent identifier. As long as the same cookie persists, they can attribute different sessions to the same consumer and, as a result, track them across time and different websites. However, privacy-conscious consumers can make use of various privacy means to control the degree of persistence of this identifier. The primary means available to them are browser-based tools, such as manual deletion of cookies, "private browsing" mode,[14] or cookie blockers.[15] These browser-based privacy means regenerate the cookie identifier but the data that is generated on the website is still sent and stored. The data is attributed to different consumers, even though they originate from the same consumer. One important detail to note is how cookie blockers work in this context. According to our discussions with employees of the intermediary, these services continually regenerate the identifier utilized by the intermediary while still allowing consumers to see the advertisements. Thus, these consumers leave a distinct mark in the data as "single searchers" who only have one observation associated with their identifier.

The GDPR opt-in rule provides another way for consumers to protect their privacy. The stipulations of GDPR, properly implemented and utilized by consumers, arguably provide a stronger protection than the aforementioned means since they block all non-essential information from being sent to the third-party website.[16] In our context, this means that by simply opting out consumers

[13] Common alternatives are other forms of storage in the browser as well as device fingerprinting, which use Internet Protocol (IP) addresses combined with device specific information to identify individuals. However, these are less commonly utilized and importantly not utilized by the intermediary.

[14] Private browsing modes create "sandbox" browser environments where cookies are only set and used for the duration of the private browsing session. As a result, the website cannot link together data from the same consumer both before and after the private browsing section.

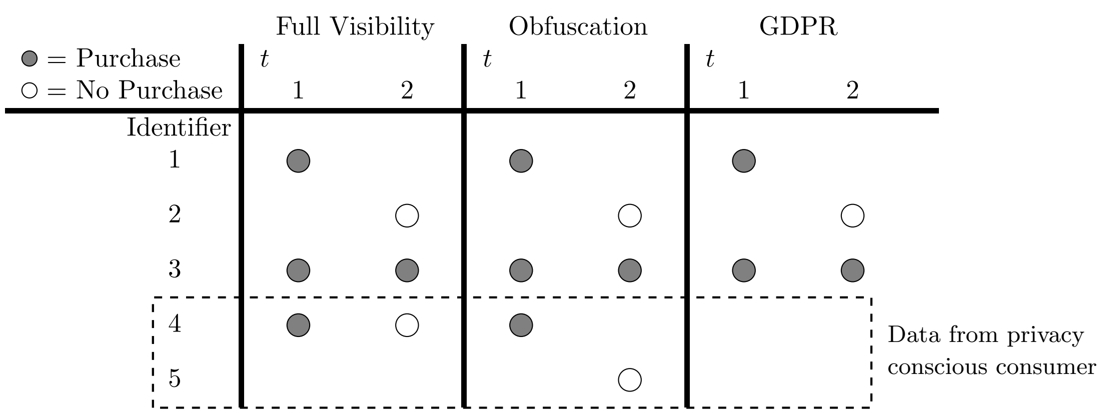
[15] There also exist industry opt-out services, such as the Ad Choices program, but these are relatively hard to use and have little usage [67]. Survey-based evidence informs us that the most utilized privacy means by consumers is manual cookie deletion [104]

[16] It is important to note that GDPR does not prevent "essential" information from being sent to a website. For instance, the ability to store consumer session cookies that allow them to provide a consistent consumer experience for the consumer may be considered "essential" information. The intermediary that we partner with, however, is a third-party service that provides complementary services to the primary functioning of the websites and so is not an "essential" service on any website where we observe data. As a result, any usage of GDPR opt-out shuts out data from being sent to the intermediary.

can keep their data from being sent to the intermediary since it provides a non-essential, third-party service.

The data generating process, therefore, depends on how consumers protect their privacy. Before GDPR a privacy-conscious consumer would rely on browser-based privacy means, in which case this consumer's data would still be sent to the intermediary but with many "bogus" identifiers associated with the same consumer. By contrast, after GDPR, such a consumer could simply opt out of data sharing, in which case no data on that consumer is sent to the intermediary. This is the important distinction for our purpose. Browser-based privacy means lead to many artificially short consumer histories that still enter the data, whereas GDPR opt-out removes the data completely.[17]

Figure 2.2: Illustration of Effects of Different Privacy Means on Data Observed



Notes: The leftmost column displays the identifier observed by the intermediary. The left panel represents the scenario where the behavior of each consumer is fully observable. The middle panel shows how, before GDPR, the privacy conscious consumer 4 has her identifier partitioned into two separate identifiers from the perspective of the intermediary. The right panel shows how, under GDPR, the data of the privacy conscious consumer, is not directly sent to the intermediary.

This is illustrated in Figure 2.2. The figure shows the data generated by four different consumers. "Full Visibility Baseline" shows a hypothetical scenario where each of the four consumers is fully identifiable. They generate spells of browsing sessions where each dot corresponds to one

[17] It's important to point out that consumers can still make use of both privacy means and do not necessarily need to substitute from exclusively using browser-based privacy means towards exclusively using GDPR-provided privacy means. However, from the perspective of the intermediary and websites in general, once a consumer utilizes GDPR opt-out then, since they no longer see any data from this consumer, the browser-based privacy means become irrelevant. As a result, from their perspective, it appears as a direct substitution.

session and the color of the dot indicates whether or not the consumer purchased a good on the website as a result of that search. Suppose that only consumer four is privacy-conscious. Before GDPR, consumer four can protect her privacy by deleting her cookies and regenerating her identifier. This is illustrated in the second panel ("Obfuscation") of the figure where the two sessions for this consumer are associated with two separate identifiers from the perspective of the intermediary. However, the third panel shows that, when GDPR opt-out is available, this consumer opts out and his data completely disappears.

The figure also illustrates how the different data scenarios impact the intermediary's ability to predict consumer behavior, and in particular, how a consumer's choice of privacy means may affect that ability. The four consumers have distinct histories, and these differences may signal different future behavior for them. For example, consumer 4 may be less likely than consumer 1 to purchase from the website next time she visits the website. Under Full Visibility, the prediction machine will correctly recognize this distinction and assign a different prediction score to consumer 4 than to consumer 1. Suppose, however, in the pre-GDPR regime, consumer 4 deletes her cookies and gets partitioned into two separate identifiers, 4 and 5. This behavior confounds the intermediary's ability to predict not only 4's behavior but also 1 and 2's: consumer 1 is now indistinguishable from consumer 4 and consumer 2 is indistinguishable from consumer 5 (the same person as consumer 4) from the intermediary's view point. For instance, the intermediary will assign a lower than accurate purchase odds to consumer 1, influenced by the fact that consumer 4 with the same history simply disappears after the visit at $t = 1$. Note that this problem exists even when the intermediary's prediction machine eventually "learns" about the presence of obfuscators, since it cannot tell who obfuscates and who does not. Under GDPR, on the other hand, consumer 4's data is not observed at all. While this leads to a loss in the amount of data, it removes the confounding that the intermediary suffered from 4's obfuscation in understanding and predicting 1 and 2's behavior.[18]

[18]Importantly, the pre-GDPR intermediary cannot simply replicate the same dataset as post-GDPR since the obfuscators' identities are latent to the intermediary, so their data cannot be surgically cleaned away; for instance, eliminating single-search data will eliminate not only 4 but also 1 and 2 from the data.

2.3 Data and Empirical Strategy

We obtained access to a new and comprehensive dataset from an anonymous intermediary that records the entirety of consumer search queries and purchases across most major online travel agencies (OTAs) in the United States and Europe as well as most prominent travel meta-search engines. We observe consumer searches, online advertising, and the intermediary's prediction of consumer behavior. Our primary analysis utilizes data from this intermediary ranging from April to July 2018.

2.3.1 Data Description

The disaggregated data contains each search query and purchase made on these platforms as well as the associated advertising auction for each query. In a single search query the data contains: the identifier of the consumer, the time of the query, the details of the query (i.e. travel information), an identifier for the platform, the browser, the operating system, and the estimated probability of purchase on the website according to the predictive machine learning algorithm employed by the intermediary. For a subset of the websites, we observe purchase information containing the consumer identifier and time of purchase.

Each query can trigger an advertising auction. In that case, the data contains: the number of bidders in the auction, the values of the winning bids, and an identifier for the winning bidders. Furthermore, if a consumer clicks on the resulting advertisement, the click itself and the resulting transfer between the advertiser and the intermediary are recorded.

Our analysis utilizes an aggregation of this dataset by week, operating system, web browser, website identifier, and country.[19] The data was aggregated on a weekly level to remove unimportant day-of-the-week fluctuations. Furthermore, the GDPR compliance date was May 25th, 2018, which was on a Friday and, as a result, our data was aggregated on a Friday-to-Friday level. Note that the GDPR compliance date corresponds to the beginning of the 22nd week in the year

[19] We drop from this aggregation observations which are labeled as coming from bots.

according to our labeling.[20]

2.3.2 Empirical Strategy

To understand the causal effect of GDPR we rely on a difference-in-differences design that exploits the geographic reach of the EU GDPR regulation. The regulation stipulates that websites that transact with EU consumers were required to ask consumers for explicit consent to use their data through an opt-in procedure, while those who processed non-EU consumers data were not obligated to do so. Even though many online travel companies transact with consumers in several countries around the world this specification works well in our setting since it is common for online travel websites to have separate, country-specific, versions of their websites and only the websites intended for EU countries are made GDPR compliant.

Our analysis focuses on the effect of the overall policy and not the effect of specific implementations of the policy. Thus, the treatment date of the policy corresponds to the GDPR compliance date, which was May 25th, 2018 (or the beginning of week 22). Our treatment group consists of nearly the universe of travel websites in major EU countries (at the time): Italy, the United Kingdom, France, Germany, and Spain. Our control group consists of nearly the universe of travel platforms in the United States, Canada, and Russia. These countries were chosen as controls since EU laws do not directly apply to them, but their seasonal travel patterns are similar to those in the EU countries as a result of similar weather and vacation patterns in the time period of interest.

Our primary regression specification is the following for the outcome variables of interest where c denotes country, j denotes the website, o denotes operating system, b denotes web browser, p denotes product type (hotels or flights), and t denotes the week in the year:

$$y_{tcjobp} = \alpha_t + \delta_{jc} + \gamma_o + \zeta_b + \omega_p + \beta(EU_j \times after) + \epsilon_{tcjobp} \tag{2.1}$$

[20] Note that we further enforce a balanced panel by dropping any observation that has zero logged searches in any period during our sample period. We do this in order to ensure that our estimates are not biased from entry / exit of websites into our data during the sample period. According to discussions with our data provider, this entry and exit is usually a result of varying contractual relations between the intermediary and the websites and so is largely orthogonal to our variables of interest.

EU_j denotes a website subject to the regulation, $after$ denotes whether the current week is after the GDPR compliance date (i.e. week 22 or later), α_t denotes time fixed effects, δ_{jc} denotes country-specific website fixed effects, ω_p denotes product type fixed effects, γ_o denotes operating system fixed effects, and ζ_b denotes browser fixed effects. Our standard errors are clustered at the website-country level.[21]

In order to validate parallel trends and to understand the persistence of the treatment effect, we further utilize a regression specification that captures the potentially time-varying nature of the treatment:

$$y_{tcjobp} = \alpha_t + \delta_{jc} + \gamma_o + \zeta_b + \omega_p + \sum_{k=\underline{T}}^{\bar{T}} \beta_k EU_j + \epsilon_{tcjobp} \tag{2.2}$$

The variable definitions are the same as before and we similarly cluster our standard errors at the website-country level.

We run our regressions over the time period between weeks 16 and 29 of 2018, which is between April 13th and July 20th. The GDPR compliance date aligns with the beginning of week 22. Furthermore, week 20 is consistently the baseline week in our regressions since there are some firms that began to implement GDPR near the end of week 21 and so week 20 is the last week where there should be no direct impact from GDPR as a result of website implementation.[22,23]

Our empirical strategy centers around the official GDPR implementation date. However, each website had to individually implement the changes stipulated by GDPR and there is evidence that there was considerable heterogeneity in compliance among firms. Furthermore, even within the subset of firms that complied with the regulation, the degree to which consumers responded varied

[21] We cluster at the website-country level because of differences in privacy concerns across countries [105] and differences in consent implementations across websites within jurisdiction [80].

[22] Our dataset ends on July 31st, 2018, which is a Tuesday, and an important measure that we want to track is the amount of consumer persistence on a weekly level, which looks at the fraction of observed cookies that remain observable in the data after some number of weeks. Since this measure requires a complete week of data to compute properly, we drop the incomplete week at the end of July as well as the full last week in July so that we can have consistency between the regressions on aggregate consumer response and those on consumer persistence.

[23] Our analysis ends at the end of July since this was the time period over which we were able to obtain data from our data provider.

considerably based on the nature of implementation [80]. As a result, we would want to include information on the timing and degree of implementation across the various websites in our sample. However, due to technical limitations, we cannot directly observe the timing and degree of GDPR implementation during the time period we study.[24]

Thus, any effects that we observe with our empirical specification are a combination of the explicit consequences as a result of implementing the stipulations of GDPR for the subset of websites that implemented it and any changes in advertiser and consumer behavior in response to the increased saliency of privacy considerations on the Internet.[25] Since we do not observe the full extent of non-compliance, our estimates can be viewed as a lower bound on the true impact of the policy had websites all fully complied with it.

2.4 Consumer Response to GDPR

In this section we quantify the extent to which consumers utilize the GDPR-mandated ability to opt out. We measure how GDPR opt-out impacts the total number of cookies and searches observed by the intermediary. We then explore whether there were any changes in the composition of the remaining, opted-in consumers.

2.4.1 Opt-Out Usage

Recall that we do not directly observe opt-out in our dataset because consumers who opt out are no longer part of our dataset. As a result, at time t, the total number of consumers on a website j is given by the true number of consumers subtracted by the number of consumers who have opted

[24] We attempted to utilize tools such as the Wayback Machine, which takes snapshots of websites across the entire Internet frequently. However, the coverage of relevant websites on the Wayback Machine is spotty and, given that many of the consent dialogs for GDPR consent are dynamically generated, are not always picked up by the snapshot taken of the website.

[25] It would be interesting to isolate the effects of each possible channel, though our data limitations prohibit us from doing so. We were able to verify that several websites in our sample implemented GDPR consent guidelines around the time of the policy and that several websites in our sample did not, though there are a considerable number for which we are uncertain when they implemented the policy.

out.[26]

$$U_{jt}^{OBS} = U_{jt}^{TRUE} - U_{jt}^{OPT-OUT}$$

In the control group, $U_{jt}^{OPT-OUT} = 0$, whereas post-GDPR $U_{jt}^{OPT-OUT} \geq 0$. We assume parallel trends in U_{jt}^{TRUE}, which means that any change in U_{jt}^{OBS} allows us to identify $U_{jt}^{OPT-OUT}$.[27,28]

Figure 2.3 displays the total unique cookies for two multi-national websites, one of which implemented the consent guidelines of the GDPR and the other which does business in the EU but did not immediately comply with the regulations. The multi-national website which implemented the consent guidelines shows a clear drop in observed cookies on European websites at the onset of GDPR. Columns (1) and (2) of Table 2.1 report the result of regression (2.1) with total number of observed unique cookies as the outcome variable. We consider the specification in both levels and logs. The estimates show that, in aggregate, GDPR reduced the total number of unique cookies by around 12.5%. As previously mentioned, our estimates should be interpreted in the context of mixed compliance with the consent guidelines of GDPR as evidenced from Figure 2.3.

It is important to note that this result *does not* imply that 12.5% of consumers made use of the opt-out features. This is because the unit of observation is a cookie, rather than a consumer. A single consumer can appear under multiple cookie identifiers if they make use of the aforementioned browser-based privacy means. Nonetheless, the results point to a relatively large usage of the opt-out features by consumers.

[26] Note that a website here serves as a first-party affiliate of our intermediary; so the true number of consumers for website j is not the true number of consumers for the intermediary, as opt-out consumers become out of its reach.

[27] As noted in [76], another possible complication is that this could be a result of firms changing the type of data that they send to third party services. To our knowledge there is no change in the data the websites send to the intermediary as a result of GDPR since the intermediary and the data are crucial for generating advertising revenue for these websites. Furthermore, if a website decided to stop using the intermediary altogether then, as noted previously, they would not be part of our sample.

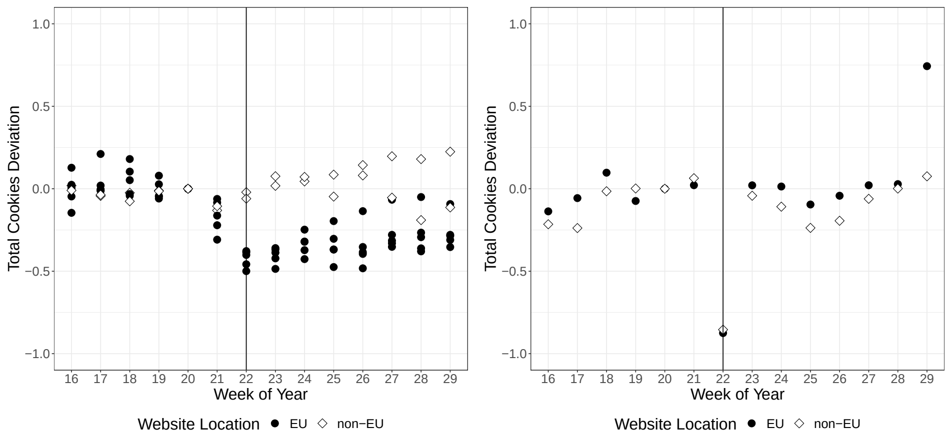
[28] Another possible confounding factor is the sales activity of the intermediary. For instance, it's possible for the intermediary to sell additional advertising units to a website that can appear on pages of the website where the intermediary previously was not tracking before. If there was a differential effect from this around the date of the treatment then this could systematically bias the number of unique cookies and searches that we observe. To test the plausibility of this hypothesis, we run our difference-in-differences specification with total advertising units and total pages on which the intermediaries advertising appears as the outcome variables. The results in Table B1 show that there was no significant change in either of those two variables. Thus, we rule this alternative explanation out.

Another measure of consumer response is the total number of searches that are recorded by the intermediary. This outcome measure can also be interpreted as the overall data size observed by the intermediary and how it is affected by GDPR. We re-run the same specification with recorded searches as the dependent variable and report the results in columns (3) and (4) of Table 2.1. We find that there's a 10.7% drop in the overall recorded searches which is qualitatively consistent with the effect size of the specification using the number of unique cookies.

In order to provide evidence for the validity of the difference-in-differences strategy we rely on our time-varying treatment specification. Figure B1 displays the resulting treatment effect over time and points to parallel pre-trends as well as a consistent treatment effect size over our sample period though there is a slight decrease in the estimated treatment effect as we approach the end of our sample period. Finally, as further evidence of robustness, we employ a synthetic control approach, which is reported in subsection B.2.1 and produces qualitatively similar results.

We want to discuss two further potential threats to the validity of our empirical strategy. The first is a potential contamination between treatment and control groups that may result from multinational companies implementing the consent mechanisms across all of their websites. The second is that the results may be driven by seasonal travel differences between the treatment and control groups. The first is not a big concern in our setting because multinational online travel agencies serve customers through country-specific websites and have incentives to only make their EU domains compliant with GDPR. For the online travel agencies where we can directly verify compliance we do indeed see that most of them only implement it for their respective EU domains as evidenced by Figure 2.3. Furthermore, to the extent that there is still residual contamination, it would mean that our estimates are a lower bound of the true effect size.

Figure 2.3: Total Number of Unique Cookies for Two Multi-National Website.



Notes: Each point on the graph represents the total number of unique cookies for a single country, reported in terms of its percent deviation relative to week 20, or $\frac{U_t - U_{t=20}}{U_{t=20}} \quad \forall t \neq 20$. The figure on the left presents a multi-national website that we were able to verify implemented the consent guidelines of GDPR. In this figure, the black dots represent the represented European countries (United Kingdom, France, Germany, Italy, Spain) and the two white dots represent the two non-EU countries where this website functions - the United States and Canada. The figure on the right presents a multi-national website that we were able to verify did not implement the consent guidelines of GDPR. The black dots represent the values from the United Kingdom and the white dots represent the values from the United States.

Table 2.1: Difference-in-Differences Estimates for Cookies and Searches

	(1) log(Unique Cookies)	(2) Unique Cookies	(3) log(Recorded Searches)	(4) Recorded Searches
DiD Coefficient	-0.125**	-1378.1*	-0.107*	-9618.3**
	(-2.43)	(-1.71)	(-1.87)	(-2.24)
Product Type Controls	✓	✓	✓	✓
OS + Browser Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Website × Country FE	✓	✓	✓	✓
Observations	63840	63840	63840	63840

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 29, including both weeks 16 and 29 (April 13th - July 20th). The dependent variables in the regression reported in the first and second column are the log and overall level of the number of unique cookies observed. The dependent variables in the regression reported in the third and fourth column are the log and overall level of the number of total recorded searches.

For the second issue, this is the reason that we focus our analysis on a tight window around the GDPR implementation date and select control countries that ought to have similar travel patterns during this time period. However, since European travel patterns have a somewhat steeper summer gradient than US travel patterns we would expect this to bias against our results. We therefore further supplement our analysis with Google Trends data on travel searches, which should be unaffected by GDPR and provide a good picture into travel trends across these different countries. Using this data, we first graphically show that in the period of the year that we consider, travel patterns between the countries in the analysis are similar. When we augment our primary analysis with country-specific seasonal controls based on Google Trends data we find quantitatively very similar results with slightly stronger effect sizes than before. The full details of this exercise are deferred to subsection B.2.2.

2.4.2 Persistence of Identifier

A natural question is whether GDPR affects the ability to persistently track consumers. To address this question, we define an *identifier persistence* measure that tracks how often cookies that we see in a given week return after k weeks, where we explore different values for k (1,2,3, and 4 weeks). Let C_{jt} be the set of cookies seen in week t on website j, the measure is then given by:

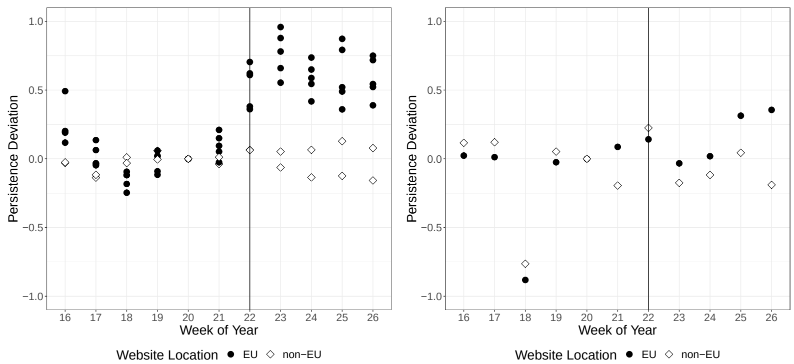
$$persistence_{kt} = \frac{|C_{j,t} \cap C_{j,t+k}|}{|C_{j,t}|}$$

In Figure 2.4 we set $k = 4$ and display the persistence measure for the same two multi-national websites with country-specific versions of their website over time. At the onset of GDPR there is a clear increase in persistence on the EU-based websites, but no noticeable difference in the non-EU websites. We further validate this increase by running our baseline difference-in-differences specification using the persistence outcome variable for $k \in \{1, 2, 3, 4\}$.[29,30]

[29] In order to run specification (2.1) we drop the last 4 weeks of our sample so that we are utilizing the same sample as we vary k. However, our results are qualitatively robust to including these weeks when the data for them is available.

[30] Note that the units on the regression and Figure 2.4 are not the same. Figure 2.4 displays the persistence measure

Figure 2.4: Four Week Persistence for Two Multi-National Websites



Notes: Each point on the graph represents the four week persistence fraction for a single country, reported in terms of its percent deviation relative to week 20, or $\frac{persistence_{4,t} - persistence_{4,t=20}}{persistence_{4,t=20}} \quad \forall t \neq 20$. The figure on the left presents a multi-national website that we were able to verify implemented the consent guidelines of GDPR. In this figure, the black dots represent the represented European countries (United Kingdom, France, Germany, Italy, Spain) and the two white dots represent the two non-EU countries where this website functions - the United States and Canada. The figure on the right presents a multi-national website that we were able to verify did not implement the consent guidelines of GDPR. The black dots represent the values from the United Kingdom and the white dots represent the values from the United States.

Table 2.2: Difference-in-Differences Estimates for Consumer Persistence

	(1) 1 Week Persistence	(2) 2 Weeks Persistence	(3) 3 Weeks Persistence	(4) 4 Weeks Persistence
DiD Coefficient	0.00308*	0.00416***	0.00382***	0.00505***
	(1.96)	(3.40)	(3.10)	(3.50)
Product Type Controls	✓	✓	✓	✓
OS + Browser Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Website × Country FE	✓	✓	✓	✓
Observations	50160	50160	50160	50160

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 26, including both weeks 16 and 26 (April 13th - June 29th). The dependent variables in the regression are the consumer persistence measures for $k = 1, 2, 3, 4$, respectively.

in terms of percent deviations from week 20 whereas the coefficients in Table 2.2 are changes in levels.

Table 2.2 shows the results of this regression, which indicate that there is a statistically significant and meaningful increase in consumer persistence and that this effect gets more pronounced as k increases.[31] We further run the time-varying treatment specification (2.2) in order to validate that parallel trends holds and to understand the consistency of the effect over time. Figure B2 shows that while for $k = 1$ the time dependent treatment effects are more noisy, for all $k \geq 2$ parallel trends hold and the treatment effect is stable over time.[32] The treatment effect remains roughly the same as k grows, even though Table B2 shows that the mean persistence declines as k increases. For instance, in the pre-treatment period, the mean persistence for EU websites was 0.0597 and the estimated treatment effect is 0.005 indicating a roughly 8% increase in persistence.

There are two possible hypotheses for the increased persistence. The first is a *selective consent hypothesis* where consumers only consent to data processing by websites that they frequently use. According to this hypothesis, infrequent users of a website are more likely to opt out of data sharing than frequent users, so the opt-in set of consumers will naturally appear to be more persistent. The second is a *privacy means substitution hypothesis* where privacy conscious consumers who were previously making use of browser-based privacy means now utilize GDPR opt-in to protect their privacy. Recall that the utilization of these privacy means would result in many artificially short-lived consumers. If these same consumers utilize GDPR opt-in instead, they would no longer show up in the intermediary's dataset and the remaining set of consumers would appear to be more persistent even though their true search and purchase behavior may not have changed.

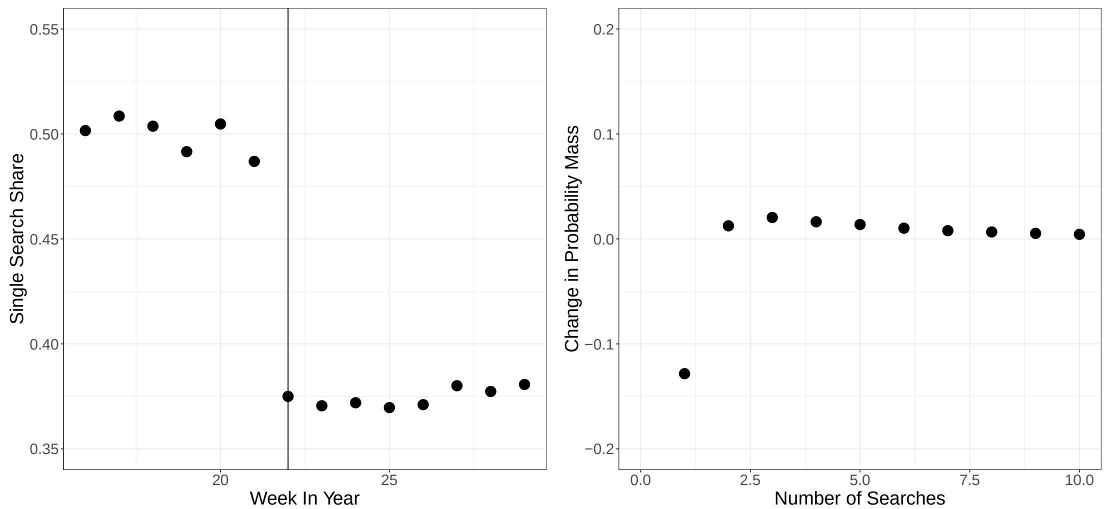
These alternative hypotheses have different economic implications. If the selective consent hypothesis is the predominant explanation for the increased persistence, then privacy regulation may favor firms with more established reputations or offer a wider variety of services.[33] The

[31] It is important to note that the persistence measure may have some noise when $k = 1$ due to consumer activity near the end of the week that spills over into the next week and falsely appears as persistence. As a result, the most reliable measures of consumer persistence are for $k \geq 2$, but we report $k = 1$ for completeness.

[32] Furthermore, Figure B3 in the appendix shows the overall distributions of consumer persistence for the EU vs. non-EU and note that there are some outliers. In particular, there is a large mass of high persistence observations and persistence measures close to 0. Our results are qualitatively robust to running our specifications winsorizing and dropping these observations as well. They are also robust to the addition of seasonal travel controls using the same procedure as in subsection B.2.2.

[33] There is a connection of this hypothesis to the theoretical predictions in [106], who argue that consent-based data collection practices would allow larger firms to collect more data than smaller firms since they offer a wider scope of

Figure 2.5: Change in Search Distribution for One Site



Notes: The figure on the left breaks down the share of cookies associated with only one search week by week, as opposed to pooling the full sample periods before and after GDPR. The figure on the right shows the difference in the share of consumers with x searches in the full sample after GDPR compared to before GDPR. For instance, the leftmost point indicates that there was a roughly 12.8% decrease in the share of cookies associated with a single search.

hypothesis would imply that in the long run consent for data collection can serve as a barrier to entry for newer firms with less established reputations and a smaller variety of services.

If the privacy means substitution hypothesis is the predominant explanation for the increased persistence, then there are several economically relevant consequences. First, the benefit of GDPR would be the marginal benefit over existing privacy protection. Thus, even though a significant fraction of consumers opted out of data collection, the welfare gains for the opted out consumers depends on the marginal privacy gain relative to these pre-existing means. Second, the usage of GDPR opt-out would lead to an externality on the opt-in consumers and, as a result, their privacy protection may be weakened. This would mean that firms relying on prediction may not suffer as much as the number of opt out indicates since this would enhance their prediction capabilities. Finally, it would allow for better advertisement attribution and measurement of advertising effectiveness which would directly influence the price advertisers are willing to pay.

While the two explanations are not mutually exclusive, we provide suggestive evidence that

services. As a result, consumers may utilize these websites more and trust the website with their data more.

the *privacy means substitution hypothesis* is the more plausible one. Both hypotheses imply that the drop in relative probability mass should be concentrated towards the lower end of the support. However, recall from section 2.2 that in our context one signature of browser-based privacy protection is a large mass of “single search" consumers. This is due to the continuing regeneration of cookie-identifiers after every request. Indeed, Figure 2.5 shows that the fraction of single searchers significantly dropped after the implementation of GDPR. Instead, under the selective consent hypothesis, we would expect that the loss in probability mass would be more evenly distributed across search counts.

Based on this observation, we test for the presence of excess single searchers using a [107] test both before and after the implementation of GDPR. The test is based on a simple model in which obfuscating and non-obfuscating consumers together give rise to an observed distribution of cookie counts. We implement this test under two different distributional assumptions. Under the first assumption, the true number of visits per consumer follows a conditional Poisson distribution. To allow for more dispersion we, alternatively, assume that the true underlying distribution follows a negative binomial distribution. For the empirical implementation, we focus on a large website which we know faithfully implemented the consent mechanism. In short, the test suggests the presence of excessive single searchers in the pre-GDPR period but not in the post-GDPR period. The full details of the exercise are deferred to section B.4.

We implement the test separately for the pre- and post-GDPR period. In the pre-GDPR period, the test rejects a model without excess single searchers in favor of a model in which the number of single searchers is inflated under both distributional assumptions. The same is true for the post-GDPR period under the Poisson distribution, but the estimated fraction of single searchers is lower in this period relative to the pre-GDPR period. However, under the more flexible negative binomial distribution, we still find statistical evidence for excessive single searchers in the pre-GDPR period whereas the evidence is not significant in the post-GDPR period.

Finally, we analyze the entire set of websites again and estimate heterogeneous treatment effects across popular web browsers and operating systems. We find that the increase in persistence occurs on all browsers except for Internet Explorer and find weak evidence that the increase in persistence is more prominent on desktop operating systems compared to mobile operating systems. While the differences on these dimensions are difficult to explain according to the selective consent hypothesis, they are plausible under the privacy means substitution hypothesis, in light of the alleged lack of technical sophistication by the consumers who use Internet Explorer and the technical difficulty of utilizing browser-based privacy means on mobile/Internet Explorer.[34] The results and a full discussion are deferred to section B.3. Overall, these results provide additional evidence in favor of the privacy means substitution hypothesis although additional research on this distinction is certainly warranted.

2.5 GDPR and Online Advertising

The advertisements in our setting are sold via real-time auctions that are held when a consumer makes a search query.[35] Advertisers bid on search keywords such as the origination, destination, or dates of travel. For example, an advertiser may submit a bid to show an advertisement for a consumer searching for a flight from JFK to LAX and upon winning displays a price comparison advertisement for this particular route. Thus, bids reflect the value of the set of consumers that search for certain keywords and not particular consumer histories. Bids are submitted per click and a payment from the advertiser to the intermediary occurs only if the consumer clicks on the advertisement. An important fact for the interpretation of our results is that consumers who opt out are never shown any advertisements. Thus, the intermediary generates no advertising revenues from these consumers.[36]

We separately investigate the changes in advertising revenue, prices, and quantity of advertise-

[34]In the time period of interest, the new Microsoft Edge browser was the default on Windows computers and Internet Explorer is predominantly utilized on computers running the Windows OS. Microsoft Edge was the default on Windows since 2015, thus users of Internet Explorer are predominantly those on older computers. Internet Explorer users tend to be older than Chrome or Firefox users (`https://elie.net/blog/web/survey-internet-explorer-users-are-older-chrome-seduces-youth/`) and thus less inclined to adopt browser-based privacy practices [108].

[35]The auction format is a linear combination of a generalized first and second price auction where there are N advertisers and k slots.

[36]An implication of this is that although GDPR opt-out restricts the data observed by the intermediary and the website, we observe the advertising revenue for the intermediary generated from opt-in consumers.

ment. First, we look for the change in the number of clicks for advertisements following GDPR. Columns (1) - (2) of Table 2.3 show that there is a statistically significant decrease of 13.5% in the total number of clicks. The magnitude of this effect is in line with the drop in total cookies and searches. We next look for changes in the number of clicks from distinct cookies to see if any changes were driven by some small set of consumers. Columns (3) - (4) show that this measure also decreases significantly. Figure 2.6 displays the time-varying specification for these outcome variable and shows that the effect on the number of clicks is relatively constant.

Columns (5) and (6) of Table 2.3 show the effects on revenue. The magnitude of the point estimates suggests an economically significant drop, though it is imprecise and not statistically significant. The time-varying treatment effect displayed in Figure 2.6 shows that revenue initially falls sharply after the implementation of GDPR and then begins to recover. Importantly, column (7) of Table 2.3 shows that the average bid of the advertisers *increases*. At roughly 12% this increase is economically sizable.[37] The time-varying coefficient in Figure 2.6 shows that the average bid does not change initially after the policy and then increases gradually. In summary, the immediate drop in clicks following GDPR leads to a sharp drop in revenue, but the gradual increase in the average bids leads to a recovery of some of the lost revenue for the intermediary and advertisers.

In light of these results one may wonder how the quantity of advertisements is affected. Using the same difference-in-differences specification with total number of advertisements as the dependent variable we find that the number of advertisements has dropped but that this change is not significant (see Table B6 and Figure B6 in section B.5 for the time varying treatment effect).

We now discuss the plausible mechanisms behind the increase in prices (bids). The first mechanism, which is consistent with the evidence that we establish above, is that remaining consumers are of higher average value to advertisers.[38] Our discussion with the intermediary indicates that advertisers' value is determined according to the *observed* conversion rate of their advertisements, which is the fraction of consumers that end up purchasing a good after clicking on an advertise-

[37]Note that in order to preserve the privacy of our intermediary, the bid and revenue values are obfuscated by a common multiplier. However, the interpretation of percentage changes is preserved under this transformation.

[38]In contemporary work and in a different e-commerce setting, [76] reach a similar conclusion about the value of consumers post-GDPR.

ment. Since the measurement of conversion rests on the ability to track consumers, it is plausible that the increased trackability of consumers following GDPR improved the measurement of conversion rates, thus contributing to an increase in value of consumers as perceived by the advertisers.[39] Suppose, instead, that GDPR opt-out is available and consumer *A* is removed from the sample of the advertiser and therefore never clicks on an advertisement. The advertiser's estimated conversion rate is 0.5 now, as opposed to 0.4 and so the perceived value of consumers weakly increases regardless of consumer *A*'s true behavior. More generally, dropping individuals similar to consumer *A* from the observed sample can only weakly increase the advertiser's perceived value. This points to the explanation that the increased ability to accurately measure conversion rates has led advertisers to gradually increase prices (bids) over time.

However, there might be two plausible alternative explanations. The first is that GDPR has decreased the "supply" of consumers to whom advertisements can be served. As we have demonstrated above, there is a significant reduction in the number of advertisements served because consumers opt out. This reduction in advertising targets might increase the value of the marginal remaining consumer. While this is certainly plausible, the pattern of price increases is not fully consistent with the supply shock explanation. Since this shock materializes right after the implementation date and advertising budgets are set daily, one would expect a sharp price increase. Instead, we observe a gradual price increase (Figure 2.6), which is more consistent with advertisers slowly adjusting to the increase in conversion rates.

[39] To illustrate, suppose that there are five consumers who click on an advertisement. Suppose one of them (from here on consumer *A*) makes us of cookie blockers but ends up purchasing and, from the remaining four, suppose two of them end up purchasing. Thus, regardless of the behavior of consumer *A*, the advertiser's estimated conversion rate is 0.4 as opposed to 0.6—a correct rate including *A*.

Figure 2.6: Week by Week Treatment Effect for Total Clicks, Revenue, and Average Bid

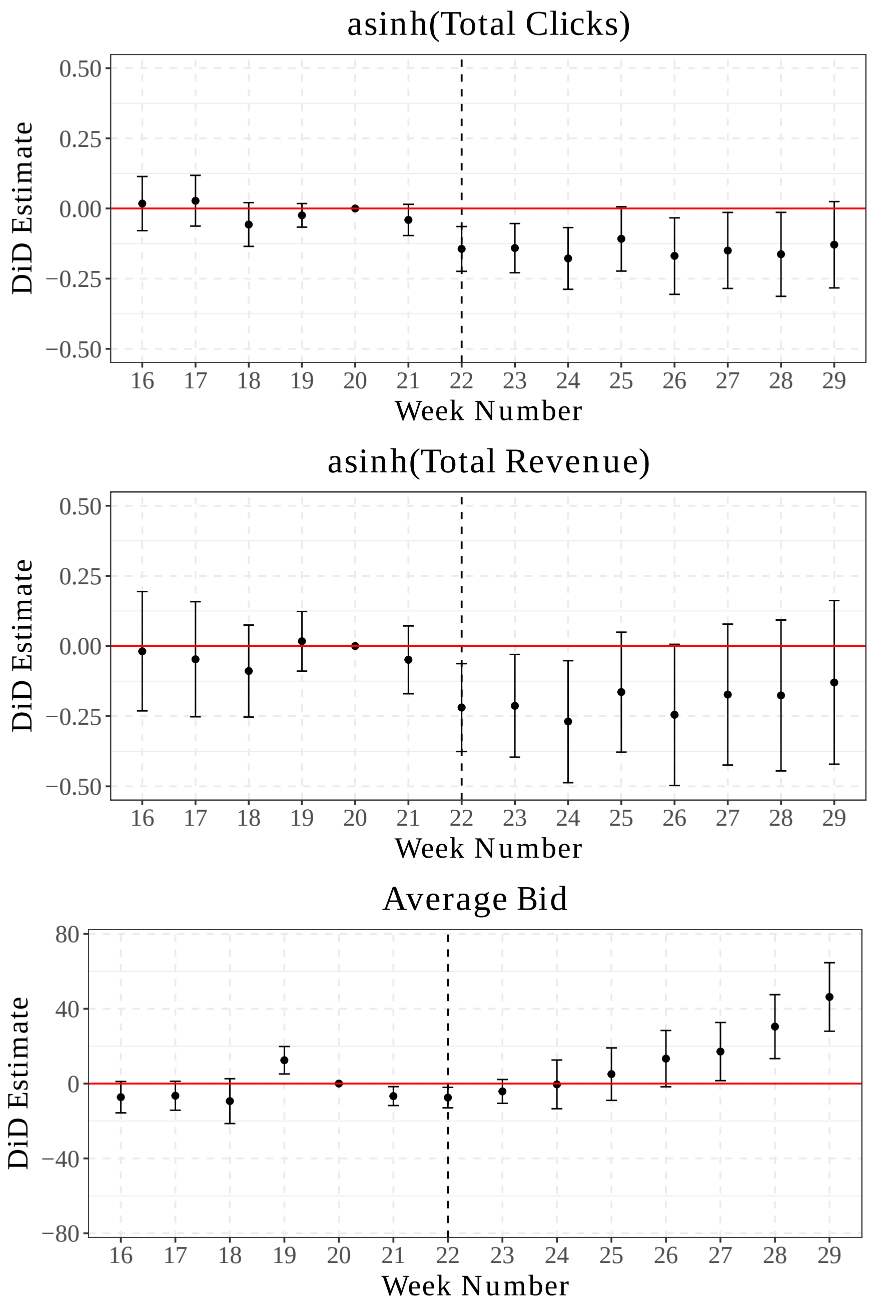


Table 2.3: Difference-in-Differences Estimates for Advertising Outcome Variables

	(1) asinh(Total Clicks)	(2) Total Clicks	(3) asinh(Distinct Clicks)	(4) Distinct Clicks	(5) asinh(Revenue)	(6) Revenue	(7) Average Bid
DiD Coefficient	-0.135**	-251.9*	-0.133**	-214.9*	-0.168	-32972.3	15.41***
	(-2.32)	(-1.91)	(-2.33)	(-1.84)	(-1.54)	(-0.75)	(2.90)
OS + Browser Controls	✓	✓	✓	✓	✓	✓	✓
Product Category Controls	✓	✓	✓	✓	✓	✓	✓
Website× Country FE	✓	✓	✓	✓	✓	✓	✓
Week FE	✓	✓	✓	✓	✓	✓	✓
Observations	62328	62328	62328	62328	62328	62328	62328

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 29, including both weeks 16 and 29 (April 13th - July 20th). The dependent variable in the regression reported in the first column is the total number of clicks associated with each observation and the second column is the inverse hyperbolic sine transform of this value. Likewise, the dependent variables in the third and fourth columns are the total number and inverse hyperbolic sine transform of the total number of unique cookies who interacted with advertisements. The dependent variables in the fifth and sixth column are the total number and inverse hyperbolic sine transform of the total revenue. The dependent variable in the seventh column is the average bid by advertisers. We utilize the inverse hyperbolic sine transform instead of the logarithm as in previous sections as some of the outcome variables we consider in this section can take zero values. The inverse hyperbolic sine transform is given by $\bar{y} = arcsinh(y) = \ln(y + \sqrt{y^2 + 1})$ and results in a similar coefficient interpretation as taking logarithms [54], but does not remove the zero valued observations from the data. We retain the zero values here so that there is a clearer comparison between the estimates before and after the transformation.

Another plausible alternative explanation is that GDPR is a positive “demand shock” for the type of advertising offered by the intermediary. Advertisers in our setting submit bids based on the context in which advertising is shown (e.g. based on travel search details) instead of on individual consumer histories. The relative efficiency of such “contextual advertising" compared to behaviorally targeted display advertising, which is even more dependent on consumer tracking, may have increased as a result of GDPR.[40] However, our setting also contains a personalized element: the decision to place advertisements is personalized based on the predictions of the intermediary. It is therefore less plausible that the intermediary would be one of the clear-cut winners under such a shift in the market.

To sum up, it seems plausible that advertising prices, at least in part, increased because the average consumer is more trackable. This interpretation is in line with the evidence of previous sections and alternative explanations are less plausible based on our institutional knowledge and prevailing patterns in the data.

2.6 GDPR and Prediction of Consumer Behavior

In this section we investigate whether the changes due to GDPR have affected the intermediary’s ability to predict consumer behavior. Beyond this particular context, such an investigation is also of broader interest. Sophisticated machine learning technologies that attempt to predict consumer purchase behavior are becoming increasingly common[41] and our results provide a case study on how their accuracy is affected by data privacy regulation.

Based on our analysis we expect there to be three predominant reasons why we might observe a change in the ability to predict. First, GDPR has significantly reduced the overall amount of data. Second, remaining consumers have longer histories and are more trackable. Third, in line with our illustration in Figure 2.2, GDPR might reveal correlation structures between consumer behavior

[40] Personalization diminished: In the GDPR era, contextual targeting is making a comeback. `https://digiday.com/media/personalization-diminished-gdpr-era-contextual-targeting-making-comeback/`. Accessed on December 15th, 2020.

[41] See, for example, Retailers Use AI to Improve Online Recommendations for Shoppers, `https://www.wsj.com/articles/retailers-use-ai-to-improve-online-recommendations-for-shoppers-11604330308`, Accessed on March 31st, 2021.

and the length of consumer histories that were previously obfuscated by the use of alternative privacy means. We would expect the first effect to decrease prediction performance and the second and third to increase prediction performance.

We take as given both the setup of the prediction problem and the algorithm that the intermediary uses. This allows us to understand the effects of GDPR on the prediction problem "in the field." Its problem is to predict whether a consumer will purchase from a site she visits based on utilizing the history that the intermediary observes about this consumer. Specifically, its algorithm classifies a search by a consumer into two categories: purchasers and non-purchasers, based on whether the consumer will purchase a product on the current website *within some time window*. Formally, each query is classified into

$$y_{ijk} = \begin{cases} 1, & \text{if } i \text{ is a purchaser on website } j \text{ after search } k \\ 0, & \text{if } i \text{ is not a purchaser on website } j \text{ after search } k, \end{cases}$$

for a consumer i on website j on the kth query observed by the intermediary. We denote the classification made in real-time by the intermediary as $\hat{y}_{ijk}$. For every consumer i we observe a series of searches on website j, $X_{ij1}, X_{ij2}, ..., X_{ijn}$ and, if the consumer ended up making a purchase on this website, the timestamp of when consumer i purchased on website j. This allows us to further construct the ground truth label, y_{ijk}^{TRUE}, which we use to evaluate the performance of the classifier.[42] We will denote the *class proportion* as the proportion of searches whose ground truth label is purchaser.

For each search, the intermediary produces a probability estimate that the consumer is a pur-

[42]The ground truth labels are constructed by setting $y_{ijk}^{TRUE} = 1$ if the purchase occurs within N_j days of the search and $y_{ijk}^{TRUE} = 0$ otherwise. While in practice the value of N_j is website-dependent, we do not observe this value for each website so we restrict focus to $N_j = 2$ across all websites. For the majority of websites in our sample ,the intermediary informed us that they set $N = 1$ or $N = 2$. Furthermore, from our preliminary analysis, the results do not qualitatively differ between $N = 1$ and $N = 2$.

chaser:

$$p_{ijk} = \Pr(y_{ijk}^{TRUE} = 1 \mid X_{ij1}, ..., X_{ijk}), \forall i, j, k \tag{2.3}$$

We observe the intermediary's predicted $\hat{p}_{ijk}$ and $\hat{y}_{ijk}$ for every search as well as the y_{ijk}^{TRUE} which we construct. The conversion of probability estimate, $\hat{p}_{ijk}$, to actual classification, $\hat{y}_{ijk}$, is based on whether the consumer's "score" $\hat{p}_{ijk}$ is above or below a chosen threshold $\hat{P}$. The threshold is chosen based on revenue considerations and other factors irrelevant to the quality of the predictions and, as a result, we focus on analyzing the prediction error associated with the probabilistic estimate $\hat{p}_{ijk}$ and not $\hat{y}_{ijk}$.

2.6.1 Prediction Evaluation Measures

To evaluate the performance of the classifier deployed by the intermediary, we use two standard measures from the machine learning literature: the Mean Squared Error (MSE) and Area under the ROC Curve (AUC).[43]

The MSE computes the mean of the squared errors associated with the predicted estimate $\hat{p}_{ijk}$ relative to the realized binary event. Specifically, let $\mathcal{I}_j$ be the set of all consumers on website j and let $\mathcal{K}_{ij}$ be the set of all events for consumer i on website j. Then, the MSE of website j is given by,

$$MSE_j = \frac{1}{\sum_{i \in \mathcal{I}_j} |\mathcal{K}_{ij}|} \sum_{i \in \mathcal{I}_j} \sum_{k \in \mathcal{K}_{ij}} (\hat{p}_{ijk} - y_{ijk}^{TRUE})^2, \tag{2.4}$$

with a low MSE indicating a good prediction performance.

[43] [109] and [110] provide a comprehensive analysis of classification evaluation metrics and differentiate between three classes of evaluation metrics. (1) Metrics based on a threshold that provide an error rate on actual classifications as opposed to predicted probabilities. (2) Metrics based on a probabilistic interpretation of error, which capture the difference between the estimated and true probabilities. (3) Metrics based on how the classifier ranks the samples in terms of likelihood to be a purchaser as opposed to a non-purchaser. As mentioned previously, we ignore the first class of metrics since there are idiosyncrasies in how the threshold is set across websites and so do not analyze the actual classifications. We select the most commonly utilized metrics from the latter two classes. From the second class of evaluation metrics we choose the MSE and from the third class we choose the Area Under the ROC Curve (AUC) metric.

Although commonly used, the MSE has a couple of drawbacks for the current purpose. First, the measure is sensitive to the skewness of, and the change in, the class distribution. In the current context, about 90% of the searches result in non-purchase, which means that the estimate $\hat{p}_{ijk}$ tends to be low; intuitively, the estimate would tolerate more errors associated with the "infrequent" event (purchase) in order to minimize the errors associated with the more "frequent" event (non-purchase). Suppose now the class distribution changes so that more searches result in purchases. This is indeed what happens in our data after GDPR. Then, even though the consumer may not have become less predictable, MSE would rise artificially, due to the convexity associated with the formula, especially if the prediction algorithm does not adjust to the change in the distribution. Second, perhaps not unrelated to the first issue, the MSE is not the measure that the intermediary focuses on for its operation as well as for communicating with its partners. Instead, it focuses on AUC (the area under the curve), which we now turn to.

The AUC measures the area under the Receiver Operating Characteristic (ROC) curve.[44] The ROC curve in turn measures how well the classifier trades off Type I ("false positive") with Type II ("false negative") errors. The AUC provides a simple scalar measure of the prediction performance. If either the prediction technology improves or the consumer becomes more predictable, then the ROC will shift up and AUC will increase. Aside from the fact that the intermediary focuses on this measure, the AUC is invariant to the change in class distribution [111]. Suppose for instance the proportion of purchasers increases. As long as the prediction technology remains unchanged the ROC and AUC remain unchanged.

These two measures capture different aspects: AUC captures the ability for the classifier to separate the two different classes whereas MSE captures the accuracy of the estimated probabilities. Hence, we will report the effect on both since they provide two qualitatively different measures of prediction performance.

[44] We provide additional details on the construction of the AUC and its interpretation in Appendix B.6.1. For an extended discussion of ROC analysis, see [111].

2.6.2 Prediction Performance

In this section we investigate the impact of GDPR on predictability at the immediate onset of its implementation. We utilize the same empirical strategy that we described in section 2.3. The same empirical design is valid because the intermediary trains separate models for each website using only the data from the respective website. As a result, any changes to the collected data from EU websites due to GDPR should not impact non-EU websites. However, there are two limiting factors in our analysis. The first is the restriction on the data; unlike the search and advertising data, the prediction performance requires additional purchase data, which is available only for a subset of websites.[45] The second is that the models are trained utilizing a sliding window of the data, which means that, even if there is a sudden change to the underlying data distribution, there may be a slow adjustment period that would vary across the different websites. Since the pool of consumers has changed with GDPR our predictability regressions compare the larger set of consumers before GDPR with a smaller set of consumers after GDPR. Changes in predictability are therefore a function of both the quantity of data and the selection of consumers where consumers with longer histories remain in the data.

Table 2.4 displays the difference-in-differences estimates for all of the relevant prediction related outcome variables. First, column (1) shows that GDPR results in a small but significant increase in the proportion of purchasers. Meanwhile, the insignificant coefficient for average prediction probability (i.e. $\hat{p}_{ijk}$) in column (2) shows that little adjustment by the classifier of the firm to this change. Figure B8 in section B.7 displays the time-varying specification for these outcome variables indicating that the average predicted probability remains constant whereas the class proportion fluctuates but appears to increase.

Columns (3) and (4) show the impact of GDPR on the prediction performance of the intermediary as measured in MSE and AUC, respectively. Column (3) shows a significant increase in MSE

[45] We drop observations that either have no purchase data or where the class proportion is degenerate. There are also two websites that we know had a reporting error for purchase data during our sample period and we drop them from our analysis. Further, we drop any $(browser, OS, product, website, country)$ tuple that, on average, has fewer than 50 consumers a week since these observations are very noisy due to low sample sizes and the performance of the prediction problem is less interesting in these cases.

after GDPR. However, rather than indicating the worsened prediction performance, this is likely to be an artifact of the change in class proportion and the lack of adjustment by the classifier.[46] Indeed, columns (5) and (6) show that MSE conditional on true class has not gone up; if anything, they have gone down albeit statistically insignificantly. As mentioned above, given the skewed distribution, an increase in the proportion of purchasers will raise the MSE. In fact, column (4) shows a positive estimate for the treatment effect on AUC indicating a marginal improvement in prediction, though it is not statistically significant. The marginal improvement in AUC indicates that the intermediary's ability to separate the two classes has increased. This observation is consistent with what we would expect from the aforementioned hypothesis of privacy means substitution.

Table 2.4: Difference-in-Differences Estimates for Prediction Outcome Variables

	(1) Class Proportion	(2) Average Predicted Probability	(3) MSE	(4) AUC	(5) Purchaser MSE	(6) Non-Purchaser MSE
DiD Coefficient	0.00915*	0.00129	0.0130***	0.0124	-0.00579	-0.00126
	(1.77)	(0.17)	(3.74)	(1.12)	(-0.43)	(-0.45)
Product Type Controls	✓	✓	✓	✓	✓	✓
OS + Browser Controls	✓	✓	✓	✓	✓	✓
Week FE	✓	✓	✓	✓	✓	✓
Website × Country FE	✓	✓	✓	✓	✓	✓
Observations	15470	15470	15470	15470	14298	15470

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 29, including both weeks 16 and 29 (April 13th - July 20th). The dependent variable in the regression reported in the first column is the proportion of purchasers associated with each observation and the second column is the average predicted probability. The dependent variables in the third and fourth column are the MSE and AUC, respectively. Finally, in the fifth and sixth columns the dependent variables are the MSE conditional on the true class of the observation.

Finally, Figure B9 in section B.7 displays the results from the time-varying specification for MSE and AUC, indicating that there was an initial increase in MSE followed by an eventual decline. This is consistent with the claim that much of the increase in MSE was a result of the lack of

[46] Appendix B.6.2 decomposes the change of MSE to accounts for the extent to which the increase may have resulted from the classifier's lack of rapid adjustment to the post-GDPR consumer distribution leading the estimated class probabilities to no longer as closely match the empirical class probabilities.

rapid adjustment. Furthermore, the increases in AUC do not occur directly after GDPR but rather also occur gradually.

Overall, our results suggest that GDPR has not negatively impacted the ability to predict consumer behavior and if at all, the sign of the treatment effect suggests the opposite. This is further validated by the exercise in section B.8 which identifies the expected "long run" changes in prediction performance as a result of the changes to the data observed in section 2.4. This exercise shows that an increase in trackability will likely improve prediction performance, whereas the change in the overall size of data as a result of GDPR should not adversely impact prediction performance significantly.

2.7 Conclusion

In this paper we empirically study the effects of data privacy regulation by exploiting the introduction of GDPR as a natural experiment. We use data from an intermediary that contracts with many online travel agencies worldwide, which allows us to investigate the effect of GDPR on a comprehensive set of outcomes. Our analysis focuses on the stipulation of GDPR that requires firms to ask consumers for explicit consent to store and process their data.

Our results paint a novel and interesting picture of how a consumer's privacy decision— particularly the means by which she protects her privacy—may impact the rest of the economy, including other consumers, and the firms and advertisers relying on consumer data. The strong and effective means of privacy protection made available by laws such as GDPR and the recent CCPA (California Consumer Privacy Act) should help the privacy-concerned consumers to protect their privacy by eliminating their digital footprints. These consumers are thus clear winners of the laws. However, the impacts on the others are less clear. Our results suggest the possibility that a consumer's switching of the means of privacy protection makes the opt-in consumers who share their data more trackable and possibly more predictable to the firms with which they share data. If this increased trackability makes up for decreased data (resulting from opt-outs), as indicated by section B.8, then the firms using consumer data could also come out as winners. What about those consumers who opt in? Their welfare will depend on how their data is used by the firms. If their data is used to target advertising and services to their needs, they too could very well be winners of privacy laws, even if their decision to opt in may not have accounted for the externality. However, if their data is used for extracting consumer surplus, e.g., via personalized pricing, the externalities could harm them.

While these qualitative implications are clear, our reduced-form approach does not allow us to quantify the welfare implications for both consumers and advertisers. We leave for future work a structural analysis of the interactions that we identify in order to better understand the magnitude of each of the channels by which consumers and advertisers are affected. Given the large compliance costs associated with data privacy regulation, decomposing the welfare effects in this manner is a fruitful direction for research and important for further building on our insights in order to guide the design and understanding the value of such regulation.

Finally, our paper has broader implications beyond the online travel industry and keyword-based advertising markets. Firms in this industry, as with many markets in the digital economy, increasingly compete with the large technology firms such as Google whose reach expands across many different online markets and for whom consumers have little choice but to accept data processing. As a result, while our results highlight that increased consent requirements may not be wholly negative for firms, if consumers are similarly using such opt-out capabilities at our estimated rates in other markets (such as behaviorally-targeted advertising markets) then such regulation may put firms in these markets at a disadvantage relative to these larger firms. It would be important to study the extent and magnitude of these adverse effects. We believe that these insights and directions for future work are useful for the design of the many proposed regulations in the US and around the world that follow in the footsteps of GDPR.

Chapter 3: Recommenders' Originals: The Welfare Effects of the Dual Role of Platforms as Producers and Recommender Systems

3.1 Introduction

An increasing number of online platforms deploy recommender systems to assist consumers with purchase decisions by providing information on available goods, which determinedly impacts consumer choice. These systems facilitate information acquisition on product quality by consumers in environments where there are thousands or even millions of alternatives available. Existing experimental literature supplies causal evidence of these systems' immense power in steering demand, with market shares being significantly affected even by recommendation systems that supply simple information to consumers [112]. Moreover, anecdotal evidence reflects the huge influence the information provided by these systems has on consumption choices individuals make: recommendations are said to account for 75% of consumed content on Netflix and 35% of page views on Amazon [113].

However, online platforms increasingly not only deploy recommender systems, but also produce and make available their own goods alongside other firms', with unclear implications to consumer welfare. Major platforms and technological leaders in the development and deployment of recommender systems — such as Amazon, Netflix, and Spotify — all now develop their own goods that are then made available on their platforms: Amazon produces more than 22,000 goods that are available on the firm's platform [114], Netflix hosts more than 2,300 "Netflix Originals" titles [115], and Spotify is now investing in producing its own audio content [116].

One possible consequence of this dual role of the platforms as both a recommender system and a producer is that platforms may systematically bias their search and recommendation systems towards their own goods. Indeed, not only is there a substantial amount of popular press coverage suggesting this,[1] there are also a number of recent legislative initiatives that appear motivated by potential abuse of this dual role, both in trying to prevent platforms in this dual role from biasing their recommendation systems in favor of their own goods and in proposing the separation of the roles of recommender and producer.[2] While the goal of regulators is to increase consumer welfare in such markets, it is unclear whether these regulatory initiatives will ultimately harm or hurt consumers once equilibrium effects are taken into account.

In this paper we study the welfare consequences of a platform acting as both a producer and a recommender and consider the specific role played by the deployment of a recommender system. We set up a stylized model of a pay-for-access platform where producers make investment decisions about the quality of their goods and revenue is split according to each goods' market share. Our main focus is in contrasting resulting consumer welfare, investments, and market shares across three different scenarios: the *no platform production* case, where only a good by an independent firm is available; the *platform dual role* case, where the platform can both produce a good and design recommendations, and both the platform's good and the independent firm's are available; and the *unbiased recommendations* case, where we modify the dual role case by imposing an exogenous policy that requires that recommendation be unbiased, or truthful and neutral.

Unlike other papers that study the consequences of platform steering (e.g. [122, 123, 124]), we model the platform's recommendation as providing information on good quality to consumers as opposed to directly influencing the search order or choosing the consumed good for a fraction of consumers. Producers have access to stochastic investment technology that affects the likelihood that goods are of high versus low quality. Consumers' prior beliefs on good quality stem from the observed investments, and they update their beliefs on each good's quality based on the recommendation policy of the platform. This allows us to build credibility of recommendation directly into

[1]For instance, [117] discusses Amazon's bias towards its own goods via recommendation and search. [118] discusses how Google manipulates its search results to steer consumers. According to [119], upon release of *House of Cards*, Netflix recommended that consumers watch it regardless of their past behavior. Pandora has stated in court that it manipulates its recommendations based on the ownership of the sound recordings.

[2]An example of tackling possible biases in recommendations is the European Commission's legislative proposal called "Digital Markets Act" [120], whereas the recent bill H.R. 3825 - "Ending Platform Monopolies Act" proposed to the House of Representatives of the United States aims to terminate situations of this dual role altogether [121].

our model, where the platform's ability to steer the behavior of rational and Bayesian consumers is naturally limited and depends on the design of its recommendations.

In our model, the revenue generated by the platform is based on pay-for-access and producers are compensated according to their (expected) consumption share. We believe this captures the fundamental elements that platforms in this dual role face (e.g. Spotify, Netflix), and constitutes one of the primary drivers in motivating the platforms to bias recommendations towards their own goods. Additionally, as a reduced-form proxy for the extent to which the independent firm is reliant on the platform for revenues, we allow the independent firm to have access to alternative sources of revenue, and characterize their impact on equilibrium investment decisions and consumer welfare. For instance, in the movie industry, [125] reports that box office revenues are an essential source of revenue for movie producers, while [126] highlights that in the music industry, recording artists made most of their revenues from touring despite the widespread usage of online streaming services.[3] We show that the degree to which the independent firm depends on the platform as its main source of revenue is decisive for whether the platform's dual role has a positive or a negative effect on consumer welfare.

The platform's entry into the upstream market affects consumer welfare both via investments in good quality, as well as through the platform's recommendation policy, which provides consumers with information on realized good quality.

Although limited by the need for recommendations to be credible in order for consumers to choose the recommended good, the platform is able to bias recommendations towards its own good so as to reappropriate the gains from information provision to increase its market share. Hence, the platform's dual role leads to biased recommendations that induce a significant pressure on the independent firm's equilibrium investment decisions: for the independent firm to appropriate a share of platform demand it need not only to invest more than the platform, but it also needs the realized quality of its good to be strictly higher than the platform's.

[3] Exploiting the exogenous negative demand shock on movie theaters as a result of COVID-19 illustrates the marginal incentives of movie producers to respond to the relative weight of platform vs. offline sources of revenue [127].

While it would be natural to expect consumer welfare to increase with the platform's entry — as product variety increases and as more information cannot harm consumers — the prospect of biased recommendation and its resulting equilibrium effect on investment levels leads to an ambiguous effect on consumer welfare. When the independent firm is heavily dependent on the platform and recommendation bias favoring the platform is most significant, the platform's entry results in lower investment levels and consumer welfare. However, if the alternative sources of revenue are sufficiently large, the platform's entry increases consumer welfare.

This result is driven by two opposing forces which influence the marginal incentives of the independent firm to invest in quality, compared to when the independent firm is the sole producer. The first is a revenue expansion channel, which expresses the ability of the firm to expand the revenue it obtains from the platform by increasing its investment level. This channel is depressed by platform entry and its use of biased recommendations as these significantly decrease the independent firm's market share and, therefore, the independent firm receives but a fraction of its marginal impact on total platform revenue. The second is a new share expansion channel, whereby additional investment by the independent producer allows it to increase its market share. The platform uses its recommendation policy to appropriate a share of the demand, but its ability to bias is limited by the difference in investments in good quality. Then, larger investment by the firm naturally limits the platform recommendation bias, which then expands the firm's market share. These two opposing forces generate a threshold effect for the investment levels of the independent firm in terms of its reliance on the platform as its main source of revenue: When the independent firm overly relies on revenue obtained from the platform, the platform's dual role effectively depresses its incentives to invest in quality, resulting in lower consumer welfare; if instead it has access to other sources of revenue that are significant enough, the independent firm invests more strongly than when it is the sole producer, driving up consumer welfare.

One important consequence of the platform's dual role and the resulting recommendation bias is the possibility of foreclosure of the independent firm. In equilibrium, this occurs when the independent firm's alternative revenue sources are small relative to the platform size, in which case the platform finds it profitable to become the product-quality leader. Then, due to to the ability to bias recommendations towards its own good, it completely drives demand away from its competition, capturing the entire demand on the platform. Although this is a sharp prediction, it echoes recent trends in video streaming markets, where platforms' original content quality — as indicated by awards received — has notably risen while their own content has simultaneously dominated platform viewership. When the independent firm's revenue sources are large enough compared to the platform's revenue potential, the platform becomes a product-quality follower. Even then, the platform still partially forecloses the independent firm by biasing recommendations in favor of its own goods, enabling it to achieve a higher market share and profit than otherwise.

We explore a natural policy remedy: ensuring that the platform cannot simultaneously provide recommendation services and produce goods; or, equivalently, a policy that prevents the platform from providing biased recommendations towards its own goods.[4] This analysis further provides insight into how much of the distortion in consumer welfare and investment decisions is due to the platform's ability to bias recommendations relative to entry and information provision alone. Although it would be reasonable to expect an unambiguous improvement in consumer welfare — since more informative recommendations should only help consumers relative to biased recommendations — we find that this policy can actually harm consumers under certain conditions.

There are two observations that lead to this result, which stem from the fact that the unbiased recommendation policy induces downward equilibrium adjustments in investment decisions that can outweigh the welfare gains arising from unbiased recommendation. The first is that the revenue expansion channel is more responsive to investments by the quality follower, and less to investments by the quality leader. This is because, under the dual role, the platform's use of recommendations to expand its demand share leads to fully dissipating any informational gains to consumers, and revenue is completely determined by the investments of the quality leader. Unbiased recommendations maximize informational gains and expand overall revenue, but lead to the aforementioned effect on how investments by the platform and the firm affect total revenue at

[4] We define *unbiased* recommendations as recommendations that are both truthful (i.e. always recommending the highest quality good) and neutral (breaking ties uniformly at random).

the margin. The second is that imposing unbiased recommendations results in a depressed share expansion channel relative to the dual role for both the independent firm and the platform. Thus, imposing a fairer competition for demand share between the platform and the independent firm by requiring unbiased recommendations has the unexpected negative effect of reducing pressure to invest in quality by both parties.

Combined, these two observations result in an ambiguous welfare effect of imposing unbiased recommendations. Relative to the dual role, unbiased recommendations improve consumer welfare when the platform's market size is comparable to the firm's alternative market size. However, when the platform's market size is large enough or when the independent firm relies mostly on alternative revenue sources, stripping the platform from its power to bias recommendations in its favor overwhelmingly depresses incentives to investment and entails a loss in consumer welfare. Further, the magnitude of welfare effects of such a policy depends crucially on the industry's structure and the relative weight of different revenue sources.

Our results illustrate how platforms entering the upstream production market benefit from biasing recommendations. However, the resulting distortion from this entry does not necessarily harm consumers, as it may spur other producers to invest more aggressively in good quality to counter not only increased product competition, but especially recommendation bias. An important element to consider is how dependent the other producers are on revenues from the platform. Only when this dependence is significant will policies targeting bias in recommender systems or separating recommendation and production altogether have a positive effect on consumer welfare. While platforms and online retailers have other means at their disposal to distort consumption choices that are not explicitly addressed in this paper, our model suggest caution when considering policy interventions: the bias in recommender system may be inducing independent firms to produce higher quality goods than what they otherwise would. Indeed, our results provide a rationale for the stipulations of the *Ending Platform Monopolies Act* that ends the dual role only for dominant, "gatekeeper" firms. In such cases the welfare effect for consumers is likely to be positive, whereas for smaller platforms it will have an ambiguous or lesser effect.

Related Work

Our paper lies in the intersection of three different literatures: biased intermediation, recommender systems and, more broadly, vertical integration and foreclosure.

Most relevant to our paper is the nascent biased intermediation literature and, more generally, biased information provision. This emerged from the traditional intermediation and two-sided market literature [128, 129], and focuses on the incentives of an intermediary to bias consumption decisions. While the majority of the literature focuses on the consequences of an intermediary manipulating the search process of a user, we instead model the intermediary as providing information to consumers. The intermediary in our model, the recommender, is thus an information designer as in the Bayesian persuasion literature [130, 131]. This has two main advantages. The first is that it provides a more accurate model of how recommender systems function, where recommendations provide imperfectly informed consumers with information about goods on the platform whose true consumption values are only learned from experience. The second is that it requires recommendations to be credible in order to affect consumer behavior, thereby disciplining the ability of platforms to steer demand.

Within this literature, the papers closest to ours are [132], [123] and [122]. [132] study the incentives of a pay-for-access platform to bias their recommendation in order to reduce the market power of the upstream content providers. They also consider that the payouts between the producers and the platforms are split via royalty fees that depend on consumer's consumption choices. They focus on perfectly horizontal preferences with fixed good characteristics, whereas our model considers the effect that platform recommendation bias has on quality investments. [123] study a model of biased intermediation where the recommendation is sold through an auction while [122] examine a setting where consumers perform costly and sequential search and the intermediary directs the consumers to a seller; in both cases, uninformed consumers naively follow the recommendation, without any credibility constraint. In contrast, in our model recommendations have to be credible and depend on investment levels, and both are endogenously determined. Other related papers — where investment decisions are absent — focus on price competition among sellers on the platform [133], advertising and search [134, 135, 136], and commission and price setting (Inderst and Ottaviani [138, 137], [124]).

Naturally, this paper contributes to the literature on recommender systems which analyzes the consequences of recommendation on consumer choice, pricing, and sales. [139] analyzes model with horizontally differentiated products where a platform with a recommender system competes with a fringe of distribution channels without such system. In this paper, the recommender's information advantage is modeled as deriving observing past users' experience in a two-period model and consumers are able to obtain a recommendation at no cost. Opposite to our model, the emphasis in this paper is on the optimal pricing by the recommender, and investment decisions are absent as all firms are intermediaries selling the same products. Other, less related works are [140], [141], [142]. [140] characterizes the optimal information provision by a welfare maximizing recommender that learns a good's quality through consumer feedback. [141] discuss the role that recommender systems can play in diversifying sales due to their personalized nature, but do not endogenize production or consider incentives to bias recommendation. [142] examines how a recommender would trade-off between optimizing for profit and maintaining reputation amongst consumers, but do not consider good investment or platform production.

Subsequent to our work, [143] and [144] study economic models of the impact of recommender systems on consumption choices with implications for competition policy. [143] focus on how recommender systems lead to excessive market concentration, and [144] explore recommendation performance as a function of consumer data and consumption choices, taking into account the degree of misalignment between the platform and consumers.

Finally, our paper broadly contributes to a classic literature in industrial organization that studies vertical integration, upstream entry, and investment [145, 146, 147], and vertical foreclosure [148, 149]. We are most interested in the dual role of platforms as producers — upstream entry — and information providers, which has not received as much attention in the literature. Two papers that look at related problems are [150] — studying the role of vertical information restraints in a retail market that involves search frictions with a focus on understanding minimum advertising price restrictions — and [151], who introduces a vertical industry structure into a consumer search model where consumers are uninformed about wholesale prices. These papers are complementary to ours, but, to our knowledge, we are the first to study the integration of recommendation and production and how this can lead to a novel form of vertical foreclosure.

The remainder of the paper is structured as follows: Section 3.2 provides the setup for the model. The impact of the platform's dual role on equilibrium investment decisions and consumer welfare consequences is analyzed in Section 3.3. In Section 3.4, we explore the value of recommendation and characterize the equilibrium welfare consequences of a policy that imposes unbiased recommendations. We discuss the robustness of the results to the different assumptions in Section 3.5, before concluding with some final remarks in Section 3.6. Proofs are omitted from the main text and can be found in Appendix C.1.

3.2 Model Setup

This section introduces the main elements of our model.

Production. There are two firms, the independent firm F and the platform P. We consider two cases: one in which only F, the independent firm, makes production decisions, and another where both F and P make production decisions. We denote by J the set of firms making production decisions. In the case where both F and P make production decisions, we suppose that the independent firm's investment q_F is observable by the platform before deciding its investment. We argue that this timing assumption is realistic as platforms are usually second-movers in production decisions. However, as we discuss in Section 3.5, our conclusions do not rely on this: a setting where production decisions are simultaneous yields the same qualitative results.

Each firm $j \in J$ produces a single good x_j, which is either of high quality, $x_j = 1$, or low quality, $x_j = 0$. The realized quality of the goods ultimately produced (x_j) is stochastic and depends on the firms' investments. Each firm initially makes investment decisions $q_j \in [0, 1]$ which determine the probability that the realized quality of the good is high, that is, $q_j = \mathbb{P}(x_j = 1)$. This need not be taken as pure vertical differentiation: high quality can be interpreted as idiosyncratic to a given consumer and q_j then refers to the probability that a given consumer will enjoy the good and deem it high quality. For simplicity, we assume that firms face a quadratic cost to this investment in (stochastic) quality: $C_j = q_j^2$.

Good Distribution. We focus on a subscription-based pricing model, which is ubiquitous in many markets where recommender systems are widely deployed — such as media streaming or news platforms. Firm P serves as a platform for consumers in the market to access the goods produced. It sets a single price τ, a subscription fee, that consumers need to pay in order to gain access to the platform. Once on the platform, consumers can choose which of the available goods to consume without incurring additional cost.

Consumers. All consumers prefer high-quality goods to low-quality goods, but vary in their willingness-to-pay. Consumer i's utility from joining the platform ($e_i = 1$), consuming good x and paying access fee τ is given by $u(x, \theta_i, \tau) = \theta_i x - \tau$, where θ_i denotes consumer i's willingness-to-pay for high quality goods; not joining the platform ($e_i = 0$) gives the consumer zero utility. Consumers' willingness-to-pay θ_i is uniformly distributed on $[0, \bar{\theta}]$, and its distribution function is denoted by G. Consumers maximize expected utility and we assume they are well-informed in that they observe investments prior to deciding whether to access the platform and which good to consume. Finally, we denote by M_P the measure of consumers that access the produced goods exclusively through the platform; for simplicity we denote a given consumer as $i \in [0, M_P]$.

Recommendation Policy. We model platform recommendations as providing consumers with information on the realized good qualities. We define a recommendation policy ρ as the probability of recommending firm j's good to consumers contingent on good quality.[5] The platform then acts as an information designer who chooses a recommendation policy before the quality of goods x_P and x_F is realized. Upon observing the realized recommendation $m \in J$, consumers update their beliefs on the quality of the goods and choose the one that maximizes their expected utility.

A key goal is to understand how different properties of the recommendation policy affect con-

[5] Note that this specification is without loss: While information provision by the platform would generically consist of a a mapping from the set of possible states, the realized good qualities $\{0, 1\}^J$ to distributions over an arbitrary message space $\mathcal{M}$, it is well-known that it is without loss of optimality to consider the message spaces that correspond to the available actions.

sumer welfare. Two important aspects of recommendations for policy considerations are whether they convey truthful information to consumers about which good is best, and that they do not confer undue advantage to any given good. Formally, we say that a recommendation policy is truthful if, given realized qualities x_P, x_F, the recommended good is of weakly higher quality than the non-recommended good ($\mathbb{P}_\rho(m \in \arg\max_{j \in J}\{x_F, x_P\}) = 1$); and recommendations are said to be neutral if ties in quality are broken uniformly at random ($\mathbb{P}_\rho(m = x_P \mid x_P = x_F) = 1/2$). Unbiased recommendation policies are those that are both truthful and neutral.

Although allowing the platform to commit to a recommendation policy is ultimately a simplifying modeling device, commitment is a common and arguably well-grounded assumption in the literature in economics and computer science studying strategic interactions between recommender systems and Bayesian consumers [140, 152, 153]: Not only can it be justified by the need for credibility in the context of repeated interactions, but also by the fact that existing recommender systems are stable deployed algorithms which imply commitment to a predefined recommendation strategy.

We focus on the role of recommendations as being utilized purely for steering subject to the constraint of rational, Bayesian consumers. For that purpose, we will assume that consumers' decision whether to enter the platform occurs prior to the choice of a recommendation policy by the platform. This timing is not devoid of justification: consumers' decision to join the platform or not is arguably more stable when compared to the more frequent adjustment of recommendation systems, taking as given (and make use of the information from) the pool of consumers on the platform.

Revenue Sources Outside the Platform. We suppose that firm P only gets revenue from the platform itself, as is common for private-label goods — "recommender's originals" such as Netflix's, Spotify's, Hulu's, where their good is only available on their own platform. Contrastingly, we allow the independent producer, F, to obtain additional revenue streams off the platform, R_F, depending implicitly on its investment in quality. We interpret these additional revenue as the reliance of the independent firm on the platform revenues. This can be seen as a reduced-form proxy for revenues from offline alternatives to the online platform. For instance, in the case of Spotify, this could be the revenue achieved from concerts or album purchases; for Netflix, it could be the revenue achieved from movie theaters. In order to study how equilibrium and consumer welfare change as the strength of the outside option for the producing firms increases we parameterize $R_F = r_F \cdot q_F$ with $r_F > 0$. We interpret the parameter r_F as the size or relevance of this alternative market in which firm F operates.[6] We discuss the robustness of our results to the existence of alternative revenue sources for the independent firm in Section 3.5.

Revenue on the Platform. The platform revenue R_P will depend on the recommendation policy, pricing and quality investments of both firms. We suppose that it is split between the platform and the independent firm according to their expected consumption share on the platform, which we denote as α_P and α_F, $\alpha_P + \alpha_F = 1$. This split rule can be interpreted as the reduced form of a linear contract agreed to by the platform and the independent producer where the independent producer gets a royalty fee for each consumer that consumes her good. This type of contractual relationship is common on pay for access platforms — for instance, Spotify pays musicians a royalty in accordance with the number of times their song is played on the platform, and on YouTube Premium the membership fees are distributed based on how many members watch a producer's content. We take this split rule as an exogenous industry benchmark and study its consequences.

The overall expected payoffs for each firm is therefore given by:

$$\pi_P = \alpha_P \cdot R_P - C_P$$

$$\pi_F = \alpha_F \cdot R_P + R_F - C_F,$$

where the dependence on investment levels, recommendation policy, and access fee is implicit.

Timing. In line with the setup defined above, the timing of events in the model is summarized as follows:

[6] If consumers preferences in this unrelated market are also quasilinear and their willingness-to-pay is uniformly distributed on $[0, \hat{\theta}]$, then were firm F a monopolist in such market optimally setting a uniform price given the chosen investment level, one would have that the generated revenue would be $r_F \cdot q_F$, with $r_F = M_F \cdot \hat{\theta}/4$.

1. Production decisions are made sequentially, first by the independent firm, then by the platform.
2. The platform determines access fee τ and consumers decide whether to join the platform or not, resulting in revenue R_P.
3. The platform commits to a recommendation policy.
4. Good qualities realize.
5. The platform makes recommendations.
6. Consumers decide which good to consume conditional on investment probabilities and platform recommendation.
7. The independent firm and the platform split the platform revenue according to their consumption share, and the independent firm accrues outside revenue R_F.

Other variations on the timing are discussed in Section 3.5.

3.3 Consequences of the Platform's Dual Role

In this section we study the consequences of the platform's dual role. We contrast the case where only the independent firm's good is available on the platform to the equilibrium in the case where the platform can itself choose to produce a good and steer consumers via its recommender system.

3.3.1 No Platform Production

We first consider the case where the independent firm is the sole producer in the market. The timing is the same, but the platform does not make production decisions. As there is only one good available on the platform and the platform does not observe good quality before consumers make the decision to join the platform, there is no scope for recommendation policy to impact consumers' valuation of paying to access the platform.

We assume that pricing is still profit maximizing.[7] The expected value of subscribing to the platform for a consumer of type θ is then $\mathbb{E}u(x, \theta, \tau) = \theta\mathbb{E}[x] - \tau$, where in the case we are analyzing we have the expected quality of the consumed good is $\mathbb{E}[x] = \mathbb{E}[x_F] = q_F$. A consumer of type θ then subscribes to the platform whenever $\mathbb{E}u(x_F, \theta, \tau) \geq 0$,[8] and therefore $e_i = \mathbf{1}_{\theta_i \geq \tau/\mathbb{E}[x_F]}$.

The platform's pricing problem is then

$$\tau \in \arg\max_{t\geq 0} M_p \cdot t \cdot \int_0^{\bar{\theta}} e_i \, dG(\theta) = \arg\max_{t\geq 0} t \cdot \left(1 - G\left(\frac{t}{q_F}\right)\right).$$

Given the uniform distribution assumption, the solution to the pricing problem is given by $\tau = \frac{1}{2}\bar{\theta}q_F$. This implies that $R_P = \frac{M_P \cdot \bar{\theta}}{4} q_F$.

For convenience, we define $r_P := \frac{M_P \cdot \bar{\theta}}{4}$, so that $R_P = r_P \cdot q_F$. The parameter r_P represents the total potential revenue that can be accrued on the platform. Both r_P and r_F can be taken as reduced form measures of market size and relative comparison between the two provides us with a notion of the relative dependence of the independent firm on the platform. The implications of the platform's dual role will crucially depend on how large the platform market size is relative to alternative markets available to the independent firm.

The firm's production decision problem is then given by

$$\max_{q_F \in [0,1]} \alpha_F \cdot R_P + R_F - C_F = \max_{q_F \in [0,1]} r_P \cdot q_F + r_F \cdot q_F - q_F^2$$

where $\alpha_F = 1$, as the firm is the only producer in the market. The firm's optimal production decision in the "no-platform-production" (NP) case is

$$q_F^{NP} = \min\left\{\frac{r_P + r_F}{2}, 1\right\}.$$

[7]Although the platform has no incentive to set profit maximizing prices in the current setup, as it effectively accrues no revenue, it can be seen — and indeed it is — the limit case of a related setup where the platform gets a fixed share s of the overall revenue, where this share is arbitrarily small. This analysis of this exact case yields the same qualitative results.

[8]Although the tie-breaking rule favors subscribing, this is not consequential for investment decisions as types are continuously distributed.

Finally, we note that consumer welfare is linear in expected quality of the good consumed,[9] given that

$$W^{NP} = \mathbb{E}[e_i \cdot u(x_F, \theta, \tau)] = \int_{\frac{\bar{\theta}}{2}}^{\bar{\theta}} \theta q_F - \tau \, dG(\theta) = \frac{3}{8}\bar{\theta}^2 \, q_F^{NP}.$$

3.3.2 Platform's Dual Role

We now characterize the equilibrium investment levels of the case where the platform can also make production decisions. Given that now more than one good is available on the platform, recommendations play a role in determining how much consumers value having a platform subscription.

A subgame-perfect equilibrium is a tuple (ρ, τ, q_P, q_F) where the platform P chooses a recommendation policy ρ, an access fee τ, and an investment level q_P, the independent firm F chooses investment level q_F in order to maximize their respective profits, and each consumer $i \in [0, M_P]$ makes a decision on whether to join the platform (e_i) and which good to consume (x_i) so as to maximize their expected utility.

As is standard, we solve for equilibrium via backward induction. We recall the (inverse) timing of events:

9. The independent firm and the platform split the platform revenue according to their consumption share and the independent firm accrues outside revenue R_F.

8. Each consumer i selects the good x_i with the highest expected utility conditional on recommendation and investment probabilities:

   $x_i \in \arg\max_{x_j \in \{x_P, x_F\}} \mathbb{E}[u(x_j, \theta_i, \tau) \mid \rho, m]$ given the recommendation policy ρ and the realized recommendation, breaking ties in favor of the platform.

7. The platform's recommendations realize:

[9] If one considers a similar market structure underlying the alternative revenue sources for the independent firm (see Footnote 6), this linearity of consumer welfare in expected quality of the independent firm's good extends also to this alternative independent market.

$m \sim \rho(x_P, x_F)$, where $\rho : \{0,1\}^2 \to \Delta(J)$ and $m \in J$.

6. Good qualities realize:

   $x_j \sim \mathbb{P}(x_j = 1) = q_j$ and $\mathbb{P}(x_j = 0) = 1 - q_j$, with $j = P, F$.

5. The platform commits to a recommendation policy ρ:

   $\rho \in \arg\max_{r:\{0,1\}^2 \to \Delta(\mathcal{M})} \alpha_P \cdot R_P - C_P$ given R_P, q_P and where the platform's market share is given by $\alpha_P = \mathbb{E}\left[\mathbf{1}_{x_P = x_i} \mid e_i = 1\right]$.

4. Each consumer i decides whether to join the platform ($e_i = 1$) or not ($e_i = 0$):

   $e_i \in \arg\max_{e \in \{0,1\}} e \cdot \mathbb{E}[u(x_i, \theta_i, \tau) \mid \rho]$.

3. The platform determines access fee τ, resulting in revenue R_P:

   $\tau \in \arg\max_{t \geq 0} t \cdot M_P \cdot \mathbb{E}_\theta[e_i]$.

2. The platform determines its investment level, q_P:

   $q_P \in \arg\max_{q'_P \in [0,1]} \alpha_P \cdot R_P - C_P$.

1. The independent firm determines its investment level, q_F:

   $q_F \in \arg\max_{q'_F \in [0,1]} \alpha_F \cdot R_P + R_F - C_F$.

Optimal Recommendation Policy

The first step is to solve for the optimal recommendation policy. The platform faces a standard Bayesian persuasion problem [130], choosing a conditional signal distribution to maximize its profits. As the consumer has two actions — choosing either the platform's or the independent firm's good — it is without loss to consider recommendation policies with at most two messages, $J = \{F, P\}$, where we interpret $\rho(x_P, x_F) = P$ $(= F)$ as recommending the consumer to choose the platform's (resp. firm's) good. The only constraint is one of credibility: the consumers cannot be left worse off by following the recommendation than if they were not to follow it. The problem can then be written as

$$\max_{\rho:\{0,1\}^2 \to \Delta(J)} \alpha_P \cdot R_P - C_P$$

subject to credibility constraints

$$\mathbb{E}[u(x_P, \theta_i, \tau) \mid \rho(x_P, x_F) = P] \geq \mathbb{E}[u(x_F, \theta_i, \tau) \mid \rho(x_P, x_F) = P]$$
$$\mathbb{E}[u(x_F, \theta_i, \tau) \mid \rho(x_P, x_F) = F] \geq \mathbb{E}[u(x_P, \theta_i, \tau) \mid \rho(x_P, x_F) = F].$$

The next proposition characterizes the platform's optimal recommendation policy and its implications for the expected good quality and market shares:

Proposition 1. *The optimal recommendation policy by the platform is such that*

(i) if $q_P \geq q_F$, the platform always recommends its good;

(ii) if $0 \leq q_P < q_F \leq 1$, then the platform always recommends its own good whenever it has weakly higher quality than the independent firm's, $\mathbb{P}(\rho(x_P, x_F) = P \mid x_P \geq x_F) = 1$, and, when its own good has strictly lower quality, it is recommended with probability $\mathbb{P}(\rho(x_P, x_F) = P \mid x_P < x_F) = \frac{q_P}{1-q_P}\frac{1-q_F}{q_F}$.

Moreover, the expected good quality is given by $\mathbb{E}[x_m \mid \rho] = \max\{q_P, q_F\}$, and the expected market shares of the platform and the independent firm are $\alpha_P = \min\{1, 1-(q_F - q_P)\}$ and $\alpha_F = 1 - \alpha_P = \max\{0, q_F - q_P\}$, respectively.

Proposition 1 highlights how the platform is able to use recommendations to heavily favor its own goods and brings forth two key insights. First, that in order to have its good recommended and subsequently consumed on the platform, the firm not only needs to initially invest strictly more than the platform, but it also needs its good's realized quality to be strictly higher than that of the platform's. And, even then, the platform will in general still be able to steer consumers to choose its good with positive probability. Second, that, with the platform-optimal recommendation policy, consumers are left as well off from following recommendations than from defaulting to their prior and choosing the good that entailed the largest investment. As we explain below, this is because rent extraction by the platform warrants recommendation bias and, even though recommendations cannot hurt consumers — more information is always weakly beneficial to decision-makers — optimal recommendation bias leads to dissipating any informational gains to consumers.

Let us discuss the intuition underlying the results in Proposition 1. When the platform invests more than the independent firm ($q_P \geq q_F$), it can capture all the market share ($\alpha_P = 1$) by always recommending its good. This is because, without additional information, consumers always default to choosing the good with greater probability of realizing of high quality. Then, in this case, the optimal recommendation policy is credible, given that consumers are left no worse off by following the recommendation; and, as recommendations are uninformative and independent of realized good quality, it actually leaves consumers exactly as well off by following the recommendation as they would be defaulting to their prior. Furthermore, we note that if there is ex-ante uncertainty about which good is best ($1 > q_P \geq q_F > 0$), then the optimal recommendation policy is also biased, as the platform always recommends its good even when the independent firm's is of strictly higher realized quality.

When instead the independent firm invests strictly more than the platform in good quality ($q_P < q_F$), the platform has to choose an informative recommendation policy in order to garner some market share for its own good. If we consider the cases where it is ex-ante known that the independent firm's good will always be of weakly higher quality than the platform's ($1 = q_F$ or $0 = q_P$), since credibility constraints require consumers not to be left worse off by following recommendations, only truthfully recommending the best good available is credible. To see this, note that any small bias toward the platform's good beyond tie-breaking leads to consumers strictly preferring to ignore the recommendation and choose the independent firm's good. Hence, the best the platform can do is to commit to truthfully recommend the best good available and, if possible, use recommendations only to break ties in favor of its own good and capture some market share.

As soon as we step away from that extreme case and have ex-ante uncertainty over which good is of higher realized quality ($1 > q_F > q_P > 0$), the platform will be able to credibly recommend its good even when the strictly better alternatives are available. In such a case, there is a possibility that the platform's realized good quality is high and the independent firm's is low —

despite the firm having invested more. It is then immediate that truthful recommendations strictly increase the expected quality for consumers relative to defaulting to choose the independent firm's good, as $\mathbb{E}[x_m \mid \rho] = q_F + (1 - q_F)q_P > q_F$. However, the platform can profitably deviate from truth-telling by, with small probability, recommending its good when the independent firm's realized quality is strictly higher: this improves the platform's market share, while still leaving consumers better off by following the recommendation. And, for any credible recommendation policy that leaves consumers better off, the platform can then continue increasing recommendation bias in this manner and retain credibility, but only up to the point in which consumers are left indifferent, with the expected consumption quality obtained by following the recommendations equalling the expected quality from choosing the independent firm's good. At an optimum, while recommendations are informative about realized good qualities,[10] recommendations are biased in such a manner that they bring no informational value to consumers. As a result — and despite the increase in number of goods on the platform — consumer welfare depends exclusively on the quality of the independent firm's good.

Optimal Access Fee

The pricing problem faced by the platform is such that

$$\tau \in \arg\max_{t \in \mathbb{R}} M_P \cdot t \cdot \int_0^{\bar{\theta}} e_i dG(\theta)$$

where $e_i \in \{0, 1\}$ describes consumer i's decision of whether or not to join the platform, given the optimal recommendation policy, that is, $e_i = \mathbf{1}_{\mathbb{E}[u(x_m, \theta_i, \tau) | \rho] \geq 0}$.

The next proposition shows that the optimal access fee is similar to the no-platform-production case:

Proposition 2. *The optimal access fee is given by $\tau = \frac{1}{2}\bar{\theta}\max\{q_P, q_F\}$, resulting in a total revenue collected by the platform of $R_P = r_P \max\{q_P, q_F\}$. Moreover, the resulting consumer welfare is given by $\mathbb{E}[e_i \cdot u(x_m, \theta_i, \tau) \mid \rho] = \frac{3}{8}\bar{\theta}^2 \max\{q_P, q_F\}$.*

[10] Whenever the independent firm's good is recommended, it must be of high quality and the platform's of low quality, and thus recommendations do inform consumers about realized qualities.

Proposition 2 provides the optimal access fee, total revenue, and consumer welfare given production decisions. Each of these quantities directly follows from the expected quality of the goods that consumers experience on the platform, which is characterized in Proposition 1, together with the fact that the optimal recommendation policy is independent from the consumer's type θ_i and the access fee.

One implication of the above result is that, whenever $q_F \geq q_P$, the optimal access fee under the dual role is identical to the access fee charged in the no-platform-production case, where only the independent firm is producing. This partly is a result of the timing of recommendation in the model, which was set up to isolate the role of credibility and gives no explicit incentive for the platform's recommender system to optimally trade off revenue per consumer and overall platform demand. Nevertheless, the induced bias implicitly affects the platform demand by reducing the expected good quality; and, hence, the price consumers are willing to pay to join the platform. Furthermore, while the access fee and welfare are identical in the case when $q_F \geq q_P$, the independent firm's expected good quality q_F is endogenous and determined in equilibrium. This implies that the platform's entry influences consumer welfare on the platform only through the endogenous response of the independent firm's investment decisions to the ability of the platform to bias its recommendations.

Investment Decisions

Finally, we characterize the optimal investment decisions of the firms, with the platform as a second-mover.

The platform's investment problem is given by

$$\max_{q_P \in [0,1]} \alpha_P \cdot R_P - C_P$$

As $\alpha_P = \min\{1, 1 - (q_F - q_P)\}$ and $R_P = r_P \max\{q_P, q_F\}$, we have that the objective function is continuous and piecewise strictly concave in q_P, with

$$\pi_P(q_P, q_F) = \alpha_P \cdot R_P - C_P = \begin{cases} r_P \cdot (1 - (q_F - q_P)) \cdot q_F - {q_P}^2 & \text{if } q_P < q_F \quad (3.1) \\ r_P \cdot q_P - {q_P}^2 & \text{if otherwise.} \quad (3.2) \end{cases}$$

Proposition 3. *The equilibrium investment levels for the platform are given by:*

$$q_P(q_F) = \begin{cases} \frac{r_P}{2} q_F & \text{if } q_F \geq \tilde{q}_F \text{ and } r_P < 2 \\ \min\left\{1, \frac{r_P}{2}\right\} & \text{if } q_F < \tilde{q}_F \text{ and } r_P < 2, \text{ or } r_P \geq 2 \end{cases}$$

where $\tilde{q}_F \equiv \frac{r_P}{4 - r_P}$.

Proposition 3 immediately follows from two observations. The first is that if $r_P \geq 2$, then the maximizer of both (3.1) and (3.2) is $q_P = 1$. The second is that if $r_P < 2$, then the maximizer of (3.2), $\frac{r_P}{2} < 1$, is always weakly larger than the maximizer of (3.1), $\frac{r_P}{2} q_F < q_F$, and so the platform will choose to invest at $\frac{r_P}{2}$ when $\pi_P(\frac{r_P}{2} q_F, q_F) \leq \pi_P(\frac{r_P}{2}, q_F)$ and $\frac{r_P}{2} \geq q_F$, which is equivalent to $q_F \leq \tilde{q}_F$. Furthermore, we break indifference in favor of the second-arm, when $q_F = \tilde{q}_F$. The tie-breaking will be immaterial in characterizing the equilibrium investment levels, and so is without loss for the overall conclusions.

The solution to the platform's investment decision has a very natural interpretation: If the independent firm's investment in quality is too low, then the platform is better off by investing as if it were the only producer on the platform; and, indeed, it is going to recommend only its own goods. If instead the independent firm's investment is high enough, then the platform invests below what it would were its good the only one on the platform – as a single good monopoly case – saving in investment costs at the expense of the independent firm. It can still enjoy some positive market share and get a part of the revenue R_P as it will bias recommendations towards its own goods when its realized quality is weakly higher than the firm – and, sometimes, even when it is not.

The independent firm's investment problem is then to choose q_F in order to maximize $\pi_F(q_F) = \alpha_F \cdot R_P + R_F - C_F$, given $q_P(q_F)$. By backward induction, the independent firm's payoffs can be written as follows:

$$\pi_F(q_F) = \begin{cases} (q_F - q_P(q_F)) \cdot r_P \cdot q_F + r_F \cdot q_F - {q_F}^2 & \text{if } r_P < 2 \text{ and } q_F \geq \tilde{q}_F = \frac{r_P}{4-r_P} \quad (3.3) \\ r_F \cdot q_F - {q_F}^2 & \text{if otherwise} \quad (3.4) \end{cases}$$

Proposition 4. *The equilibrium investment level for the independent firm is given by*

$$q_F^{DR} = \begin{cases} \min\left\{1, \frac{r_F}{2}\right\} & \text{if } r_F \leq \underline{r_F} \text{ and } r_P < 2\text{, or if } r_P \geq 2 \\ \tilde{q}_F & \text{if } \underline{r_F} \leq r_F < \overline{r_F} \text{ and } r_P < 2 \\ \min\left\{1, \frac{r_F}{2(1-r_P)+{r_P}^2}\right\} & \text{if } \overline{r_F} \leq r_F \text{ and } r_P < 2 \end{cases}$$

where $\tilde{q}_F \equiv \frac{r_P}{4-r_P}$, $\underline{r_F} \equiv \frac{r_P}{4-r_P}\left(2 - \sqrt{2r_P\,(2-r_P)}\right)$, *and* $\overline{r_F} \equiv \frac{r_P}{4-r_P}\left(2(1-r_P)+{r_P}^2\right)$. *Furthermore, except when* $\underline{r_F} = r_F$, *the investment levels are uniquely determined.*

The intuition for the proof of Proposition 4 is easily summarized. If $r_P \geq 2$, the platform will always set $q_P = 1$ and recommends only its good, regardless of the firm's investment level, thereby excluding the independent firm from considering platform revenue. Consequently, the firm's optimal investment considers only the revenue that it is able to garner from outside markets. When instead $r_P < 2$, there are three cases to consider which depend on the magnitude of r_F relative to r_P. In the first case, where r_F is small enough relative to r_P, we have again that the independent firm's investment decisions only depend on the relative strength of the outside markets, since the platform's ability to bias recommendations is sufficiently strong to foreclose the independent firm entirely from the platform. In the second case, where r_F attains intermediate values relative to r_P, the independent firm invests in quality just enough to leave the platform indifferent between foreclosing the independent firm and investing in lower (expected) quality, allowing the independent firm some market share. In this case, the investment that the independent firm would make in the absence of platform revenue would be considerably lower, and fighting for larger consumption share on the platform is not worthwhile. However, the independent firm strictly benefits from getting enough of the platform demand and investing more than it would when considering only the outside revenue. Finally, in the last case, where r_F is large relative to r_P, it is now profitable for the independent firm to invest in even higher quality, securing a larger share of the platform demand and leading to higher revenue from platform subscriptions.

3.3.3 Welfare Consequences of the Dual Role

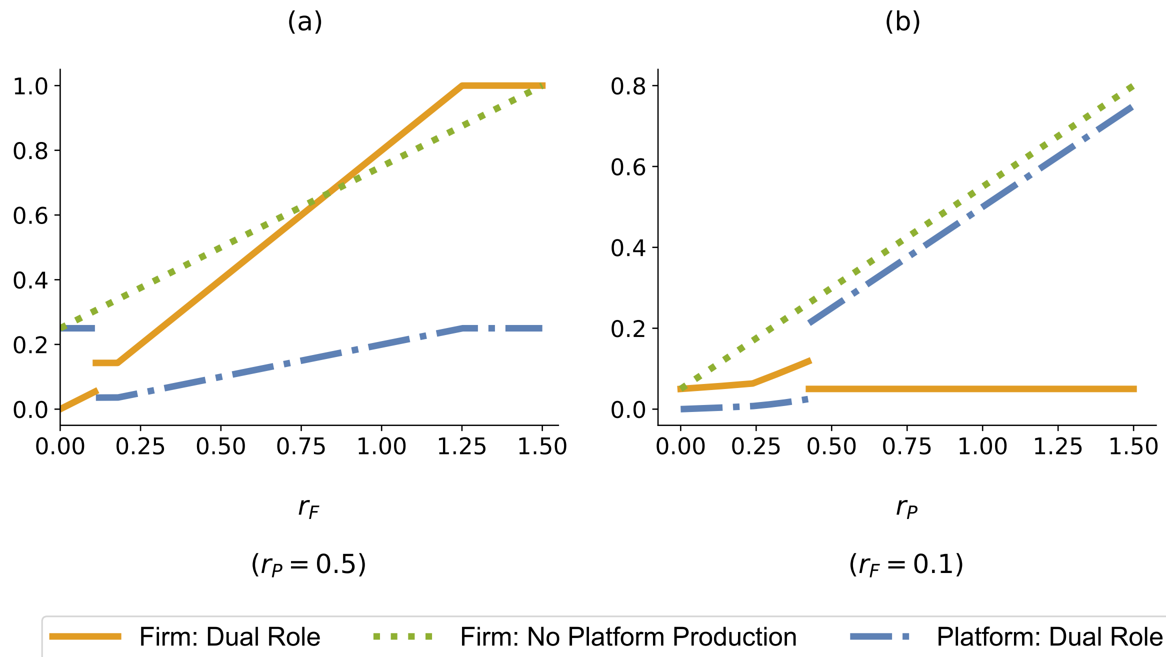
We now study the implications of the dual role on consumer welfare. Our primary question of interest is whether the increased competition in the production market through the entry of platform increases or decreases consumer welfare. We now state the main result comparing welfare between the two cases:

Proposition 5. *Consumer welfare is weakly higher under the dual role than under the no platform production case if and only if* $r_F \geq \frac{2(1-r_P)+r_P^2}{\max\{1,2-r_P\}}$ *or* $r_P \geq 2$.
It is strictly higher if and only if $2 - r_P > r_F > \frac{2(1-r_P)+r_P^2}{2-r_P}$.

Proposition 5 shows that, despite the increased competition in the upstream market, the platform's dual role results in lower quality investments and lower welfare when the independent firm's alternative market relevance is small compared to the platform's – cf. Figures 3.1 and 3.2.[11] This shows that when the platform steers a substantial fraction of the demand utilizing biased recommendations, platform upstream competition decreases consumer welfare. It not only adversely impacts consumer welfare, but also strictly depresses the industry's total profits since, if the independent firm is the sole producer, its profits attain the industry's maximum. Therefore, the increased competition leads to an overall, unambiguous, decrease in total surplus. However, in situations where the independent firm has access to significant alternative revenue sources, the platform's dual role becomes welfare improving.

[11] While Proposition 5 only focuses on consumer welfare on the platform, if one further assumes identical consumer preferences in the revenue sources outside of the platform, then the conclusion from Proposition 5 that for the dual role can either improve or harm consumer welfare extends to including these sources as well, although the thresholds would differ. Moreover, if the dual role strictly improves on on-platform consumer welfare, it also improves on off-platform consumer welfare. A notable difference would be that if $r_P \geq 2$, the platform invests such that $q_P = 1$, leaving on-platform consumer welfare unchanged, but as the independent firm is foreclosed, it will invest strictly less than in the no-platform-production case, with negative welfare consequences for off-platform consumers.

Figure 3.1: Equilibrium Investment Levels



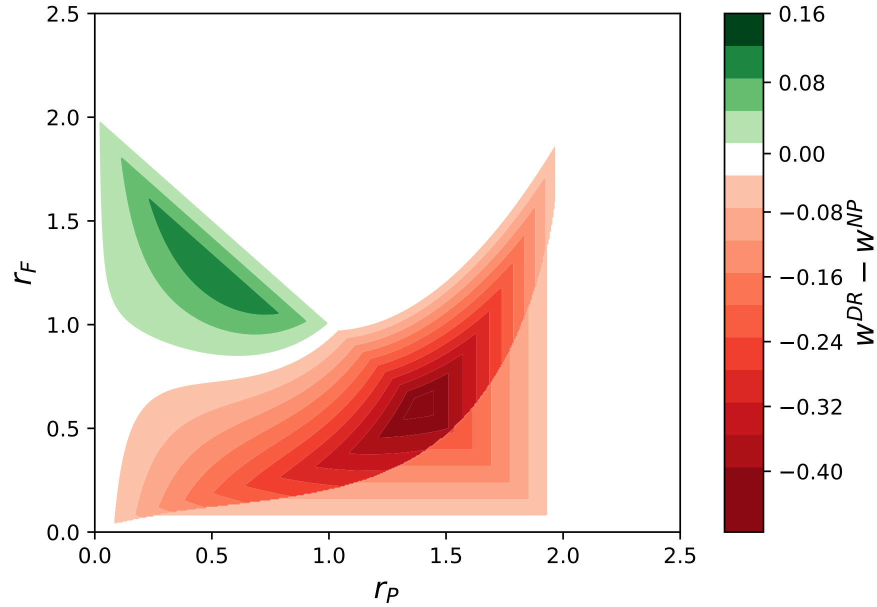
Notes: This figure displays the investment levels across the no platform production and dual role cases. Panel (a) shows how investment levels change as we increase the alternative revenue sources for a fixed level of platform potential revenue. Panel (b) shows how investment levels change as we increase the platform potential revenue for a fixed level of alternative revenue sources.

In order to better understand the reason that the platform's entry and recommendation bias change the investment incentives of the independent firm, consider the total derivative of the independent firm's profit function with respect to its investment choice:

$$\frac{d\pi_F}{dq_F} = \underbrace{\frac{\partial \alpha_F}{\partial q_F} \cdot R_P}_{\text{Share Expansion}} + \underbrace{\alpha_F \cdot \frac{\partial R_P}{\partial q_F}}_{\text{Revenue Expansion}} + \frac{\partial R_F}{\partial q_F} - \frac{\partial C_F}{\partial q_F}$$

In the no platform production case, the *share expansion* term is always zero since the independent firm always has $\alpha_F = 1$ due to its being the only good on the platform. This allows the independent firm to extract the full impact of its investment on expanding the overall platform revenue through the *revenue expansion* term. In the dual role case, the marginal incentives to increase investment are driven by both of these terms: while the entry of the platform softens the marginal gains from revenue expansion, it introduces the marginal gains coming from the introduction of business

Figure 3.2: Average Consumer Welfare: Dual Role – No Platform Production



Notes: This figure displays the difference in expected good quality, which corresponds to average consumer welfare on the platform, between the dual role and the no platform production cases for varying r_F and r_P. The regions in green (red) correspond to cases in which consumer welfare is strictly greater (lower) under the dual role than under no platform production.

stealing through the share expansion component. Focusing on the case when $q_F > \tilde{q}_F$ and $r_P < 2$, $\frac{d\pi_F}{dq_F} = (1-\frac{r_P}{2})\cdot r_P\cdot q_F+(q_F-\frac{r_P}{2}q_F)\cdot r_P+r_F-2\cdot q_F$ under the dual role, and $\frac{d\pi_F}{dq_F} = r_P+r_F-2\cdot q_F$ under no platform production. Thus, the marginal investment incentive is larger under the dual role when $q_F > \frac{1}{2-r_P}$, indicating that the role of the share expansion term is strong enough to countervail the negative effects of entry on the revenue expansion term and increase overall marginal incentives to investment. It is then the need to counter the bias in recommendations that can effectively drive the independent firm to invest more than in the no-platform-production case.

The model holds another significant implication: Once the platform relevance becomes significant relative to other revenue sources, the platform becomes the good quality leader and uses recommendations to perform vertical foreclosure. As illustrated in Figure 3.1b, in equilibrium the platform may benefit from completely disregarding the independent firm, producing as if it were a monopolist. This occurs when the independent firm's alternative revenue sources are not strong enough relative to platform potential revenue to induce it to compete for market share with the platform as a producer. The outcome of this interaction is then a form of vertical foreclosure by means of the platform's use of biased recommendations — the independent firm's good is never recommended to consumers, even if it is of higher realized quality than the platform's — which establishes the platform as the quality leader by precluding the independent firm's access to the platform's demand. This necessarily has a negative effect on consumer welfare as the expected good quality on the platform is lower than it would be when compared to a case where only the independent firm's good is available.

This prediction echoes anecdotal evidence on streaming platforms. First, the already mentioned concerns that platforms bias their recommendation systems towards their own goods to the detriment of goods produced by other firms manifests itself in our model through the biased recommendations the platform uses to steer consumers towards its own goods. Second, data on viewer and subscriber patterns on streaming platforms indicates that there is a positive correlation between number of subscribers, platform original content's share of the total platform viewership, and ranking of platform's original content.[12] Even though the model's prediction is sharp and reality is necessarily more complex and nuanced, the model suggests one possible mechanism underlying such anecdotal evidence.

Nevertheless, the need to compete for market share on the platform can still drive the independent firm to stronger quality investments as the platform's audience loses its overwhelming relevance within the industry, possibly due to the emergence of alternative platforms. When the independent firm has a base incentive to support sufficiently high quality investments — when the alternative revenue sources are significant enough relative to the platform's — it is worthwhile for the firm to invest even more and compete for market share on the platform. In this case, the platform becomes a quality follower relative to the independent firm, making use of recommendations to appropriate a substantial fraction of the market share that the independent firm's production decisions attract.

[12] See, for the case of Netflix, e.g. "Netflix Original Series Viewing Climbs, but Licensed Content Remains Majority of Total U.S. Streams" [154] and "Netflix Subscriber Numbers Soar, Showing the Sky Is Not Falling" [155].

3.4 Unbiased Recommendations

In this section we consider a natural policy remedy: requiring the platform's recommendations to be unbiased — truthful and neutral. Truthfulness requires that the platform always recommends a good when it is of strictly higher quality than the other, while neutrality requires that the platform always breaks ties uniformly. Another interpretation of the unbiased recommendation case is as a separation (or divestiture) between the platform's production and recommendation activities. Therefore, this also corresponds to a benchmark of two producers, independent from the platform. As it is unclear the extent to which the platform's dual role is distinct from a case of simple upstream entry with information provision — known to have ambiguous welfare consequences in cases of pure vertical differentiation — this section further provides clarification on the distinctive effects of the platform's dual role.

3.4.1 Equilibrium Characterization

The model remains the same, up to imposing unbiased recommendations: The platform now recommends whichever good realizes the highest quality, with uniform tie-breaking. This implies that the expected good quality that consumers end up getting is

$$q^U := q_P \cdot q_F + q_P \cdot (1 - q_F) + (1 - q_P) \cdot q_F = q_P + (1 - q_P) \cdot q_F$$

The solution to the optimal access fee is analogous to the one from before, leading also to a similar expression for platform revenue R_P. In fact, the revenue maximizing access fee is the same up to replacing $\max\{q_P, q_F\}$ with the new expression for expected good quality q^U. Thus, the optimal access fee and resulting revenue are given by $\tau = \frac{1}{2}\bar{\theta} \cdot q^U$ and $R_P = r_P \cdot q^U$.

Expected market shares are given by $\alpha_P = 1 - \alpha_F = \frac{1}{2}(1 - (q_F - q_P))$, resembling the form of market share under biased recommendations with two noteworthy differences. First, market shares are always strictly positive for any strictly positive quality investment. This immediately implies that the independent firm is now able to capture a share of the market, even when it invests less than the platform, as imposing unbiased recommendations precludes the use of this policy instrument by the platform to induce vertical foreclosure. Second, the incentives for the firm to compete for market share are dampened: the marginal change in the firm's market share from increasing its investment is halved when compared to the biased recommendation case with positive market share.

The platform's production problem is given by the same expression up to the changes in R_P and α_P:

$$\pi_P(q_P, q_F) = \alpha_P R_P - C_P = \frac{1}{2}\left(1 - (q_F - q_P)\right) \cdot r_P \cdot (q_F + (1 - q_F)q_P) - {q_P}^2 \tag{3.5}$$

We note that the platform has a unique best response to the independent firm's investment level:

Lemma 1. *The platform's optimal investment, $q_P^U(q_F) := \arg\max_{q_P \in [0,1]} \pi_P(q_P, q_F)$ is unique and continuous in q_F.*

The firm's investment problem is now given by

$$\pi_F(q_P, q_F) = \alpha_F R_P + R_F - C_F = \frac{1}{2}\left(1 - (q_P - q_F)\right) \cdot r_P \cdot (q_F + (1 - q_F)q_P) + r_F \cdot q_F - {q_F}^2$$

with $q_P = q_P^U(q_F)$.

It results that this problem has a unique maximizer and therefore we obtain a unique equilibrium:

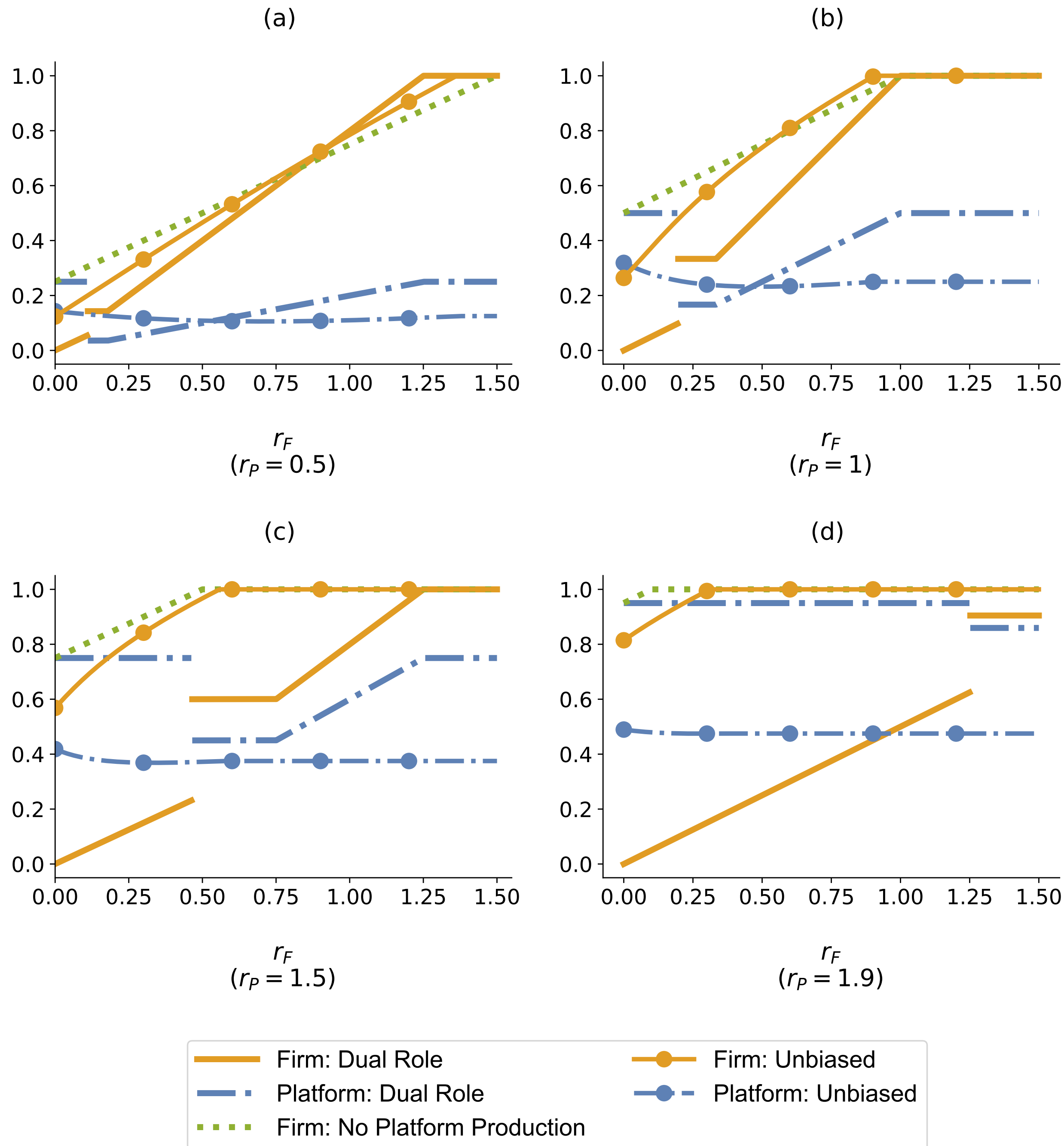
Proposition 6. *Equilibrium investment levels with unbiased recommendations are uniquely defined.*

3.4.2 Comparison to Dual Role Equilibrium

Investment Levels

We now directly compare the equilibrium investment levels in the different cases. Figure 3.3 depicts the equilibrium investment levels as we vary the significance of the firm's alternative revenue,

Figure 3.3: Equilibrium Investment Levels including Unbiased Recommendation



Notes: This figure displays the investment levels across the unbiased, no platform production, and dual role cases. Each figure plots the changes in investment levels as we vary the strength of the alternative revenue sources for representative values of platform potential revenue.

r_F, for different representative values of platform market sizes, r_P. The figure shows that imposing unbiased recommendations has an ambiguous effect on firm equilibrium investment levels.

We first focus on the independent firm. Similarly to before, the independent firm invests less in the unbiased case compared to the dual role when r_F is high and r_P is low (see Figure 3.3b). There are two channels that depress the incentives for the independent firm to invest. First, we note that the *share expansion* term, the marginal gain on market share $\left(\frac{\partial \alpha_F}{\partial q_F}\right)$, is halved under unbiased recommendations compared to the dual role. Second, we have that the *revenue expansion* term, the marginal increase in overall platform revenue as a result of increasing independent firm investment $\left(\frac{\partial R_P}{\partial q_F}\right)$, is r_P in the dual role case when $q_F > q_P$, but is only $r_P(1-q_P)$ under unbiased recommendations.

However, there are also two forces that act in the opposite direction and encourage the independent firm to increase investment. The first is that the platform's reaction to the independent firm's quality investment is milder as it can no longer bias recommendations in its favor. The other is that even though the marginal effect of investment on market share is lower, unbiased recommendation still fosters higher market shares for the independent firm.

When r_F is high enough and r_P is low enough, the mechanisms that depress incentives are stronger and drive the independent firm to invest less under unbiased recommendations compared to biased recommendations. With higher r_P, the channels that drive the independent firm to invest more become stronger than those that depress investment. Thus, when r_P is sufficiently high, the forces that lead to higher investment dominate the investment-depressing channels, leading to overall higher investment levels in the unbiased case.

We now investigate the effect that unbiased recommendation has on the platform's production decision, which can similarly be seen in Figure 3.3. We highlight three main results.

First, when the platform potential revenue is high relative to the firm's alternative markets, we previously noted that the platform is able to use recommendation to effectively become a monopolist. When recommendations have to be unbiased, the resulting competition leads to a decrease in the platform's equilibrium investments. Although it is reasonable to expect that the platform being unable to capture the whole market would lead to more aggressive investment by the platform, this is not the case. As the independent firm has higher returns to investments than the platform — due to its alternative revenue sources — it results that it will always obtain at least half of the market share under unbiased recommendations. This leads to competition resulting in lower investment by the platform.

Second, unbiased recommendations depress platform investment when the independent firm's alternative revenue is large enough. When the platform can bias recommendations, it has a stronger incentive to keep up with the independent firm's investments as r_F increases. In contrast, under unbiased recommendations, this effect turns negative as the alternative revenue sources grow more and more significant. In this case, as happens for the independent firm, unbiased recommendations actually dampen competition incentives, making it less worthwhile for the platform to produce higher quality goods given the halved effect on the market share and virtually no marginal change in total revenue when q_F is close to 1.

Finally, if the platform's potential revenue is high enough and the firm's alternative markets are not too significant, the firm's investment under unbiased recommendations is lower than the platform's when it can bias recommendations. As it is possible to observe in Figures 3.3c and 3.3d, when r_P is high enough and r_F is low, in order to capture full platform demand in the dual role case, the platform invests more in expected quality than it does in the unbiased recommendations case. Moreover, the platform's equilibrium investment levels are then consistently higher under the dual role, than it is when imposing unbiased recommendations.

The two main findings regarding to investment levels are then the following:

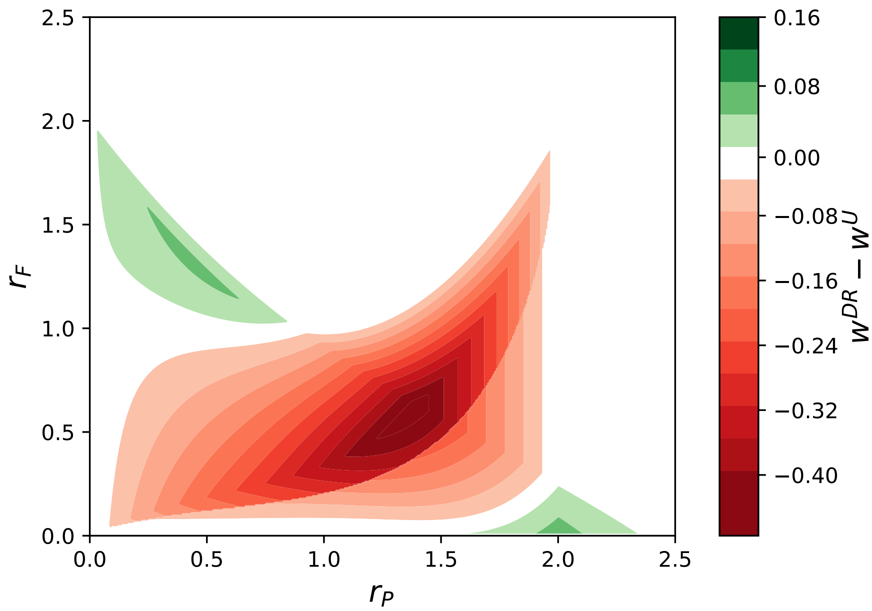
Remark 1.

1. *When the platform's market size is small and the firm's alternative market size is large, imposing unbiased recommendations leads to lower investments by the independent firm.*

2. *When the platform's market size is large relative to the firm's alternative market size or when the latter is large enough, imposing unbiased recommendations leads to lower investments*

by the platform.

Figure 3.4: Average Consumer Welfare: Dual Role – Unbiased



Notes: This figure displays the difference in expected good quality, which corresponds to average consumer welfare on the platform, between the dual role and unbiased recommendations cases for varying r_F and r_P. The regions in green (resp. red) correspond to cases in which consumer welfare is strictly greater (resp. lower) under the dual role than under no platform production.

Consumer Welfare

The analysis of the resulting differences in consumer welfare between the two regimes does not follow immediately from assessing the platform's and the independent firm's investment levels separately since the expected market shares further differ as a result of the change in recommendation policy. In both of these regimes consumer welfare depends in the exact same manner on expected good quality and so our results here have direct parallels to the previously discussed changes in investment levels. There are two channels through which this policy affects welfare. The first is that, fixing investment levels and prices, unbiased recommendations have an unambiguously positive effect on consumer welfare, owing to consumers being better informed about each good's realized quality. This leads to differences in expected market shares for the producers, and, as a result, the second channel comes from responses in equilibrium investment levels.

Interestingly, imposing unbiased recommendations can lower consumer welfare both when the platform market size relative to the size of the firm's alternative market is large and when it is small — cf. Figure 3.4. Immediately, Remark 1 implies that when r_P is low and r_F is high, both the independent firm and the platform are investing less in quality under the new policy than when the platform is able to bias the recommendations. By itself, this need not imply that welfare is lower, as de-biasing recommendations leads to a higher informational value of recommendations for consumers, which could potentially outweigh the lower investments. When looking at Figure 3.4, we can observe this is not always the case: When the platform's market size is small and the firm's alternative market size is large enough, imposing unbiased recommendation leads to a welfare loss.

There is also a second case where unbiased recommendations can be welfare depressing: When the platform's market size is large relative to the firm's alternative market. In this case, in order for the platform to completely foreclose the independent firm utilizing biased recommendations would require it to undertake higher investments in quality than both the platform and the independent firm do with unbiased recommendations. This results in the platform increasing its investment in order to shut out the independent firm when it can take advantage of designing the recommendation policy to its favor, and this increased investment can be strictly beneficial for consumers. Therefore, it is exactly the ability to engage in anti-competitive practices enabled by the ability to bias recommendations that leads to a higher consumer welfare with biased recommendations in this case.

While the two cases identified above result highlight the striking result that preventing platforms from biasing their recommender systems may result in non-negligible losses in consumer welfare, Figure 3.4 shows that such a policy is welfare improving when the platform's market size is comparable to the independent firm's alternative market. Furthermore, in such circumstances, the magnitude of the welfare gains due to unbiased recommendations can be very significant. Then, when considering the consequences of such a policy, it becomes crucial to understand not only the industry's structure, but also its returns.

Following the above discussion, we summarize the main observations of this section in the following remark:

Remark 2.

1. *When the platform's market size is small and the firm's alternative market size is large, or vice-versa, imposing unbiased recommendations leads depresses consumer welfare.*

2. *When the platform's market size is comparable to the firm's alternative market size, imposing unbiased recommendations strongly increases consumer welfare.*

Decomposing the Foreclosure Effects

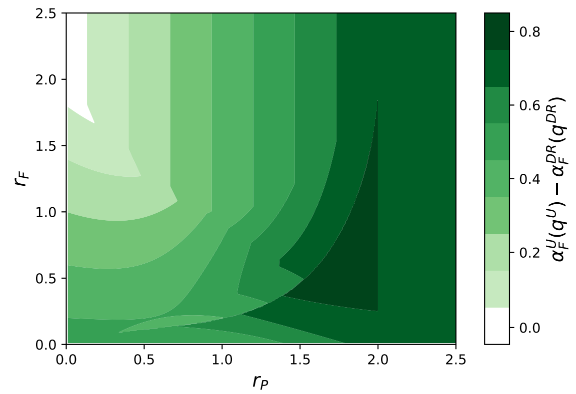
Finally, we decompose the extent to which the ability for the platform to bias recommendation allows it to shift market share to itself from the independent firm. In order to shed some light on the idiosyncratic effects of the dual role — platform upstream entry coupled with biased information provision — relative to a situation of upstream entry (with unbiased information provision) as that characterized by divestiture, we characterize the overall difference in market share between the dual role and unbiased case and identify to which extent of is due to biased recommendations received by consumers and to the equilibrium adjustment in investment levels.

Figure 3.5a displays the overall differences in the independent firm's market share $\alpha_F \in [0, 1]$ between the two regimes. As expected, overall the unbiased recommendation regime leads to a larger market share for the firm across all parameter values. Figure 3.5b compares the difference in market shares as a result of biased recommendation alone by fixing the equilibrium investment levels from the dual role and varying the recommendation policy between the two cases. Figure 3.5c imposes that the recommendation policy is always unbiased and compares the resulting difference in market shares across the dual role and unbiased equilibrium investment levels.

These figures suggest that the influence of both is non-trivial. On the one hand, the effect of de-biasing recommendations (Figure 3.5b) appears to play a larger role when the firm was already obtaining positive market share under biased recommendations, besides the case where it was already producing at maximum quality. On the other hand, equilibrium adjustments in quality

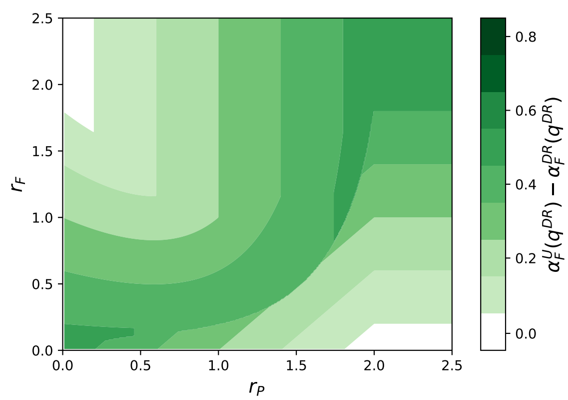
Figure 3.5: Difference in Market Share for Independent Firm

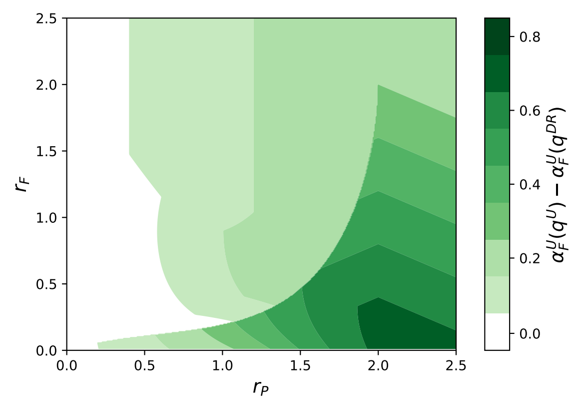
(a) Difference in Market Share: Unbiased – Dual Role



(b) Difference in Market Share: Unbiased with Dual Role Investments – Dual Role

(c) Difference in Market Share: Unbiased - Unbiased with Dual Role Investments





Notes: This figure displays the differences in the independent firm's market share, $\alpha_F \in [0, 1]$, between the unbiased and dual role cases. Panel (a) displays the overall difference in market share in equilibrium between the unbiased and dual role case. Panel (b) displays the difference in market share for the independent firm between the unbiased recommendation regime with investment levels fixed at the dual role levels, and the equilibrium in the dual role. Panel (c) displays the difference in market share between the equilibrium in the unbiased recommendation regime, and the market share under unbiased recommendations but with investment levels fixed at the equilibrium values for the dual role.

lead to extremely significant changes in market share when the firm's alternative revenue sources are meager, as under unbiased recommendations it can do always at least as well as the platform, increasing market share from zero to over 50% (Figure 3.5c). This underscores the extent to which the ability of platforms to bias recommendation can undeservedly shift substantial market share from independent firms' goods to the platform's goods. The clearly identifiable discontinuity is given by $r_F = \underline{r_F}$ (as given in Proposition 4), which determines the threshold at which the independent firm starts enjoying strictly positive market share in the dual role case.

3.5 Additional Discussion and Robustness Exercises

In this section we provide additional discussion about the assumptions in the model as well as several robustness exercises in order to show that the qualitative conclusions from our analysis are robust to different modeling assumptions.

Heterogeneous Costs: The model that we consider crucially relies on the ability of the independent firm to access outside revenue sources as this provides it with some advantage relative to the platform. Although we consider such outside revenue sources as an important element in reality, one could alternatively consider the independent firm to have a more efficient production process and thus has a lower marginal cost to invest in quality. This is can be an important difference in situations where independent producers are likely to have mature production processes for their goods, whereas the platforms are usually new entrants and unlikely to have as efficient production processes.

In Appendix C.2 we consider a setup where there is no outside revenue for the independent producer ($R_F = 0$), and, instead, there is a cost advantage, leading to potentially heterogeneous cost structures: The platform's production cost will still be $C_P(q_P) = q_P^2$, but the independent firm's is now $C_F(q_F) = c_F \cdot q_F^2$, with $c_F \in (0, 1)$. Keeping the remaining elements of the setup in the baseline model, we highlight the robustness of the main conclusions regarding the ambiguous welfare effects of both platform upstream entry and the requirement of recommendations be unbiased to this alternative specification.

While, in contrast to our benchmark model specification, recommendation bias and upstream entry by the platform results in the independent firm investing either maximally ($q_F = 1$) or minimally ($q_F = 0$), the dual role of the platform as both a recommender and a producer retains an ambiguous effect on consumer welfare in comparison to the other two situations: it will strictly improve consumer welfare when the independent producer's cost advantage is neither too significant nor too negligible and the potential market size of the platform —- as given by r_P —- is not too large; and depress it if otherwise.

Simultaneous Investment Timing: We show that our main conclusions are robust to the assumption on the timing of investments. The baseline model that we consider has investment decisions decided in a sequential manner with the independent firm moving first followed by the platform. While this timing is most natural, we further consider the case in which investments are simultaneous rather than sequential, keeping everything else in the model the same.

In Appendix C.3 we characterize the equilibria in the dual role and unbiased case under this timing. This modification of the timing implies that there might be multiple equilibria in the dual role case for a given range of parameters r_P, r_F. In spite of this, the resulting welfare comparisons are remarkably consistent with the sequential timing. Specifically, we obtain conclusions that are analogous to Proposition 5 and Remark 2: the platform's dual role may induce either higher or lower consumer welfare relative to both the no-platform-production and the unbiased recommendation benchmarks. The welfare comparisons are summarised in Figure C1 in Appendix C.3, and show that nearly the same parameter regions induce the no platform production case to be welfare improving relative to the dual role case as well as the unbiased recommendation case to be welfare improving relative to the dual role case.

Information Disclosure Timing: The recommendation provision by the platform was modeled as an optimal information design problem with commitment [130]. An alternative modeling assumption is that the platform could determine its recommendations after quality realizations, akin to a cheap talk problem [156]. While this could be an interesting avenue in other setups, in the context of our model, this would result in no information being conveyed in equilibrium, with only "babbling" equilibria persisting.[13]

Pricing: We consider a model of subscription-based pricing in order to restrict attention to the interplay between good quality and recommendation without introducing the additional complexity due to item pricing and issues of price competition. Many of the prominent online platforms which rely on recommender systems, such as media streaming platforms and news websites, follow a subscription-based model and, to our knowledge, platforms such as Spotify and YouTube have similar revenue splitting rules between independent producers and themselves.[14] In contrast, the insights of our model are not directly applicable to e-commerce platforms, such as Amazon or Wayfair, due to this modeling specification.

3.6 Conclusion

In this paper we study a stylized model of strategic interaction between a platform that deploys a recommender system and producers of the goods distributed on this platform. Using this model we explore the welfare consequences of the entry of the platform into the production market. Opposite to the common intuition that increased competition in good production is welfare improving, we find that the ability by the platform to deploy a recommender system enables it to steer demand towards its own goods which leads to lower consumer welfare in equilibrium as a result of the platform's entry. When the primary revenue sources for the independent producers are from the platform, the bias in recommendation leads to depressed incentives to invest both for the independent producers and the platform itself.

The policy implications from our model are clear — the increasing trend of online platforms to produce their own goods should be viewed with caution by regulators. A unique element of these platforms is their deployment of recommender systems, which provide utility for consumers by providing them with information on which goods on the platform they should consume. As our

[13]Note that if the platform's message were able to persuade the consumer to choose its good given knowledge of the realized qualities, then it would do so regardless of the realized qualities. This mechanism renders equilibrium messages uninformative.

[14]While it does not directly map to the procedure on a platform such as Netflix, one can interpret the contractual agreements between the independent film distributors and Netflix as being determined by the expected consumption share on the platform.

model points out, the clear evidence of bias in both search and recommendation can lead to negative equilibrium effects on the quality of the goods that get produced and threatens the ability of independent producers to thrive when they are dependent on the platform as their primary revenue source. It must be noted that while we primarily consider the role of strategic information design to consumers as a mechanism for the platform to appropriate rents and foreclosure competition, online retailers have other channels to distort consumption patterns, such as the ability to make consumers unaware of the existence of certain goods or through the pricing channel. Thus, the extent of concern arising from this trend is underestimated by our analysis.

A natural policy remedy is to require that platforms have unbiased recommendations or, equivalently, force a separation between recommendation and production. Surprisingly, we find that the equilibrium effects of biased recommendation lead to this policy not being unambiguously welfare-improving for consumers. In the case in which the platform is the primary revenue source for the independent producers, we find that this policy does improve welfare for consumers: unbiased information disclosure directly generates positive consumer welfare gains. In contrast, when the alternative revenue sources for the independent producers are large relative to the platform potential revenue, biased recommendations induce higher investment levels and lead to higher consumer welfare when compared to unbiased recommendations. It is important to further note that beyond the sign of the difference in welfare, the magnitude of the effects depends on the relative weight of the different revenue sources.

As a result, if independent producers are primarily dependent on the platform for revenue then policies enforcing the separation between recommendation and production will be welfare-improving. But if independent producers have access to large alternative revenue sources relative to the platform's potential revenue, then these policies targeting the integration of recommendation and production may have adverse effects on consumer welfare.

Finally, there are several aspects of the integration between recommendation and production that warrant further study. The first is a better understanding of the interaction between the information accumulated about consumer preferences due to intermediation and dynamic production decisions. This is particularly amplified in the case of recommender systems since, in order to develop a good recommendation system, the intermediary needs to collect fine-grained information about consumer preferences. For instance, Netflix and Amazon are primarily relying on “data-driven" approaches to production decisions using the data they get to power their recommendation systems. Moreover, consumer choices in these markets may have a path-dependence as illustrated by [157], and so, understanding the dynamic consequences of the integration of production and recommendation seems a fruitful and important direction for future work. As the role of recommender systems in online platforms increases and platforms increasingly integrate production and recommendation, examining their effects on competition, investment, and consumer welfare of all these considerations becomes increasingly more important for better policy design for the digital economy.

Chapter 4: Deconstructing the Filter Bubble: User Decision-Making and Recommender Systems

4.1 Introduction

Recommender systems (RS) have become critical for assisting users in navigating the large choice sets that they face on many online platforms. For instance, users have to choose from thousands of movies on Netflix, millions of products on Amazon, and billions of videos on YouTube. Users in many cases are not aware of most items, let alone have full information about their preferences over them. To make matters worse, the items in these contexts are usually experience goods whose true value for users can only be learned after consumption.

RS have driven a significant fraction of consumer choice on these platforms with 75% of movies watched on Netflix and 35% of page-views on Amazon coming from recommendations.[1] While there are many positive effects from these systems, there is an increasing worry about their unintended side-effects. There have been claims that personalized RS lead users into *filter bubbles* where they effectively get isolated from a diversity of viewpoints or content [158], and that personalized RS may also lead users to become increasingly homogenized at the same time [159, 160].

Understanding how RS influence user behavior is important not only for characterizing the broader consequences of such systems but also for guiding their design. In this paper, we develop a theoretical model of user decision-making in contexts where RS are traditionally deployed. We utilize previous empirical studies that characterize how RS influence user choice as a benchmark and our theoretical model provides an intuitive mechanism that can explain these empirical results.

[1] MacKenzie et al. (2013, Oct.), How retailers can keep up with consumers. `https://www.mckinsey.com/industries/retail/our-insights/how-retailers-can-keep-up-with-consumers`. Retrieved on October 3, 2019.

The key insight of our model is that user *beliefs* drive the consumption choices of users and that recommendations provide them with information that leads them to update their beliefs and alter their choices. A crucial component of our model is that users' beliefs about items are driven not only by recommendations, but also from their previous experiences with similar items. We use these insights to provide guidance for RS design, highlighting that understanding users' beliefs about the quality of the available items is essential to design recommendations and evaluate their impact.

Our Model. We analyze a model of user choice with four central components.

The first component of our model is that users sequentially consume items and face large choice sets. In our setting of interest, users are long-lived, but they only consume a small fraction of this choice set over their lifetime. This is traditionally the case on online platforms that have thousands or millions of options for users.

The second component is that, prior to consuming them, users are *uncertain* about how much they value the different items. This is motivated both by the fact that recommender systems are traditionally deployed in contexts with experience goods, whose true value can only be learned after consumption, and the fact that such uncertainty is why RS exist in the first place. Thus, users face a sequential decision-making problem under uncertainty.

The third, and most crucial, element is that consumption of an item reveals information that changes user beliefs about their valuation of similar items. Unlike in standard sequential decision-making problems, once an item is consumed all uncertainty about its valuation is resolved and provides information that enables users to update their beliefs about similar items. This exploits the fact that the valuations of similar items are correlated which assists users in navigating the vast product space. The idea that users make similarity-based assessments to guide their choice has grounding in empirical evidence on how users navigate large choice sets [161].

Finally, in our model recommendation provides users with information about the true valuations. We model the realized valuations as being a weighted sum of a common-value and an idiosyncratic component. This formulation gives a stylized notion of predictability of user preferences where the idiosyncratic component is inherently unpredictable given other users' preferences and the common-value component is what the recommender can learn from previous users' data. We suppose that the recommender knows the common-value component for each item and combines it with users' beliefs over the product space when designing personalized recommendation.

Our Contributions. We provide a clear mechanism that explains the empirical results in [1] who show that, in the context of movie consumption, user behavior is consistent with filter-bubble effects even without recommendation and that recommendation leads to users being less likely to fall into such filter bubbles. In this context, filter-bubble effects are defined as users consuming items in an increasingly narrow portion of the product space over time. The simple and intuitive driving force of this is that preferences for similar items are correlated, which implies that when an item is consumed and the user learns its value, it provides information about similar items. Crucially, this not only impacts the underlying belief about the expected value of similar items, but also how uncertain the user is about their valuation of them. Consequently, this learning spillover induces users to consume items similar to those they consumed before that had high realized value, leading to an increasing narrowing of consumption towards these regions of the product space. This effect is further amplified when users are *risk-averse*, a concept from decision theory where all else being equal, users have a preference for items with lower uncertainty to those with higher uncertainty. However, by providing information to users, recommendation leads users to be more likely to explore other portions of the product space, limiting the filter bubble effect.

We find that, while recommendation leads a single user to be more likely to explore diverse portions of the product space, it also coordinates consumption choices across users. This leads to an increase in homogeneity across users, resulting in a trade-off between homogenizing across-user consumption and diversifying within-user consumption. We explore the relationship between the overall diversity of consumed items and user welfare and find that more diverse sets of consumed items do not always correspond to higher user welfare.

Lastly, we discuss how our model and findings can be used to inform the design and evaluation of RS as well as the data that is traditionally collected for them. This highlights the importance of user beliefs in determining user consumption choices and how both recommendation and informational spillovers determine how these beliefs change over time. By collecting information on user beliefs, RS designers can understand what items a user would consume *without* recommendation and then predict how providing information to the user would change her beliefs and resulting consumption decisions. Thus, our evaluation measure determines the value of a recommendation based on the marginal welfare gain associated with providing a user with a recommendation over what the user would do without it. We discuss how this provides an additional rationale as to why "accurate" recommendations are not always good recommendations.

4.2 Related Work.

The first set of related works studies the extent and implications of filter bubbles. [158] first informally described the idea of the "filter bubble" which is that online personalization services would lead users down paths of increasingly narrower content so that they would effectively be isolated from a diversity of viewpoints or content. Following this, a number of empirical studies in various disciplines, have since studied the extent to which this phenomenon exists in a wide range of contexts [162, 160, 163, 1]. The most relevant to our study is [1] who study whether this effect exists in the context of movie consumption. They find that even users whose consumption choices are not guided by recommendations exhibit behavior consistent with "filter bubbles" and that RS can actually increase the diversity of the content that users consume. To our knowledge there are no theoretical models that rationalize these empirical findings and we provide a theoretical framework through which to view this problem. Moreover, we provide a clear mechanism that drives such effects and how recommendation interacts with them.

Another set of papers has examined whether RS can lead users to become increasingly homogenized. [164, 165] show that incorporating content popularity into RS can lead to increased user homogenization. [159] shows how user homogenization may arise from training RS on data from users exposed to algorithmic recommendations. [141] show that homogenization can increase due to a popularity recommendation bias that arises from lack of information about items with limited consumption histories. We show similar results as previous work where RS lead to increased user homogenization. However, the mechanisms behind this differ from existing work as homogenization arises due to the fact that recommendation leads users to coordinate their consumption decisions in certain portions of the product space.

A third strand in the literature studies the impact of human decision-making on the design and evaluation of RS. [166] surveys the literature on the relationship between human decision making and RS. The closest set of papers pointed out in this survey are those related to preference construction [167, 168] whereby users develop preferences over time through the context of a decision process. We point out that the true underlying preferences of users may be stable over time, but, due to the nature of items in contexts where RS are deployed, they have incomplete information of their valuations and both consumption and recommendation provide them with information to reduce their uncertainty. Thus, the primary insight of our paper is that user beliefs and how users update their beliefs about similar items after consumption are important and previously unconsidered elements of human decision making that are critical for understanding the design and consequences of RS. Within this literature, [169, 170, 171] focus on "user-centric" approaches to recommendation whereby user evaluation of the usefulness of recommendation is a key evaluation measure. Our evaluation measure is similar, but, unlike previous approaches, emphasizes the importance of user beliefs. Finally, [172] considers a similar model as ours where users engage in "spatial learning" and exploit the correlation of their preferences in the environment, but consider it in the context of search for a single item.

4.3 Our Model and Preliminaries

4.3.1 Preliminaries on Expected Utility Theory

For every item n in the product space $\mathcal{J}$, we assume that each user i assigns a monetary equivalent $x_{i,n} \in \mathbb{R}$ to the experience of consuming it. Each user can value the same item differently. However, we assume that users have the same utility over money, given by a utility function $u : \mathbb{R} \to \mathbb{R}$, strictly increasing and continuous. So, ex-post, the value of item n for user i is given by $u(x_{i,n})$.

Before consuming the item, the user does not know exactly how she will value it. In particular, even users that will end up having the same ex-post valuation of item n may differ in their ex-ante valuation because they hold different beliefs about it. We denote by p_i the beliefs user i has about how she will value each of the items in the product space. Note that this implies that consuming item n is the same as taking a gamble. Each user evaluates the item according to the expected utility associated with the item, i.e. $U_i(n) = \mathbb{E}_{p_i}[u(x_n)]$.

Risk aversion captures how different users react to the risk associated to a particular consumption opportunity. It is formalized as follows: a given gamble x takes real values and follows distribution p. Then, for every gamble x, there is a certain amount of money that makes the user indifferent between taking the gamble or taking the sure amount of money. This sure amount of money is called the *certainty equivalent* of gamble x and is denoted as $\delta(x)$. A user i is more risk-averse than another user j if whenever user j prefers a sure thing to the gamble, then user i does too. Therefore, a more risk-averse user is more willing to avoid the risk of taking the gamble. We assume that the utility function takes a flexible functional form $u(x) = 1 - \exp(-\gamma x)$ for $\gamma \neq 0$ and $u(x) = x$ for $\gamma \to 0$ – known as constant absolute risk-aversion preferences (from hereon CARA). Higher γ implies higher risk-aversion, with $\gamma \to 0$ corresponding to the risk-neutral case and $\gamma > 0$ to the risk-averse one. Our formulations here follow standard economic consumer theory (see [173] for a textbook treatment of these topics).

4.3.2 Model

Users. We consider a set of users I where each user $i \in I$ faces the same finite set of N items $\mathcal{J} = \{0, 1, ..., N-1\}$. For simplicity, we assume that users only derive pleasure from item $n \in \mathcal{J}$ the first time they consume it.

We denote by $x_{i,n}$ user i's realized value from consuming item n. In particular, we consider that the realized value derived from a given item can be decomposed in the following manner: $x_{i,n} = v_{i,n} + \beta v_n$, where $v_{i,n}$ denotes an idiosyncratic component – i.e. user i's idiosyncratic taste for item n – and v_n, a common-value component. One can interpret v_n as a measure of how much item n is valued in society in general and, in a sense, $v_{i,n}$ denotes how i diverges from this overall ranking. The scalar $\beta \in \mathbb{R}_+$ denotes the degree to which valuations are idiosyncratic to each user or common across users. If $\beta = 0$, it is impossible to generate meaningful predictions of any one's individual preferences based on others, while if β is large, every individual has similar preferences.

Stacking values in vector-form, we get the vector of values associated with each item

$$\left(x_{i,n}\right)_{n \in \mathcal{J}} =: X_i = V_i + \beta V,$$

where $V_i = \left(v_{i,n}\right)_{n \in \mathcal{J}}$ and $V = (v_n)_{n \in \mathcal{J}}$.

User Decision-Making. We assume the user makes T choices and therefore can only consume up to T items, where T is a small fraction of N. This captures the idea that users are faced with an immense choice set, but that ultimately they end up experiencing (and learning) about just a small fraction of it. For tractability, we impose that users are myopic and every period consume the item that they have not yet tried (n_i^t) that gives them the highest expected utility given the information from past consumption ($C_i^{t-1} = (n_i^1, ..., n_i^{t-1})$) and their initial beliefs.

User Beliefs. We assume that all the true realized values are drawn at $t = 0$. However, users do not know the realized values before consuming an item, but rather have beliefs over them. Formally, user i starts with some beliefs about X_i, namely that the idiosyncratic and common-value parts of the valuations are independent – $V_i \perp\!\!\!\perp V$ – and that each is multivariate normal:

1. $V_i \sim \mathcal{N}(\overline{V}_i, \Sigma_i)$; and
2. $V \sim \mathcal{N}(\overline{V}, \Sigma)$ with $\overline{V} = 0$.

We impose the normality assumption for two reasons. The first is that this allows for simple and tractable belief updating. The second is that it allows us to incorporate an easily interpretable correlation structure between the items. The precise formulation of Σ and Σ_i that we consider is defined below when we discuss user learning.

Recalling that V_i represents idiosyncratic deviations from V, we assume that, on the population level, prior beliefs $\overline{V}_i = \left(\overline{v}_{i,n}\right)_{n \in \mathcal{J}}$ are drawn independently from a jointly normal distribution, where $\overline{v}_{i,n} \sim \mathcal{N}(0, \overline{\sigma}^2)$ are independent and identically distributed. These $\overline{v}_{i,n}$ denote the prior belief that individual i holds about her valuation over item n. As people are exposed to different backgrounds, their beliefs about how much they value a given item also varies and $\overline{v}_{i,n}$ denotes this idiosyncrasy at the level of prior beliefs.

We assume users are expected utility maximizers. User i's certainty equivalent for item n, the sure value that makes user i indifferent between it and consuming the item n, conditional on the consumption history, is given by $\delta_i(n) \mid C_i^{t-1} = \mu_n - \frac{1}{2}\gamma\Sigma_{nn}$, where μ_n and Σ_{nn} are the expected value and variance for item n that the user has given their initial beliefs and consumption history up until time t. Note that this expression is known to be the certainty equivalent for CARA preferences in a Gaussian environment [173]. As it is immediate from this expression, the user assigns greater value to items for which the expected monetary equivalent, μ_n, is higher, but penalizes those about which there is greater uncertainty Σ_{nn}, the more so the greater the user's degree of risk aversion γ.

User Learning. When a user consumes an item n she learns the realized value for that item. We consider the case where learning about the value of item n reveals more about the value associated to items that are closer to it, which captures the idea that trying an item provides more information about similar items than about dissimilar ones.[2] In order to have a well-defined notion of similarity we need to define a distance function between items, which we define as $d(n,m) := \min\{|m - n|, N - |m - n|\}$ where m and n are indices of items in $\mathcal{J}$. This distance function is not intended to model the intricacies of a realistic product space, but instead to provide a stylized product space to help us understand the effects of informational spillovers on user behavior. The basic intuition, in the context of movie consumption, is that a user's valuation of *John Wick* (item n) provides more information about how much she will like *John Wick: Chapter Two* (item m) than *Titanic* (item q) since $d(n,m) < d(n,q)$.

We consider that the entry of n-th row and the (m)-th column of Σ_i is given by $\sigma_i^2\rho^{d(n,m)}$, and that of Σ is given by $\sigma^2\rho^{d(n,m)}$. The scalar $\rho \in [0,1]$ therefore impacts the covariance structure:

[2][161] empirically studies how individuals solve sequential decision-making problems under uncertainty in large choice sets in the context of mobile food delivery orders. They find that individuals engage in similarity-based generalizations where learning about the realized value of a particular item provides them with information about similar items. We incorporate this finding into our model in a stylized manner.

a higher ρ implies that learning the utility of n is more informative about items nearby and, for $\rho \in (0,1)$, this effect is decreasing in distance. The particular distance function that we rely on leads to a simple covariance structure, where the $(n, n+1)$-th entry in the covariance matrix is $\sigma^2\rho$, the $(n, n+2)$-th entry is $\sigma^2\rho^2$, etc.[3]

The precise updating rule is as follows. Recall that at time t the user's consumption history is given by C_i^t and we denote the utility realizations of these items as c_t. We denote μ_t as the initial mean beliefs the user has over the items in C_i^t and μ_{N-t} as the initial mean beliefs the user has over the remaining $N-t$ items, $\mathcal{J} \setminus C_i^t$. We partition the covariance matrix as follows:

$$\Sigma = \begin{pmatrix} \Sigma_{(N-t,N-t)} & \Sigma_{(N-t,t)} \\ \Sigma_{(t,N-t)} & \Sigma_{(t,t)} \end{pmatrix}.$$

After consuming the items in C_i^t, the resulting beliefs over the remaining items are given by $\mathcal{N}(\bar{\mu}, \bar{\Sigma})$ where $\bar{\mu}$ and $\bar{\Sigma}$ are as follows:

$$\begin{aligned} \bar{\mu} \mid c_t &= \mu_{N-t} + \Sigma_{(N-t,t)}\Sigma_{(t,t)}^{-1}(c_t - \mu_t); \\ \bar{\Sigma} \mid c_t &= \Sigma_{(N-t,N-t)} - \Sigma_{(N-t,t)}\Sigma_{(t,t)}^{-1}\Sigma_{(t,N-t)}. \end{aligned}$$

An Illustrative Example. We illustrate the main intuitions of our model with a simple example. Suppose that there are four items: 0, 1, 2, 3. The items are in different places of the product space, where 0 is close to 1 and 3 but more distant from 2. For the sake of expositional clarity, suppose that the initial mean beliefs are given by $\mu = (\mathbb{E}[x_n])_{n=0}^3 = (0)_{n=0}^3$.

In period 1, every item is ex-ante identical since they have the same mean and variance and so suppose that the user breaks the tie arbitrarily and consumes item 0. The underlying correlation structure implies that upon observing that $x_0 = y$ the user will update beliefs about the remaining three items according to the previously specified updating rule. For concreteness, we suppose that

[3] This exponential decay correlation structure can be related to the tenet of case-based similarity of [174] – see [175] for an axiomatization of exponential similarity.

$\sigma = 1$ and $\rho = 0.5$, but the intuitions hold for any value of σ and $\rho > 0$. First, we consider the case when the realization of $y > 0$ and, specifically, $y = 0.5$ – though the general intuitions hold for any $y > 0$. The resulting beliefs after observing y are then as follows:

$$\bar{\mu} = (\mu \mid x_0 = y) = \begin{pmatrix} \mathbb{E}[x_1 \mid x_0 = y] \\ \mathbb{E}[x_2 \mid x_0 = y] \\ \mathbb{E}[x_3 \mid x_0 = y] \end{pmatrix} = \begin{pmatrix} \rho y \\ \rho^2 y \\ \rho y \end{pmatrix} = \begin{pmatrix} \frac{1}{4} \\ \frac{1}{8} \\ \frac{1}{4} \end{pmatrix}$$

$$\bar{\Sigma} = (\Sigma \mid x_0 = y) = \begin{pmatrix} \frac{3}{4} & \frac{3}{8} & 0 \\ \frac{3}{8} & \frac{15}{16} & \frac{3}{8} \\ 0 & \frac{3}{8} & \frac{3}{4} \end{pmatrix}$$

Thus, upon learning $x_0 = y$, the user updates beliefs about the remaining items. Note that $\mathbb{E}[x_1 \mid x_0 = y] = \mathbb{E}[x_3 \mid x_0 = y] > \mathbb{E}[x_2 \mid x_0 = y]$ since item 0's value is more informative about similar items' values, items 1 and 3, than items further away in the product space such as item 2. Moreover, $\bar{\Sigma}_{11} = \bar{\Sigma}_{33} < \bar{\Sigma}_{22}$ as the uncertainty about items 1 and 3 is further reduced compared to item 2. Thus, since $y > 0$, the user in the next period will consume items nearby to item 0 since, even though initially she believed that all items had the same mean, the spillover from consuming item 0 leads her to believe that items 1 and 3 have higher expected valuations. Since both the mean is higher for these items and the variance is lower, the user will consume items 1 and 3 regardless of her risk aversion level.

Now we consider the case when item 0 ends up having a negative valuation so that $y = -0.5 < 0$. This results in $\mathbb{E}[x_1 \mid x_0 = y] = \mathbb{E}[x_3 \mid x_0 = y] = -\frac{1}{4} < -\frac{1}{8} = \mathbb{E}[x_2 \mid x_0 = y]$ with $\bar{\Sigma}$ remaining the same as when $y = 0.5$. In this case the risk-aversion levels of the user determine the choice in the next period. If the user is risk-neutral ($\gamma = 0$), then she will go across the product space to consume item 2 in the next period since it has a higher expected value. However, if she is sufficiently risk-averse then she may still consume item 1 or 3 since her uncertainty about these items is lower than item 2. In particular, this will happen when

$$\delta(3) = \delta(1) = \rho y - \frac{1}{2}\gamma\bar{\Sigma}_{11} > \rho^2 y - \frac{1}{2}\gamma\bar{\Sigma}_{22} = \delta(2)$$

Given the aforementioned parametrization and $y = -0.5$, the user will consume item 1 or 3 when $\gamma > \frac{4}{3}$ and will consume item 2 when $\gamma < \frac{4}{3}$. Thus if the user is risk averse enough, then she might be willing to trade-off ex-ante lower expected values for lower risk and stick to consuming nearby items just because these items have lower uncertainty.

This example illustrates the main mechanisms that can lead to excessive consumption of similar items. Once the user finds items in the product space with high valuations she will update her beliefs positively about items in this portion of the product space and continue consuming these items regardless of her level of risk aversion. However, this same updating leads to a reduction in uncertainty of these items and so, if she is sufficiently risk-averse, she still may continue consuming items in this portion of the product space, even if she has bad experiences with them, since they are perceived to be less risky.

Recommendation. Our model of recommendation is stylized in order to provide qualitative insights into how recommendation shapes behavior, instead of focusing on the details of how RS are implemented in practice. We model recommendation as giving users information about the valuation of the items.

We will consider three cases. The case of primary interest is *recommendation* where the recommender observes values accrued and knows V but does not know V_i.[4] However, the recommender does know the users' beliefs $\bar{V}_i$. Thus, at any given period, the recommender provides a personalized recommendation that combines the knowledge of the common value component V with the user beliefs $\bar{V}_i$. Knowing the user's beliefs about her valuation of each item become crucial in this case: just providing the user with information about V may change the user's original ranking, but, without considering the user's beliefs, she will not necessarily follow the recommendation.[5]

[4] We do not consider the acquisition of information for the recommender to know V and suppose that she has sufficient data to learn V with arbitrary precision at $t = 0$.

[5] The notion of recommendation that we consider is idealized where the recommendation does the Bayesian up-

We further consider two cases that serve mainly as benchmarks. The first is *no recommendation*, where users get no additional information and make consumption choices based on their beliefs and consumption history. This gives us a benchmark as to how users would behave *without* recommendation so that we can analyze what changes with the introduction of recommendation. The second is the *oracle recommendation* where the recommender knows the true realized utility of each item for each user and can therefore recommend the best remaining item in every period. This gives us a full information benchmark, which is the optimal consumption path for a user if all uncertainty about their preferences was resolved. Comparison to the oracle regime benchmark provides an analog to the standard regret measures utilized in the multi-armed bandit literature, which look at the difference in the expected value of the ex-post optimal action and the expected value of actions that were taken.

Simulation Details. We analyze our model using numerical simulation since the sequential decision-making component paired with the rich covariance structure between the items makes it hard to characterize optimal behavior analytically.[6]

We explore how consumption patterns differ as we consider different recommendation regimes and report representative results from our simulations. We run this simulation over 100 populations of users with 100 users per population for each combination of parameters. A given set of parameters and a user are a single data point in our dataset.

We simulate over risk-aversion parameters $\gamma \in \{0, 0.3, 0.6, 1, 5\}$, standard-deviation $\sigma \in \{0.25, 0.5, 1.0, 2.0, 4.0\}$, correlation parameters $\rho \in \{0, 0.1, 0.3, 0.5, 0.7, 0.9\}$ and degree of idiosyncrasy of individual valuations $\beta \in \{0, 0.4, 0.8, 1, 2, 5\}$. The range of considered parameter values cover the relevant portions of the parameter space in order to provide full insight into the behavior of the model. When we consider results varying a single parameter, we group the results over the other parameters and provide reports varying only the parameter of interest. We report results for a relatively small consumption history $T = 20$ with a product space size $N = 200$.[7]

dating for users, but the results are equivalent to if the users did the updating themselves.

[6]The Gaussian assumption allows for closed form belief updating which enables us to simulate our model, but does not provide much help in analytical characterizations.

[7]Due to space constraints, we only report the values for $N = 200$. However, we further conducted the same

4.4 Results

4.4.1 Local Consumption and Filter Bubbles

We characterize "filter bubble" effects as the degree to which users engage in *local consumption*. We define local consumption in terms of the average consumption distance between the items consumed by the users at time $t-1$ and t. Thus, in the context of our model, filter-bubble effects arise when the average consumption distance decreases over time and, across regimes, when the levels are lower for a given recommendation regime compared to another.

Our first finding can be summarized as follows:

Finding 1. *The impact of recommendation on local consumption:*

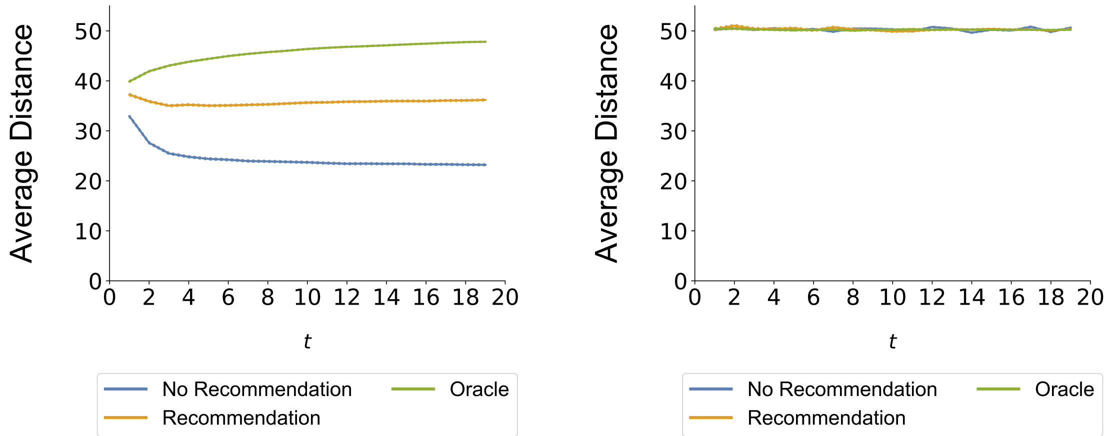
1. *When $\rho = 0$, there is no difference in consumption distance between the three recommendation regimes.*

2. *When $\rho > 0$, no recommendation induces more local consumption than both recommendation and oracle regimes. This effect is amplified as ρ increases as well as when users are more risk averse (γ increases).*

First, the right panel of Figure 4.1 shows that, when $\rho = 0$, there is no difference in consumption distance between the three regimes. This is due to the fact that when $\rho = 0$, there is no reason that items that are close in the product space should have similar values and so the optimal consumption path does not depend on the similarity of the items. However this also means that users do not learn anything about neighboring items and so there is limited path-dependence in consumption. Not only is there no difference in the levels between the three regimes, but they all have the same, flat, average consumption distance path. This underscores the fact that if there were no correlation between the realized utilities then there would be no reason for users to consume similar items and thus no narrowing effect, regardless of the information on the true utility of the items that users had.

exercises for $N = 100$ and $N = 500$ and the results are qualitatively similar to those reported here.

Figure 4.1: Local Consumption and Correlation (ρ)



Notes: The figure shows the consecutive consumption path difference between the no recommendation, recommendation, and oracle regime. The figure on the left displays the average consecutive consumption distance aggregating over simulations where $\rho \in (0, 1)$ and the figure on the right displays the average consecutive consumption distance aggregating over simulations where $\rho = 0$. The shaded area represents the 95% confidence interval.

The left panel of Figure 4.1 shows that, when $\rho \in (0, 1)$, both recommendation and no recommendation lead to increasingly local consumption compared to the oracle benchmark case. Moreover, the average consumption path between periods is *decreasing* for the no recommendation case whereas it is *increasing* for the oracle case. The recommendation regime decreases the degree of local consumption, but not as much as the oracle benchmark. Due to the correlation of values, the oracle consumption path exploits this and leads to the consumption of more similar items than in the case when $\rho = 0$. However, since these spillovers also impact user learning in the no recommendation case, users *over-exploit* these and increasingly consume items similar to high value items that they have consumed before. This is also illustrated in the top row of Figure 4.2, which shows how the consumption paths in the oracle and no-recommendation regimes vary as ρ increases and is in line with this intuition.

As shown in the bottom row of Figure 4.2, this effect is further amplified as the level of risk aversion increases: the degree of local consumption drastically increases as γ increases. This is due to the fact that the spillovers not only impact the mean expected belief about quality but also the degree of uncertainty. Local consumption therefore leads to users having less uncertainty about certain areas of the product space and risk aversion may lead them to increasingly consume nearby items.

In sum, filter-bubble effects only arise when there is an inherent correlation between the realized utilities of the items in the product space. When there is a correlation between the realized utilities then filter bubbles can naturally arise due to the nature of how individuals acquire additional information about the remaining items. We have shown that, unless users are provided with additional information to guide their consumption choices, then these information spillovers and user risk-aversion can lead users into filter bubbles. When users consume high valued items, they exploit the underlying correlation across different items' values, stronger for similar items, which leads them to increasingly consume items in narrower and narrower portions of the product space. Risk aversion may lead users into performing local consumption even when they have a low valuation of nearby items just because they know what to expect from the item. Recommendation leads to these effects being mitigated by providing users with additional information on items outside the already explored portions of the product space. Logically, if all uncertainty were resolved as in the oracle regime, then such behavior is not present.

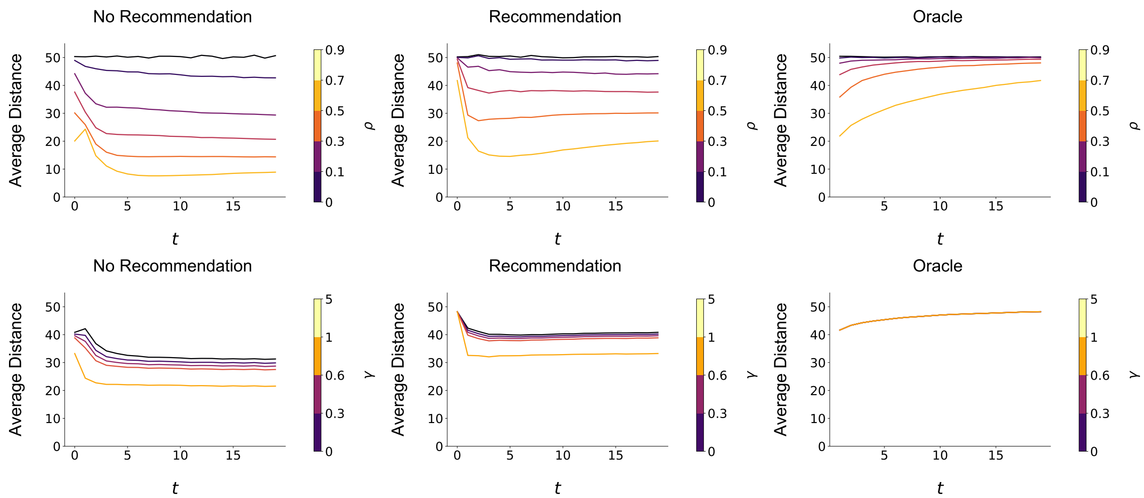
4.4.2 User Welfare and Item Diversity

In this section we primarily focus on the impact of recommendation on user welfare and the overall diversity of the items that they consume. While in the previous section we looked at the distance between consecutive items, in this section we focus on a diversity measure that considers the entire consumed set of items. The diversity measure we utilize is common in RS literature (e.g. [176]) – the average normalized pairwise distance between the consumed items:

$$D_i := \frac{1}{N}\frac{1}{T(T-1)} \sum_{n,m \in C_i^T : n \neq m} d(n,m).$$

Finding 2 summarizes the main results on item diversity:

Figure 4.2: Relationship between Local Consumption and Correlation (ρ), Risk Aversion (γ)



Notes: Each figure plots the average consecutive consumption distance across time as the inherent correlation between the valuation of the items, ρ, varies (top row) and the level of risk-aversion, γ, varies (bottom row). In both rows, the left displays the no-recommendation regime, the center displays the recommendation regime, and the right displays the oracle regime.

Finding 2. *The impact of recommendation on item diversity:*

1. *When $\rho = 0$, item diversity is the same across all three recommendation regimes;*
2. *When $\rho \in (0, 1)$, item diversity decreases across all recommendation regimes but decreases the most in the no-recommendation regime. This effect is amplified as ρ increases as well as when users become more risk-averse.*

As before, when there is no correlation between valuations, item diversity is the same across different recommendation regimes. The over-exploitation of information spillovers when $\rho \in (0, 1)$ leads to item diversity being lowest in the no-recommendation regime. As a result, this effect gets amplified as ρ increases, which leads to the gap in diversity between the regimes to increase as ρ increases. The top row of Figure 4.2 shows how diversity varies as ρ increases across the three regimes that we consider. There is a similar increasing diversity gap as γ, or the level of risk-aversion, increases as can be seen in the bottom row of Figure 4.2. The mechanisms behind these effects directly parallels those discussed in the previous section since low average sequential consumption distance directly leads to low diversity.

We now study how recommendation impacts user welfare. In our model users make consumption decisions that maximize their current period ex-ante utility that depends on their beliefs in that period. Thus, from an ex-ante perspective, they make optimal decisions, but our primary interest is in understanding how the ex-post, or realized, utility varies across regimes and parameter values. We define user's *ex-post* welfare as the average of the realized values, controlling for the effect of T:

$$W_i := \frac{1}{T} \sum_{n \in C_i^T} x_{i,n}$$

Finding 3 states our main findings of the impact of recommendation on ex-post welfare, which can be seen in the rightmost plot of Figure 4.3:

Finding 3. *The impact of recommendation on consumer welfare is as follows:*

1. *Under oracle recommendation, welfare is invariant with respect to ρ.*

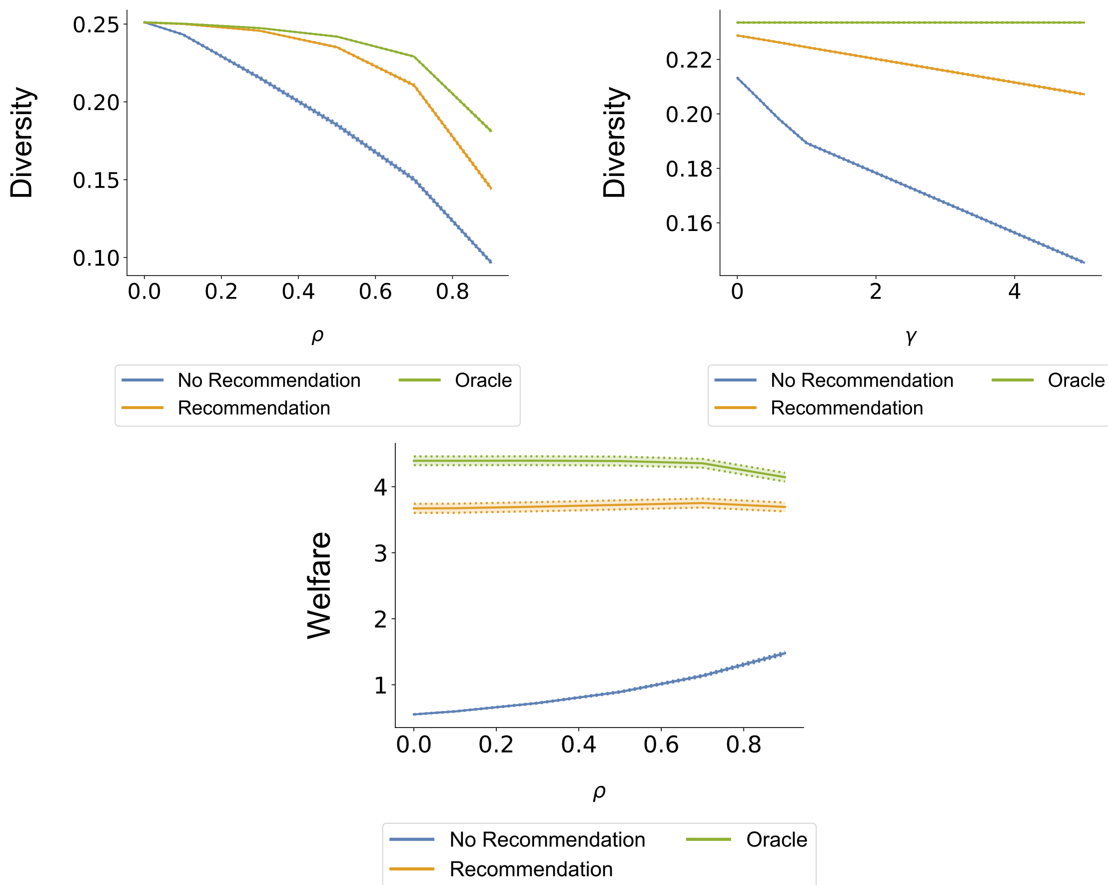
2. *Under no recommendation, welfare is increasing in ρ.*

3. *Recommendation introduces welfare gains relative to no recommendation, but these gains are decreasing as ρ increases.*

The most interesting observation is that the value of recommendation decreases as ρ decreases. While welfare in the recommendation regime is flat as we increase ρ, it is increasing in the no-recommendation regime and thus the welfare gap between the two shrinks as ρ increases. The intuition is clear as recommendation provides users with information that allows them to better guide their decisions and increase welfare. However, as ρ increases users get increasingly more information from consuming items since the realized utility is now more informative about the utility of nearby items. Thus, since consumption decisions themselves yield valuable information, the information provided by recommendation is less valuable to the user.

One striking observation is that the decrease in diversity does not appear to be associated with a decline in welfare. Indeed, it appears that the opposite is the case - that low diversity is associated

Figure 4.3: Relationship between User Welfare, Diversity and Correlation (ρ), Risk Aversion (γ)



Notes: The figures on the left and center display the relationship between ρ and overall consumed item diversity (left) as well as γ and overall consumed item diversity (center). The figure on the right displays the relationship between ρ and overall welfare. The shaded area represents the 95% confidence interval.

with higher welfare and vice versa. We next explore the relationship between welfare and diversity, which is summarized in Finding 4.

Finding 4. *In the no-recommendation regime, diversity and welfare are:*

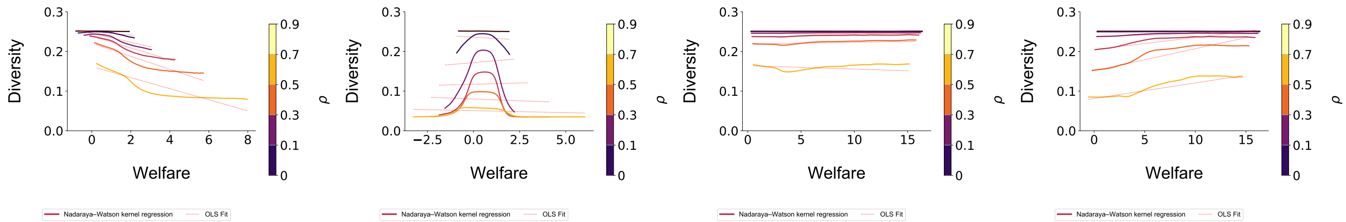
1. *Negatively correlated when users have no risk-aversion;*

2. *Uncorrelated when users have high levels of risk-aversion.*

In the recommendation regime, diversity and welfare are:

1. *Uncorrelated when users have no risk-aversion;*

Figure 4.4: Diversity vs. Welfare



Notes: The figure plots the relationship between diversity and welfare under no recommendation, with $\gamma = 0$ (first) and $\gamma = 5$ (second), and under recommendation, with $\gamma = 0$ (third) and $\gamma = 5$ (fourth).

2. *Positively correlated when users have high levels of risk-aversion.*

In the oracle regime, diversity and welfare are always uncorrelated.

Figure 4.4 shows how diversity and welfare correlate for the no recommendation case as we vary the degree of risk aversion. When there is no risk-aversion there is a negative correlation between welfare and diversity. This is since, in this case, a user will select the item that she currently believes has the highest expected value. High item diversity in this case can arise from a user who consumes an item she disliked and updates her beliefs about nearby items negatively. As a result, in the following period she will pick an item far away in the product space from the item that was previously consumed. If instead the user valued highly the item that she had consumed, then she is more likely to pick a nearby item. The information spillovers therefore lead to high item diversity being negatively correlated with welfare.

This only happens since $\gamma = 0$ leads to users only caring about the expected value of the item. However, as we saw in Findings 1 and 2, increasing γ can lead to lower diversity and increasingly local consumption due to the fact that the degree of uncertainty now impacts users' choices. This weakens the negative relationship between diversity and welfare since both negative and positive experiences with an item reduce uncertainty about surrounding items. This leads to the inverted-U shape found in Figure 4.4 when γ is relatively large (e.g. $\gamma = 5$) though diversity and welfare are virtually uncorrelated in the data. In the recommendation and oracle regimes, under risk neutrality ($\gamma = 0$), welfare and diversity are uncorrelated, while under risk aversion ($\gamma = 5$), it is possible to observe an actual positive relation between diversity and welfare as recommendations are able to reduce uncertainty and facilitate exploration of the product space.

4.4.3 User Homogenization

In this section, we focus on comparisons across users and investigate how the consumed set of items across users varies across different recommendation regimes and parameter values. In particular we look at the degree of *homogenization* between users. Similar to other papers that study the degree of homogenization in RS (e.g. [159]) we measure homogeneity via the Jaccard index between the consumption sets of users:

$$H := \frac{1}{|I|(|I|-1)} \sum_{i,j \in I : i \neq j} d_J(C_i^T, C_j^T)$$

where d_J denotes the Jaccard index and $H \in [0, 1]$.

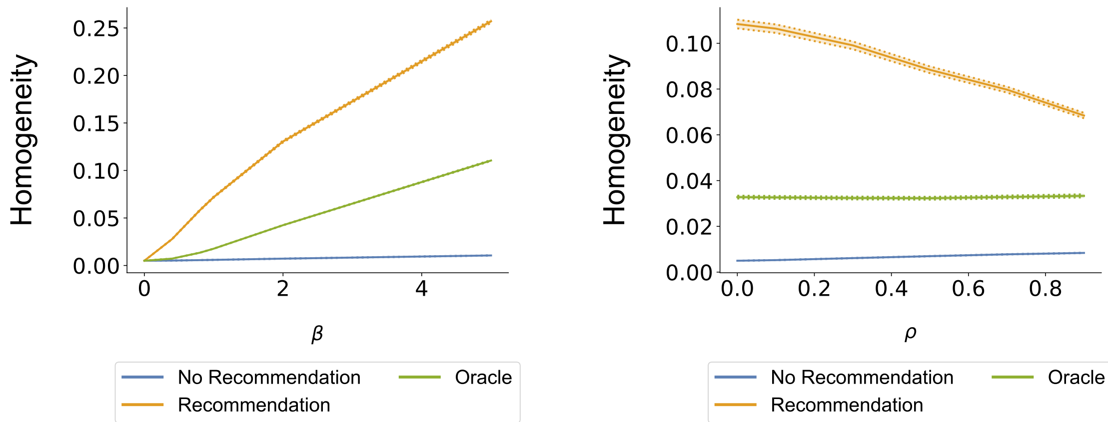
Finding 5 summarizes our findings on the impact of recommendation on user homogeneity:

Finding 5. *The impact of recommendation on homogeneity is as follows:*

1. *Highest under recommendation and lowest under no recommendation;*
2. *Increasing in β, or the weight of the common-value component;*
3. *Decreasing in ρ for partial recommendation, but weakly increasing in ρ for no recommendation.*

First, we study how the degree of homogenization varies as we increase β, the weight of the common value component. As β increases we expect that users should become increasingly homogeneous as the realized utilities of the items are now increasingly similar. Figure 4.5 confirms that as the weight of the common-value component β increases, users consume increasingly similar sets of items. The homogenization effect is strongest under the recommendation regime since the revelation of the common-value component induces users to consume items in similar areas of the product space. As β increases, some amount of homogenization is optimal as can be seen from

Figure 4.5: Relationship between Homogeneity and Common-Value Strength (β), Correlation (ρ)



Notes: This figure displays the value of the homogeneity measure as we vary the weight of the common value component, β (left) and correlation between valuations, ρ (right). Each line represents this plot for a single recommendation regime. The shaded area represents the 95% confidence interval.

the oracle case. However, since users in the no-recommendation regime do not know the common-value component they engage in local consumption in different areas of the product space which leads to less than optimal homogeneity.

We next study how the degree of homogeneity varies with ρ. Figure 4.5 shows how homogeneity decreases as ρ increases in the recommendation regime. As was highlighted in Findings 1 and 2, the degree of local consumption increases with ρ. Even though the revelation of the common-value component induces them to search in similar parts of the product space, their idiosyncratic components induce them to consume items in a more localized area of the product space as ρ increases which leads to a decline in homogeneity.

4.5 Recommender System Evaluation

In this section we discuss how the insights from our model of user decision-making can inform the evaluation and design of recommender systems. The classic approach to evaluation is to predict user ratings for items and to compare how accurate this prediction is to recorded ratings data, either explicitly given by users or inferred from behavioral data. The RS should then recommend the items with the highest predicted ratings [177].

There has been a recent movement away from such evaluation measures due to the observation that accurate recommendations are not necessarily useful recommendations [178]. Our model illustrates a mechanism behind this observation. Consider the domain of movie recommendation and suppose a user has just watched the movie *John Wick* and rated it highly. A RS attempting to predict user ratings may then predict that this user is very likely to enjoy the sequel, *John Wick: Chapter Two*, as well. However, the user herself may also have made this inference since the two movies are very similar to each other. Thus, recommending this movie would not be not useful since the recommendation gives the user little information that she did not already know. The key insight is it is not useful since *it ignores the inference the user themselves made and their updated beliefs*. The user may watch *John Wick: Chapter Two*, then, even without recommendation, and the value of the recommendation was small.

This intuition implies that RS should collect additional data beyond that which is traditionally recorded. The first and most crucial type of data to collect is individual user *beliefs* about items that they have not yet consumed. As illustrated by our model, these beliefs are what drive the future consumption decisions of users and understanding these beliefs is crucial for determining the value of recommending certain items.[8] The second type of data that is relevant for RS designers to collect is how user beliefs change over time and, in particular, not just how individuals value the item they just consumed, but also how it impacts their beliefs about the quality of similar items.[9] The third type of data is the risk-aversion levels of users as our model illustrates that the risk preferences of users are important for understanding what information RS can provide that materially leads users to alter their consumption patterns.

A natural follow-up question is how this additional data should be utilized in the design of good recommendations. Our model posits that recommendation provides value to users by providing them with information about the true valuation of an item if they were to consume it. Thus, the

[8]Additionally, user beliefs contain information that may not have been observed by the recommender that only observes user choices on the platform.

[9]Characterizing the similarity between items has been an important goal of designing content-based recommendations, though as noted by [179], how users perceive similarity between items is not always in line with how similarity is computed in content-based RS. Understanding how this impacts which items users update their beliefs about is an important direction for future work.

prediction problem for the recommender becomes predicting what item the user would choose with no recommendation and, correspondingly, what would be the most useful information to provide to the user that would lead her to consume a *better* item than she would without recommendation. This links back to the intuition our model provided for the *John Wick* example whereby collecting user beliefs and measuring how the user updated beliefs about similar items would lead the recommender to understand that the user would consume *John Wick: Chapter Two*. Our approach would therefore imply that, with this as a starting point, the recommender's problem would be to predict what is the most useful information to give the user leading them to change the item that they eventually consume.

There have been a number of alternative recommendation evaluation metrics proposed in the literature with the aim of providing more useful recommendations than those provided by accuracy metrics, such as serendipity [180], calibration [181], coverage [182], novelty [183], and many others. Our approach most closely follows the set of proposed serendipity measures which are surveyed in [180]. As discussed by [184], serendipitous recommendations are said to "have the quality of being both unexpected and useful" which is in line with the primary intuition behind our approach. The primary difference between our proposed approach and those existing in the literature is that ours crucially hinges on understanding user beliefs and the risk-preferences of users. For instance, [183, 185] propose unexpectedness metrics that look at the dissimilarity of the proposed recommended items compared to what the recommender already knows the user likes. This metric depends only on the proposed item-set and not necessarily on the user's beliefs or how such a recommendation will change the item that the user consumes. [186] provide a comprehensive overview of possible definitions of serendipity and ours is closest to their "motivational novelty" definition, which is that the user was persuaded to consume an item as a result of recommendation.

Indeed, our approach allows us to give precise definitions for what it means for a recommendation to be *unexpected* and *useful* in the spirit of serendipitous recommendations. Our evaluation measure leads to useful recommendations since it leads users towards better items than they would consume without recommendation. It further results in "unexpected" recommendations since it explicitly incorporates user beliefs and thus allows the RS to understand how “unexpected” a recommendation would be from the perspective of a user. Finally, such a measure may lead to a perceived broadening of user preferences as has been discussed in [187, 188]. However, under our interpretation, it may be that their underlying preferences are unchanged and, instead, that recommendation and consumption themselves provides information that encourages users to explore different portions of the product space.

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Appendix A: Supplementary Material for Drivers of Digital Attention: Evidence from a Social Media Experiment

A.1 Experiment Materials

A.1.1 Recruitment Materials

The following are the recruitment materials that were used for the study. Participants were either recruited from university lab pools or Facebook advertisements. For the participants who came from university lab pools they received the invitation in Section A.1.1 via email. The Facebook advertisement that was used for recruitment is shown in Figure A1.

Recruitment Letter

Hello [NAME OF PARTICIPANTS]!

We are inviting you to participate in an Internet experiment via Zoom. You will be able to earn money while contributing to science and hopefully having fun!

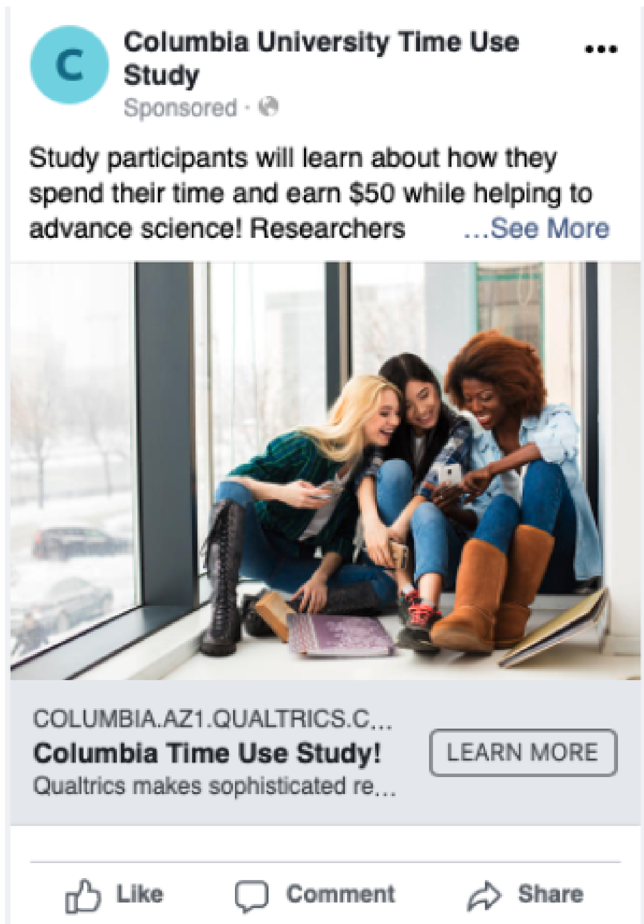
We are running an experiment to better understand how people spend their time online. We will ask you to install an application that will allow us to track how much time you spend on your phone and computer and periodically restrict access to certain applications on your phone [we only observe the time spent, not what happens on the app itself]. We will meet with you on zoom for five minutes to make sure the app is set up on your phone properly and then you will take a fifteen minute intro survey. You will not have to actively do anything during the rest of the experiment, beyond answering a short 4-minute survey once a week for five weeks.

Participants will earn $50 for successfully completing the experiment (i.e. keeping the application installed and completing all the survey questions each week). Note that only individuals with Android phones can participate in this experiment.

To sign up for the study, please click the link below to express your interest and we will follow up via email to schedule an initial meeting to set up the software and start the study: [link]

Thanks for your interest in participating in this study.

Figure A1: Facebook Advertisement



Recruitment Survey

Once the participants clicked on the link in the email sent from the lab pool or the Facebook advertisement, they were sent to an interest survey to complete. The recruitment survey had two pages. The first described the study in more detail, as shown in Figure A2, and still emphasized that the main purpose of the study was to understand how participants spent their time online. The second page elicited information on social media habits and preferences with participants who stated that they used Facebook/Instagram/WhatsApp more than WeChat/Weibo/QQ/KakaoTalk were invited to the study.

Figure A2: Recruitment Survey

We are recruiting Android users for a five-week experiment!

We are inviting you to participate in an Internet experiment via Zoom. You will be able to earn money while contributing to science and hopefully having fun!

We are running an experiment to better understand how people spend their time online. We will ask you to install an application that will allow us to **track how much time you spend on your phone and computer** [we only observe the time spent, not what happens on the app itself]. Additionally, **there may be a period of 1-2 weeks in the middle of the study where we restrict your usage of a single social media application on your phone**. This means that you will not be able to use that social media platform on your phone for that period of time, but **will be able to do so on other devices**. We will meet with you on zoom for five minutes to make sure the app is set up on your phone properly and then you will take a fifteen minute intro survey. You will not have to actively do anything during the rest of the experiment, beyond **answering a short 2-minute survey once a week for five weeks**.

Participants will earn $50 for successfully completing the experiment (i.e. keeping the application installed and completing all the survey questions each week). If you only complete a portion of the study you will receive $5 payment as compensation for your time and effort. **Note that only individuals with Android phones can participate in this experiment.**

If you are interested in participating, please fill out your contact information (phone number and email) and we will send a separate email about scheduling a time to get you enrolled into the experiment. This should happen sometime in early to mid March.

If you have more questions, you can email the researchers directly at msm2254@columbia.edu

What kind of phone do you have?

Android | iPhone | Other

1. Question # 1: Which set of social media platforms and apps do you use more often?
    - Facebook/Instagram/WhatsApp
    - WeChat/Weibo/QQ/KakaoTalk

2. Question # 2: Which of these apps do you use frequently (at least once a week)? Select all that are applicable.

    - Facebook, Instagram, Messenger, YouTube, WhatsApp, TikTok, Reddit, Snapchat, Twitter, WeChat, QQ, Weibo, KakaoTalk, Line, Telegram

3. Question # 3: Which web browser do you use most often?

    - Google Chrome, Safari, Internet Explorer, Firefox, Other

4. Question # 4: Contact Information - name, phone number, email

A.1.2 Baseline Survey

The baseline survey that participants fill out when they set up the software starts with the standard experimental consent form and study details. It then proceeds to ask a number of questions about their usage of social media applications.

Figure A3: Consent Form and Study Details

Welcome to the study!

The study you are about to participate in is an economics and marketing study. The purpose of the study is to understand how people utilize applications on their phones and spend their time more generally. In order to do so, we will ask that you install software on your phone and computer. We will restrict **a single** social media or entertainment application on your phone for a time period ranging from one to two weeks during the course of the study.

Procedure
(Must read in order to know what is going on)

Overview
(1) You will set up the software on your phone and complete the initial long survey. (This is today)
(2) We will restrict a single application from your phone, for either one week or two weeks, starting on April 3rd. We will text you on April 2nd informing you which application will be restricted.
(3) The applications will remain installed and you will complete weekly surveys until May 2nd. You will receive two short surveys every week, one on Thursday and one on Saturday. Both will take 1-3 minutes to complete.
(4) Depending on your answer to a question later in this survey, you may have the opportunity to earn \$0-\$500 on top of the \$50. We will randomly select two participants to have an additional restriction and receive additional payment.

Details
The study will start with a Zoom meeting to set up the ScreenTime application, the desktop chrome extension, and a survey (which you should currently be in). The survey will ask you about how you use several popular social media and entertainment applications as well as some personality questions. The survey should take approximately ten to twenty minutes.

The majority of the study will make use of the installed ScreenTime application on your phone. This application will allow us to collect data on how much time you spend on applications on your phone. This application will not enable us to see what you do on the phone (i.e. the actual content within the applications), but only record how much time you spend on individual applications. This portion of the study will run until May 2nd (approximately 5 weeks).

If we do not text you about an application being blocked, then all the applications on your phone should be available. We will **only block entertainment and social media applications, not any essential components of your phone (i.e. maps, SMS, calling)**. At the end of the five weeks, you will be texted a password that will enable you to delete the application from your phone and receive your payment for completing the study.

It is important to note that all personal identifiers will be removed and researchers on the project will be the only ones who will have access to the data. If you complete **ALL** parts of the study, you will receive **\$50** in compensation. Based on your survey responses, you can earn additional compensation if you are selected at the end of the study to have an additional restriction. This will become clear when you complete the current survey. If you do not complete all parts, you will be compensated \$5 for completion of this initial survey. If you wish to opt-out of the study at any point, you can contact Guy Aridor at g.aridor@columbia.edu or Maayan Malter at mmalter22@gsb.columbia.edu but, if you do so, you will be forgoing the additional \$45 payment.

The questions were then as follows:

1. Question #1: Subjective Time Use. For each application write in your best guess for the number of hours you spend on it each week (in 30 minute increments, e.g. 1.50 hours for 1 hour and 30 minutes per week). The first column asks how much time you think you spend on the application on your phone and the second column asks how much time you think you spend on the application on your other devices.

   - Facebook, Twitter, WhatsApp, TikTok, Instagram, Snapchat, Facebook Messenger, Attention Check. Write 99., YouTube, Reddit, Netflix

2. Question #2: Content Production. How frequently do you post content (including stories, re-sharing posts) on each of the following applications? For each of the following applications,

the participants were asked to select one of the following options.

- Applications: Facebook, Instagram, YouTube, Reddit, TikTok, Netflix, Snapchat, Twitter
- Options: Never, Less than once a month, At least once a month, At least once a week, 2 or 3 times per week, Every day

3. Question #3: Subjective Activity on Application. The main activity I do on each application on my phone is as follows. For each of the following applications the participants were asked to select one of the following options.

    - Applications: Facebook, Instagram, YouTube, Reddit, TikTok, Netflix, Snapchat, Twitter, Messenger, WhatsApp
    - Options: Get Information (e.g. news about politics, sports, business, etc.), Online Shopping, Keep up with my friends' lives, Communicate with my friends, Entertainment content (e.g. memes, influencers, videos, etc.), I don't use this application

4. Question #4: Connections. For each application, write in the number of people you are connected to on the application. Please put your best guess for the range, there is no need to check for the exact values. For applications with followers / following, please let us know approximately how many individuals you follow on the application. For applications without direct connections, please let us know approximately how many individuals you interact with each week on the application.

    - Facebook (Friends): 0, 1-49, 50-149, 150-299, 300-499, 500-749, 750-999, 1000-2499, 2500-4999, 5000+
    - Twitter (Following): 0, 1-49, 50-149, 150-299, 300-499, 500-749, 750-999, 1000-2499, 2500-4999, 5000+
    - WhatsApp (Contacts): 0, 1-4, 5-9, 10-19, 20-29, 30-39, 40-49, 50-99, 100-249, 250+

- TikTok (Following): 0, 1-9, 10-24, 25-49, 50-99, 100-199, 200-299, 300-399, 400-499, 500+
- Instagram (Accounts Followed): 0, 1-49, 50-149, 150-299, 300-499, 500-749, 750-999, 1000-2499, 2500-4999, 5000+
- Snapchat (Friends): 0, 1-9, 10-24, 25-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500+
- YouTube (Channels Subscribed): 0, 1-9, 10-24, 25-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500+
- Reddit (Sub-reddits Subscribed): 0, 1-9, 10-24, 25-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500+

5. Question #5: WTA. See Figure A4 for the interface and description presented to participants.

6. Question #6: Hypothetical Consumer Switching. For this question suppose the application in each row was no longer available on your phone. How do you think you would use the time you can no longer spend on that application? For each row application, let us know the category where you would spend most of your freed up time instead. For instance, if your Facebook is restricted and you think you would spend most of the gained time on other social media such as Twitter or TikTok then you would select “Social Media." If you think you would spend your most of your time painting instead, then you would select “Other Hobbies." If you don’t use the blocked app on a regular basis, then select “No Change." The interface presented to participants can be seen in Figure A5.

7. Remaining Questions: A battery of psychology questions and demographic questions. The only one reported in this paper is a social media addiction question, see Figure A6, adapted from [9].

Figure A4: WTA Elicitation Interface

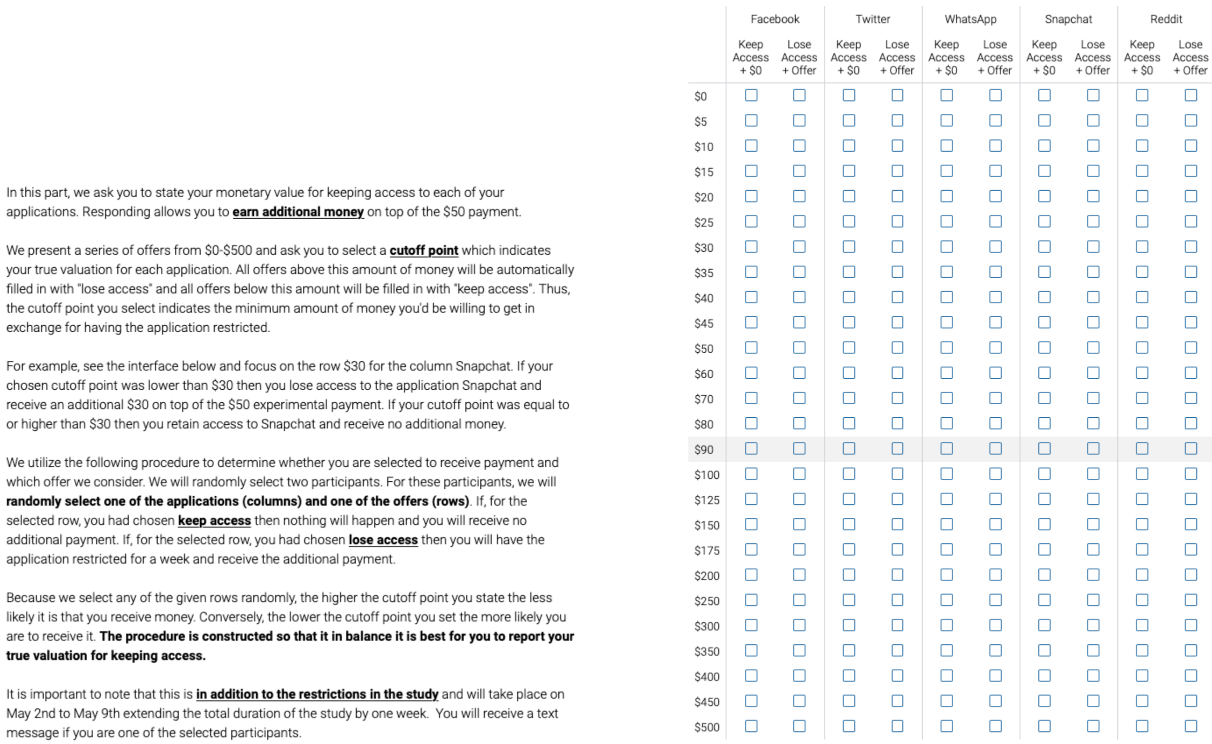


Figure A5: Hypothetical Consumer Switching Interface

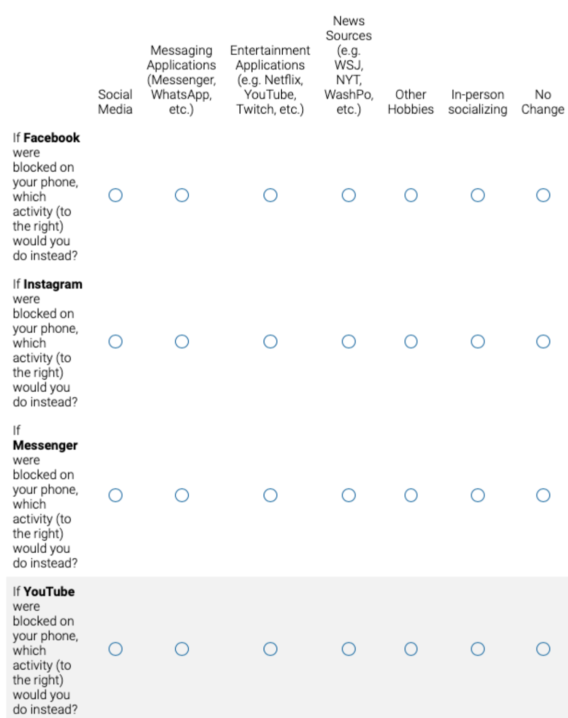
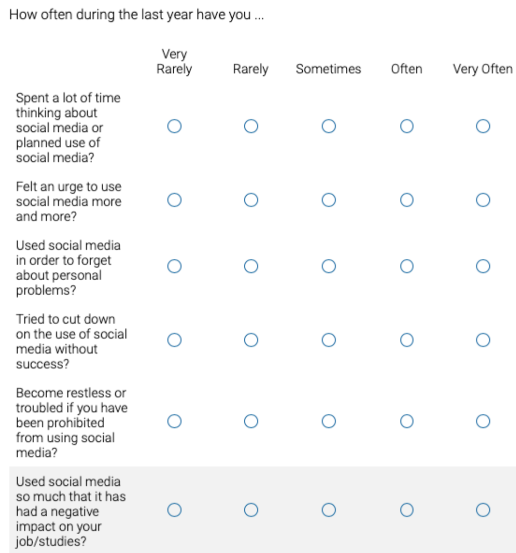


Figure A6: Social Media Addiction Scale


How often during the last year have you ...

	Very Rarely	Rarely	Sometimes	Often	Very Often
Spent a lot of time thinking about social media or planned use of social media?	○	○	○	○	○
Felt an urge to use social media more and more?	○	○	○	○	○
Used social media in order to forget about personal problems?	○	○	○	○	○
Tried to cut down on the use of social media without success?	○	○	○	○	○
Become restless or troubled if you have been prohibited from using social media?	○	○	○	○	○
Used social media so much that it has had a negative impact on your job/studies?	○	○	○	○	○

A.1.3 Additional Surveys

There are two weekly surveys throughout the study. The first is during the week and sent on Thursdays as part of the data collection partnership for this study. It is meant to capture instantaneous psychology measures, which is why it is sent during the week while the application restrictions are ongoing. The second is sent on Saturday mornings and is meant to record subjective perceptions of time usage throughout the week.

The Thursday survey asks the participants how fast they felt the week had passed, questions about their social connectedness and well-being, a question about whether they made any big purchases in the past week, and finally whether there were any major life events in the past week.

The Saturday survey is broken into three separate components. The first component asks participants how much time they felt they spent off their phones on Facebook, Instagram, YouTube, Facebook Messenger, WhatsApp, Netflix, TikTok, Twitter, and Reddit. The second component asks participants how much time they spent on life necessities, including sleeping, studying, attending classes, paid work, cooking/eating, cleaning, socializing in person, and child care. The final component asks participants how much time they spent on leisure activities off the phone, including playing video games, reading books, watching cable TV, streaming on TV / tablet, exercising, shopping (in person), artistic hobbies, and reading print media.

Finally there is an endline survey that is attached to the final weekly time use survey, which asks the following questions:

1. Question #1: Ability to revise WTA. The participants are given the same WTA question as the initial survey, but the results are pre-filled based on their initial survey responses. They are instructed to revise the values if they wish.

2. Question #2: Reason for revision. The participants are asked why they revised the WTA value.

   - Applications: Facebook, Instagram, YouTube, Reddit, TikTok, Netflix, Snapchat, Twitter, Messenger, WhatsApp
   - Options: Have a better idea of how much time I spend on the application, Reduced my usage of the application during the study period, Started using the application during the study period, Increased my usage of the application during the study period, Realized the application is more/less important to me than I thought, I realized I misunderstood this question when I first answered it, No Change

3. Question #3: What did you think the purpose of the study and the restrictions was? Open-Response.

4. Question #4: During the restriction period, select the following statement which you think most accurately describes your behavior. Multiple choice.

   - I downloaded new applications and spent most of the gained time using them.

- I spent more time on applications I already had installed and spent time curating better content on these applications (e.g. following more accounts/channels on YouTub/TikTok/Instagram, figuring out how different features worked).
- I spent more time on applications I already had installed, but did not significantly invest time in improving my experience on them.
- I spent more time on my computer.
- I spent more time off my devices.
- I had no restrictions.
- No change.

5. Question #5: After the restriction period, I started to use the restricted application on my phone. Multiple choice with the following possible responses: More time than before the restrictions, the same time as before the restrictions, Less time than before the restrictions, I had no application restriction.

6. Question #6: Select the following statement which you think most accurately how your behavior after the restrictions compares to before the restrictions. Multiple choice.

   - I spent my time more or less the same.
   - I spent more time on applications I downloaded during the restriction period.
   - I spent more time on applications I already had installed but did not significantly invest time in improving my experience on them during the restriction period.
   - I spent more time on applications I already had installed, but had invested time in making my experience on them better.
   - I spent more time on my computer.
   - I spent more time off my devices.
   - I had no application restrictions

7. Question #7: (Optional) If you want to describe in words how you responded to the restrictions, feel free to elaborate below.

8. Question #8: (Optional) How do you think you will change your behavior with respect to social media applications going forward?

A.1.4 Software

Figure A7: Chrome Extension Interface

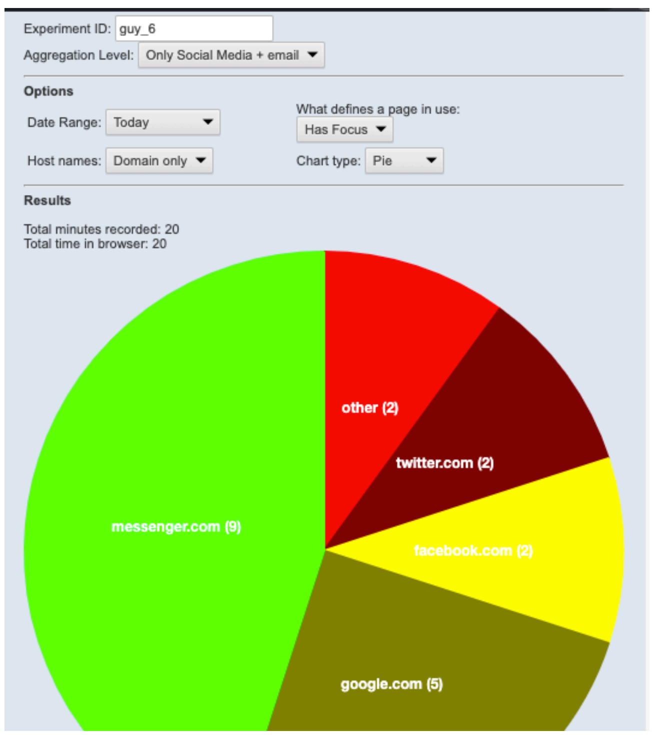
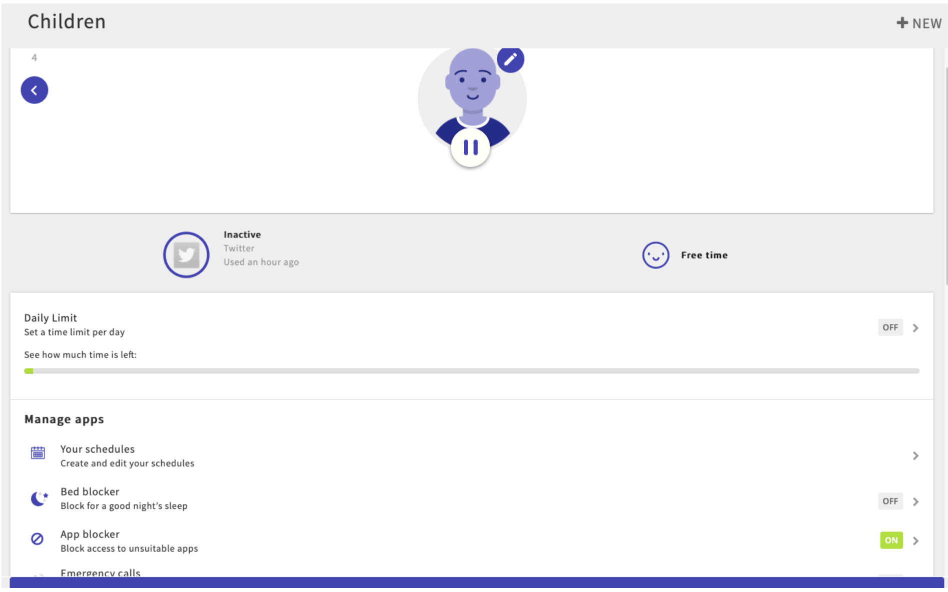
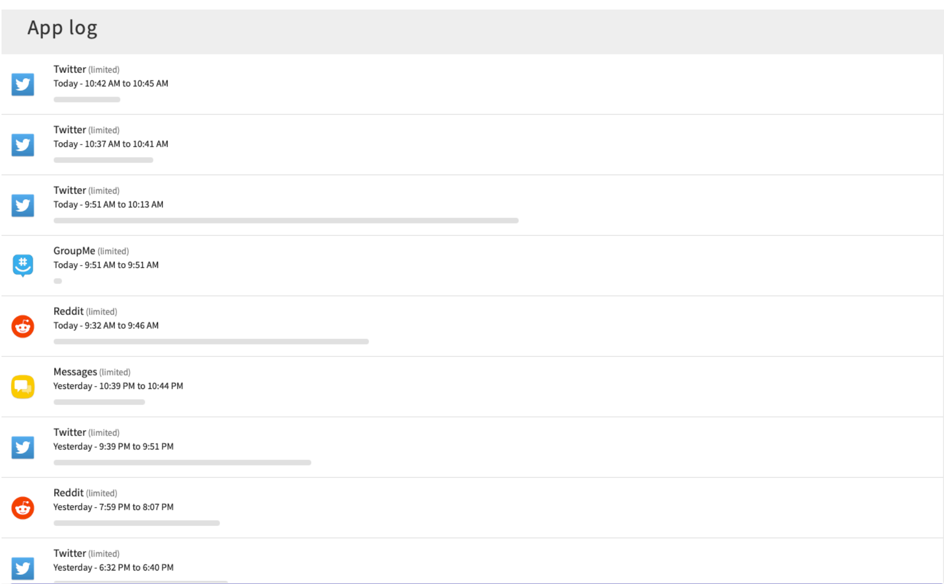


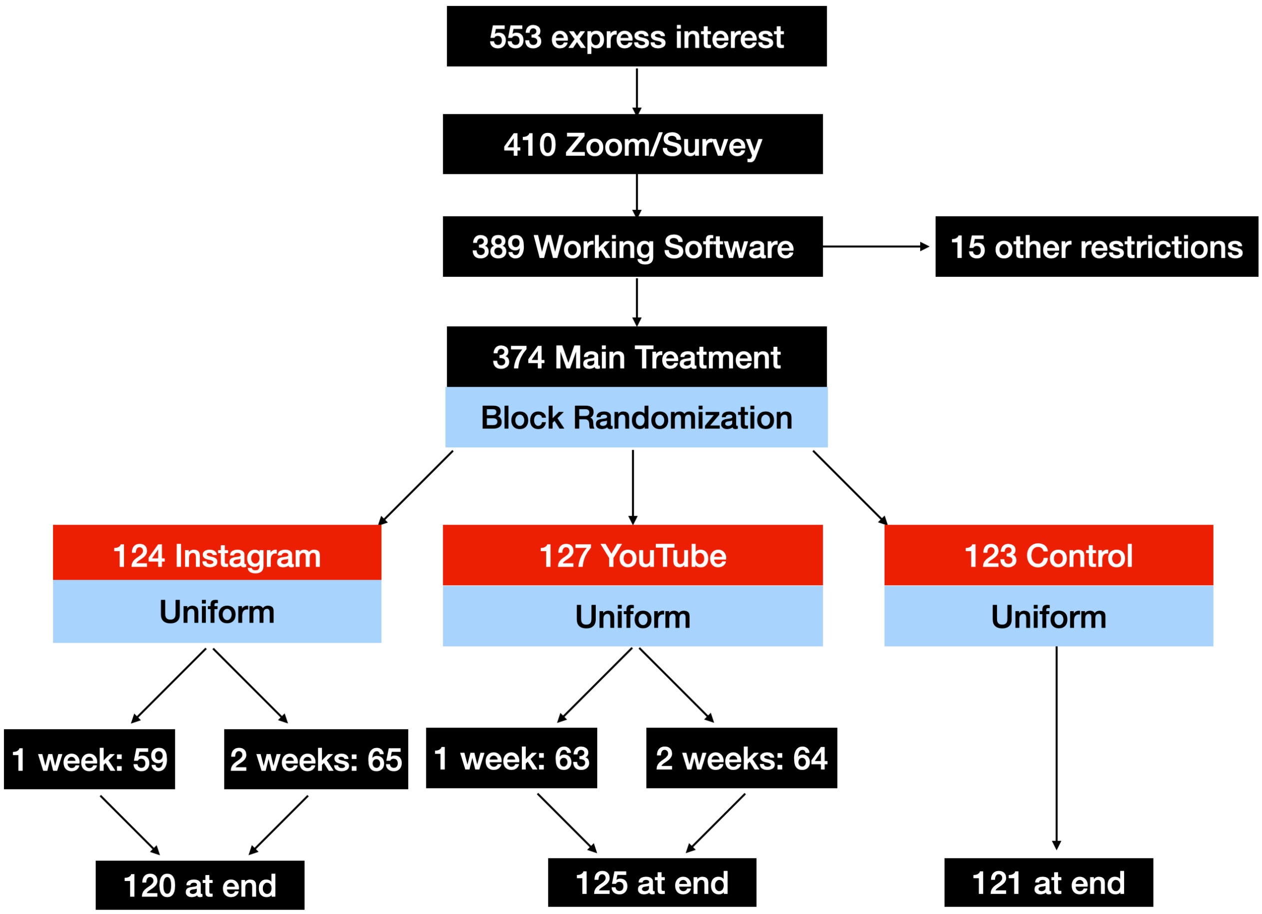
Figure A8: Parental Control Interface





A.1.5 Experiment Timeline

Figure A9: Experiment Timeline



A.2 Additional Descriptive Statistics Figures and Tables

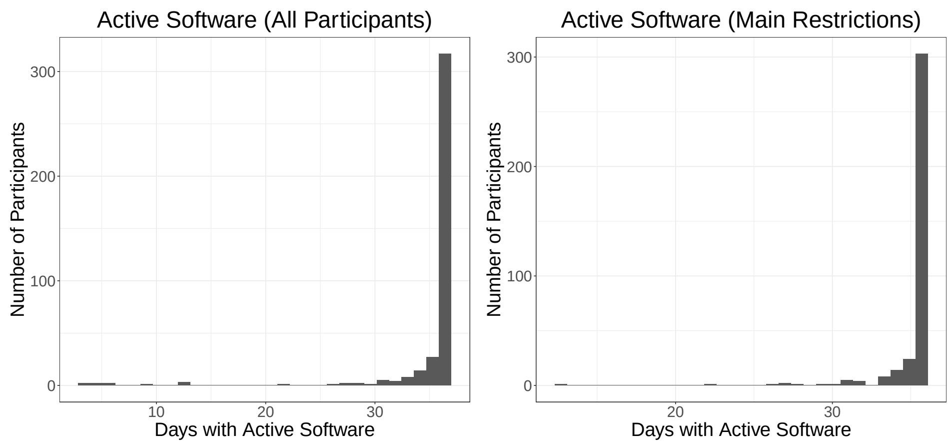
Table A1: Gender Distribution

Female	Male	Non-Binary
180	216	11

Table A2: Age Distribution

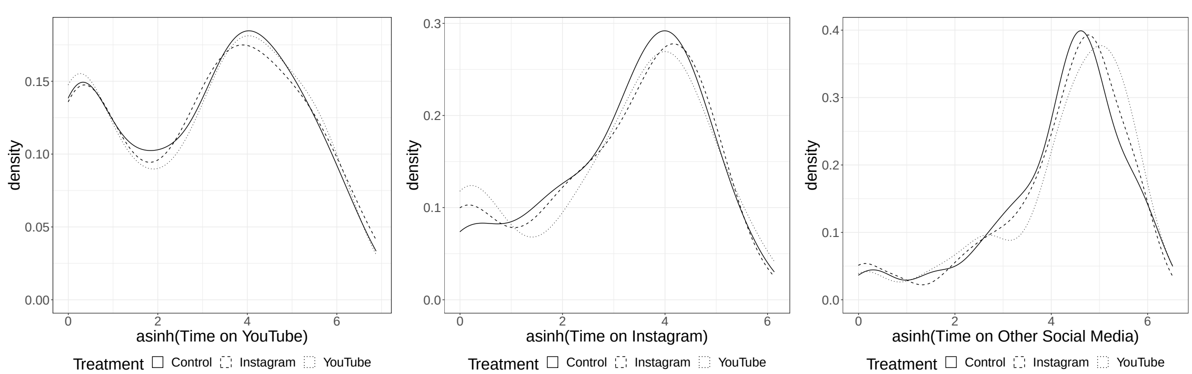
Minimum	25th Percentile	50th Percentile	Mean	75th Percentile	Maximum
18	21	23	25.92	27.0	73

Figure A10: Software Reliability



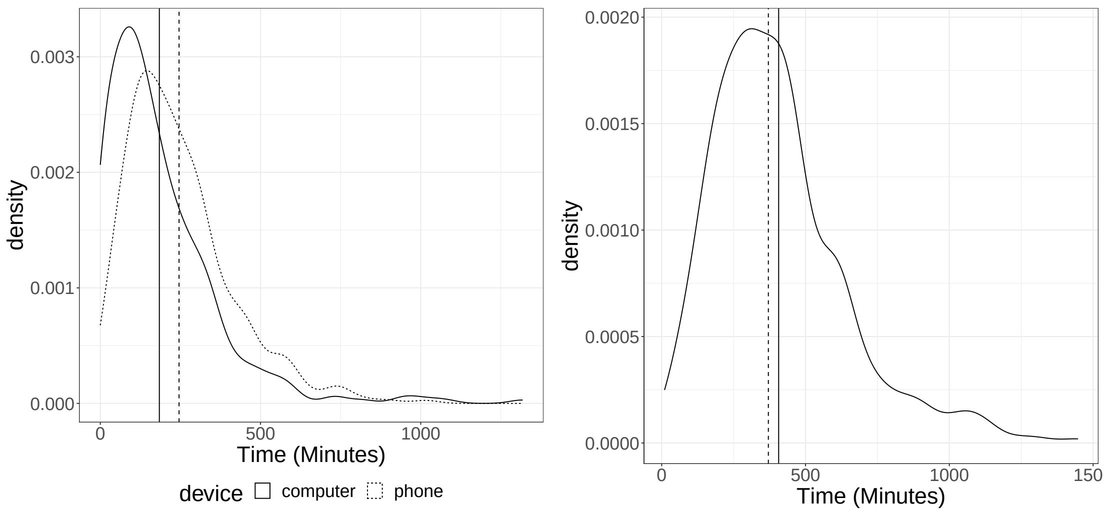
Notes: The figure on the left shows the number of days with active software for all participants, including those who dropped out but whose data I do not drop entirely. The figure on the right shows the number of days with active software for participants in the main experimental group and who stayed through the entirety of the study.

Figure A11: Distributions of Application Usage Across Treatment Groups



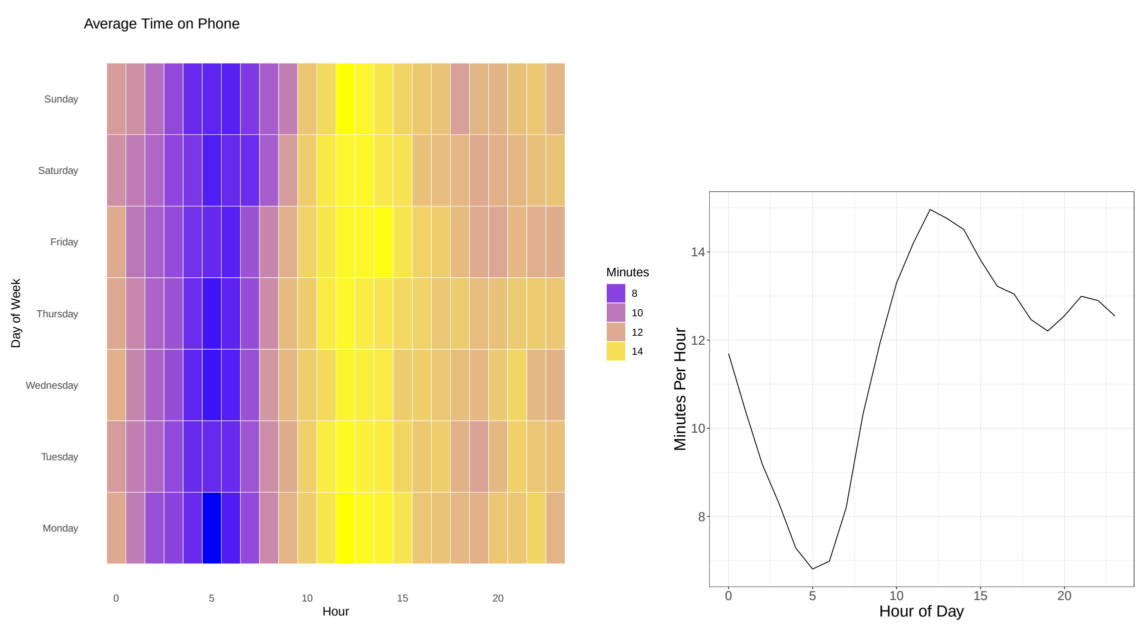
Notes: The figures show the distribution of usage on YouTube (left), Instagram (middle), and other social media (right) during the baseline period across the different experimental treatment groups.

Figure A12: Distribution of Daily Phone Usage



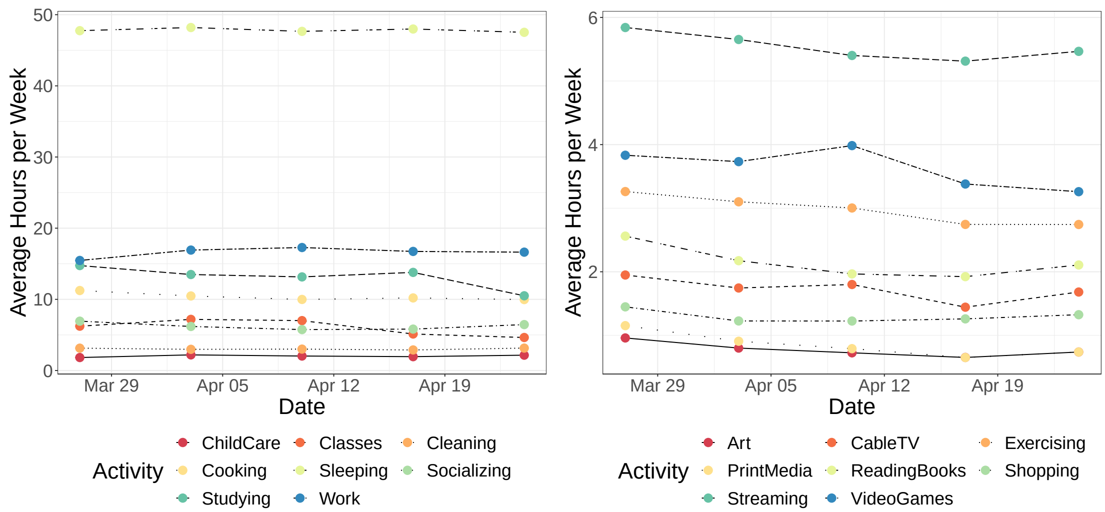
Notes: Both figures plot a kernel density fit of the observed average daily phone usage over the baseline week of the experiment. The figure on the left plots the distribution of phone and computer data separately with the dashed vertical line representing the mean phone time and the solid vertical line representing the mean computer time. The figure on the right displays the distribution of time spent across both computer and phone. The solid line represents the mean time and the dashed line represents the median time.

Figure A13: Time on Phone Across the Week



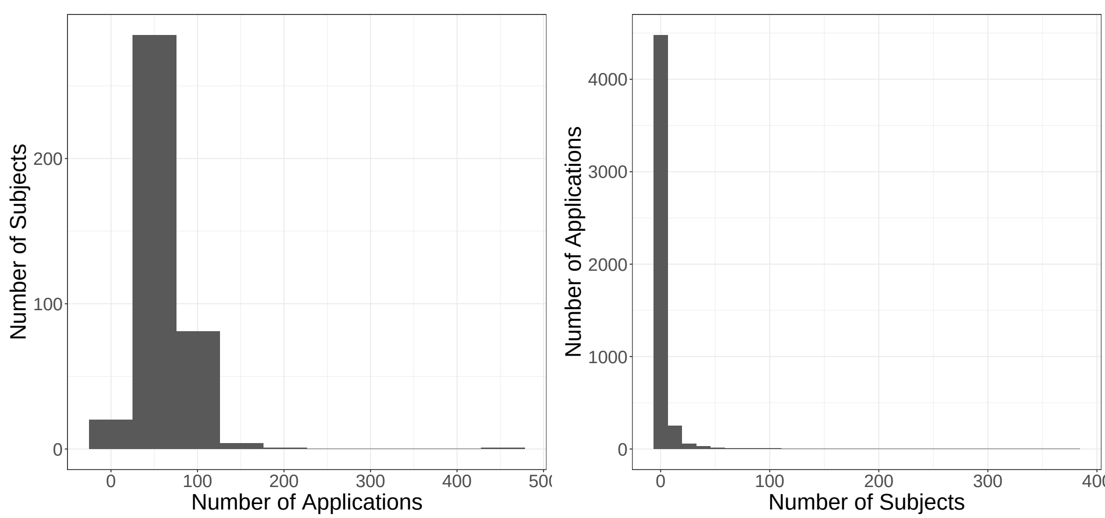
Notes: The figure on the left plots the heatmap of average minutes of usage throughout the entire study period across days of the week and hours of the day. The figure on the right plots the average minutes of usage across hours of the day.

Figure A14: Time Off Digital Devices



Notes: A single point on the graph represents the average reported time spent on a category and week. Each reported data comes from the weekly time use survey filled out by participants. The figure on the left displays the amount of time spent on necessities in life such as sleeping and working. The figure on the right displays the amount of time spent on leisure activities such as streaming movies, reading books, playing video games, etc.

Figure A15: The Long Tail of Applications



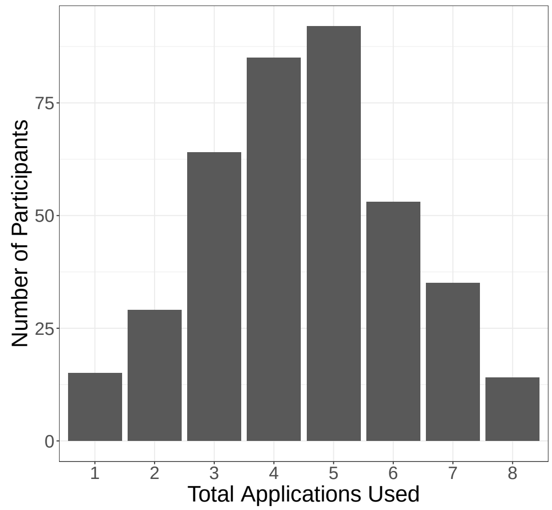
Notes: The figure on the left displays the number of users for each application observed throughout the entire study period. The figure on the right displays the histogram of the number of applications used by each participant throughout the entire study period.

Table A3: Time Spent on Application Categories on Phone

Category	Average Time	Median Time	Average Time | Usage	Average Time | Usage	Numbers of Users
social	66.09	52.22	68.74	53.73	374
entertainment	56.20	21.32	60.19	25.57	366
communication	54.75	40.86	55.17	41.00	389
game	23.77	0.57	42.38	16.93	175
tools	11.65	6.54	11.80	6.64	387
education	5.25	0.14	8.65	1.00	216
maps	4.51	0.83	6.39	2.11	276
business	4.49	0.50	6.58	2.39	254
productivity	4.32	1.43	4.73	1.64	358
news	3.77	0.00	8.51	1.50	130
shopping	3.32	0.29	5.26	1.46	230
sports	3.06	0.07	5.61	1.21	55
art	2.96	1.29	3.37	1.79	345
lifestyle	2.69	0.14	4.60	0.64	212
finance	2.19	0.71	2.64	1.29	316
dating	2.02	0.07	3.40	0.57	219
food	1.76	0.29	2.79	1.29	190
health	1.60	0.07	3.03	0.43	176
music	1.55	0.00	4.15	0.61	144

Notes: This table displays the time allocations for the product categories on the phone. The product categories are those assigned to the applications in the Google Play Store. I report average daily minutes spent on each category during the baseline week for the days when there were no known issues with application usage logging. The first column displays the name of the application. The second and third columns display the average and median minutes per day, respectively, across all participants. The fourth and fifth columns display the same quantities respectively, but conditional only on the participants that make use of those applications. The sixth column displays the number of participants that use the application.

Figure A16: Multihoming



Notes: This figure computes the set of participants that make use of Facebook, Messenger, Instagram, YouTube, Reddit, WhatsApp, TikTok, and Snapchat. It plots how many participants used 1, 2, 3, etc. of these applications over the course of the experiment.

Table A4: Extent of Multihoming

	Facebook	Messenger	YouTube	TikTok	Instagram	Snapchat	WhatsApp	Reddit	Total Users
Facebook	–	0.76	0.98	0.27	0.87	0.48	0.75	0.43	274
Messenger	0.83	–	0.98	0.27	0.84	0.5	0.68	0.42	250
YouTube	0.71	0.65	–	0.25	0.81	0.46	0.74	0.42	379
TikTok	0.80	0.72	1.0	–	0.94	0.72	0.68	0.55	94
Instagram	0.76	0.66	0.97	0.28	–	0.52	0.767	0.4	316
Snapchat	0.74	0.70	0.98	0.38	0.92	–	0.70	0.47	178
WhatsApp	0.71	0.59	0.98	0.22	0.84	0.43	–	0.40	288
Reddit	0.73	0.65	0.99	0.32	0.86	0.51	0.70	–	162

Notes: Each row represents a single application. The last column in the row indicates the total number of participants that used the application over the course of the experiment. Each cell represents the fraction of participants that made use of the (row) application that also made use of the (column) application.

Table A5: Time Spent on Applications of Interest

Application	Medium	Average	Median	Average | Usage	Median | Usage	Number of Participants
Other Applications	computer	143.69	106.71	147.49	110.00	340
Other Applications	phone	112.36	84.18	112.36	84.18	392
YouTube	phone	41.56	11.71	48.63	17.86	335
YouTube	computer	24.99	5.57	32.67	11.86	267
Instagram	phone	22.44	9.29	29.82	19.00	295
WhatsApp	phone	18.30	4.79	26.66	15.64	269
Facebook	phone	13.10	0.71	21.86	7.57	235
TikTok	phone	8.80	0.00	50.71	28.86	68
Reddit	phone	7.61	0.00	21.62	5.36	138
Netflix	computer	5.82	0.00	25.69	10.14	79
Messenger	phone	5.55	0.07	10.47	1.96	208
Twitter	phone	4.58	0.00	13.41	3.79	134
Netflix	phone	4.16	0.00	22.65	3.57	72
Snapchat	phone	3.58	0.00	9.30	3.86	151
Reddit	computer	2.81	0.00	7.73	1.00	127
Facebook	computer	2.63	0.14	5.19	1.57	177
Twitter	computer	1.81	0.00	6.79	0.86	93
Messenger	computer	1.28	0.00	13.96	6.21	32
Instagram	computer	1.00	0.00	4.05	0.43	86
WhatsApp	computer	0.15	0.00	8.52	6.07	6
TikTok	computer	0.03	0.00	0.95	0.36	12
Snapchat	computer	0.00	0.00	-	-	0

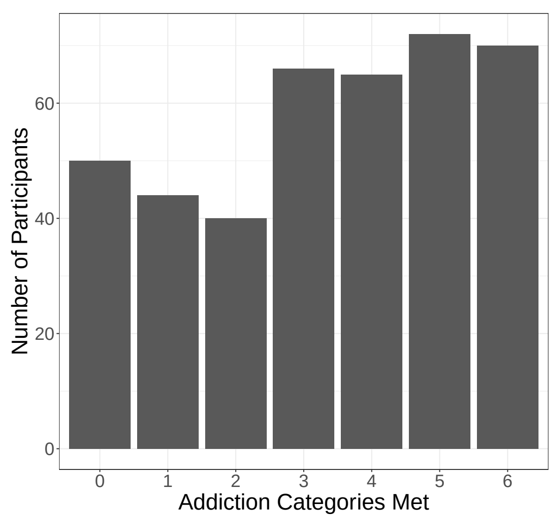
Notes: This table displays the time allocations for the applications of interest. I report average daily minutes spent on each application during the baseline week for the days when there were no known issues with application usage logging. The first and second column display the name of the application and whether it was on the computer or phone. The third and fourth columns display the average and median minutes per day, respectively, across all participants. The fifth and sixth columns display the same quantities respectively, but conditional only on the participants that make use of those applications. The seventh column displays the number of participants that use the application.

Table A6: Post Frequency on Applications of Interest

Application	Never	Less Than Once a Month	At least once a month	At least once a week	2 or 3 times per week	Every day
Facebook	0.36	0.41	0.10	0.04	0.04	0.05
Instagram	0.16	0.44	0.20	0.08	0.07	0.05
YouTube	0.81	0.11	0.03	0.02	0.02	0.02
TikTok	0.76	0.13	0.08	0.01	0.01	0.02
Twitter	0.32	0.31	0.10	0.11	0.11	0.05
Snapchat	0.24	0.28	0.09	0.12	0.11	0.16
Reddit	0.51	0.27	0.07	0.07	0.07	0.01

Notes: Each cell represents the fraction of users of the row application that reported the column post frequency. A post means that the participant actively contributes content to the selected application (including ephemeral content such as stories). For each row, I only report the proportion of participants who stated in the survey that they use this application or if there is observed time usage of the application in the baseline period of the study.

Figure A17: Distribution of Addiction Classification



Notes: This figure presents the responses to the social media addiction question in the initial survey which follows [9]. This consists of 6 questions which represent different aspects of addiction. The x-axis represents the number of aspects where the participant is classified as having that aspect above a threshold. The y-axis represents the number of participants that have that number of aspects of addiction satisfied.

A.3 Correlational Relationship Between Welfare and Time

In this section I explore the relationship between welfare (WTA) and time allocations by estimating the following specification using the time data from my experiment and the elicited WTA values:

$$WTA_{ij} = \beta \cdot time_{ij} + \gamma \cdot \mathbb{1}(app = j) + \alpha_i + \epsilon_{ij} \tag{A.1}$$

I find a positive and robust relationship between time spent on an application and associated welfare generated by it, though the R^2 is quite low. This indicates that additional time leads to an increase in welfare, but that other (unobserved) factors further play a role. Table A7 and Table A8 reports the specifications using levels and logs, respectively, with heteroskedasticity robust standard errors reported.[1] The results do not change substantially across the different specifications and my preferred specification is column (4) which includes both application controls and individual fixed effects. The estimates imply that an additional minute on average weekly time spent on an application leads to a 5.8 cents increase in welfare.[2]

Next I explore heterogeneity in this measure across different dimensions. The most obvious heterogeneity to explore is across different applications as one might expect that the relationship between time and value varies depending on the application. However, as noted by Table 1.2, the traditional social media applications have heterogeneity in how participants use them. In order to assess how this heterogeneity impacts changes in valuation, I estimate the following specification:

$$WTA_{ij} = \beta \cdot \left(time_{ij} \times activity_{ij}\right) + \alpha_i + \epsilon_{ij} \tag{A.2}$$

Table A9 reports the results of estimating (A.2). Across all activities there is a positive rela-

[1] I only provide the results for the linear specification, but including quadratic and cubic terms did not quantitatively change the resulting estimates.

[2] As a robustness check, I re-run the specifications using the subjective weekly time estimates from the initial survey. The results are quantitatively similar with the preferred specification implying that an additional minute on average weekly time spent on an application leads to a 4.4 cents increase in welfare.

tionship between time and WTA with the entertainment activity leading to a 5 cent increase per additional weekly minute. The relationship is consistent across other categories with the exception of communicating with friends which indicates a 11 cent increase per additional weekly minute.[3] I validate this distinction by estimating heterogeneous effects across different application where the takeaway is qualitatively similar with the messaging applications having higher value per additional minute than Facebook and other social media applications.[4]

Table A7: Time vs. WTA (Collected Data, Levels)

	Dependent variable:			
	WTA			
	(1)	(2)	(3)	(4)
Average Weekly Phone Minutes	0.062***	0.063***	0.057***	0.058***
	(0.009)	(0.007)	(0.009)	(0.007)
Application Controls	No	No	Yes	Yes
Participant Fixed Effects	No	Yes	No	Yes
Constant	68.957***			
	(3.046)			
Observations	2,131	2,131	2,131	2,131
R^2	0.023	0.537	0.067	0.572
Adjusted R^2	0.022	0.432	0.063	0.474
Residual Std. Error	123.970 (df = 2129)	94.455 (df = 1739)	121.350 (df = 2121)	90.952 (df = 1731)

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

Notes: Heteroskedacity-robust standard errors are reported in parentheses. The dependent variable is the elicited WTA. The data is from the average daily time spent during the baseline period. I drop the application Netflix.

[3]The point estimate for online shopping also suggests there is a larger relationship between time and welfare for this category. However, since only a handful of participants use these applications for online shopping the standard errors are quite large and thus the resulting estimate is imprecise and makes it difficult to draw definitive conclusions.

[4]I do not report this specification here to economize on space, but it is available upon request.

Table A8: Time vs. WTA (Collected Data, Logs)

	Dependent variable:			
	WTA			
	(1)	(2)	(3)	(4)
asinh(Average Weekly Phone Minutes)	10.476***	11.609***	9.963***	11.146***
	(1.092)	(0.937)	(1.128)	(0.961)
Application Controls	No	No	Yes	Yes
Participant Fixed Effects	No	Yes	No	Yes
Constant	37.829***			
	(5.056)			
Observations	2,131	2,131	2,131	2,131
R^2	0.041	0.556	0.083	0.589
Adjusted R^2	0.041	0.457	0.079	0.495
Residual Std. Error	122.780 (df = 2129)	92.408 (df = 1739)	120.309 (df = 2121)	89.118 (df = 1731)

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

Notes: Heteroskedacity-robust standard errors are reported in parentheses. The dependent variable is the elicited WTA. The data is from the average daily time spent during the baseline period. I drop the application Netflix.

Table A9: Time vs WTA, By Activity

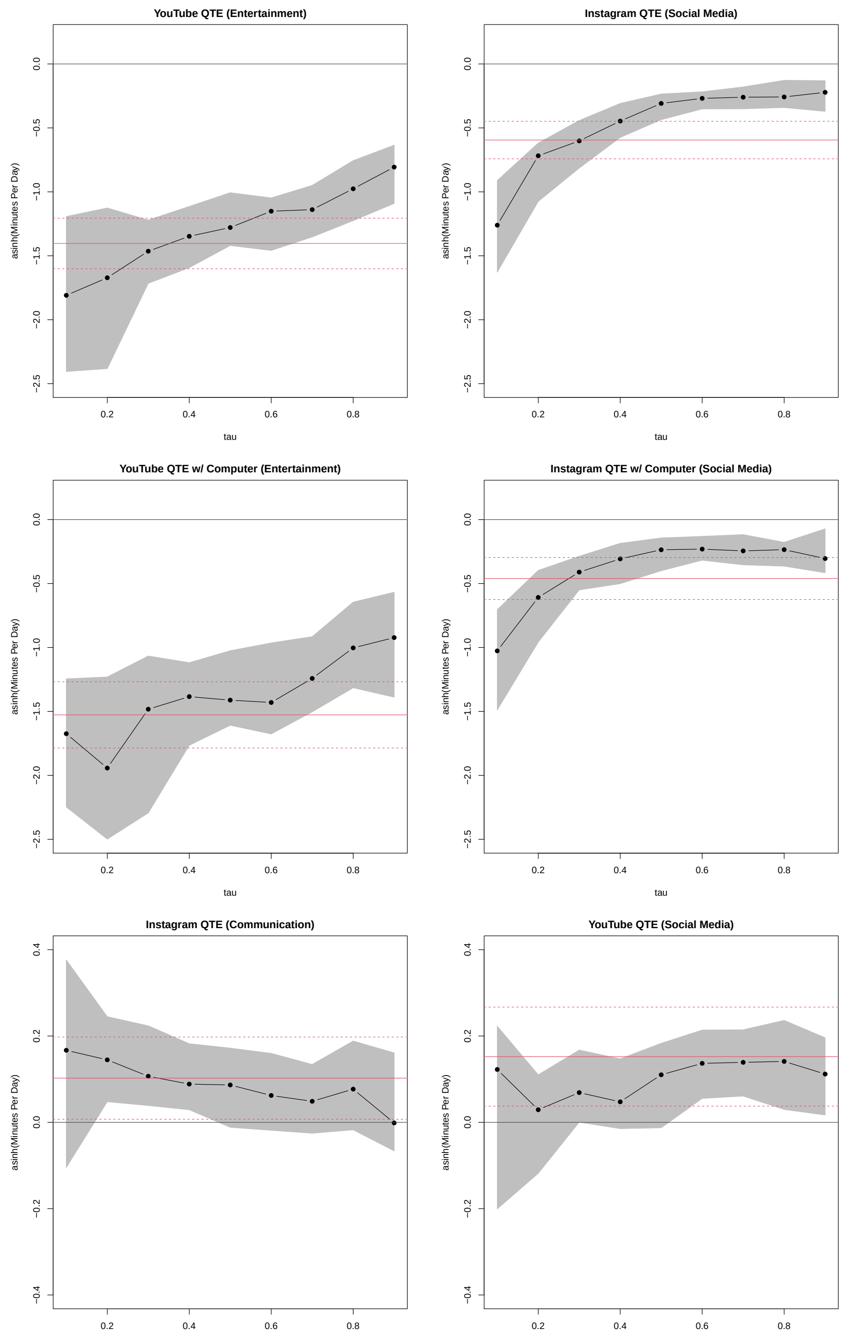
	Dependent variable:			
	WTA			
	(1)	(2)	(3)	(4)
Average Recorded Phone Time	0.040***		0.040***	
	(0.012)		(0.010)	
asinh(Average Recorded Phone Time)		7.753***		7.126***
		(1.865)		(1.563)
Communicate with my friends	24.071***	−0.748	13.916**	−25.792**
	(7.728)	(12.912)	(6.319)	(10.508)
Get Information (e.g. news about politics, sports, business, etc.)	0.085	7.422	−19.599***	−22.540*
	(8.897)	(14.574)	(7.575)	(11.982)
Keep up with my friends' lives	−12.806	−6.894	−16.184**	−7.412
	(9.708)	(16.460)	(8.122)	(13.720)
Online Shopping	−37.945	−22.816	−28.525	−31.164
	(35.760)	(48.723)	(30.326)	(39.453)
Average Recorded Phone Time × Communicate with my friends	0.095***		0.109***	
	(0.025)		(0.021)	
Average Recorded Phone Time × Get Information (e.g. news about politics, sports, business, etc.)	0.046**		0.034*	
	(0.022)		(0.018)	
Average Recorded Phone Time × Keep up with my friends' lives	0.003		−0.016	
	(0.037)		(0.031)	
Average Recorded Phone Time × Online Shopping	0.214		0.140	
	(0.327)		(0.276)	
asinh(Average Recorded Phone Time) × Communicate with my friends		9.976***		13.952***
		(2.710)		(2.203)
asinh(Average Recorded Phone Time) × Get Information (e.g. news about politics, sports, business, etc.)		0.776		2.811
		(3.144)		(2.578)
asinh(Average Recorded Phone Time) × Keep up with my friends' lives		−1.766		−3.388
		(3.440)		(2.873)
asinh(Average Recorded Phone Time) × Online Shopping		0.089		4.108
		(11.406)		(9.279)
Participant Fixed Effects	No	No	Yes	Yes
Constant	61.886***	37.109***		
	(5.564)	(9.602)		
Observations	2,056	2,056	2,056	2,056
R^2	0.053	0.072	0.562	0.587
Adjusted R^2	0.049	0.068	0.457	0.488
Residual Std. Error	121.107 (df = 2046)	119.922 (df = 2046)	91.492 (df = 1657)	88.852 (df = 1657)

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

Notes: Heteroskedasticity-robust standard errors are reported in parentheses. The baseline activity is Entertainment content (e.g. memes, influencers, videos, etc.). The dependent variable considered is elicited WTA. The data is from the average daily time spent during the baseline period. I drop the application Netflix.

A.4 Additional Experimental Results

Figure A18: Quantile Treatment Effects of Category Substitution



Notes: Each figure shows the QTE for an outcome variable. The title of the figure indicates the treatment and the parentheses indicates the outcome variable. The figure on the first row and first column is titled YouTube QTE (Entertainment) meaning that I focus on the YouTube treatment and the entertainment category. The figures in the middle row include time from the Chrome extension, whereas the rest only include time from the phone.

Table A10: Stated Substitution Patterns

Application	Social Media	Entertainment	News	Messaging	In-person	Other Hobbies	Total
Facebook	0.33	0.25	0.12	0.11	0.04	0.16	280
Messenger	0.09	0.10	0.02	0.57	0.11	0.12	250
Instagram	0.23	0.32	0.05	0.12	0.06	0.22	310
YouTube	0.10	0.46	0.10	0.02	0.03	0.31	367
TikTok	0.18	0.43	0.01	0.04	0.04	0.28	92
WhatsApp	0.10	0.08	0.003	0.55	0.16	0.10	288
Twitter	0.27	0.11	0.41	0.08	0.04	0.10	194
Snapchat	0.29	0.08	0.02	0.38	0.10	0.13	167
Reddit	0.17	0.19	0.36	0.03	0.01	0.22	201
Netflix	0.07	0.57	0.02	0.03	0.05	0.25	227

Notes: Each row corresponds to the response for each application about what the participant believes they would substitute their time with if the application was no longer available. The last column indicates the total number of participants that indicated they would substitute to one of the categories. Each cell in the row corresponds to the fraction of total participants who selected the column option. For each row, I only report the proportion of participants who stated in the survey that they use this application or if there is observed time usage of the application in the baseline period of the study as well as if they did not mark no change in response to the question.

Table A11: Instagram Type of App Substitution

	Dependent variable:			
	Facebook Ecosystem	Facebook Ecosystem (No IG)	Major	Minor
	(1)	(2)	(3)	(4)
Category Time	-21.906^{***}	1.828	-4.727	-3.322
	(4.126)	(3.194)	(6.086)	(5.253)
Category Time - Pooled	-21.575^{***}	0.950	2.231	2.321
	(3.195)	(2.646)	(4.543)	(3.928)
asinh(Category Time)	-0.577^{***}	0.160	0.061	0.015
	(0.121)	(0.109)	(0.097)	(0.089)
asinh(Category Time) - Pooled	-0.646^{***}	0.179^{**}	0.075	0.080
	(0.099)	(0.077)	(0.076)	(0.070)
Category Share	-0.059^{***}	0.051^{***}	0.027^{*}	0.025
	(0.015)	(0.014)	(0.016)	(0.016)
Category Share - Pooled	-0.067^{***}	0.044^{***}	0.024^{*}	0.029^{**}
	(0.012)	(0.011)	(0.013)	(0.012)

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

Notes: I consider the degree of substitution to Facebook-owned applications (WhatsApp, Facebook, Instagram, Messenger), "major" applications (Reddit, YouTube, TikTok, Netflix, Twitter, Discord, Snapchat, Twitch, LinkedIn, Spotify, Zoom, Telegram, Hulu, Prime Video, Signal, Google, Amazon Shopping), and the rest of the applications (excluding core phone applications). Each cell reports the estimated average treatment effect on average daily usage for the participants who have the software active for at least 3 days in the baseline and restriction periods. The first, third, and fifth rows display the primary specification estimated on data from the current experiment with heteroskedacity-robust standard errors are reported in parentheses. The second, fourth, and sixth rows display the primary specification estimated on data pooled from the current experiment and the pilot experiment with standard errors clustered at the individual level reported in parentheses. The category share row measures the on phone share of time spent on the category.

Table A12: YouTube Type of Application Substitution

	Dependent variable:			
	Facebook Ecosystem	Major	Major (No YT)	Minor
	(1)	(2)	(3)	(4)
Category Time	4.176	-47.307^{***}	-0.953	1.046
	(4.328)	(7.037)	(5.011)	(6.622)
Category Time - Pooled	1.998	-48.006^{***}	-4.054	-0.350
	(4.020)	(6.195)	(3.888)	(4.613)
asinh(Category Time)	0.027	-0.710^{***}	0.032	0.036
	(0.082)	(0.114)	(0.101)	(0.081)
asinh(Category Time) - Pooled	0.029	-0.690^{***}	0.054	0.026
	(0.070)	(0.088)	(0.077)	(0.065)
Category Share	0.060^{***}	-0.124^{***}	0.021	0.056^{***}
	(0.013)	(0.016)	(0.013)	(0.016)
Category Share - Pooled	0.044^{***}	-0.104^{***}	0.027^{**}	0.057^{***}
	(0.011)	(0.014)	(0.012)	(0.012)

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

Notes: I consider the degree of substitution to Facebook-owned applications (WhatsApp, Facebook, Instagram, Messenger), "major" applications (Reddit, YouTube, TikTok, Netflix, Twitter, Discord, Snapchat, Twitch, LinkedIn, Spotify, Zoom, Telegram, Hulu, Prime Video, Signal, Google, Amazon Shopping), and the rest of the applications (excluding core phone applications). Each cell reports the estimated average treatment effect on average daily usage for the participants who have the software active for at least 3 days in the baseline and restriction periods. The first, third, and fifth rows display the primary specification estimated on data from the current experiment with heteroskedacity-robust standard errors are reported in parentheses. The second, fourth, and sixth rows display the primary specification estimated on data pooled from the current experiment and the pilot experiment with standard errors clustered at the individual level reported in parentheses. The category share row measures the on phone share of time spent on the category.

Table A13: Survey of Time on Restricted App During Treatment Week Off Phone

	Dependent variable:			
	Other Device Instagram Time	Other Device YouTube Time	asinh(Other Device Instagram Time)	asinh(Other Device YouTube Time)
	(1)	(2)	(3)	(4)
YouTube Treatment		−8.151		−0.409**
		(6.850)		(0.207)
Instagram Treatment	−1.941		−0.042	
	(1.964)		(0.181)	
Baseline Time Controls	Yes	Yes	Yes	Yes
Block Control	Yes	Yes	Yes	Yes
Observations	231	238	231	238
R^2	0.103	0.182	0.316	0.311
Adjusted R^2	0.036	0.123	0.265	0.261
Residual Std. Error	14.854 (df = 214)	52.686 (df = 221)	1.370 (df = 214)	1.594 (df = 221)

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

Notes: Heteroskedasticity-robust standard errors are reported in parentheses. The first and third columns present the results of a regression of self-reported time on Instagram on other devices between the Instagram restriction group and the control group. The second and fourth columns present the results of a regression of self-reported time on YouTube on other devices between the YouTube restriction group and the control group. The dependent variable considered in the regressions is the average daily minutes of usage on the column variable.

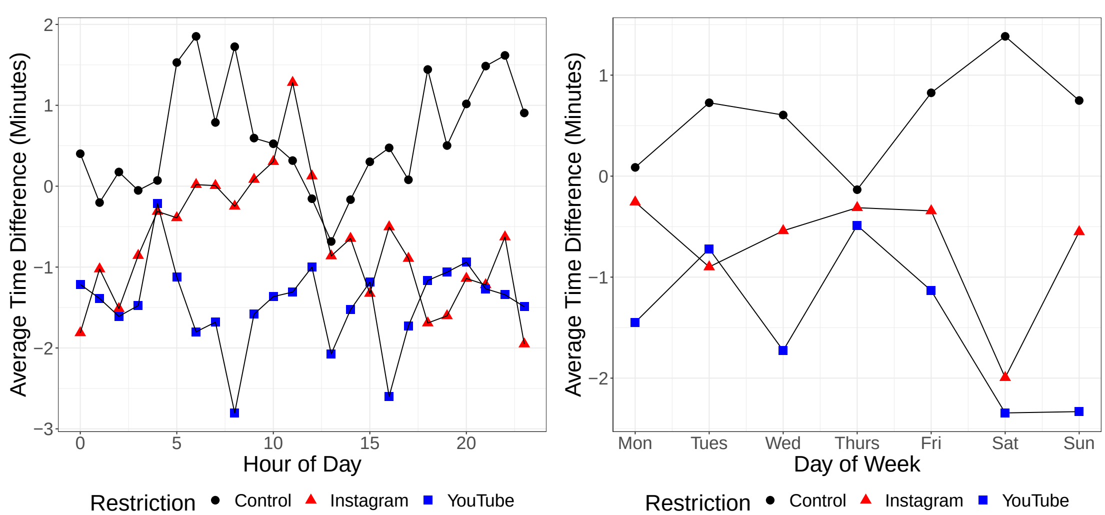
Table A14: Substitution towards the Computer During Treatment Week

	Dependent variable:					
	Overall Computer Time	asinh(Overall Computer Time)	YouTube Computer Time	asinh(YouTube Computer Time)	Instagram Computer Time	asinh(Instagram Computer Time)
	(1)	(2)	(3)	(4)	(5)	(6)
Instagram Treatment	7.978	−0.089			1.585**	0.386***
	(13.856)	(0.115)			(0.801)	(0.094)
YouTube Treatment	17.723	−0.110	9.264*	0.105		
	(13.519)	(0.112)	(5.210)	(0.167)		
Baseline Time Controls	Yes	Yes	Yes	Yes	Yes	Yes
Block Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	330	330	224	224	215	215
R^2	0.698	0.666	0.483	0.622	0.155	0.364
Adjusted R^2	0.681	0.647	0.443	0.592	0.087	0.313
Residual Std. Error	100.774 (df = 312)	0.836 (df = 312)	38.741 (df = 207)	1.241 (df = 207)	5.795 (df = 198)	0.677 (df = 198)

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

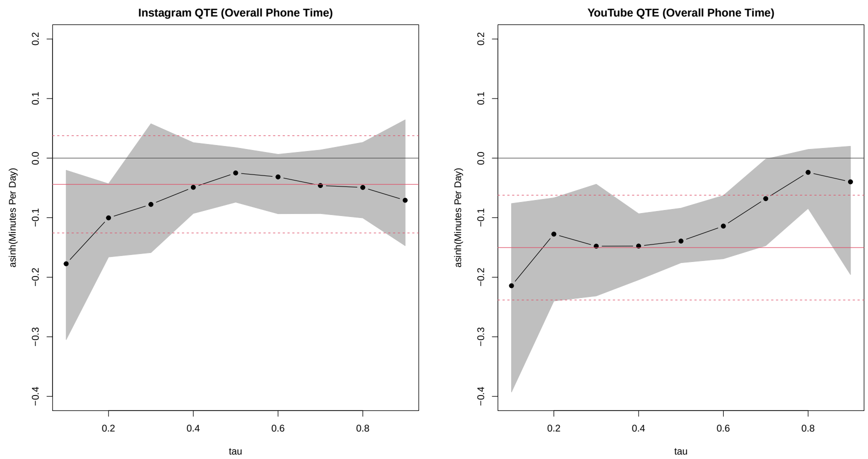
Notes: Heteroskedasticity-robust standard errors are reported in parentheses. The table presents the estimated ATE on average daily computer usage during the first week of the restriction period using the recorded data from the Chrome Extension. The first and second columns present the estimated ATE of overall computer usage for levels and logs respectively. The third and fourth columns present the estimated ATE of computer YouTube usage for levels and logs respectively. The fifth and sixth columns present the estimated ATE of computer Instagram usage for levels and logs respectively.

Figure A19: Time Spent on Phone Throughout the Week (During Treatment Period)



Notes: The figures plot the difference between the first week and the treatment week for each treatment group. The figure on the left plots the difference across different hours of the day and the figure on the right plots the difference across different days of the week.

Figure A20: Quantile Treatment Effects of Overall Phone Time



Notes: The figures present the estimated QTE of the log of overall phone usage across both treatment groups during the restriction period.

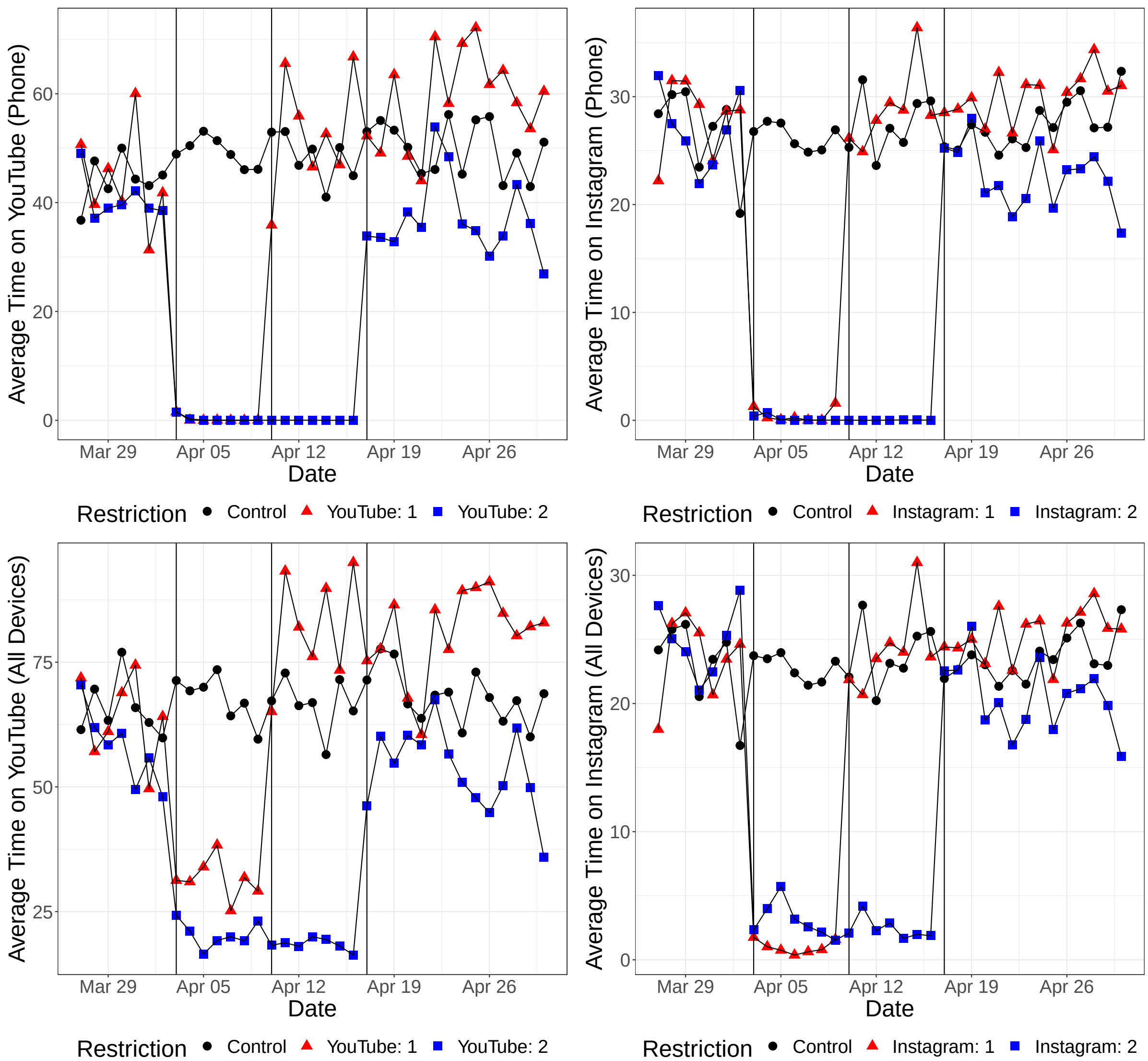
Table A15: Survey of Time Spent on Other Media During Restriction Period

	Dependent variable:			
	asinh(Time on Cable TV)	asinh(Time on Video Games)	asinh(Time on Streaming Services)	asinh(Time on Other Media Composite)
	(1)	(2)	(3)	(4)
YouTube Treatment	0.015	0.258	−0.381	−0.076
	(0.185)	(0.205)	(0.248)	(0.208)
Instagram Treatment	−0.290	0.217	−0.292	−0.079
	(0.187)	(0.207)	(0.251)	(0.210)
Baseline Time Controls	Yes	Yes	Yes	Yes
Block Control	Yes	Yes	Yes	Yes
Observations	357	357	357	357
R^2	0.471	0.565	0.344	0.386
Adjusted R^2	0.444	0.544	0.311	0.355
Residual Std. Error (df = 339)	1.423	1.575	1.911	1.604

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

Notes: Heteroskedasticity-robust standard errors are reported in parentheses. This table reports the estimated ATE on time spent on non-phone media during the restriction period. The data for this come from the weekly time use survey. The first column reports the impact of the treatment on average daily time on cable TV. The second column reports the impact of the treatment on average daily time on video games. The third column reports the impact of the treatment on average daily time on non-phone video streaming services. The fourth column reports the impact of the treatment on the sum of the average daily time on cable TV, video games, and non-phone video streaming services.

Figure A21: Time on Restricted Applications



Notes: This figure plots the average daily usage on the restricted applications across the different treatment groups. The first row shows the average daily usage of the restricted application on the phone for the YouTube (left) and Instagram (right) restriction group. The second row shows the average daily usage of the restricted application across all devices (phone and computer) for the YouTube (left) and Instagram (right) restriction group.

Table A16: Instagram Post-Restriction Usage

	Dependent variable:			
	Instagram Time	asinh(Instagram Time)	Instagram Time	asinh(Instagram Time)
	(1)	(2)	(3)	(4)
Instagram Treatment	4.845	0.177	-5.164^{**}	-0.061
	(3.438)	(0.166)	(2.483)	(0.134)
2 week restriction	3.180	0.038		
	(3.048)	(0.179)		
Instagram Treatment × 2 week restriction	-10.452^{**}	-0.231		
	(4.746)	(0.232)		
Baseline Usage	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes
Block Control	Yes	Yes	Yes	Yes
Observations	410	410	312	312
R^2	0.696	0.731	0.707	0.732
Adjusted R^2	0.680	0.717	0.689	0.716
Residual Std. Error	17.437 (df = 389)	0.912 (df = 389)	16.398 (df = 293)	0.918 (df = 293)

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

Notes: The standard errors for the regression are clustered at the participant level. The regression is estimated on the data of weekly time usage of Instagram in the weeks following the restriction period. The dependent variables reported are both the levels and logs of Instagram usage. The first two columns report the regression across all restriction groups with heterogeneous effects across restriction lengths. The last two columns report the regression on the entire control group and restricting focus to the 2 week Instagram restriction group.

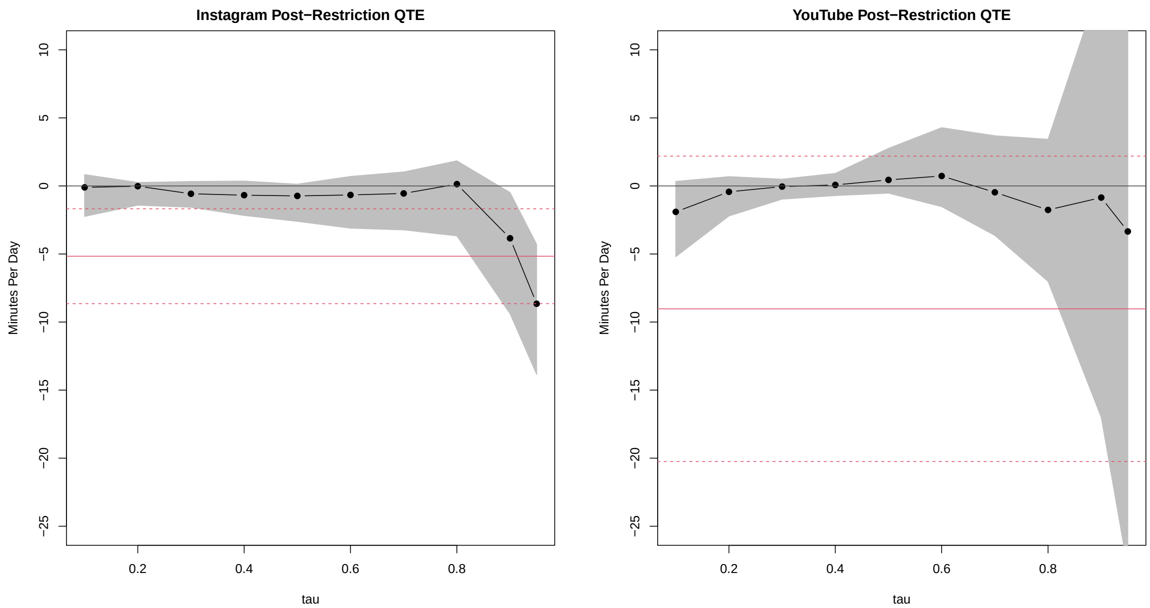
Table A17: YouTube Post-Restriction Usage

	Dependent variable:			
	YouTube Time	asinh(YouTube Time)	YouTube Time	asinh(YouTube Time)
	(1)	(2)	(3)	(4)
YouTube Treatment	1.067	−0.078	−9.028	−0.173
	(10.492)	(0.160)	(6.762)	(0.191)
2 week restriction	−9.093	−0.258		
	(9.021)	(0.213)		
YouTube Treatment × 2 week restriction	−6.640	0.004		
	(10.639)	(0.273)		
Baseline Usage	Yes	Yes	Yes	Yes
Week Fixed Effects	Yes	Yes	Yes	Yes
Block Control	Yes	Yes	Yes	Yes
Observations	480	480	360	360
R^2	0.558	0.674	0.531	0.619
Adjusted R^2	0.539	0.660	0.506	0.599
Residual Std. Error	55.046 (df = 459)	1.190 (df = 459)	56.989 (df = 341)	1.266 (df = 341)

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

Notes: The standard errors for the regression are clustered at the participant level. The regression is estimated on the data of weekly time usage of YouTube in the weeks following the restriction period. The dependent variables reported are both the levels and logs of YouTube usage. The first two columns report the regression across all restriction groups with heterogeneous effects across restriction lengths. The last two columns report the regression on the entire control group and restricting focus to the 2 week YouTube restriction group.

Figure A22: Quantile Treatment Effects of Post-Restriction Usage



Notes: The figures present the estimated QTE of post-restriction usage on the restricted applications across both treatment groups.

Table A18: Perceived Endline Substitution Patterns

Restricted Application	New Apps	Invested in Other Apps	Time on Other Apps	Computer Time	Offline	No Change
During Restriction - Instagram	0.05	0.19	0.26	0.20	0.18	0.11
After Restriction - Instagram	0.04	0.08	0.16	0.17	0.15	0.41
During Restriction - YouTube	0.10	0.15	0.30	0.22	0.15	0.08
After Restriction - YouTube	0.05	0.11	0.13	0.17	0.13	0.41

Notes: This table shows the proportion of participants in each treatment group that report their perceived substitution during the experiment. The first and third rows show the perceived changes in behavior during the restriction period. The second and fourth rows show the perceived changes in behavior following the restriction period. Column 2 represents primary substitution towards newly installed applications. Column 3 represents primary substitution towards installed applications that participants "invested" in sourcing better content from. Column 4 represents primary substitution towards other installed applications but without significant additional "investment" in them. Column 5 represents primary substitution towards the computer. Column 6 represents primary substitution towards non-digital activities. Column 7 represents no change in behavior.

Table A19: One Month Post-Experiment Survey Results

	Dependent variable:			
	Phone Time	Social Media Time	Instagram Time	YouTube Time
	(1)	(2)	(3)	(4)
Instagram Restriction	−0.115	-0.305^{**}	-0.316^{*}	−0.113
	(0.147)	(0.150)	(0.189)	(0.178)
YouTube Restriction	0.087	−0.003	0.189	−0.268
	(0.145)	(0.148)	(0.186)	(0.176)
Block Control	No	No	No	No
Constant	2.811^{***}	2.698^{***}	2.756^{***}	3.113^{***}
	(0.106)	(0.107)	(0.137)	(0.127)
Observations	168	168	149	167
R^2	0.012	0.033	0.051	0.014
Adjusted R^2	−0.00004	0.021	0.038	0.002
Residual Std. Error	0.768 (df = 165)	0.783 (df = 165)	0.920 (df = 146)	0.927 (df = 164)

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

Notes: Heteroskedasticity-robust standard errors are reported in parentheses. The data comes from the survey sent one month after the study concluded where participants indicated whether they were spending significantly less time (1), somewhat less time (2), the same time (3), somewhat more time (4), or significantly more time (5) on each outcome variable. The dependent variable in column 1 is the overall phone time, in column 2 is overall social media time, in column 3 is Instagram time, and column 4 is YouTube time. For the YouTube and Instagram time dependent variables, I drop participants who marked that they do not use the respective application or started to use it during the study.

Table A20: Instagram Post-Restriction Usage of Non-Restricted Applications

	Dependent variable:			
	Time	asinh(Time)	Time: 2 Weeks	asinh(Time): 2 Weeks
	(1)	(2)	(3)	(4)
Social Category	−1.960	−0.067	−1.037	−0.133
	(4.305)	(0.079)	(5.855)	(0.109)
Communication Category	−3.395	0.049	0.435	0.071
	(3.833)	(0.073)	(4.644)	(0.090)
TikTok	0.367	−0.199	5.876	−0.222
	(7.047)	(0.196)	(9.376)	(0.247)
Facebook	1.063	0.032	1.198	0.117
	(2.516)	(0.137)	(3.213)	(0.161)
Snapchat	−0.053	−0.063	−0.532	−0.156
	(1.049)	(0.101)	(1.520)	(0.145)
WhatsApp	−2.585	0.101	−1.315	0.259^{*}
	(2.838)	(0.118)	(3.401)	(0.146)
Messenger	−0.161	−0.010	−0.008	0.017
	(0.943)	(0.116)	(1.345)	(0.154)
YouTube	−6.288	−0.177	−5.244	−0.176
	(6.843)	(0.141)	(7.383)	(0.183)
Apps Installed During Restriction	4.293	0.262	1.554	0.123
	(2.605)	(0.194)	(1.993)	(0.255)

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

Notes: Reported standard errors in parentheses are clustered at the participant level. This table presents the ATE estimates of usage on the row application / category after the restriction period. A single data point is the average daily time on the row application one or two weeks following the restriction. The first two rows consider the average time on social and communication categories respectively. The following rows consider the average time on the specified application. The final row considers the average time on the applications that were installed during the restriction period. The first two columns report the ATE on time usage both in levels and logs. The final two column report the ATE on time usage restricting to the 2 week restriction group.

Table A21: YouTube Post-Restriction Usage of Non-Restricted Applications

	Dependent variable:			
	Time	asinh(Time)	Time: 2 Weeks	asinh(Time): 2 Weeks
	(1)	(2)	(3)	(4)
Social Category	0.541	0.014	1.951	−0.028
	(4.182)	(0.081)	(6.208)	(0.115)
Entertainment Category	−4.754	−0.144	−12.857	−0.269
	(7.140)	(0.121)	(7.993)	(0.170)
TikTok	−3.313	−0.041	−6.529	0.012
	(8.271)	(0.288)	(12.526)	(0.371)
Facebook	−0.735	−0.031	−0.954	0.068
	(2.010)	(0.127)	(2.522)	(0.146)
Instagram	3.999^{*}	0.190^{*}	2.876	0.161
	(2.262)	(0.112)	(3.276)	(0.133)
Snapchat	0.473	0.003	0.747	0.040
	(1.115)	(0.114)	(1.628)	(0.158)
WhatsApp	−0.323	0.091	0.682	0.157
	(2.496)	(0.102)	(3.245)	(0.123)
Apps Installed During Restriction	3.366^{**}	0.406^{**}	1.990	0.201
	(1.450)	(0.186)	(1.837)	(0.243)

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

Notes: Reported standard errors in parentheses are clustered at the participant level. This table presents the ATE of usage on the row application / category after the restriction period. A single data point is the average daily time on the row application one or two weeks following the restriction. The first two rows consider the average time on social and entertainment categories respectively. The following rows consider the average time on the specified application. The final row considers the average time on the applications that were installed during the restriction period. The first two columns report the ATE on time usage both in levels and logs. The final two column report the ATE on time usage restricting to the 2 week restriction group.

A.5 Alternative Estimation of Diversion Ratios

In this section I provide an alternative method of estimating the diversion ratios required for the UPP test. I follow the methods proposed in [8, 32] that directly exploit the experimental product unavailability variation to estimate the diversion ratios. The method proceeds by first using the estimated average treatment effects between the restricted applications and the other applications of interest to estimate the diversion ratios from the restricted applications to other applications. This provides a nonparametric estimate of the diversion ratio between these applications. Then, I impose a semiparametric logit assumption and, using the aggregate market shares and the estimated diversion ratios, an MPEC procedure enables the estimation of the rest of the matrix of diversion ratios.

A.5.1 Estimation Procedure

I restrict to the same set of prominent social media and entertainment applications as in the main text: Snapchat, Facebook, Reddit, TikTok, Instagram, YouTube, and Twitter. The outside option is defined as time not on the phone and other applications on the phone. Thus, I have a choice set of $J = 7$ applications plus an outside option and the goal is to estimate the $J \times (J + 1)$ matrix of diversion ratios. I aggregate time spent during these time periods as follows. I consider that each time of day has T time periods of half minute units. I aggregate time spent during these time periods in order to compute market shares for each individual. I drop the “late night" hours so that I only consider 17 hours in the day.

There are I individuals, $J + 1$ applications (including outside option), and T time periods. I denote the choice decision of each individual i for application j at time period t as a discrete choice:

$$d_{ij,t} = \begin{cases} 1, & \text{if } u_{ij,t} > u_{ij',t} \quad \forall j' \in \mathcal{J} \setminus j \\ 0, & \text{otherwise} \end{cases}$$

Thus, the individual choice shares for individual i as well as the aggregate choice shares for application j are given as follows:

$$s_{ij}(\mathcal{J}) = \frac{1}{T}\sum_{t=1}^{T} d_{ij,t} \quad s_j(\mathcal{J}) = \frac{1}{IT}\sum_{i=1}^{I}\sum_{t=1}^{T} d_{ij,t}$$

A.5.2 Estimating Diversion Ratios of the Restricted Applications

I estimate the diversion ratios for the restricted applications. I denote $\mathcal{S}$ as the vector of aggregate market shares. Following [8], I can directly compute the diversion ratios from the restricted application to other applications of interest using the estimated treatment effect of the application restrictions:

$$\tilde{D}_{kj} = \frac{\mathcal{S}_j(\mathcal{J} \setminus k) - \mathcal{S}_j(\mathcal{J})}{\mathcal{S}_k(\mathcal{J})}$$

In order to compute the numerator, I estimate the baseline specification (1.1) for each application of interest and, for the denominator, I use the average share of application k in the baseline period. However, this formulation does not guarantee that the resulting diversion ratios sum to 1 or are non-negative. I impose the assumption that the resulting diversion ratios must be non-negative (i.e. the applications are substitutes) and that they sum to 1. Thus, given the resulting estimates of the diversion ratio, I first impose that they are non-negative and then normalize them so that the resulting estimated diversion ratios all sum to 1.

For additional precision in the estimates of the diversion ratios, I make use of the empirical Bayesian shrinkage estimator used by [32]. The estimator is given as follows where q_j denotes the share of daily time on application j:

$$\hat{D}_{kj} = \lambda \cdot \mu_{kj} + (1-\lambda)\cdot \tilde{D}_{kj}, \quad \lambda = \frac{m_{kj}}{m_{kj} + q_j}$$

The idea is that one can view the diversion ratio as a binomial with $\mathcal{S}_k(\mathcal{J})$ “trials" and $\mathcal{S}_j(\mathcal{J} \setminus$

$k) - \mathcal{S}_j(\mathcal{J})$ successes in terms of consumers who chose application k but now switch to application j. Viewed in this manner, I specify a prior belief on D_{kj} and parameterize this prior as $Dirichlet(\mu_{j0}, \mu_{j1}, .., \mu_{jK}, m_{kj})$.[5] The reason to make use of this estimator as opposed to the estimate itself is that some of the estimates may be large, but noisy, especially for applications that have smaller number of users such as TikTok and we want the estimator to account for this. Note that this procedure makes no parametric assumptions about the functional form of consumer utility beyond the substitutes assumption.

Estimating the Other Entries of the Diversion Ratio Matrix

The challenge now is to estimate the remaining cells of the matrix of diversion ratios. However, of course, I do not have direct experimental variation for all of the applications of interest. Following [32], I assume that consumer utility follows a semi-parametric logit, $u_{ij} = V_{ij} + \epsilon_{ij}$ where ϵ_{ij} is the standard type-1 extreme value error. Given this assumption, then [8] show that the average second-choice diversion ratio is given by:

$$D_{kj} \equiv \mathbb{E}[D_{kj,i} \mid i \text{ chooses } k] = \sum_{i=1}^{N} \frac{\pi_i \cdot s_{ik}}{s_k} \cdot \frac{s_{ij}}{1 - s_{ik}} \tag{A.3}$$

Under this parameterization, [32] propose the following MPEC matrix completion procedure in order to estimate the rest of the diversion ratios by using the aggregate shares and the estimated diversion ratios from the experimental data. One intuition as to why this procedure works is that the logit assumption induces full support so that everything weakly substitutes with everything else (i.e. the “connected substitutes" notion discussed in [189]) so that it’s possible to get information on the substitution between Facebook and Snapchat even if I observe no experiments with these items removed. I simplify their procedure since in my case the time spent on the outside option is pinned down due to the fact that there are a limited number of minutes in the day. The notation is as follows: $\hat{D}_{kj}$ denotes the estimated diversion ratios from second choice data, $\mathcal{S}_j$ denotes the aggregate shares, and π_i denotes the probability that a consumer is of type i, OBS denotes the pairs

[5] Since $0 \leq D_{kj} \leq 1$ and $\sum_k D_{kj} = 1$.

of applications for which I have second-choice measures of diversion.

$$\min_{s_{ij},\pi_i} \sum_{(k,j)\in OBS} (\hat{D}_{kj} - D_{kj})^2 + \lambda \sum_j (\mathcal{S}_j - s_j)^2 \tag{A.4}$$

$$\text{subject to: } s_j = \sum_i \pi_i \cdot s_{ij} \tag{A.5}$$

$$D_{kj} = \sum_i \pi_i \cdot \frac{s_{ij}}{1 - s_{ik}} \cdot \frac{s_{ik}}{s_k} \tag{A.6}$$

$$0 \leq s_{ij}, \pi_i, s_j, D_{kj} \leq 1, \sum_i \pi_i = 1, \sum_j s_{ij} = 1 \tag{A.7}$$

This procedure involves an exogenous selection of I latent types of individuals each with different preferences as well as the penalization parameter $\lambda > 0$. The idea is that, as in standard random coefficients logit demand models, the resulting aggregate market shares come from a mixture of different types of consumers whose preferences each follow a different logit. Thus, (A.3) pins down the average second-choice diversion ratio and the MPEC procedure optimizes over the space of possible mixtures of different possible types of individuals in order to best fit the observed diversion ratios and aggregate market shares.

I implement this procedure and choose the exogenous parameter λ by the model with the best in-sample fit according to the mean-squared error or mean absolute error.[6] I consider the set of $I \in \{1, 2, ..., 8, 9\}$ and for each I choose $\lambda \in \{0.2, 0.6, 1.0, ..., 9.2, 9.6, 10\}$. Given the fixed λ for each I, I then choose across I by comparing whether the resulting estimate correctly fits the market shares and whether the resulting estimated diversion ratios could reasonably be implied by the noisier experiments from the pilot experiment which included two applications (Facebook-Messenger and Snapchat-Tiktok) and a smaller sample size. The nonparametric diversion ratios from the joint restrictions in the pilot experiment are reported in Table A23.

[6] I alternatively considered a cross-validation procedure where the model is estimated holding out one set of diversion ratios (i.e. holding out one of the two experiments) but found that this led to unreasonable estimates, likely since I have a small number of experiments and in this case the procedure is relying only on the estimates from one experiment.

A.5.3 Diversion Ratio Estimates

I report the nonparametric diversion ratio estimates for Instagram and YouTube that I compute directly using the experimental variation. I pool together the data from the pilot and larger-scale experiment in order to get more precise estimates. For the estimates I use an informative prior so that the prior follows the predictions of logit and the diversion is proportional to market shares, $\mu_{kj} = \frac{s_j}{1-s_k}$ and $m_{kj} = 10$. I compute standard errors using simple block bootstrap with the blocks being participants and utilizing the bootstrap percentile 95% confidence interval with 20000 replications. Table A23 reports the estimated diversion ratios as I vary the value of m_{kj}. Recall that increasing m_{kj} places additional weight on the prior, which is the predicted diversion from logit, at the expense of the experimental estimates.[7] Furthermore, I also report the estimated diversion ratios from the joint removal of Snapchat and TikTok as well as Facebook and Messenger. I do not directly incorporate these into $\mathcal{D}$ since they contain multiple application restrictions and are less precisely estimated due to smaller sample sizes and multiple restrictions, but rather use them to choose between resulting estimates.

Table A22 reports the estimated diversion ratios for the rest of the applications using the MPEC procedure for $I = 3$ and $\lambda = 6.6$. For lower values of I, the selected λ do a poor job at fitting the market shares, whereas for the higher values of I the selected λ predict very little diversion to the outside option for the other applications. The resulting estimates to the outside option for the reported specification in Table A22 are in line with what one would expect given the nonparametric diversion estimates in Table A23 for the joint Snapchat and TikTok as well as the joint Facebook and Messenger restrictions.

[7] This also varies across applications since, for instance, Snapchat and TikTok have lower aggregate usage the estimator naturally places more weight on the prior for diversion to these applications relative to diversion to diversion for more used applications like YouTube and Instagram.

Table A22: Diversion Ratio Estimates

	Instagram	YouTube	Facebook	TikTok	Snapchat	Reddit	Twitter	Outside Option
Instagram	-	$2.8e-5$	0.05	0.046	$5.4e-5$	$2.8e-5$	0.014	0.89
YouTube	0.052	-	0.033	0.019	0.0035	0.0039	$6.5e-5$	0.89
Facebook	0.024	0.077	-	0.012	0.0062	0.0092	0.0072	0.86
TikTok	0.022	0.061	0.017	-	0.0065	0.0098	0.0079	0.88
Snapchat	0.017	0.019	0.014	0.011	-	0.012	0.0099	0.92
Reddit	0.016	0.014	0.014	0.011	0.0076	-	0.01	0.93
Twitter	0.015	0.00033	0.013	0.011	0.0079	0.012	-	0.94

Notes: The presented table is of the matrix of diversion ratios, D_{kj}, where a cell in the table is the diversion from application k (row) to application j (column). The diversion ratios are estimated using the MPEC procedure.

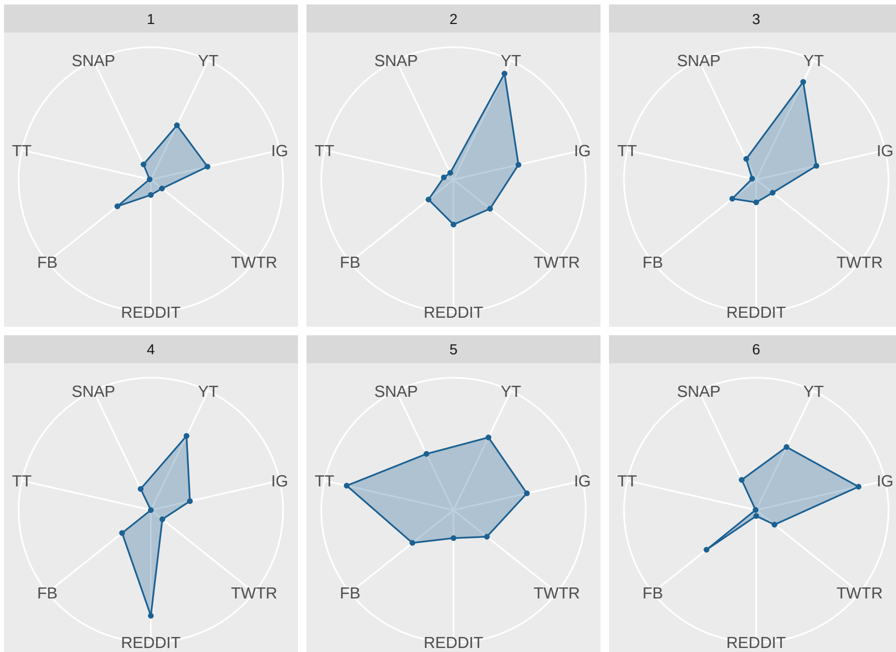
Table A23: Nonparametric Diversion Ratio Estimates

	Instagram	YouTube	Facebook	TikTok	Snapchat	Reddit	Twitter	Outside Option	m_{kj}
Instagram	–	0.0	0.07	0.08	0.0	0.0	0.027	0.82	0
		(0.0, 0.22)	(0.0, 0.18)	(0.0, 0.17)	(0.0, 0.03)	(0.0, 0.04)	(0.0, 0.11)	(0.51, 0.95)	
YouTube	0.06	–	0.05	0.03	0.002	0.0	0.0	0.86	0
	(0.0, 0.13)		(0.0, 0.10)	(0.0, 0.09)	(0.0, 0.03)	(0.0, 0.04)	(0.0, 0.02)	(0.72, 0.94)	
Instagram	–	0.0	0.05	0.05	0.0	0.0	0.01	0.89	10
		(0.0, 0.22)	(0.0, 0.13)	(0.003, 0.10)	(0.0, 0.01)	(0.0, 0.02)	(0.0, 0.05)	(0.61, 0.97)	
YouTube	0.05	–	0.03	0.02	0.003	0.004	0.0	0.89	10
	(0.005, 0.10)		(0.002, 0.07)	(0.0, 0.05)	(0.0, 0.01)	(0.0, 0.02)	(0.0, 0.01)	(0.80, 0.95)	
Snapchat and TikTok	0.03	0.04	0.03	–	–	0.003	0.002	0.90	0
	(0.01, 0.07)	(0.004, 0.09)	(0.006, 0.07)			(0.0, 0.01)	(0.0, 0.01)	(0.78, 0.95)	
Facebook and Messenger	0.08	0.0	–	0.0	0.01	0.01	0.0	0.90	0
	(0.0, 0.31)	(0.0, 0.31)		(0.0, 0.08)	(0.0, 0.10)	(0.0, 0.10)	(0.0, 0.04)	(0.42, 1.0)	

Notes: The presented table is of the matrix of diversion ratios, D_{kj}, where a cell in the table is the diversion from application k (row) to application j (column). This displays different estimates of diversion from Instagram to other applications and YouTube to other applications, depending on the value m_{kj}. I additionally compute the diversion during the Snapchat-TikTok and Facebook-Messenger restrictions which were run in the pilot study. 95% confidence intervals are constructed by simple block bootstrap and using the percentile confidence interval calculation with 20000 replications and are reported in parentheses.

A.6 Additional Figures / Tables for Time Usage Model

Figure A23: K-means Clustering of Participants



Notes: The figures display the results of k-means clustering for $k = 6$. Each pane shows the average (log) time allocations across the different applications for the participants in the cluster. For instance, if a point is closer to the outer edge for an application A than application B then that indicates that that application A has more usage on average than application B. The application names are abbreviated so that the figure is readable. TT is TikTok, FB is Facebook, YT is YouTube, IG is Instagram, SNAP is SnapChat, REDDIT is Reddit, and TWTR is Twitter.

Table A24: Demand Model Parameter Estimates

Type	(1)	(2)	(3)	(4)	(5)	(6)
h_{ijt}	0.013	0.0045	0.0087	0.0052	0.011	0.0086
	(0.00012)	(0.00017)	(8.7e-5)	(0.00016)	(0.00019)	(0.00014)
r_{ijt}	1.5	0.94	1.1	1.4	1.1	1.3
	(0.016)	(0.018)	(0.021)	(0.091)	(0.026)	(0.021)
r^2_{ijt}	-0.038	-0.033	-0.023	-0.083	-0.029	-0.062
	(0.0013)	(0.001)	(0.004)	(0.02)	(0.0019)	(0.0037)
App - Instagram	-5.0	-5.0	-4.3	-5.1	-4.8	-4.2
	(0.026)	(0.07)	(0.038)	(0.14)	(0.077)	(0.039)
App - Twitter	-5.8	-6.4	-5.2	-6.4	-5.0	-5.2
	(0.03)	(0.11)	(0.051)	(0.16)	(0.092)	(0.065)
App - YouTube	-5.5	-4.4	-4.3	-4.3	-5.1	-4.9
	(0.023)	(0.086)	(0.04)	(0.079)	(0.091)	(0.056)
App - TikTok	-5.7	-5.1	-4.7	-4.2	-4.8	-5.3
	(0.031)	(0.1)	(0.043)	(0.13)	(0.094)	(0.085)
App - Reddit	-5.9	-5.2	-5.1	-3.8	-7.0	-6.5
	(0.032)	(0.088)	(0.041)	(0.064)	(0.16)	(0.11)
App- Snapchat	-5.5	-7.2	-4.5	-5.9	-4.9	-5.0
	(0.032)	(0.19)	(0.041)	(0.18)	(0.11)	(0.051)
App - Facebook	-5.3	-4.7	-4.7	-5.7	-5.0	-4.7
	(0.025)	(0.072)	(0.041)	(0.2)	(0.085)	(0.042)
a_{ij} - Online Shopping	-0.52	0.53	0.064	0.0	0.0	-0.46
	(0.065)	(0.13)	(0.055)	(0.0)	(0.0)	(0.17)
a_{ij} - Entertainment content	0.11	0.38	0.25	-0.032	0.23	-0.057
	(0.014)	(0.05)	(0.025)	(0.047)	(0.059)	(0.02)
a_{ij} - Keep up with my friends' lives	0.0	0.0	0.0	0.0099	-0.22	0.0
	(0.0)	(0.0)	(0.0)	(0.16)	(0.068)	(0.0)
a_{ij} - Get Information	-0.26	0.24	0.25	0.0	0.0	-0.016
	(0.021)	(0.061)	(0.031)	(0.0)	(0.0)	(0.03)
a_{ij} - I don't use this application	-4.4	-5.6	-4.4	-4.4	-3.1	-2.9
	(0.086)	(2.8)	(0.12)	(0.3)	(0.26)	(0.11)
a_{ij} - Communicate with my friends	-0.096	0.75	0.077	0.7	0.41	0.29
	(0.016)	(0.095)	(0.029)	(0.18)	(0.087)	(0.041)
h_t - EVENING	-0.59	-0.74	-0.0044	-0.55	-0.04	-0.42
	(0.023)	(0.046)	(0.026)	(0.048)	(0.038)	(0.03)
h_t - LATE NIGHT	-0.75	-1.1	-0.29	-0.53	-0.69	-0.64
	(0.02)	(0.047)	(0.022)	(0.042)	(0.042)	(0.029)
h_t - MORNING	-0.77	-1.1	-0.29	-0.43	-0.72	-0.53
	(0.019)	(0.048)	(0.024)	(0.048)	(0.03)	(0.028)
h_t - AFTERNOON	0.0	0.0	0.0	0.0	0.0	0.0
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
d_t - Sunday	0.048	0.0073	0.018	0.17	0.032	-0.044
	(0.018)	(0.047)	(0.034)	(0.063)	(0.046)	(0.035)
d_t - Monday	0.046	0.093	0.061	0.19	-0.081	0.0069
	(0.018)	(0.048)	(0.027)	(0.077)	(0.047)	(0.037)
d_t - Tuesday	0.031	0.11	0.039	0.16	-0.066	0.027
	(0.018)	(0.053)	(0.027)	(0.07)	(0.046)	(0.036)
d_t - Wednesday	0.081	0.097	0.04	0.17	-0.023	0.00086
	(0.023)	(0.05)	(0.031)	(0.068)	(0.042)	(0.034)
d_t - Thursday	0.069	0.064	0.076	0.1	-0.032	-0.018
	(0.019)	(0.045)	(0.032)	(0.065)	(0.042)	(0.036)
d_t - Friday	0.0	0.0	0.0	0.0	0.0	0.0
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
d_t - Saturday	-0.008	0.11	-0.038	0.062	-0.04	0.0074
	(0.02)	(0.057)	(0.028)	(0.062)	(0.05)	(0.036)
w_t - Week 1	0.029	0.064	-0.088	-0.0075	-0.15	0.02
	(0.017)	(0.055)	(0.027)	(0.057)	(0.048)	(0.033)
w_t - Week 2	-0.11	-0.0029	-0.12	-0.096	-0.0086	-0.043
	(0.015)	(0.052)	(0.022)	(0.054)	(0.045)	(0.026)
w_t - Week 3	-0.068	-0.0036	-0.14	-0.14	0.0023	-0.058
	(0.013)	(0.046)	(0.028)	(0.057)	(0.033)	(0.022)
w_t - Week 4	-0.018	-0.0021	-0.13	0.043	0.039	-0.12
	(0.015)	(0.036)	(0.024)	(0.05)	(0.042)	(0.023)
w_t - Week 5	0.0	0.0	0.0	0.0	0.0	0.0
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)

Notes: This table presents the estimated parameters of the demand model. The estimates for each type are presented in a separate column. Standard errors in parentheses are computed by 50 bootstrap samples.

Table A25: Model Validation

Application	Baseline (Predicted)	Baseline (Actual)	Instagram Restriction (Predicted)	Instagram Restriction (Actual)	YouTube Restriction (Predicted)	YouTube Restriction (Actual)
Outside Option	0.901	0.901	0.916	0.913	0.933	0.928
Facebook	0.0122	0.0118	0.0131	0.0145	0.0122	0.0127
Reddit	0.00651	0.00624	0.00912	0.00971	0.00712	0.00721
Snapchat	0.00671	0.00694	0.00859	0.00876	0.0062	0.00689
Twitter	0.0044	0.00447	0.00576	0.00569	0.00407	0.00455
TikTok	0.00796	0.00801	0.00843	0.0092	0.00759	0.0071
Instagram	0.0276	0.0278	-	-	0.0303	0.0335
YouTube	0.0339	0.034	0.0392	0.0386	-	-

Notes: Columns 1 and 2 compare the true market shares in week 1, 4, 5 to the predicted market shares from this model during this time period. Columns 3 and 4 compare the true to predicted market shares in the week 2 restriction period for the Instagram restriction group. Columns 5 and 6 compare the true to predicted market shares in the week 2 restriction period for the YouTube restriction group.

Table A26: Second-Choice Diversion Ratios (No Inertia)

	Instagram	Twitter	YouTube	TikTok	Reddit	Snapchat	Facebook	Outside Option
Instagram	-	0.0042	0.015	0.0037	0.0033	0.0053	0.0097	0.96
Twitter	0.016	-	0.015	0.0062	0.004	0.0053	0.0093	0.94
YouTube	0.015	0.0038	-	0.0036	0.0065	0.0044	0.0089	0.96
TikTok	0.016	0.0066	0.016	-	0.0036	0.0094	0.0096	0.94
Reddit	0.011	0.0031	0.02	0.0028	-	0.0039	0.0067	0.95
Snapchat	0.016	0.004	0.014	0.007	0.0038	-	0.0091	0.95
Facebook	0.016	0.0039	0.015	0.004	0.0036	0.005	-	0.95

Notes: This table displays the estimated second-choice diversion ratios that come from the estimated model with $\beta^{q(i)} = 0$. The cell in each row k and column j is computed by $D_{kj} = \frac{s_j(\mathcal{J}\setminus\{k\}) - s_j(\mathcal{J})}{s_k(\mathcal{J})}$.

Table A27: Percentage Change in Diversion Ratio (No Inertia)

	Instagram	Twitter	YouTube	TikTok	Reddit	Snapchat	Facebook	Outside Option
Instagram	–	−11%	−35%	−41%	14%	−26%	−26%	1.7%
Twitter	−39%	–	−40%	−53%	−2.5%	−10%	−22%	3.4%
YouTube	−37%	−13%	–	−49%	−20%	−19%	−22%	1.9%
TikTok	−42%	−28%	−47%	–	23%	−50%	−20%	4.4%
Reddit	−22%	2.1%	−42%	−17%	–	−26%	−15%	2.2%
Snapchat	−39%	9.7%	−35%	−61%	−19%	–	−20%	3.4%
Facebook	−39%	−6.9%	−35%	−35%	−2.5%	−21%	–	2.4%

Notes: This table presents the percentage change in the second-choice diversion ratios when $\beta^{q(i)} = 0$.

Table A28: Percentage Change in Market Share (No Inertia)

Instagram	Twitter	YouTube	TikTok	Reddit	Snapchat	Facebook
-46.2%	-17.3%	-43.7%	-51.3%	-26.9%	-39.3%	-30.0%

Notes: This table presents the percentage reduction in predicted average market share for the column application when $\beta^{q(i)} = 0$. The predicted average market share is computed over weeks $1, 4, 5$ of the experiment when all the participants faced no restrictions.

A.7 Upward Pricing Pressure Test Derivation

In this appendix, I provide details on the derivation of the Upward Pricing Pressure (UPP) test that is utilized in the main text. As is common practice in the literature, I derive the formulation directly from the profit function of the merged firm. Recall that the notation I utilize is as follows. $P_1(t_1(\boldsymbol{a}))$ denotes the price of advertising on application 1, which implicitly depends on $t_1(\boldsymbol{a})$ where t_1 denotes the time demand for application 1 and $\boldsymbol{a}$ is the vector of advertising quantities that are set by all of the applications. a_1 denotes the quantity of advertising served by application 1 and c_1 is the marginal cost of serving advertisements.

The pre-merger profit function for application 1 is as follows:

$$\pi_1 = \Big(P_1(t_1(\boldsymbol{a})) \cdot a_1 - c_1\Big) \cdot t_1(\boldsymbol{a})$$

I suppose that the pre-merger advertising quantity is set optimally so that it is characterized implicitly by the first-order condition, which pins down the profit maximizing advertising load before the merger:

$$\frac{d\pi_1}{da_1} = \frac{dP_1}{dt_1} \cdot \frac{\partial t_1}{\partial a_1} \cdot a_1 \cdot t_1(\boldsymbol{a}) + P_1 \cdot t_1(\boldsymbol{a}) + \frac{\partial t_1}{\partial a_1}\Big[a_1 \cdot P_1 - c_1\Big] = 0$$

I consider that the application 1 merges with application 2. This changes the profit function by incorporating the efficiency gains as a result of the merger. Following [63], I suppose that the primary efficiency gains for consumers come from increased application quality as a result of a merger as opposed to a marginal cost reduction. I consider that consumers face quality-adjusted advertising loads given by $\tilde{a_1} = a_1 - v_1$. Thus, the demand for application i is as follows: $t_i(a_1 - v_1, ..., a_N - v_N) \equiv t_i(\tilde{a}_1, ..., \tilde{a}_N)$. Before the merger $\tilde{a_1} = a_1$ so that the pre-merger levels of application quality are already captured in the demand functions. Combined these imply that the profit function of the merged firm is as follows:

$$\pi_1 + \pi_2 = \Big(P_1(t_1(\boldsymbol{a})) \cdot (a_1 + v_1) - c_1\Big) \cdot t_1(\boldsymbol{a}) + \Big(P_2(t_2(\boldsymbol{a})) \cdot (a_2 + v_2) - c_2\Big) \cdot t_2(\boldsymbol{a})$$

As is customary, I derive a UPP expression for each application individually. Indeed, as noted by [63], in this case it is sufficient to focus on characterizing whether there is upward pricing pressure for each application individually as if this is the case for one of the applications then it implies that the two applications jointly have upward pricing pressure as well. The UPP expression is typically derived by solving for the first-order condition of the profit function of the merged firm and characterizing the conditions under which this term is positive. If this is positive then there is “upward pressure" for the firm to increase (quality-adjusted) prices post-merger. Therefore without loss of generality I focus on the first-order condition for application 1.

$$\begin{aligned}\frac{\partial \pi_1}{\partial a_1} = & \underbrace{\frac{dP_1}{dt_1} \cdot \frac{\partial t_1}{\partial a_1} \cdot a_1 \cdot t_1(\boldsymbol{a}) + P_1 \cdot t_1(\boldsymbol{a}) + \frac{\partial t_1}{\partial a_1}\Big[a_1 \cdot P_1 - c_1\Big]}_{\text{=0 at pre-merger}} \\ & + \frac{dP_1}{dt_1} \cdot \frac{\partial t_1}{\partial a_1} \cdot v_1 \cdot t_1(\boldsymbol{a}) + \frac{\partial t_1}{\partial a_1} \cdot v_1 \cdot P_1 \\ & + \frac{\partial t_2}{\partial a_1} \cdot (P_2 \cdot (a_2 + v_2) - c_2) + \frac{dP_2}{dt_2} \cdot \frac{\partial t_2}{\partial a_1} \cdot t_2(\boldsymbol{a}) \cdot (a_2 + v_2) = 0\end{aligned}$$

The term noted in the brackets corresponds to the first-order condition of application 1 which, at the pre-merger advertising quantities, evaluates to 0. Recall that the diversion ratio is defined as $D_{12} \equiv -1 \cdot \frac{\frac{\partial t_2}{\partial a_1}}{\frac{\partial t_1}{\partial a_1}}$. I divide the remaining terms through by $-1 \cdot \frac{\partial t_1}{\partial a_1}$ and reorganize to obtain the following UPP expression:

$$UPP_1 = D_{12} \cdot \Bigg(\underbrace{P_2 \cdot (a_2 + \nu_2) - c_2}_{\text{Revenue from Diversion}} + \underbrace{\frac{dP_2}{dt_2} \cdot (a_2 + \nu_2) \cdot t_2(\boldsymbol{a})}_{\text{Price Change from Diversion}} \Bigg) - \underbrace{\Bigg(\nu_1 \cdot \Bigg(\frac{dP_1}{dt_1} \cdot t_1(\boldsymbol{a}) + P_1 \Bigg) \Bigg)}_{\text{Quality Efficiency Gains}}$$

A.8 Collection of Survey Responses

In this section are the responses to the optional question in the endline survey which asked the participants to describe in words how they responded to the restrictions.

- Addiction
  - I hated it while it happened, but it really broke the app's addictive nature.
  - I never realized that I am tsuch addicting to instagram until I found myself opened it absentmindedly several times during mY restrictions period. my usage time of ig has decreased from averagely 6.5 hrs before the restrictions to 3 hr in the first week, but bounce back to 7 hrs this week, even exceeding the number before.
  - It's strange, because it didn't feel like I needed YouTube, I just knew I had spent a lot of time on it. However, when it became restricted, I noticed how much time I had spent simply laying about and watching YouTube. It felt weird knowing that my instinct was to immediately press the YouTube button when I got bored, and I realized I perhaps need/use it more than I think.
  - It was crazy how addicted I am to these apps. During the restrictions, I kept accidentally trying to open the app -all the time. I didn't realize how much time I spent on them.
  - I kept opening instagram time after time forgetting that is was blocked
  - I had one restriction on Instagram and it was weird breaking the habit of accessing and took some getting used to avoiding the app
  - When the restriction started I got a feeling I was gonna be a little anxious. I was wrong.

- It was frustrating - did not know I was so addicted to YouTube
- I felt out of the loop so I often tried to access Instagram using my laptop.
- At first restricting instagram was frustrating as i had the application on my home screen and built muscle memory for the past 4 years to press that part of the screen where the instagram shortcut is. I removed instagram from my home screen and after 5 days of the restriction i completely realized instagram was nor important at all for me and only time i open it is when i receive a direct message.

• Shifted Towards Other Apps

  - It wasn't easy at first as I tried to access the restricted application about two different times but I received the restriction message from screen time app with a grin on my face....lol. I had to figure out what I want from other applications I didn't know offered similar content before time, after the restriction elapsed, I had adjusted to sourcing for such content on both apps.
  - Well at first after my YouTube was restricted, I thought I could access it using my browser but then i realised that was also impossible. I was like, how will I cope without streaming videos on YouTube? But after some time I adjusted and got used to it.
  - At the beginning i felt like damn this is an important application (Youtube) and what if i need it for anything Turns out i dont need it as much and there are other options available
  - Pre-COVID, I would listen to a lot of podcasts when driving, walking to class, etc. So when Youtube was restricted, I mostly just listened to more podcasts like I used to. I think I also probably watched more Youtube on my PC and smart TV during this period.
  - At the beginning it felt like something was missing but eventually I started using other apps and filled that vacancy

- I spent time on twitch watching streamers vs. Youtube where I had watched them before.
- I think the restriction gave me the opportunity to spend more time on other applications i had already installed but hardly use.
- I often use youtube for music on my phone when I don't want to pay for Spotify premium, but during the restriction period I ended up resubscribing to Spotify Premium for $5 so I could listen to music on my phone easily

- Realized Value of Application
  - It was a bit hard to adapt at first but I eventually got used to it.Eventually I realized I am better off without it so I ended up deleting it and till now am okay with my decision.
  - After the restriction I definitely started spending more time on the app that was restricted. I started to use the app more because I wanted to track local businesses which can be hard to discover by googling. I'm not sure if it was a coincidence that I developed an interest in small businesses and increased my app usage or if it was the restriction that caused me to appreciate what I could do on the app more.
  - I felt that I missed using it I realized I was spending to much time on the app
  - Struggling to access Instagram, but when there's no restrictions, i found that the content i wanna access previously is very trivial
  - I felt minorly inconvenienced since I could still access on my computer if it were an emergency like an insta dm I needed to respond to. Having time away from insta definitely helped me mentally.
  - Sometimes I misses to use but nothing as bad as I thought. Most of time I have not importante to do, Its just a way to spend time
  - I felt after restrictions that I need this application more and I can't take this restrictions for a long.

- YouTube was restricted, so it was a little difficult when my baby was having a meltdown in public, but it also wasn't as often as usual, thankfully. It was difficult also if I needed to learn something off of YouTube pertaining to my career like a how-to or new technique.

- Shifted Towards Non-Digital Activities
  - Honestly I spent more time outdoor and with friends.
  - I initially felt bored, since a common reflex I had was to open up Youtube whenever I had nothing to do. However, within a few days, I started doing other things instead, such as reading. It was actually a good experience.
  - At first it was difficult because YouTube is the most used app by me.Whatever it is YouTube is a go to for me in my daily life.After that I made up my mind to concentrate in different things and spent more time off the devices.I tried to concentrate more on my studies and spent time with my family.
  - I was surprised my youtube was restricted. For me its a big part of the content i consume and it is was hard to not have it on my phone. Initially I tried watching it on my computer but it was something i couldn't keep up all the time. Over time my useage dropped from watching a lot to, mainly watching when i am on my computer taking a small break (even then only watching the videos i really like and not wasting time on YT)

- Impact on Socializing
  - I realized I spent a lot of time on an app establishing really ineffective communication. I changed the way in which I communicate online.
  - I didn't think I used Instagram very much but the restriction turned out to be very annoying as friends would message me there and wonder why I wasn't responding

- I used Instagram to communicate with friends less frequently when it was restricted, but used WhatsApp more instead. These were reverted after restrictions were lifted
- I felt frustrated because I feel like I was missing out. I wasn't able to keep up with the people I followed on Instagram as much because the app was restricted
- I felt it was a very interesting experience. I don't feel like I have an addiction to certain applications and could probably live my life without it. The only limit I faced was that I could not contact certain people, who I only talk with on that application. But to be honest, I could live even without those conversations or certain people and would probably find other apps to contact them on. But I did not do that.
- Instagram was restricted for me and because I mainly use it as a communication app, I was not significantly affected. I just used regular text, video call, and Snapchat to keep up socially.
- It was a little annoying especially whenever my friend shared something that can only open on that platform. But after a couple of days I was able to make my peace with it
- I did a bit of communication on Instagram, so told the person I was chatting to to switch and that didn't really happen so it ended up reducing how much we messaged

Appendix B: Supplementary Material for The Effect of Privacy Regulation on the Data Industry: Empirical Evidence from GDPR

B.1 Additional Consumer Response Figures

Figure B1: Week by Week Treatment Effect (Cookies and Recorded Searches)

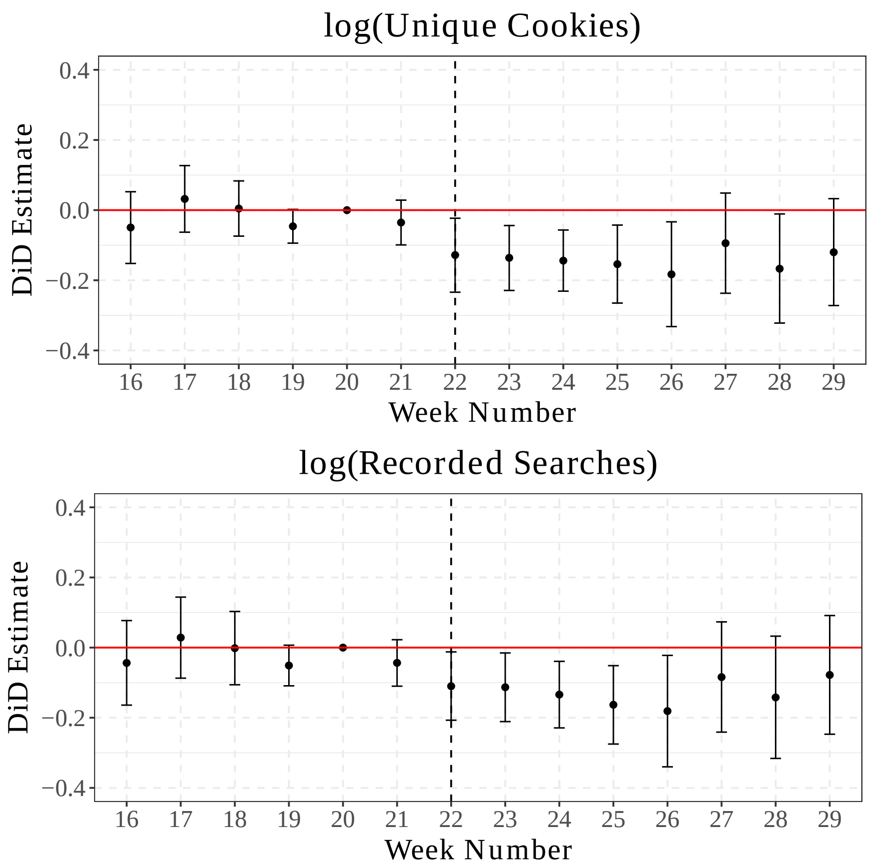


Table B1: Difference-in-Differences Estimates for Sales Activity

	(1) Total Pages	(2) Total Advertising Units
DiD Coefficient	-0.0387	0.0837
	(-0.58)	(1.11)
Product Category Controls	✓	✓
Week FE	✓	✓
Website × Country FE	✓	✓
Observations	3731	3731

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-product type-week level between weeks 16 and 29, including both weeks 16 and 29 (April 13th - July 20th). The dependent variable in the first regression is the total number of pages where the intermediary is present. The dependent variable in the second regression is the total number of advertising units associated with the intermediary.

Figure B2: Week by Week Treatment Effect (Consumer Persistence)

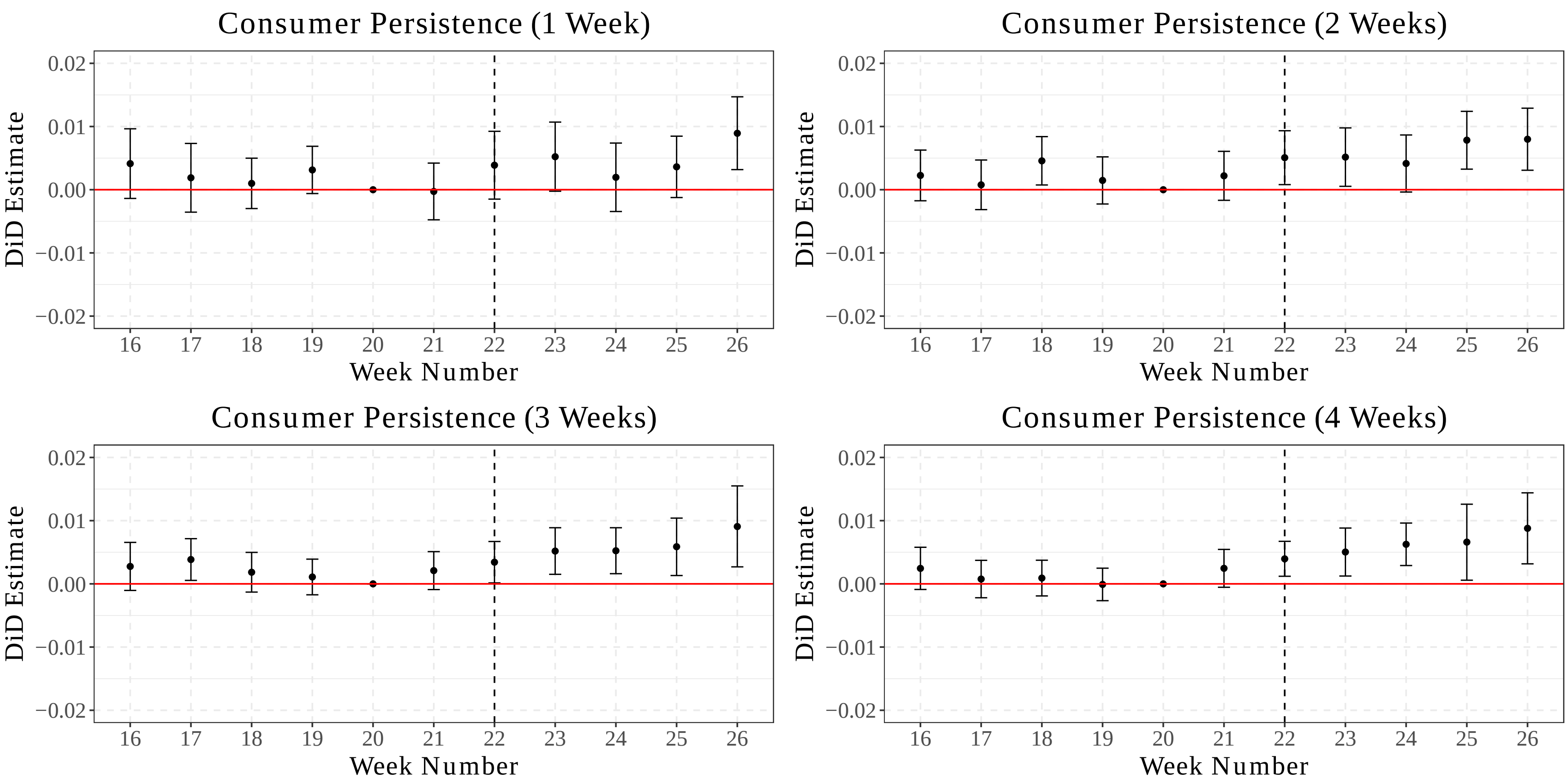


Figure B3: Distribution of Consumer Persistence (1 Week)

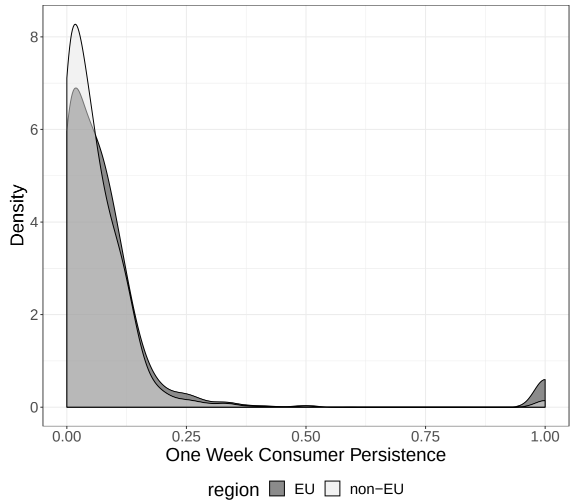


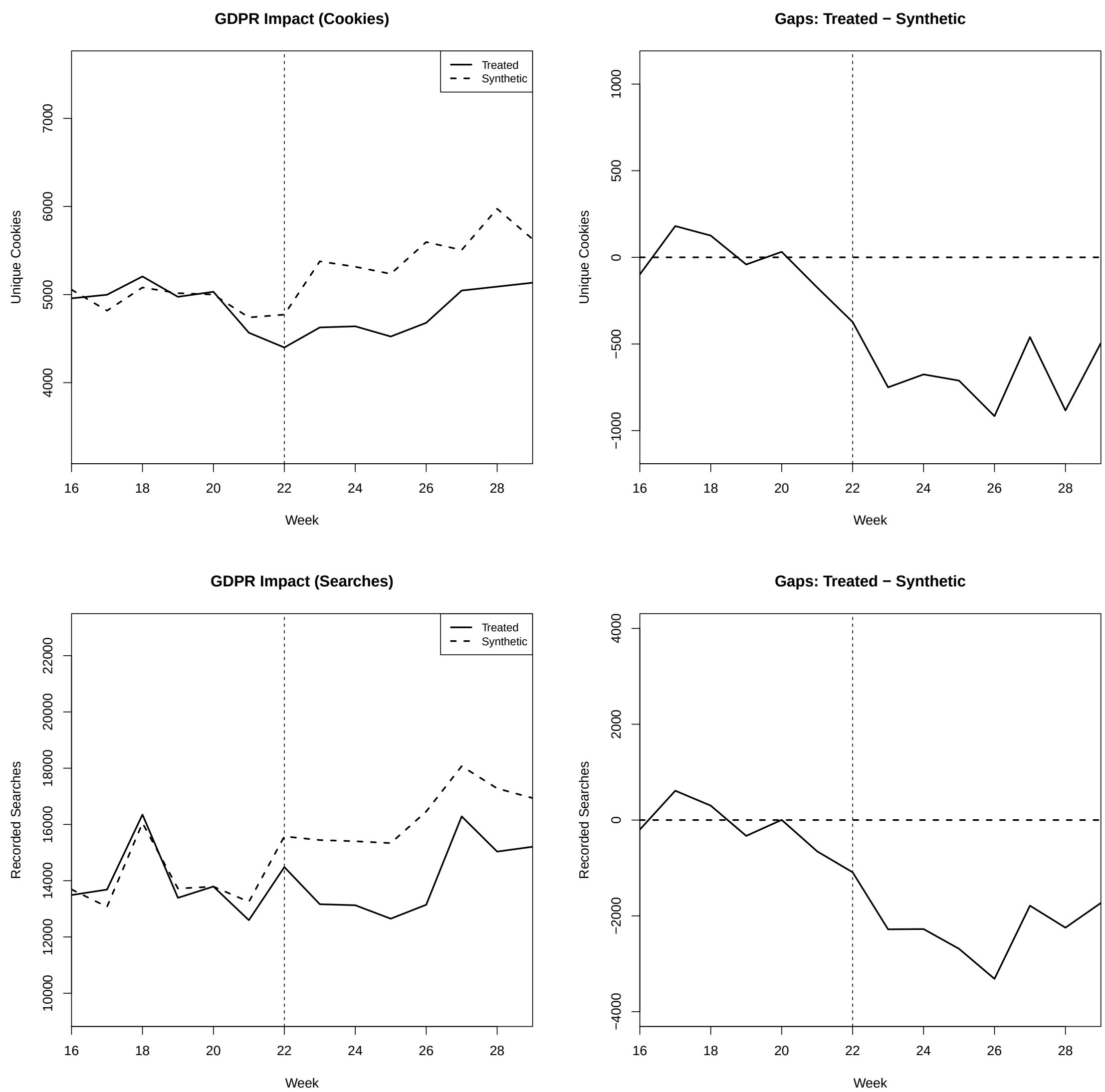
Table B2: Summary Statistics of Consumer Persistence

Treatment Group	**1 Week**	**2 Weeks**	**3 Weeks**	**4 Weeks**
non-EU	.0640	.0417	.0330	.0282
EU	.0962	.0730	.0644	.0597

Notes: The summary statistics are computed on the sample period before GDPR and show the mean consumer persistence values across the EU and the non-EU for $k = 1, 2, 3, 4$.

B.2 Robustness for Consumer Response Results

Figure B4: Synthetic Controls for Cookies and Recorded Searches



Notes: The plots in the leftmost column display the time series of the average treated unit and the constructed synthetic control for the number of unique cookies (top) and number of recorded searches (bottom). The plots in the rightmost column display the difference at every point in time between the averaged treated unit and the constructed synthetic control for the number of unique cookies (top) and number of recorded searches (bottom).

We provide additional evidence of robustness for our estimated effects on the usage of consent-based opt-out as a result of GDPR. subsection B.2.1 mimics the exercise in the main text, but utilizes a standard synthetic control based approach and recovers similar results as our difference-in-differences approach. subsection B.2.2 augments our analysis with Google Trends data and uses this to control for seasonality differences in travel patterns across the countries in our analysis.

B.2.1 Synthetic Controls

In order to provide additional validation for the difference-in-differences results in subsection 2.4.1, we supplement our primary analysis with a synthetic controls analysis, following [190] and utilizing the corresponding R package, `Synth`. We aggregate the data to the same level as we do in the primary analysis.[1] We expand the set of control countries beyond the United States, Canada, and Russia to include Argentina, Brazil, Australia, Japan, and Mexico in order to allow for additional flexibility in the design of the synthetic control group.[2] Thus, the travel websites in these countries serve as the possible donor pool for the construction of the synthetic control. In order to apply the synthetic control method to our data we construct a single average treated unit for each outcome variable from the set of treated units. The set of predictor variables that we utilize in order to fit the weights assigned to each control unit are the two outcome variables that we consider—the total number of searches and the total number of unique cookies observed. We fit the weights to match the outcome variable between weeks 16 and 21. The results of applying this method are reported in Figure B4. Qualitatively, the results match what we find utilizing the difference-in-differences analysis with a stark drop at the onset of GDPR with a small recovery nearing the end of our sample period.

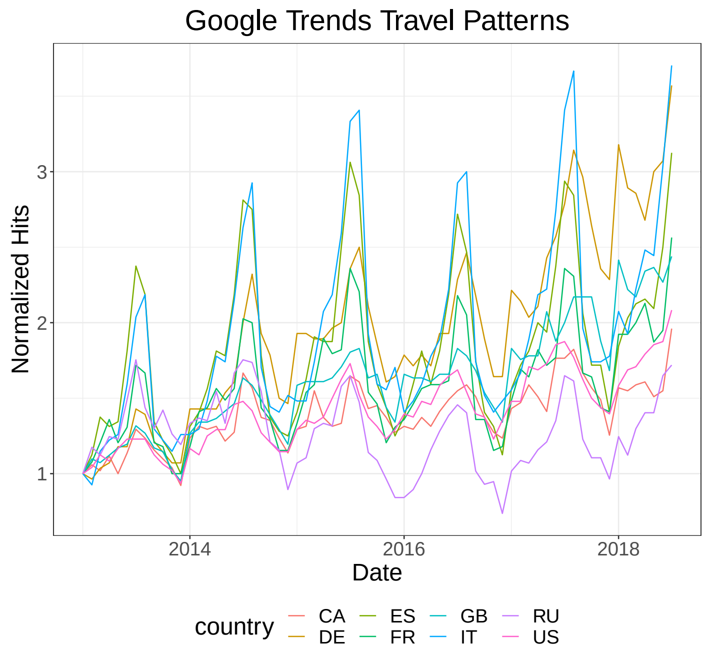
B.2.2 Controlling for Differences in Travel Patterns Across Countries

Since our paper tries to understand the impact of privacy regulations utilizing data from the online travel industry, a potential concern is that differential seasonality trends in travel across

[1] We also do this exercise aggregating at the website-country level and find qualitatively similar results. One might argue for this aggregation since the way we utilize the synthetic control method involves collapsing all treated units into a single average treated unit and it seems more natural to do so at the website-country level so that the synthetic control represents a synthetic European website. However, this makes the comparison of estimated treatment effects to the primary specification more difficult since the underlying units are different.

[2] Our results are nearly identical if we use the same set of control countries as we do in the baseline difference-in-differences specification, but use the larger set of countries due to the flexibility of the synthetic control method which makes this a special case of the reported exercise.

Figure B5: Historical Google Trends Travel Patterns



Notes: The graph is constructed by pulling Google Trends data for keyword "booking" for the time period ranging from 1/1/2013 - 7/31/2018. We pull the data for each country separately. We further normalize the score returned from Google Trends by dividing by the first observation for each country in order to ease cross-country comparisons.

countries may influence the results. We selected the set of control countries in our analysis specifically to have similar seasonal travel patterns as the major EU countries impacted by GDPR in a short period around the GDPR implementation date. To further validate this we make use of data from Google Trends. The Google Trends data is useful since it provides an estimate of similar quantities observed in our data, but without the possibility that data can be removed as a result of GDPR. We first plot the relative trends over time of a common travel keyword and provide evidence that the travel trends are relatively similar in the period that we study. If anything, such trends seem to cause our estimates to understate the treatment effects of GDPR. We then make use of the historical data from Google Trends to better control for seasonal patterns and investigate the impact of these controls on our estimates of the change in total recorded searches and unique cookies.

According to the Google Trends documentation, their data is constructed by a representative sample of searches done through Google Search. Instead of reporting the raw number of searches, Google Trends reports a normalized score that is constructed by dividing the number of searches for the keyword by the total searches of the selected geography and time range. The resulting number is scaled on a range of 0 to 100 based on the topic's proportion to all searches.[3]

Table B3: Difference-in-Differences Estimates With Google Trends controls

	(1) log(Unique Cookies)	(2) Unique Cookies	(3) log(Recorded Searches)	(4) Recorded Searches
DiD Coefficient	-0.129**	-1373.1*	-0.113**	-9555.9**
	(-2.52)	(-1.75)	(-1.98)	(-2.25)
Google Trends Seasonality Controls	✓	✓	✓	✓
OS + Browser Controls	✓	✓	✓	✓
Product Category Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Website × Country FE	✓	✓	✓	✓
Observations	63840	63840	63840	63840

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 26, including both weeks 16 and 26 (April 13th - June 29th). These regressions are identical to those presented in the main text, but with the addition of Google Trends data in order to control for potential differences in seasonal travel patterns across the countries in our analysis.

Given this data construction, in order to compare the relative intensity of travel queries across countries we pull the data for each country and keyword individually. The first important detail is

[3] `https://support.google.com/trends/answer/4365533` provides additional details.

that Google Trends aggregates across specific strings and not terms, which means that when we do cross country comparisons we have to be careful about the precise keyword we utilize. In order to overcome this difficulty, we use the term of a common and popular OTA across all the countries in our analysis: booking. Figure B5 plots the results from Google Trends for the trends for this keyword from January 1st, 2013 until July 31st, 2018. Figure B5 shows that the keyword appears to pick up the seasonal trends we would expect across the different countries as well as that these appear to be similar across this set of countries, especially in the periods of our analysis.

We now consider the same specification as in (2.1), but make use of the Google Trends data to additionally construct controls for seasonal travel trends. We run the following regression in order to construct these controls, using the daily Google Trends data from 2013-2018:[4]

$$google_{ct} = \chi\left[week \times country\right] + \epsilon_{ct} \tag{B.1}$$

where as in the main specification, t denotes week and c denotes country. We then take $\hat{\chi}$ and add into our primary specification:

$$y_{tcjobp} = \alpha_t + \hat{\chi}_{tc} + \delta_{jc} + \gamma_o + \zeta_b + \omega_p + \beta(EU_j \times after) + \epsilon_{tcjobp} \tag{B.2}$$

where the notation is identical to that utilized in the main text and $\hat{\chi}_{tc}$ denotes the coefficient on $week \times country$ that comes from from running (B.1). The regression results are reported in Table B3 and are qualitatively consistent with the results from our main specification.

B.3 Consumer Persistence Heterogeneous Treatment Effects

We further investigate the mechanisms behind the increased consumer persistence by estimating heterogeneous treatment effects across web browsers and operating systems. We exploit the fact that different browsers and operating systems attract different types of individuals with dif-

[4]For this analysis we aggregate the daily Google Trends normalized scores to a weekly level. We define a week in an identical manner as in the primary analysis, from Friday-to-Friday, and take the average normalized score over the week in order to construct this data.

ferent levels of technical sophistication as well as provide different levels of privacy protection. This exercise provides additional evidence to disentangle the selective consent and privacy means substitution hypotheses since the selective consent hypothesis would predict that there should be no heterogeneity in persistence across these dimensions whereas the privacy means substitution hypothesis would predict the opposite.

First, we study heterogeneous treatment effects across web browsers and restrict attention to the most popular web browsers: Google Chrome, Microsoft Edge, Mozilla Firefox, Internet Explorer, Opera, and Apple Safari. We consider the following specification:

$$y_{tcjobp} = \alpha_t + \delta_{jc} + \gamma_o + \zeta_b + \omega_p + \beta(EU_j \times after \times browser) + \epsilon_{tcjobp} \tag{B.3}$$

There are two dimensions on which we could think that the differential change in persistence would vary across web browsers. The first is that there is a demographic selection into browsers and the ability to substitute between various privacy means requires technical sophistication (i.e. consumers need to know how to manage cookies). For instance, Internet Explorer (IE) is a web browser primarily used on older computers and is known to attract older, less technologically sophisticated, users. Thus, the privacy means substitution hypothesis seems more plausible if the effects are stronger on browsers with more technologically sophisticated consumers. The second is that there are different levels of privacy protection among browsers. For instance, Apple Safari at the time of the GDPR had a broad set of privacy protection means built into it, whereas Google Chrome had laxer privacy controls.[5] The lack of JavaScript extensions on Internet Explorer makes cookie blockers substantially more difficult to implement on the browser and thus we would expect less “single searchers" and less usage of browser-based privacy means due to the relative lack of automated means of doing so.[6] In sum, in order to be consistent with the privacy means substitution hypothesis we would expect a smaller increase in persistence on Internet Explorer and Safari

[5] Safari also is the default browser on OS X and so one would expect users to potentially be less technically sophisticated than those that make use of non-default browsers.

[6] See, for instance, `https://help.getadblock.com/support/solutions/articles/6000055833-is-adblock-available-for-internet-explorer-`.

relative to the other browsers.

Table B5 displays the regression results for this specification with Chrome as the omitted browser. The treatment effect is consistent across browsers with the exception of Internet Explorer which has almost no change in persistence, consistent with our hypothesis. The estimated treatment effect is lower in Safari relative to Chrome, but the difference is not statistically significant. Both of these observations are consistent with the privacy means hypothesis.

Next, we study heterogeneous treatment effects across operating systems and narrow down the sample to only look at the most popular operating systems: Android, Chrome OS, iOS, Linux, Mac OS X, and Windows. We consider the following specification:

$$y_{tcjobp} = \alpha_t + \delta_{jc} + \gamma_o + \zeta_b + \omega_p + \beta(EU_j \times after \times OS) + \epsilon_{tcjobp} \tag{B.4}$$

We are mainly interested in differences in the treatment effects between mobile and desktop consumers. The reason is that there are less readily available privacy means for cookie management on the mobile web compared to desktop and consumer behavior in general tends to be different on mobile compared to desktop. For consistency with the privacy means substitution hypothesis, we would expect a larger difference in persistence on desktop compared to mobile whereas for consistency with the selective consent hypothesis we should expect a smaller difference.

Table B4 displays the regression results with Windows as the omitted operating system that indicates that Android and iOS have no or weak increases in persistence for $k = 1, 2$ but appear to have an increase in persistence for $k = 3, 4$. Otherwise, the treatment effect is approximately the same across the different operating systems. Since there seems to be a weak difference between persistence on mobile and desktop this appears to be suggestive of the privacy means substitution hypothesis, but does not provide conclusive evidence.

Table B4: Consumer Persistence by Week - OS Heterogeneous Treatment Effects

	(1) 1 Week	(2) 2 Weeks	(3) 3 Weeks	(4) 4 Weeks
Treated	0.00603***	0.00462***	0.00460***	0.00476***
	(2.70)	(2.76)	(2.65)	(2.91)
Treated × (OS =ANDROID)	-0.00886***	-0.00429*	-0.00256	0.000311
	(-3.19)	(-1.96)	(-1.26)	(0.17)
Treated × (OS = CHROME_OS)	-0.00384	-0.00592	-0.00593	0.00176
	(-0.67)	(-1.24)	(-1.44)	(0.52)
Treated × (OS =iOS)	-0.00367	-0.00184	0.000438	0.00132
	(-1.29)	(-0.77)	(0.19)	(0.70)
Treated × (OS = LINUX)	-0.000856	0.00326	-0.000188	0.000463
	(-0.18)	(0.77)	(-0.06)	(0.12)
Treated × (OS = MAC_OS_X)	-0.00291	-0.000367	-0.00209	-0.00184
	(-1.08)	(-0.19)	(-1.26)	(-1.10)
OS = ANDROID	0.0105***	0.00565**	0.00335	0.00296
	(3.56)	(2.01)	(1.20)	(1.18)
OS = CHROME_OS	0.00307	0.00221	-0.000749	-0.00117
	(0.89)	(0.59)	(-0.27)	(-0.45)
OS = iOS	0.00712***	0.000500	-0.0000303	-0.0000989
	(2.66)	(0.22)	(-0.01)	(-0.05)
OS = LINUX	-0.0164***	-0.0119***	-0.0105***	-0.00732***
	(-4.37)	(-3.46)	(-4.17)	(-2.87)
OS = MAC_OS_X	-0.000548	-0.00115	-0.00299*	-0.00297***
	(-0.24)	(-0.58)	(-1.96)	(-2.68)
Constant	0.0835***	0.0619***	0.0557***	0.0497***
	(33.88)	(29.13)	(31.75)	(29.66)
Product Type Controls	✓	✓	✓	✓
OS × Week, OS × EU Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Website × Country FE	✓	✓	✓	✓
Browser Controls	✓	✓	✓	✓
Observations	48301	48301	48301	48301

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 26, including both weeks 16 and 26 (April 13th - June 29th). We restrict focus only to the most popular operating systems. The dependent variables in the regression are the consumer persistence measures for $k = 1, 2, 3, 4$, respectively. $treated$ indicates whether the observation is associated with an EU website and past the GDPR implementation date. $treated \times os$ indicates the heterogeneous treatment effect for the specified os. The coefficients on os indicate the estimated values for the os fixed effect. The held-out operating system is Windows.

Table B5: Consumer Persistence - Browser Heterogeneous Treatment Effects

	(1) 1 Week	(2) 2 Weeks	(3) 3 Weeks	(4) 4 Weeks
Treated	0.00615***	0.00645***	0.00519***	0.00628***
	(2.99)	(3.51)	(3.29)	(3.49)
Treated × (Browser = EDGE)	-0.00134	-0.00169	0.00230	0.000132
	(-0.35)	(-0.61)	(0.74)	(0.04)
Treated × (Browser = FIREFOX)	-0.00413	-0.00214	-0.00260	-0.00166
	(-1.60)	(-0.89)	(-1.43)	(-0.84)
Treated × (Browser = IE)	-0.0101**	-0.00838***	-0.00375	-0.00497**
	(-2.53)	(-2.67)	(-1.54)	(-2.03)
Treated × (Browser = OPERA)	-0.00935*	-0.00396	-0.00344	-0.00335
	(-1.95)	(-0.83)	(-0.94)	(-0.86)
Treated × (Browser = SAFARI)	-0.00185	-0.00332	-0.00280	-0.00225
	(-0.69)	(-1.43)	(-1.44)	(-1.12)
Browser = EDGE	0.00125	-0.00226	-0.00144	-0.000568
	(0.36)	(-0.78)	(-0.42)	(-0.18)
Browser = FIREFOX	-0.00503**	-0.00381*	-0.00465***	-0.00409***
	(-2.29)	(-1.96)	(-3.13)	(-2.92)
Browser = IE	-0.0164***	-0.0113***	-0.00801***	-0.00764***
	(-6.73)	(-5.15)	(-3.29)	(-4.18)
Browser = OPERA	-0.00151	-0.00337	-0.00665**	-0.00596**
	(-0.39)	(-1.00)	(-2.22)	(-2.15)
Browser = SAFARI	-0.00315	-0.00229	-0.00309*	-0.00211
	(-1.22)	(-1.06)	(-1.80)	(-1.20)
Constant	0.0861***	0.0647***	0.0575***	0.0568***
	(32.30)	(29.30)	(34.48)	(12.53)
Product Type Controls	✓	✓	✓	✓
OS × Week, OS × EU Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Website × Country FE	✓	✓	✓	✓
OS Controls	✓	✓	✓	✓
Observations	40810	40810	40810	40810

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 26, including both weeks 16 and 26 (April 13th - June 29th). We restrict focus only to the most popular web browsers. The dependent variables in the regression are the consumer persistence measures for $k = 1, 2, 3, 4$, respectively. $treated$ indicates whether the observation is associated with an EU website and past the GDPR implementation date. $treated \times browser$ indicates the heterogeneous treatment effect for the specified $browser$. The coefficients on $browser$ indicate the estimated values for the $browser$ fixed effect. The held-out browser is Google Chrome.

B.4 Additional Evidence for "Single Searcher" Inflation

In this section we more formally investigate the "single searcher" observation from subsection 2.4.2. Our main objective is to determine statistically whether there is an excess amount of single searchers, which is consistent with the privacy means substitution hypothesis. The test results in this section provide evidence for the following two claims. First, consumers were making use of such privacy means prior to the introduction of GDPR. Second, the fraction of consumers doing so has decreased after GDPR is introduced.

B.4.1 Setup and Hypotheses

We first describe a simple model that motivates our empirical exercise. Suppose that there are two types of consumers – obfuscators (o) and non-obfuscators (n) – and that each type of consumer generates an observed search history length of k. Each consumer has a history of length $k \geq 1$. The distribution of observed history length for the obfuscation consumer type o is degenerate with probability mass 1 at $k = 1$. For the obfuscation type o, we hypothesize that their observed history length is $k = 1$ with probability one. For the non-obfuscation consumer type n, the probability of observing search history length $k \geq 1$, conditional on observing a consumer, is denoted by $Q(k; \theta(x))$, where θ contains the relevant parameters of probability distribution Q and x denotes a set of observable consumer characteristics. In our setting the lowest count is one, which is why we subtract one from each observation to map it into a standard count model. We denote the fraction of consumers that are obfuscators conditional on observable characteristics x as

$$\pi(x) := \Pr\{\text{obfuscator} \mid x\}$$

This setup maps into the following observed share of visits S_k, where k denotes history length:

$$S_1 = \pi(x) + (1 - \pi(x)) \cdot Q(1; \theta(x)) \tag{B.5}$$

$$S_k = (1 - \pi(x)) \cdot Q(k; \theta(x)), \quad \forall k \geq 1 \tag{B.6}$$

We note that given this set-up π and θ are identified and that we can separately estimate π and θ for the pre-GDPR and post-GDPR period, giving us estimates for $\hat{\pi}^{PRE}, \hat{\pi}^{POST}, \hat{\theta}^{PRE}, \hat{\theta}^{POST}$. Given this setup, our informal hypotheses can be stated as the following null hypotheses:

1. $H_0 : \hat{\pi}^{PRE} = 0, H_a : \hat{\pi}^{PRE} \neq 0$
2. $H_0 : \hat{\pi}^{POST} = \hat{\pi}^{PRE}, H_a : \hat{\pi}^{POST} \neq \hat{\pi}^{PRE}$

B.4.2 Data and Estimation

For this exercise we restrict attention to the same large hotel website shown in Figure 2.5 which exhibited the noticeable change in “single searchers" at the onset of GDPR). We measure how many searches are associated with each identifier observed before and after GDPR is introduced. In total, we observe more than three million unique identifiers.

We allow the parameters of the model to depend on both the web browser and the operating system. Thus, we allow both the arrival rates and the fraction of obfuscators to vary across these dimensions. Next, we parameterize $\pi(x)$ as follows:

$$\pi(x) = \Big[\exp(x'\gamma)\Big] / \Big[1 + \exp(x'\gamma)\Big],$$

where γ is a parameter to be estimated. We consider two possible distributional assumptions for Q: a Poisson distribution and a negative binomial distribution, where the latter allows for additional dispersion. For the Poisson distribution we allow the arrival rate $\lambda(x)$ to vary across observables and we do similarly for the negative binomial parameters $\mu(x)$, $\alpha(x)$.[7]

Our setup maps almost directly to standard zero-inflation Poisson models (e.g. [192]. We follow [192, 191] and estimate the parameters of the model via maximum likelihood estimation. The model with a positive share of obfuscators is tested against either a standard Poisson regression or a negative binomial regression. We then conduct a Vuong test [107] to evaluate whether a model

[7] See section 20.4.1 of [191] for full details of the parameterization for Poisson and Negative Binomial regressions that we utilize.

with type o consumers leads to a better fit to the observed data [193]. In order to test our second hypothesis of interest, we do a t-test comparing the vectors of $\hat{\pi}^{PRE}$ and $\hat{\pi}^{POST}$.

B.4.3 Results

We first consider the specification where we assume that Q follows a Poisson distribution. The results of the Vuong test strongly conclude that there is evidence for the existence of type o consumers in both periods with a z-statistic of -244.85 in the pre-GDPR period and -246.28 in the post-GDPR period.

We then compare the resulting $\hat{\pi}$ in the pre-GDPR and post-GDPR periods, denoted by $\hat{\pi}^{PRE}$ and $\hat{\pi}^{POST}$, respectively. We run a t-test with the null hypothesis that $\hat{\pi}^{POST} = \hat{\pi}^{PRE}$. We are able to reject the null with $p < 2.2e - 16$. The difference is also economically significant as we note that $\overline{\hat{\pi}^{PRE}} = 0.478$ and $\overline{\hat{\pi}^{POST}} = 0.354$, suggesting a significant drop of obfuscators after GDPR.

One concern with the parameterization of Q as Poisson is that it does not account for overdispersion or underdispersion. We can directly test for overdispersion. Let Y_i denote the observed history length for consumer i and $\hat{\lambda}_i$ the implied variance of the poisson distribution. One can then test the null that $\alpha = 0$ for $\texttt{VAR}(Y_i) = \hat{\lambda}_i + \alpha \cdot \hat{\lambda}_i$ against the alternative that α is larger than zero [194]. We reject the null ($p < 2.2e - 16$) in both the pre and post period. Thus, we conclude that the data is overdispersed and consider the common remedy that imposes that Q follows a negative binomial distribution, instead of a Poisson distribution [191].

Under the the assumption of a negative binomial, the Vuong test still concludes that there is evidence for the presence of type o consumers ($z = -6.97$) in the pre-GDPR period. However, we no longer reject the model without excess single searchers in the post-GDPR period ($z = -1.81$,AIC-corrected: $z = -0.84$, BIC-corrected: $z = 4.94$). Furthermore, we are able again to reject the null hypothesis that $\hat{\pi}^{POST} = \hat{\pi}^{PRE}$ with $p < 2.2e - 16$.

In sum, we document statistical evidence for excess “single-searchers" in the pre-GDPR period under both distributional assumptions. Once we take into account the overdispersion relative to a Poisson count model, we do not find evidence for excess single searchers in the post-GDPR period.

B.5 Additional Advertisement and Auction Figures

Table B6: Difference-in-Differences Estimates for Advertisements Delivered

	(1) Total Advertisements Delivered	(2) asinh(Total Advertisements Delivered)
DiD Coefficient	-2627.2	-0.145
	(-1.61)	(-1.52)
OS + Browser Controls	✓	✓
Product Category Controls	✓	✓
Week FE	✓	✓
Website × Country FE	✓	✓
Observations	62328	62328

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 29, including both weeks 16 and 29 (April 13th - July 20th). The dependent variables are the log and overall level of total advertisements delivered to consumers.

Figure B6: Week by Week Treatment Effect (Total Advertisements Delivered)

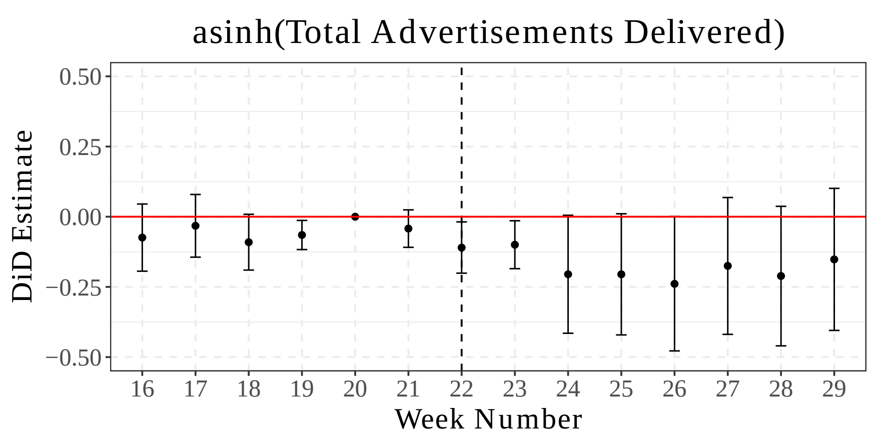


Table B7: Summary Statistics, Bids

Treatment Group	Average Bid
non-EU	394.053
EU	126.947

Notes: The table reports the mean of the average bid across observations in the pre-GDPR time period for the EU and non-EU respectively.

B.6 Prediction Evaluation Measures

B.6.1 AUC Primer

In this section we provide additional details on how to calculate the AUC measure and its interpretation. To begin, fix the classification threshold at any $\hat{P}$. Then, a consumer with score $\hat{p}_{ijk}$ is classified as a purchaser if $\hat{p}_{ijk} > \hat{P}$ and a non-purchaser if $\hat{p}_{ijk} < \hat{P}$. This would result in a false positive rate—a rate at which a non-purchaser is misclassified into a purchaser:

$$FPR := \frac{\text{False Positive}}{\text{True Negative} + \text{False Positive}} = \frac{\sum_{ijk} |\{\hat{p}_{ijk} > \hat{P}, y_{ijk}^{TRUE} = 0\}|}{\sum_{ijk} |\{y_{ijk}^{TRUE} = 0\}|}.$$

At the same time, it would result in a true positive rate—or a rate at which a purchaser is correctly classified as a purchaser:

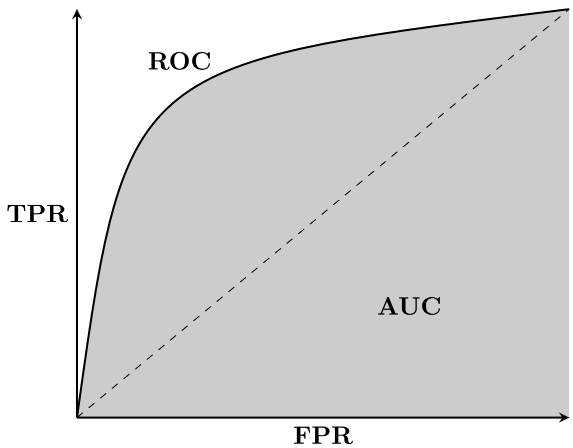
$$TPR := \frac{\text{True Positive}}{\text{True Positive} + \text{False Negative}} = \frac{\sum_{ijk} |\{\hat{p}_{ijk} > \hat{P}, y_{ijk}^{TRUE} = 1\}|}{\sum_{ijk} |\{y_{ijk}^{TRUE} = 1\}|}.$$

The ROC then depicts the level of TPR a prediction machine achieves for each level of FPR it tolerates.

The ROC is obtained by tracing the locus of (FPR, TPR) by varying the classification threshold $\hat{P}$.[8] The slope of the ROC corresponds to the additional power (in rate) the prediction gains

[8]For extreme cases, with $\hat{P} = 1$, all consumers are classified as non-purchasers, which yields $(FPR, TPR) =$

Figure B7: Sample ROC Curve



Notes: This figure depicts an ROC curve, which maps out the trade-off between type I and type II errors for a classifier as the classification threshold varies. The area under the ROC curve is denoted by AUC and provides a scalar measure of prediction performance.

for an additional unit of type I error (in rate) it tolerates. For a random predictor, this slope would be one, and the ROC will be a 45 degrees line. A better than random predictor would produce an ROC which lies above that 45 degrees line. Figure B7 depicts a typical ROC curve.

B.6.2 Breakdown of MSE

In this section we further investigate the cause of the increase in MSE in our difference-in-differences analysis in section 2.6. In order to do so we utilize a standard decomposition for the MSE in the classification context and study the effects of GDPR on each component of the decomposition. The MSE for binary classification problems can be decomposed into a *calibration* and *refinement* component [195]. The *calibration* component indicates the degree to which the estimated probabilities match the true class proportion. The *refinement* component indicates the usefulness of the prediction where a more refined prediction is one that is closer to certainty (i.e. closer to 0 or 1 with 0.5 being the most uncertain). Thus, a classifier with a good MSE is well-calibrated and more refined. This decomposition requires a discretization of the estimated

$(0,0)$, and with $\hat{P} = 0$ all consumers are classified as purchasers, which yields $(FPR, TPR) = (1,1)$.

probabilities into a series of K bins.[9] For notation, p_k denotes the kth estimated probability bin, n_k denotes the number of probability estimates falling into the kth bin and $\bar{o}_k$ denotes the true class proportion in the kth bin in the data. This allows us to rewrite (2.4) as:

$$MSE_j = \underbrace{\frac{1}{\sum\limits_{i \in \mathcal{I}_j} |\mathcal{K}_{ij}|} \sum_{k=1}^{K} n_k (p_k - \bar{o}_k)^2}_{\text{calibration error}} + \underbrace{\frac{1}{\sum\limits_{i \in \mathcal{I}_j} |\mathcal{K}_{ij}|} \sum_{k=1}^{K} n_k \bar{o}_k (1 - \bar{o}_k)}_{\text{refinement error}} \tag{B.7}$$

Table B8: Difference-in-Differences Estimates for Relevance and Calibration

	(1) Calibration	(2) Refinement
DiD Coefficient	0.00735***	0.00576**
	(2.84)	(2.64)
OS + Browser Controls	✓	✓
Product Category Controls	✓	✓
Week FE	✓	✓
Website × Country FE	✓	✓
Observations	15470	15470

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-browser-OS-product type-week level between weeks 16 and 29, including both weeks 16 and 29 (April 13th - July 20th). The dependent variable in the regression reported in the first column is the calibration component of the MSE. The dependent variable in the regression reported in the second column is the refinement component of the MSE.

We run the same specification utilizing each component of the decomposition of the MSE as the outcome variable. These results are reported in Table B8. They indicate that both the refinement and calibration components increased after GDPR. Both of the components are approximately equally responsible for the increase in MSE with the calibration component being only slightly

[9] Throughout this paper, when calculating the decomposed MSE we will primarily utilize equally spaced bins of size 0.01. Note that since the decomposition requires this discretization, the decomposed MSE and the standard MSE are not precisely the same quantities but are approximately the same.

larger. The increase in calibration error is driven by the classifier's lack of rapid adjustment to the post-GDPR consumer distribution leading the estimated class probabilities to no longer as closely match the empirical class probabilities. However, the increase in refinement error points to a partial adjustment since this increase is a result of the increased uncertainty in the predicted class (i.e. the class proportion moving closer to 0.5).

B.7 Additional Prediction Figures

Figure B8: Week by Week Treatment Effect (Average Predicted Probability and Class Proportion)

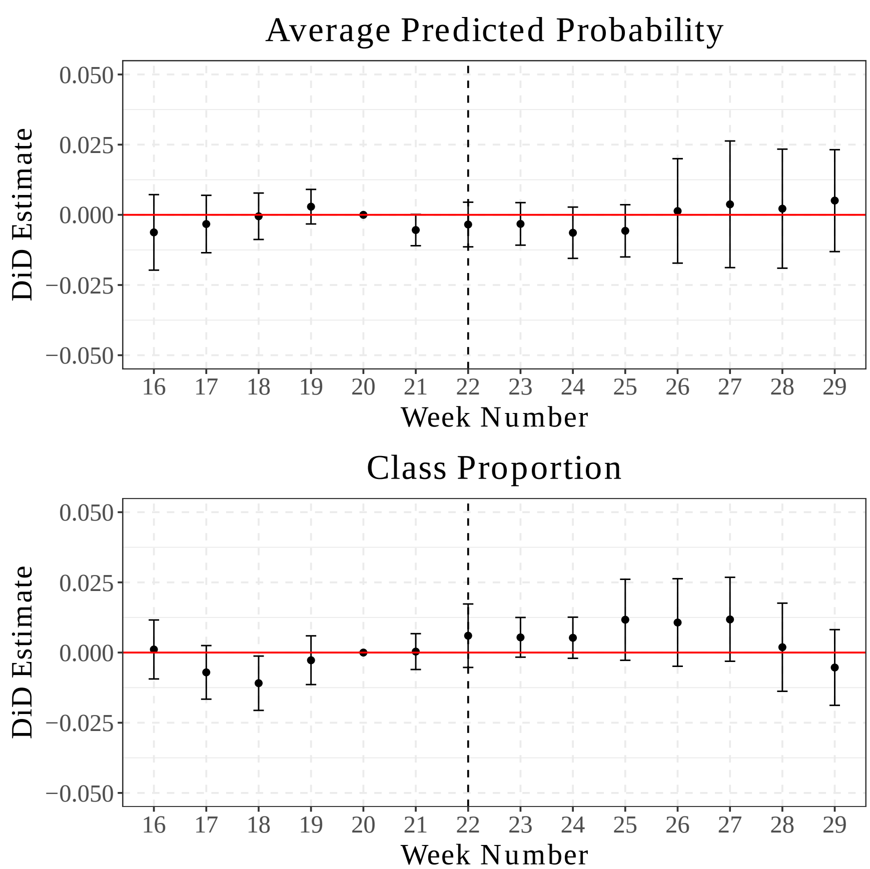
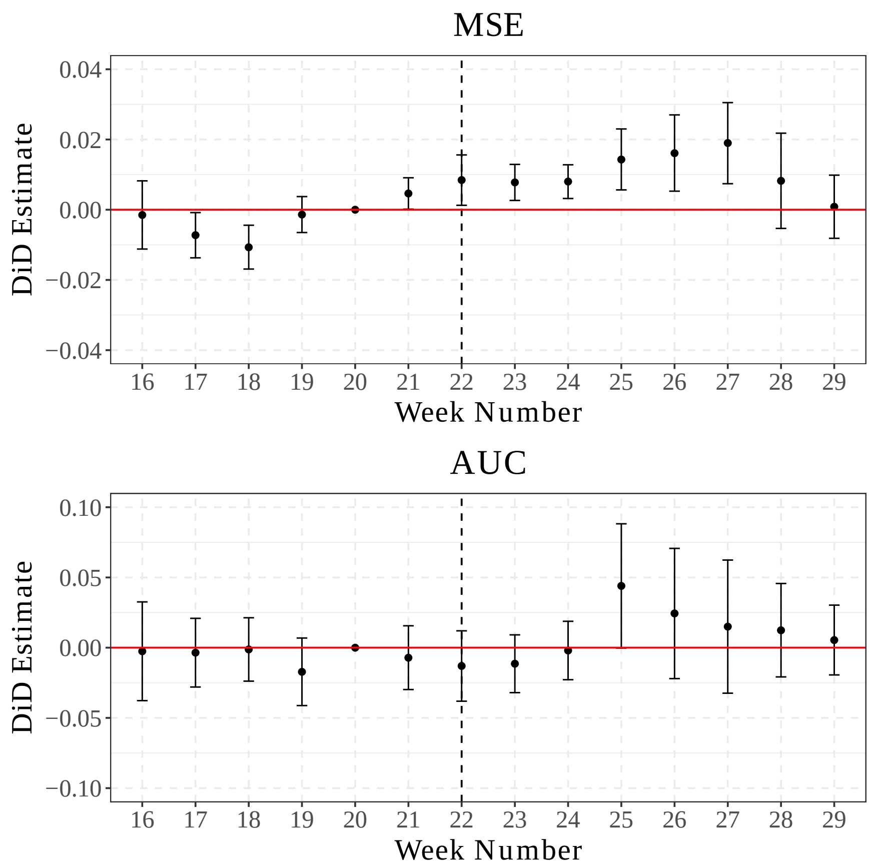


Figure B9: Week by Week Treatment Effect (MSE and AUC)



B.8 The Impact of Consumer Persistence and Data Scale on Prediction

The analysis in section 2.6 on the effect of GDPR on the firm's ability to predict is limited by the data restrictions and the apparent lack of adjustment by its prediction algorithm to the post-GDPR environment. To fully understand the implications for prediction, therefore, we now take a different approach. Instead of asking how the firm's prediction was actually impacted in the immediate aftermath, we now ask what would happen to predictive performance in the long run when the algorithm were fully adjusted.

As observed in section 2.4, GDPR reduces the number of consumers that the intermediary observes but remaining consumers are more persistently trackable. Our approach is to study how these two features—number of observed consumers and the persistence of observed consumers—

impact the two measures of prediction performance cross-sectionally by comparing across websites differing in these two dimensions. We use a dataset aggregated at the website-product type-week level. We restrict attention to the pre-GDPR period between January 19th and April 6th. We rely again on the fact that the intermediary only utilizes the data from each individual website in order to train the model for that website. This ensures that predictions for each website are only responsive to the data size and persistence of that website.

We run the following regressions where the dependent variable, $pred_{tcjp}$ represents the prediction error of website j in country c for product type p at time t. The fixed effects are the same as in the primary empirical specification and the standard errors are clustered at the website-country level, the same as with the previous specifications:

$$pred_{tcjp} = \beta \cdot \log(Recorded\_Searches) + \alpha_t + \delta_{jc} + \omega_p + \epsilon_{tcjobp} \tag{B.8}$$

$$pred_{tcjp} = \beta \cdot Consumer\_Persistence + \alpha_t + \delta_{jc} + \omega_p + \epsilon_{tcjobp} \tag{B.9}$$

Table B9 displays the OLS estimates of the regression relating total recorded searches on prediction error, using both the MSE and AUC as the dependent variables. We report the results of running the regressions with and without the website and website-country fixed effects, but our preferred specification is the one without the website and website-country fixed effects.[10] This corresponds to the regression results in Columns (1) and (3) of Table B9. As expected, an increase in the total recorded searches increases AUC significantly and decreases MSE, albeit insignificantly. Recall that our point estimate of the magnitude of lost data from the GDPR was 10.7%. With this data loss, the magnitude of the predicted decline in prediction error is relatively small with a 10.7% decrease in recorded searches only leading to a 0.0007 decrease in AUC.[11]

Table B11 displays the OLS estimates of the regression relating four week consumer persis-

[10] The reason is that the website-country fixed effects soak up the variation in different dataset sizes across websites, even though understanding how this variation impacts prediction error is our main interest.

[11] In reality the intermediary does not train its models only on data from the current week, but rather utilizing a sliding window of data that includes previous weeks. Table B10 shows the results for the same specification, but uses a sliding window total of recorded searches instead of the weekly total number of recorded searches, and shows that the point estimates do not change much when taking this into account.

tence to prediction error, using both the MSE and AUC as the dependent variable. As before, we have regressions with and without website and website-country fixed effects, and focus primarily on the regressions without them. Recall that we previously found a 0.00505 increase in the four week persistence as a result of GDPR. Combined with the point estimates from Table B11, this implies an increase of 0.013 for AUC and a decrease of 0.007 for MSE.

Putting these two results together point to the fact that the decline in the overall scale of data should have little impact on predictability, but the change in the nature of the data towards more identifiable consumers should marginally improve prediction according to both AUC and MSE. However, this does not imply that the scale of data is unimportant which would run counter to standard statistical intuition; on the contrary, prediction ability improves substantially as the scale of data increases. Rather, the change in the scale of the data as a result of GDPR is not large enough to cause meaningful changes in prediction error in the long run. However, the increase in persistence as a result of GDPR should lead to an improvement in prediction capabilities in the long run.

Table B9: Prediction Error and Scale of Data

	(1) AUC	(2) AUC	(3) MSE	(4) MSE
log(Recorded Searches)	0.0154*	0.0178	-0.00435	0.000937
	(1.84)	(0.98)	(-0.88)	(0.15)
Constant	0.505***	0.510**	0.191***	0.0987
	(4.60)	(2.45)	(2.82)	(1.31)
Product Category Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Website × Country FE		✓		✓
Observations	874	874	874	874
R^2	0.129	0.699	0.138	0.936

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-product type-week level between weeks 4 and 14, including both weeks 4 and 14 (January 9th - April 5th). The dependent variable in the regression reported in the first and second column is AUC. The dependent variables in the third and fourth column is the MSE. The regression results reported in column (1) and (3) do not include website or website-country fixed effects, whereas those reported in column (2) and (4) include these fixed effects.

Table B10: Sliding Window Data Scale and Aggregate Prediction Error

	(1) AUC	(2) AUC	(3) MSE	(4) MSE
log(Two Week Search Total)	0.0158*		-0.00439	
	(1.88)		(-0.87)	
log(Three Week Search Total)		0.0161*		-0.00440
		(1.92)		(-0.86)
Constant	0.651***	0.479***	0.0942	0.192**
	(5.34)	(4.05)	(1.28)	(2.56)
Product Category Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Website × Country FE		✓		✓
Observations	868	861	868	861
R^2	0.129	0.129	0.140	0.142

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-product type-week level between weeks 4 and 14, including both weeks 4 and 14 (January 9th - April 5th). The dependent variable in the regression reported in the first and second column is AUC. The dependent variables in the third and fourth column is the MSE. The regression results reported in column (1) and (3) do not include website or website-country fixed effects, whereas those reported in column (2) and (4) include these fixed effects. The Two Week Search Total and Three Week Search Total variables are computed by summing the total number of searches observed for each observation over a sliding window of two weeks and three weeks, respectively.

Table B11: Consumer Persistence and Prediction Error

	(1) AUC	(2) AUC	(3) MSE	(4) MSE
Four Week Persistence	2.621***	0.758	-1.401**	0.611*
	(4.55)	(0.95)	(-2.58)	(1.67)
Constant	0.542***	0.686***	0.221***	0.0852***
	(11.35)	(20.17)	(4.91)	(5.30)
Product Category Controls	✓	✓	✓	✓
Week FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Website × Country FE		✓		✓
Observations	874	874	874	874
R^2	0.230	0.691	0.223	0.938

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: t-statistics are reported in parentheses. The standard errors for every regression are clustered at the website-country level. We aggregate every dependent variable to the website-country-product type-week level between weeks 4 and 14, including both weeks 4 and 14 (January 9th - April 5th). The dependent variable in the regression reported in the first and second column is AUC. The dependent variables in the third and fourth column is the MSE. The regression results reported in column (1) and (3) do not include website or website-country fixed effects, whereas those reported in column (2) and (4) include these fixed effects.

Appendix C: Supplementary Material for Recommenders' Originals: The Welfare Effects of the Dual Role of Platforms as Producers and Recommender Systems

C.1 Omitted Proofs

C.1.1 Proof of Proposition 1

We assume, as is customary in the information design literature [131], that the recommender is able to select its preferred equilibrium. Noting that

$$\begin{aligned}\arg\max_{x_j \in \{x_P, x_F\}} \mathbb{E}[u(x_j, \theta, \tau) \mid \rho(x_P, x_F) = m] &= \arg\max_{x_j \in \{x_P, x_F\}} \theta \mathbb{E}[x_j \mid \rho(x_P, x_F) = m] - \tau \\ &= \arg\max_{x_j \in \{x_P, x_F\}} \mathbb{E}[x_j \mid \rho(x_P, x_F) = m]\end{aligned}$$

we have that the event $\left\{x_P \in \arg\max_{x_j \in \{x_P, x_F\}} \mathbb{E}[u(x_j, \theta, \tau) \mid \rho(x_P, x_F) = m]\right\}$ is independent from θ for any recommendation policy ρ and so

$$\alpha_P = \mathbb{P}\left(x_P \in \arg\max_{x_j \in \{x_P, x_F\}} \mathbb{E}[x_j \mid \rho(x_P, x_F) = m]\right).$$

As the recommendation policy is determined after revenue R_P is collected and q_P is chosen, and letting $\mathcal{M}$ denote a set of arbitrary messages (containing at least two distinct messages) the problem of designing a recommendation policy that maximizes the platform's payoffs collapses to maximizing the platform's market share subject to consumer credibility constraints:

$$\begin{aligned} &\arg \max_{\rho:\{0,1\}^2 \to \Delta(\mathcal{M})} \alpha_P \cdot R_P - C_P \\ = &\arg \max_{\rho:\{0,1\}^2 \to \Delta(\mathcal{M})} \mathbb{P}\left(x_P \in \arg \max_{x_j \in \{x_P, x_F\}} \mathbb{E}[x_j \mid \rho(x_P, x_F) = m] \right). \end{aligned} \tag{RP}$$

Given that there are only two relevant actions that the recommendation policy induces x_P, x_F, the problem is equivalent to having (stochastic) direct recommendations, that is, to having $\rho : \{0,1\}^2 \to \Delta(J)$. We can then recast the optimal recommendation policy from the optimization problem given in (RP) to:

$$\max_{\rho:\{0,1\}^2 \to \Delta(J)} \sum_{a,b \in \{0,1\}} \mathbb{P}\left(\rho(x_P, x_F) = P \mid x_P = a,\, x_F = b\right) \mathbb{P}(x_P = a,\, x_F = b)$$

subject to credibility constraints

$$\mathbb{E}[x_P \mid \rho(x_P, x_F) = P] \geq \mathbb{E}[x_F \mid \rho(x_P, x_F) = P] \tag{1}$$

$$\mathbb{E}[x_F \mid \rho(x_P, x_F) = F] \geq \mathbb{E}[x_P \mid \rho(x_P, x_F) = F] \tag{2}$$

Given independence of x_P and x_F, the objective function becomes $q_P q_F \mathbb{P}\left(\rho(1,1) = P\right) + q_P(1 - q_F)\mathbb{P}\left(\rho(1,0) = P\right) + (1-q_P)q_F \mathbb{P}\left(\rho(0,1) = P\right) + (1-q_P)(1-q_F)\mathbb{P}\left(\rho(0,0) = P\right)$, which is linear and increasing in $\mathbb{P}\left(\rho(a,b) = P\right)$, $a, b \in \{0,1\}$.

Note that the unconstrained optimum is setting $\mathbb{P}\left(\rho(x_P, x_F) = P\right) = 1$ regardless of the quality realizations, implying that the platform always recommends its own goods. This is indeed the solution to the optimal recommendation policy problem whenever $q_P \geq q_F$ as, in this case, the unconstrained optimum is feasible as, without further information, the consumers will always consume the platform's good.

The solution to the case where $0 = q_P < q_F$ is similarly straightforward, as recommendations are ineffective and thus the only policy that complies with obedience is to send consumers truthful recommendations to choose the independent firm's good whenever it is of high quality, and break indifference in favor of the platform's good when $x_F = 0$. Similarly, when $q_P < q_F = 1$, it should be straightforward that the optimal recommendation policy is to send consumers truthful recommendations, but breaking indifference in favor of the platform's good. For the case where $0 < q_P < q_F < 1$, as the constraints do not depend on $\mathbb{P}(\rho(j,j) = P)$, $j = 0,1$, we can set $\mathbb{P}(\rho(j,j) = P) = 1$ noting that the objective function is strictly increasing in $\mathbb{P}(\rho(j,j) = P)$ given that $q_P > 0$ and $q_F < 1$. When $q_P < q_F$, the constraint (2), is redundant as $\mathbb{P}(x_P = 1, x_F = 0) - \mathbb{P}(x_P = 0, x_F = 1) = q_P(1-q_F) - (1-q_P)q_F < 0$. Rearranging the the terms in the constraint (1), we have $0 \leq \mathbb{P}(\rho(x_P = 0, x_F = 1) = P) \leq \frac{q_P(1-q_F)}{q_F(1-q_P)}\mathbb{P}(\rho(x_P = 1, x_F = 0) = P) \leq 1$. Again by monotonicity of the objective function in $\mathbb{P}(\rho(x_P, x_F) = P)$, the optimal policy is given by setting $\mathbb{P}(\rho(x_P = 1, x_F = 0) = P) = 1$ and $\mathbb{P}(\rho(x_P = 0, x_F = 1) = P) = \frac{q_P}{1-q_P}\frac{1-q_F}{q_F}$.

C.1.2 Proof of Proposition 2

Since the optimal recommendation policy is independent of the access fee and the consumer's type, $\mathbb{E}[u(x_i, \theta_i, \tau) \mid \rho] = \theta_i \mathbb{E}[x_m \mid \rho] - \tau$. As, from Proposition 1, $\mathbb{E}[x_m \mid \rho] = \max\{q_P, q_F\}$, we obtain that $e_i = \mathbf{1}_{\theta_i \geq \tau/\max\{q_P, q_F\}}$. Then, the problem simplifies to

$$\tau \in \arg\max_{t \geq 0} t \cdot \left(1 - \frac{t}{\bar{\theta}\max\{q_P, q_F\}}\right)$$

which implies that $\tau = \frac{1}{2}\bar{\theta}\max\{q_P, q_F\}$, $R_P = r_P \max\{q_P, q_F\}$, where r_P is defined as before, and $\mathbb{E}[e_i u(x_m, \theta_i, \tau) \mid \rho] = \frac{3}{8}\bar{\theta}^2 \max\{q_P, q_F\}$.

C.1.3 Proof of Proposition 4

Note that $\pi_F(q_F)$ is also a piecewise strictly concave function, but it is not continuous. Immediately, if $r_P \geq 2$, we have that the independent firm sets quality at $\frac{r_F}{2}$. We now consider the case where $r_P < 2$. Let $\pi^{(1)}(q_F) := r_F \cdot q_F - {q_F}^2$ and $\pi^{(2)}(q_F) := (q_F - q_P(q_F)) \cdot r_P \cdot q_F + r_F \cdot q_F - {q_F}^2$. The maximizer of $\pi_F^{(1)}$ is $\min\left\{1, \frac{r_F}{2}\right\}$, while that of $\pi_F^{(2)}$ is $\min\left\{1, \frac{r_F}{2(1-r_P)+{r_P}^2}\right\}$. We have split the exogenous parameters into different cases and find the maximum under each of these cases.

1. When $\tilde{q}_F \geq \frac{r_F}{2}$, then the maximizer can only be that of $\pi_F^{(2)}$. This follows by strict concavity of $\pi_F^{(1)}$ which then leads to the fact that $\frac{d}{dq_F}\pi_F^{(1)}(q_F) \mid_{q_F=\bar{q}_F} > 0$. As such, $\max_{q_F \in [\bar{q}_F,1]} \pi_F^{(2)}(q_F) \geq \pi_F^{(2)}(\bar{q}_F) > \pi_F^{(1)}(\bar{q}_F)$. Finally, when $\tilde{q}_F \geq \frac{r_F}{2}$ we also have that $\frac{d}{dq_F}\pi_F^{(2)}(q_F) \mid_{q_F=\bar{q}_F} \geq 0$, which implies that $\arg\max_{q_F \in [0,1]} \pi_F^{(2)}(q_F) = \min\left\{1, \frac{r_F}{2(1-r_P)+r_P{}^2}\right\}$.

2. When $\frac{r_F}{2} \geq \tilde{q}_F \geq \frac{r_F}{2(1-r_P)+r_P{}^2}$, then simple but cumbersome algebraic manipulations show that $\max_{q_F \in [0,\bar{q}_F]} \pi^{(1)}(q_F) < \max_{q_F \in [\bar{q}_F,1]} \pi^{(2)}(q_F)$ whenever this is the case and therefore $\arg\max_{q_F \in [0,1]} \pi_F^{(2)}(q_F) = \min\left\{1, \frac{r_F}{2(1-r_P)+r_P{}^2}\right\}$.

   From the conditions in this and the above case, we have that $\min\left\{1, \frac{r_F}{2(1-r_P)+r_P{}^2}\right\}$ is a maximizer whenever (i) $\tilde{q}_F \geq \frac{r_F}{2}$ or (ii) $\frac{r_F}{2} \geq \tilde{q}_F \geq \frac{r_F}{2(1-r_P)+r_P{}^2}$, which, given $r_P < 2$, leads to the condition that $\frac{r_F}{2} \geq \tilde{q}_F \frac{2(1-r_P)+r_P{}^2}{2}$.

3. Finally, when $\frac{r_F}{2(1-r_P)+r_P{}^2} < \tilde{q}_F$, there are two candidates for maximizers: the discontinuity point, $\bar{q}_F$, which corresponds to the unique (corner) solution to $\arg\max_{q_F \in [\bar{q}_F,1]} \pi^{(2)}(q_F)$, and $\frac{r_F}{2} = \arg\max_{q_F \in [0,\bar{q}_F]} \pi^{(1)}(q_F)$. The discontinuity point is a maximizer whenever, together with the above inequalities,

$$
\begin{aligned}
&\pi_F\left(\tilde{q}_F\right) \geq \pi_F\left(\frac{r_F}{2}\right) \\
\Longleftrightarrow &\left(r_P \frac{2-r_P}{2} - 1\right)(\tilde{q}_F)^2 + r_F \cdot \tilde{q}_F \geq \frac{r_F{}^2}{4} \\
\Longleftrightarrow &\frac{r_F}{2} \in \left[\tilde{q}_F\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right), \tilde{q}_F\left(1+\sqrt{r_P\frac{2-r_P}{2}}\right)\right]
\end{aligned}
$$

   As $\tilde{q}_F\left(1+\sqrt{r_P\frac{2-r_P}{2}}\right) \geq \frac{r_F}{2(1-r_P)+r_P{}^2}$ whenever $r_P < 2$, we have that the discontinuity point is a maximizer whenever $\frac{r_F}{2} \in \left\{\tilde{q}_F\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right), \tilde{q}_F\frac{2(1-r_P)+r_P{}^2}{2}\right)$ and $\frac{r_F}{2}$ is a maximizer when $\frac{r_F}{2} \leq \tilde{q}_F\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right)$.

C.1.4 Proof of Proposition 5

As welfare in both cases is given by $K \cdot \max\left\{q_P^{DR}, q_F^{DR}\right\}$ and $K \cdot q_F^{NP}$ for the same positive constant K, it suffices to compare the resulting quality investments in both cases. If $r_P \geq 2$, then $q_P^{DR} = q_F^{NP} = 1$, attaining the same welfare. We proceed by analyzing the case where $r_P < 2$. Note that

$$\max\left\{q_P^{DR}, q_F^{DR}\right\} = \begin{cases} \min\left\{1, \frac{r_F}{2(1-r_P)+r_P^2}\right\} & \text{if } \frac{2(1-r_P)+r_P^2}{2}\tilde{q}_F \leq \frac{r_F}{2} \\ \tilde{q}_F & \text{if } \tilde{q}_F\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right) \leq \frac{r_F}{2} < \frac{2(1-r_P)+r_P^2}{2}\tilde{q}_F \\ \frac{r_P}{2} & \text{if } \frac{r_F}{2} \leq \tilde{q}_F\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right) \end{cases}$$

As $q_P^{DR} \leq \frac{r_P}{2} < \frac{r_P+r_F}{2} \leq q_F^{NP}$, if $\max\left\{q_P^{DR}, q_F^{DR}\right\} = q_P^{DR}$, welfare is lower in the dual role case.

Suppose that $\max\left\{q_P^{DR}, q_F^{DR}\right\} = \tilde{q}_F \geq \frac{r_P+r_F}{2} = q_F^{NP} \iff \tilde{q}_F - \frac{r_P}{2} \geq \frac{r_F}{2}$. As $\max\left\{q_P^{DR}, q_F^{DR}\right\} = \tilde{q}_F$ implies that $\tilde{q}_F\frac{2\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right)}{2} \leq \frac{r_F}{2}$. However, $\tilde{q}_F\frac{2\left(1-\sqrt{r_P\frac{2-r_P}{2}}\right)}{2} > \tilde{q}_F - \frac{r_P}{2}\ \forall r_P < 2$, which then leads to a contradiction.

We then have the case where $\max\left\{q_P^{DR}, q_F^{DR}\right\} = \min\left\{1, \frac{r_F}{2(1-r_P)+r_P^2}\right\}$. Note that $\frac{r_F}{2(1-r_P)+r_P^2} \geq \frac{r_P+r_F}{2} \implies r_F \geq \frac{2(1-r_P)+r_P^2}{2-r_P}$. Moreover, as $\max\left\{q_P^{DR}, q_F^{DR}\right\} = \frac{r_F}{2(1-r_P)+r_P^2} \implies r_F \geq \left(2(1-r_P)+r_P^2\right)\frac{r_P}{4-r_P}$ and $\frac{1}{2-r_P} \geq \frac{r_P}{4-r_P}$, we have that only if $r_F \geq \frac{2(1-r_P)+r_P^2}{2-r_P}$ do we have $q_F^{DR} \geq q_F^{NP}$ and that if $r_F \geq \frac{2(1-r_P)+r_P^2}{2-r_P}$ and if $q_F^{NP} = \min\left\{1, \frac{r_P+r_F}{2}\right\} = \frac{r_P+r_F}{2}$ then $q_F^{DR} \geq q_F^{NP}$.

Finally, note that $\frac{r_P+r_F}{2} \geq q_F^{NP} = 1 > \frac{r_F}{2(1-r_P)+r_P^2}$ implies that $\frac{2(1-r_P)+r_P^2}{2-r_P} > r_F$, which is necessary and sufficient for this case. To see this note that $\max\left\{q_P^{DR}, q_F^{DR}\right\} = \frac{r_F}{2(1-r_P)+r_P^2}$ implies $r_F \geq \left(2(1-r_P)+r_P^2\right)\frac{r_P}{4-r_P}$ and as $\frac{1}{2-r_P} \leq \frac{r_P}{4-r_P}$ when $r_P < 2$, this imposes no further constraint.

Consequently, $r_F \geq \frac{2(1-r_P)+r_P^2}{\max\{1,2-r_P\}}$ is a necessary and sufficient for $q_F^{NP} \leq \max\left\{q_P^{DR}, q_F^{DR}\right\}$. Moreover, $q_F^{NP} < \max\left\{q_P^{DR}, q_F^{DR}\right\}$ if and only if $r_F > \frac{2(1-r_P)+r_P^2}{2-r_P}$ and $1 \geq \frac{r_P+r_F}{2}$.

C.1.5 Proof of Lemma 1

If $r_P \geq \frac{2}{1-q_F}$, then $\pi_P(q_P, q_F)$ is convex and strictly increasing in q_P, which immediately implies that the platform optimally sets $q_P = 1$. If $\frac{2}{1-q_F} > r_P \geq \frac{4}{3(1-q_F)+q_F{}^2}$, then $\pi_P(q_P, q_F)$ is strictly concave but $\frac{\partial}{\partial q_P}\pi_P(q_P, q_F) \mid_{q_P=1} \geq 0$ and still implies that the platform optimally sets $q_P = 1$. Finally, if $\frac{4}{3(1-q_F)+q_F{}^2} > r_P$, then $\pi_P(q_P, q_F)$ is strictly concave and the platform sets investments optimally at $q_P = \hat{q}_P(q_F) := \frac{r_P}{2}\frac{1-q_F(1-q_F)}{2-r_P(1-q_F)}$. As $\forall q_F \in [0,1]$ and $\forall r_P > 0$, π_P is either strictly increasing or strictly concave in q_P, it is strictly quasiconcave in q_P. Moreover, as π_P is continuous in (q_P, q_F), then we have that $q_P(q_F)$ is continuous, by Berge's theorem of the maximum. Hence, the platform's optimal investment, as a function of the firm's investment, is given by

$$q_P(q_F) = \begin{cases} 1 & \text{if } \frac{4}{3(1-q_F)+q_F{}^2} \leq r_P \\ \hat{q}_P(q_F) & \text{if otherwise} \end{cases}$$

and is a continuous function of q_F.

C.1.6 Proof of Proposition 6

Recall that $q_P(q_F) = 1$ if $\frac{4}{3(1-q_F)+q_F{}^2} \leq r_P$. Note that $\frac{4}{3(1-q_F)+q_F{}^2} \leq 4$ for any $q_F \in [0,1]$. Hence, if $r_P \geq 4$, then $q_P(q_F) = 1$ for any $q_F \in [0,1]$, in which case $q_F^U := \arg\max_{q_F \in [0,1]} \pi_F(1, q_F) = \min\left\{1, \frac{r_F + r_P/2}{2}\right\}$.

If $r_P < 4$, then $1 > q_P(q_F) \iff 1 > \hat{q}_P(q_F) \iff q_F > \hat{q}_F := \frac{1}{2}\left(3 - \sqrt{\frac{16-3r_P}{r_P}}\right)$, where $\hat{q}_F < 1$. We define:

$$\pi_F^{(1)}(q_F) = \pi_F(1, q_F)$$
$$\pi_F^{(2)}(q_F) = \pi_F(\hat{q}_P(q_F), q_F)$$

where $\pi_F(q_P(q_F), q_F) = \pi_F^{(1)}(q_F)$ if $q_F \leq \hat{q}_F$ and $\pi_F(q_P(q_F), q_F) = \pi_F^{(2)}(q_F)$ if otherwise. Note that

1. $\pi_F^{(1)}$ is strictly concave

2. When $r_P < 4$, it is also the case that $\frac{d}{dq_F}\pi_F^{(1)}(q_F) > 0 \; \forall q_F \in [0, \hat{q}_F]$.

3. Straightforward computations show that $\frac{d}{dq_F}\pi_F^{(2)}(q_F) \mid_{q_F=\hat{q}_F} > 0$

4. $\frac{d^2}{(dq_F)^2}\pi_F^{(2)}(q_F) < 0 \; \forall q_F \in [0, 1]$ when $r_P < 4$.

(1) - (3) directly imply that $\arg\max_{q_F\in[0,1]} \pi_F(q_P(q_F), q_F) = \arg\max_{q_F\in[\hat{q}_F,1]} \pi_F^{(2)}(q_F)$.

(4) implies that $\arg\max_{q_F\in[\hat{q}_F,1]} \pi_F^{(2)}(q_F)$ is a singleton.

The direct implication of these two results is that $\arg\max_{q_F\in[\hat{q}_F,1]} \pi_F^{(2)}(q_F) = \arg\max_{q_F\in[0,1]} \pi_F^{(2)}(q_F) = \arg\max_{q_F\in[0,1]} \pi_F(q_P(q_F), q_F)$ is uniquely defined.

C.2 Model with Heterogeneous Costs

In this appendix, we consider the a setup where there is no outside revenue for the independent producer ($R_F = 0$), and, instead, there is a cost advantage, leading to potentially heterogeneous cost structures: the platform's production cost will still be $C_P(q_P) = q_P^2$, but the independent firm's is now $C_F(q_F) = c_F \cdot q_F^2$, with $c_F \in (0, 1)$. Keeping the remaining elements of the setup in the main text, we show that the results on the comparison of the dual role case with both the no-platform-production and the unbiased recommendations cases are robust to this alternative specification.

C.2.1 Dual Role: Equilibrium, Welfare Comparison

Keeping the same timeline as in the main text, this change in the setup affects only the investment decisions. In the no-platform-production benchmark, the independent firm solves

$$\max_{q_F\in[0,1]} r_P \cdot q_F - c_F \cdot q_F^2$$

which results in

$$q_F^{NP} = \min\left\{\frac{r_P}{2 \cdot c_F}, 1\right\}.$$

Under the platform's dual role, Proposition 3 continues to hold as before. However, given the cost-advantage specification, the independent firm's payoff is now given by

$$\pi_F(q_F) = \begin{cases} (q_F - q_P(q_F)) \cdot r_P \cdot q_F - c_F \cdot {q_F}^2 & \text{if } r_P < 2 \text{ and } q_F \geq \tilde{q}_F = \frac{r_P}{4-r_P}, & \text{(C.1)} \\ 0 & \text{if otherwise.} & \text{(C.2)} \end{cases}$$

Then, the firm's equilibrium investment is

$$q_F^{DR} = \begin{cases} 1 & \text{if } 0 < c_F \leq \frac{1}{2} r_P \left(2 - r_P\right), \\ 0 & \text{if otherwise.} \end{cases}$$

The difference in consumer welfare between the dual role case and the no-platform-production benchmark is proportional to $\max\left\{q_F^{DR}, q_R^{DR}\right\} - q_F^{NP}$, as consumer welfare is linear in expected quality of the good consumed (given the recommendations). Consequently, this implies that welfare is the same whenever $r_P \geq 2$ and strictly higher under the dual role than under absent of platform production when $\frac{1}{2} r_P < c_F \leq \frac{1}{2} r_P \left(2 - r_P\right)$.

C.2.2 Unbiased Recommendation: Equilibrium, Welfare Comparison

We turn to the case of allowing for platform production but imposing unbiased recommendations. Here again we will have uniquely defined equilibrium investments as a function of the exogenous parameters r_P and c_F. The platform's investment problem is the same as in the main text, maximizing the profits as given in (3.5). It is straightforward to verify that the solution to the platform's problem yields a best response given by

$$q_P(q_F) = \begin{cases} 1 & \text{if } r_P \geq \frac{4}{3} \text{ and } q_F \leq \frac{1}{2}\left(3 - \sqrt{16/r_P - 3}\right), \\ \frac{r_P}{2} \frac{1 - q_F(1-q_F)}{2 - r_P(1-q_F)} & \text{if otherwise.} \end{cases}$$

The independent firm's payoffs are given by

$$\pi_F(q_F) = \frac{1}{2}\left(1 - (q_P(q_F) - q_F)\right) \cdot r_P \cdot (q_F + (1 - q_F)q_P(q_F)) - c_F \cdot {q_F}^2$$

It is easy to see that if we substitute $q_P(q_F) = 1$, then the firm's profit function simplifies to

$$\frac{1}{2} q_F \cdot r_P - c_F \cdot {q_F}^2$$

being maximized at $q_F = \min\left\{1 \ , \ \frac{r_P}{4 \cdot c_F}\right\}$.

Given that, $\forall c_F \in (0, 1)$ and $r_P \in \left[\frac{4}{3}, 4\right)$, $\frac{r_P}{4 \cdot c_F} > \frac{1}{2}\left(3 - \sqrt{16/r_P - 3}\right)$, then $q_P^U = 1$ if and only if $r_P \geq 4$, in which case $q_F^U = 1$. Moreover, as $\frac{d\pi_F(q_F)}{dq_F}\Big|_{q_F=1} = \frac{r_P}{4}\left(6 - 2r_P + \frac{3}{8}r_P^2\right) - 2c_F$, then equilibrium investments (q_F^U, q_P^U) are both strictly smaller than 1 if and only if $c_F > \frac{r_P}{8}\left(6 - 2r_P + \frac{3}{8}r_P^2\right)$. This also imposes a constraint on the size of the platform, as $1 > \frac{r_P}{8}\left(6 - 2r_P + \frac{3}{8}r_P^2\right)$. Let $\bar{r}_P \in (0, 4)$ be the solution to $1 = \frac{r_P}{8}\left(6 - 2r_P + \frac{3}{8}r_P^2\right)\}$, which is uniquely defined (with $\bar{r}_P \approx 2.38$). Then $c_F > \frac{r_P}{8}\left(6 - 2r_P + \frac{3}{8}r_P^2\right)$ requires $r_P < \bar{r}_P$. In contrast, when $c_F \leq \frac{r_P}{8}\left(6 - 2r_P + \frac{3}{8}r_P^2\right)$, we again have $q_F^U = 1$, with $q_P^U < 1$ if and only if $r_P < 4$.

Here too, the dual role can yield greater welfare than imposing unbiased recommendation policies. If $1 = \max\left\{q_P^{DR}, q_F^{DR}\right\} > q_P^U + (1 - q_P^U)q_F^U$, then this is trivially the case, occurring for intermediate values of the cost advantage of the independent firm (fixing $r_P < \bar{r}_P$): $\frac{1}{2}r_P(2 - r_P) \geq c_F > \frac{r_P}{8}\left(6 - 2r_P + \frac{3}{8}r_P^2\right)$. In this case, the platform's threat of sending non-truthful recommendations induces larger investments by the independent firm to the point of strictly exceeding the investments that would have occurred under unbiased recommendations, to the benefit of consumers.

However, it can also be the case that $1 > \max\left\{q_P^{DR}, q_F^{DR}\right\} = q_P^{DR} > q_P^U + (1 - q_P^U)q_F^U$ for any fixed $\bar{r}_P$ in so far as the cost advantage of the independent firm is small enough (i.e. c_F is close enough to 1).[1] Note that this implies that, in the dual role case, even when the threat of

[1] The derivation of the exact expression for the lower bound on c_F is tedious and provides no significant additional insight.

biased recommendations leads to foreclosure of the independent firm and the platform alone is producing, it can still lead to a higher consumer welfare than when unbiased recommendations are imposed.

C.2.3 Discussion

Providing the independent firm with a cost advantage works similarly to it having alternative revenue sources available. When the cost advantage is (not) large enough, the firm will commit to a larger (resp. lower) investment in face of the platform's dual role than when the platform is not involved in production, leading to higher (lower) consumer welfare. The reason for why this is the case is exactly the same: while having to split the revenue under the dual role depresses the independent firm's marginal incentives to invest, higher investment levels now increase both the total revenue as well as the share of the total revenue that is accrued by the independent firm.

However — and again similarly to what occurs in the benchmark model — this is not the whole story. The (threat of) untruthful recommendations arising from the optimal information design policy by the platform can also contribute to stronger investment incentives. If biasing recommendations negatively affects the value of the platform to consumers (relative to unbiased recommendations), it can induce the independent producer to investment more in order to expand its market share. We can then observe that only when the firm's cost advantage is large (analogous to having significant sources of outside revenue) does letting the platform set up the recommendation system to its own advantage improve consumer welfare relative to having unbiased recommendations.

A final note on this variation and how it differs relative to the benchmark setup. The cost structure and the cost advantage specification result in a "bang-bang" solution in the dual-role case, in that either the independent firm expects to be foreclosed from the platform — its only source of revenue — and therefore does not even invest, or the threat of non-truthful recommendations induces the firm to invest as much as possible to counter it.[2] This then effectively results in there being truthful recommendations in equilibrium recommendation policies, as the consumers know

[2] Note that recommendations are still *biased* according to our definition since they are not neutral.

that, if the independent firm's good is available, it is a high quality good for sure. Then, Bayes plausibility requires the recommendation to guarantee at least as good quality in expectation, the platform has no leeway to send non-truthful recommendations (in contrast to what occurs in our benchmark setup), implying that the consumers will only be told to choose the platform's good instead if and when it is at least as good as the available alternative. Therefore, the threat of non-truthful recommendations alone is enough to drive up the firm's investment incentives.

C.3 Model with Simultaneous Investment

In this appendix we show that our main conclusions are robust to our assumption on the timing of investments. In particular, we consider the case in which investments are simultaneous rather than sequential, keeping everything else the same case as in the main text.

C.3.1 Dual Role Equilibrium Characterization

Naturally, the only changes that occur are for the independent producer in the dual role and in the unbiased recommendations cases, as they no longer take as given the platform's sequential reaction to their investment choices. Hence, in the dual role we still have that the platform's best response to the producer's chosen quality is given by

$$q_P(q_F) = \begin{cases} \frac{r_P}{2} q_F & \text{if } q_F \geq \tilde{q}_F \text{ and } r_P < 2 \\ \min\left\{1, \frac{r_P}{2}\right\} & \text{if } q_F < \tilde{q}_F \text{ and } r_P < 2, \text{ or } r_P \geq 2. \end{cases}$$

Differently from the baseline, the independent firm's profit function is now given by

$$\pi_F(q_F, q_P) := \max\{0, q_F - q_P\} r_P q_F + r_F q_F - q_F^2.$$

Proposition 7. *For any $r_P, r_F > 0$, there is an equilibrium in the dual role with simultaneous investments. There are r_P, r_F such that consumer welfare is strictly higher under the dual role than under the no-platform-production case.*

The proof for Proposition 7 provides a complete characterization of the equilibria. In general, a result similar to our baseline model follows, that is, consumer welfare is weakly higher under the dual role than under the no-platform-production case if and only if r_F is large enough relative to r_P. However, given potential multiplicity of equilibria when $1 > r_P > r_F > 0$, the characterization holds only when the equilibrium selection across the parameter space is monotone in $\max\{q_P, q_F\}$. There are multiple of such selection rules, for instance, selecting the equilibrium that attains the highest (or the smallest) $\max\{q_P, q_F\}$ whenever multiple equilibria exist.

Proof. We characterize all equilibria by studying different cases of the parameter space.

Case 1: $r_P \geq 2$ and $r_F > 0$.

In this case, the platform will choose $q_P = 1$ regardless of the firm's choice of investment, leading a unique equilibrium where $q_P = 1$ and $q_F = \min\{1, r_F/2\}$.

Case 2: $2 > r_P \geq 1$ and $r_F \geq r_P$.

Note that $r_P \geq 1 \implies \pi_F$ is convex on $[q_P, 1]$. Moreover, π_F is strictly concave on $[0, q_P]$.

Any equilibrium has either $q_P = \frac{r_P}{2}$ or $q_P = \frac{r_P}{2} q_F < q_F$. Then, if $q_P = \frac{r_P}{2}$, we have that

$$0 \leq \frac{\partial}{\partial q_F} \pi_F(q_F, q_P) \bigg|_{q_F = \frac{r_P}{2}^-} = -r_P + r_F < -r_P + r_F + \frac{r_P^2}{2} = \frac{\partial}{\partial q_F} \pi_F(q_F, q_P) \bigg|_{q_F = \frac{r_P}{2}^+}.$$

This implies that $\arg\max_{q_F \in [0,1]} \pi_F(q_F, q_P) = \arg\max_{q_F \in [q_P, 1]} \pi_F(q_F, q_P) = 1$, where the last equality follows by convexity of π_F on $[q_P, 1]$. As $q_F = 1 \geq \frac{r_P}{4 - r_P}$ and $q_P = \frac{r_P}{2} q_F = \frac{r_P}{2}$, we have that $(q_P, q_F) = \left(\frac{r_P}{2}, 1\right)$ is an equilibrium.

To see that, in this case, this is the unique equilibrium, suppose that we have an equilibrium where $q_F < 1$. Then we must have that $q_P = \frac{r_P}{2} q_F$. But then $q_P < q_F$ and, by convexity of π_F on $[q_P, 1]$, $q_F \in \{q_P, 1\}$. By assumption $q_F < 1$ and if instead $q_F = q_P$, then $q_P = \frac{r_P}{2} q_P = 0 = q_F$, which contradicts the fact that $\frac{\partial}{\partial q_F} \pi_F(q_F, q_P) \Big|_{q_F = q_P = 0} > 0$.

Case 3: $2 > r_P > 1$ and $r_P > r_F > 0$.

As $r_P \geq 1$, π_F remains convex on $[q_P, 1]$, but $r_P > r_F$ implies that there is a unique interior maximum on $[0, q_P]$ whenever $q_P = \frac{r_P}{2}$ due to π_F being strictly concave on this region and

$$0 > -r_P + r_F = \frac{\partial}{\partial q_F}\pi_F(q_F, q_P)\bigg|_{q_F=\frac{r_P}{2}^-}.$$

The associated maximizer is $\frac{r_F}{2}$, that is, the investment that the independent firm when foreclosed.

Thus, if, at an equilibrium, $q_P = \frac{r_P}{2}q_F \leq q_F$, we must have that $q_F = 1$. If not, as by convexity of π_F on $[q_P, 1]$, $\arg\max_{q_F\in[q_P,1]} \pi_F(q_F, q_P) \in \{q_P, 1\}$, we would again have $q_F = q_P = \frac{r_P}{2}q_F = 0$, a contradiction. Then, as $1 \geq \frac{r_P}{4-r_P}$, it suffices to check that $\pi_F\left(\frac{r_F}{2}, \frac{r_P}{2}\right) \leq \pi_F\left(1, \frac{r_P}{2}\right)$, which holds whenever $r_F \geq 2 - \sqrt{2r_P}\sqrt{2-r_P}$.

Now suppose that, at an equilibrium, $q_F = \frac{r_F}{2} < q_P \Longrightarrow q_P = \frac{r_P}{2} \Longrightarrow \frac{r_F}{2} \leq \frac{r_P}{4-r_P}$. Furthermore, when $2 > r_P \geq 1$ and $r_P > r_F$, some algebra shows that

$$2 - \sqrt{2r_P}\sqrt{2-r_P} \leq 2\frac{r_P}{4-r_P}.$$

Then, given $2 > r_P > 1$ and $r_P > r_F > 0$,

(1) if $r_F < 2 - \sqrt{2r_P}\sqrt{2-r_P}$, there is a unique equilibrium with $(q_P, q_F) = \left(\frac{r_P}{2}, \frac{r_F}{2}\right)$;

(2) if $2-\sqrt{2r_P}\sqrt{2-r_P} \leq r_F \leq 2\frac{r_P}{4-r_P}$, there are two equilibria, where $(q_P, q_F) \in \left\{\left(\frac{r_P}{2}, 1\right), \left(\frac{r_P}{2}, \frac{r_F}{2}\right)\right\}$;

(3) if $2\frac{r_P}{4-r_P} < r_F$, there is a unique equilibrium, where $(q_P, q_F) = \left(\frac{r_P}{2}, 1\right)$.

Case 4: $r_P = 1 > r_F > 0$.

Note that $\forall q_P < q_F$, $\frac{\partial}{\partial q_F}\pi_F(q_F, q_P) = -q_P + r_F$. Then, not only do we have the same equilibria from Case 3,[3] we have one additional equilibrium, where $q_P = r_F = \frac{r_P}{2}q_F \Longrightarrow q_F = 2r_F$, whenever $2r_F \geq \frac{r_P}{4-r_P} \Longleftrightarrow r_F \geq \frac{1}{6}$.

Case 5: $1 > r_P > 0$ and $r_F \geq r_P$.

In this case we have that π_F is strictly concave on $[0, q_P]$ and on $[q_P, 1]$. At an equilibrium where

[3] The only difference is that instead of having that $\arg\max_{q_F\in[q_P,1]} \pi_F(q_F, q_P) \in \{q_P, 1\}$, we have $\arg\max_{q_F\in[q_P,1]} \pi_F(q_F, q_P) \cap \{q_P, 1\} \neq \emptyset$.

$q_P = \frac{r_P}{2}$, as $r_P \leq r_F$, we have that

$$0 \leq \frac{\partial}{\partial q_F}\pi_F(q_F, q_P)\bigg|_{q_F = \frac{r_P}{2}^-} = -r_P + r_F < -r_P + r_F + \frac{r_P^2}{2} = \frac{\partial}{\partial q_F}\pi_F(q_F, q_P)\bigg|_{q_F = \frac{r_P}{2}^+}.$$

This implies that

$$\arg\max_{q_F \in [0,1]} \pi_F(q_F, q_P) = \arg\max_{q_F \in [q_P,1]} \pi_F(q_F, q_P) = \min\left\{1, \frac{r_F - r_P q_P}{2(1-r_P)}\right\}.$$

Then, given $q_P = \frac{r_P}{2}$, as

$$\min\left\{1, \frac{r_F - r_P q_P}{2(1-r_P)}\right\} = 1 \iff r_F \geq \max\left\{r_P, \frac{(2-r_P)^2}{2}\right\},$$

and

$$\frac{(2-r_P)^2}{2} > r_F \geq r_P \implies \frac{r_F - r_P q_P}{2(1-r_P)} \geq \frac{r_P}{4-r_P},$$

there is an equilibrium with $q_P = \frac{r_P}{2}$ if and only if $r_F \geq \max\left\{r_P, \frac{(2-r_P)^2}{2}\right\}$; in which case the equilibrium is given by $(q_P, q_F) = \left(\frac{r_P}{2}, 1\right)$.

Noting that

$$\frac{\partial}{\partial q_F}\pi_F(q_F, q_P)\bigg|_{q_F = q_P^-} = r_F - 2q_P < r_P q_P + r_F - 2q_P = \frac{\partial}{\partial q_F}\pi_F(q_F, q_P)\bigg|_{q_F = q_P^+},$$

if at an equilibrium $q_P = \frac{r_P}{2} q_F$, then we further have that $r_F - 2q_P = r_F - r_P q_F \geq 0$, which again delivers

$$\arg\max_{q_F \in [0,1]} \pi_F(q_F, q_P) = \arg\max_{q_F \in [q_P,1]} \pi_F(q_F, q_P) = \min\left\{1, \frac{r_F - r_P q_P}{2(1-r_P)}\right\}.$$

Replacing $q_P = \frac{r_P}{2} q_F$ and solving for q_F yields $q_F = \min\left\{1, \frac{4}{(2-r_P)^2}\frac{r_F}{2}\right\}$, and when $1 > r_P > 0$ and $r_F \geq r_P$, $q_F > \frac{r_P}{4-r_P}$. Moreover, $r_F \geq \frac{(2-r_P)^2}{2} \iff q_F = 1$.

Therefore, we have shown there is a unique equilibrium, where $(q_P, q_F) = \left(\frac{r_P}{2}, 1\right)$ if $r_F \geq$

$\max\left\{r_P, \frac{(2-r_P)^2}{2}\right\}$ and $(q_P, q_F) = \left(\frac{r_P}{2}\frac{4}{(2-r_P)^2}\frac{r_F}{2}, \frac{4}{(2-r_P)^2}\frac{r_F}{2}\right)$ if $\frac{(2-r_P)^2}{2} > r_F \geq r_P$.

Case 6: $1 > r_P > 0$ and $r_P > r_F > 0$.

As before, given that $1 > r_P$, π_F is strictly concave on $[0, q_P]$ and on $[q_P, 1]$. Then, in any equilibrium, $q_P \in \left\{\frac{r_P}{2}, \frac{r_P}{2}q_F\right\}$ and $q_F \in \left\{\frac{r_F}{2}, \min\left\{1, \frac{r_F - r_P q_P}{2(1-r_P)}\right\}\right\}$.

Immediately we see that there is no equilibrium where $(q_P, q_F) = \left(\frac{r_P}{2}q_F, \frac{r_F}{2}\right)$, seeing that if $q_P < q_F$, then q_F corresponds to the maximizer of π_F on $[q_P, 1]$, which is given by $\min\left\{1, \frac{r_F - r_P q_P}{2(1-r_P)}\right\}$, and this quantity is always strictly larger than $\frac{r_F}{2}$.

Case 6.1: There is an equilibrium where $(q_P, q_F) = \left(\frac{r_P}{2}, 1\right)$ if and only if (i) $\pi_F\left(\frac{r_F}{2}, \frac{r_P}{2}\right) \leq \pi_F\left(1, \frac{r_P}{2}\right) \iff 2 - \sqrt{2r_P}\sqrt{2-r_P} \leq r_F$, and (ii) $\frac{\partial}{\partial q_F}\pi_F(q_F, q_P)|_{q_F=1, q_P=r_P/2} \geq 0 \iff \frac{(2-r_P)^2}{2} \leq r_F$. Note that if $q_F = 1$, then the platform best-responds by choosing $q_P = \frac{r_P}{2}$ as their investment level. Consequently, such an equilibrium exists whenever

$$1 > r_P > r_F \geq \max\left\{2 - \sqrt{2r_P}\sqrt{2-r_P}, \frac{(2-r_P)^2}{2}\right\}.$$

Case 6.2: An equilibrium in which $(q_P, q_F) = \left(\frac{r_P}{2}q_F, \min\left\{1, \frac{r_F - r_P q_P}{2(1-r_P)}\right\}\right) \neq \left(\frac{r_P}{2}, 1\right)$ implies $(q_P, q_F) = \left(\frac{r_P}{2}\frac{4}{(2-r_P)^2}\frac{r_F}{2}, \frac{4}{(2-r_P)^2}\frac{r_F}{2}\right)$. Note that, if $\frac{r_P}{2}\frac{4}{(2-r_P)^2}\frac{r_F}{2} \leq \frac{r_F}{2} \iff r_P \leq \frac{(2-r_P)^2}{2}$, we have that π_F is maximized at $q_F > \frac{r_F}{2}$. Then, there is such an equilibrium if

(i) $r_P \leq \dfrac{(2-r_P)^2}{2}$ or $\pi_F\left(\dfrac{r_F}{2}, \dfrac{r_P}{2}\dfrac{4}{(2-r_P)^2}\dfrac{r_F}{2}\right) \leq \pi_F\left(\dfrac{4}{(2-r_P)^2}\dfrac{r_F}{2}, \dfrac{r_P}{2}\dfrac{4}{(2-r_P)^2}\dfrac{r_F}{2}\right)$

(ii) $q_F = \frac{4}{(2-r_P)^2}\frac{r_F}{2} < 1 \iff \min\left\{r_P, \frac{(2-r_P)^2}{2}\right\} > r_F$, and

(iii) $\pi_P\left(\dfrac{r_P}{2}, \dfrac{4}{(2-r_P)^2}\dfrac{r_F}{2}\right) \leq \pi_P\left(\dfrac{r_P}{2}\dfrac{4}{(2-r_P)^2}\dfrac{r_F}{2}, \dfrac{4}{(2-r_P)^2}\dfrac{r_F}{2}\right) \iff r_F \geq 2\dfrac{r_P}{4-r_P}\dfrac{(2-r_P)^2}{4}$.

Combining these, we find

$$\min\left\{r_P, \frac{(2-r_P)^2}{2}\right\} > r_F \geq \frac{r_P}{4-r_P}\frac{(2-r_P)^2}{2} \qquad \text{and} \qquad \frac{(2-r_P)^2}{2} \geq 2\frac{r_P}{4-r_P}.$$

Case 6.3: Finally, the last possible equilibrium to consider is one such that $(q_P, q_F) = \left(\frac{r_P}{2}, \frac{r_F}{2}\right)$. If $\min\left\{1, \max\left\{0, \frac{r_F - r_P^2/2}{2(1-r_P)}\right\}\right\} \leq q_P = \frac{r_P}{2}$, then $\arg\max_{q_F \in [0,1]} \pi_F(q_F, q_P) = \frac{r_F}{2}$. So equilibrium conditions are given by

(i) $\pi_P\left(\frac{r_P}{2},\frac{r_F}{2}\right) \le \pi_P\left(\frac{r_P}{2}\frac{r_F}{2},\frac{r_F}{2}\right) \Longleftrightarrow r_F \le 2\frac{r_P}{4-r_P}$, and

(ii) $\frac{r_F - r_P^2/2}{2(1-r_P)} \le \frac{r_P}{2}$ or
$\frac{r_F - r_P^2/2}{2(1-r_P)} > \frac{r_P}{2}$ and $\pi_F\left(\frac{r_F}{2},\frac{r_P}{2}\right) \ge \pi_F\left(\min\left\{1,\frac{r_F - r_P^2/2}{2(1-r_P)}\right\},\frac{r_P}{2}\right)$.

Simplifying the above, we find the conditions supporting this equilibrium to be

$$\min\left\{2\frac{r_P}{4-r_P}, 2-\sqrt{2r_P}\sqrt{2-r_P}\right\} \ge r_F > 0.$$

We now verify that there is always an equilibrium where $1 > r_P > r_F$. First, note that Cases 6.1 and 6.3 have disjoint conditions, as

$$\min\left\{2\frac{r_P}{4-r_P}, 2-\sqrt{2r_P}\sqrt{2-r_P}\right\} < \max\left\{2-\sqrt{2r_P}\sqrt{2-r_P}, \frac{(2-r_P)^2}{2}\right\}.$$

If $\frac{(2-r_P)^2}{2} \ge 2\frac{r_P}{4-r_P}$, then

$$\frac{(2-r_P)^2}{2} = \max\left\{2-\sqrt{2r_P}\sqrt{2-r_P}, \frac{(2-r_P)^2}{2}\right\}$$

and

$$2\frac{r_P}{4-r_P} = \min\left\{2\frac{r_P}{4-r_P}, 2-\sqrt{2r_P}\sqrt{2-r_P}\right\}.$$

Consequently, the conditions for either Case 6.1, 6.2, or 6.3 are satisfied, as

$$(0,r_P) = \left(0, 2\frac{r_P}{4-r_P}\right) \cup \left(2\frac{r_P}{4-r_P}\cdot\frac{1}{2}\frac{(2-r_P)^2}{2}, \frac{(2-r_P)^2}{2}\right) \cup \left[\frac{(2-r_P)^2}{2}, r_P\right).$$

If $\frac{(2-r_P)^2}{2} \le 2\frac{r_P}{4-r_P}$,

$$2-\sqrt{2r_P}\sqrt{2-r_P} = \max\left\{2-\sqrt{2r_P}\sqrt{2-r_P}, \frac{(2-r_P)^2}{2}\right\} = \min\left\{2\frac{r_P}{4-r_P}, 2-\sqrt{2r_P}\sqrt{2-r_P}\right\},$$

and therefore,

$$(0, r_P) = \left(0, 2 - \sqrt{2r_P}\sqrt{2 - r_P}\right) \cup \left[2 - \sqrt{2r_P}\sqrt{2 - r_P}, r_P\right),$$

implying that the conditions for either Case 6.1 or 6.3 are satisfied.

□

C.3.2 Welfare Comparison – Dual Role and No Platform Production

We now directly compare the resulting consumer welfare in the dual role case with simultaneous investment decisions to the no platform production case. Recall that in the no platform production case, consumer welfare is given by $q_F^{NP} = \frac{r_P + r_F}{2}$, whereas in the dual role case, consumer welfare is given by $\max\left\{q_P^{DR}, q_F^{DR}\right\}$.

In Case 1, consumer welfare is maximal under both the dual role and the no-platform-production scenarios. In Cases 2, 3, and 4, i.e. $2 > r_P \geq 1$, consumer welfare is (strictly) higher under the dual role in some equilibrium if and only if $2 - \sqrt{2r_P}\sqrt{2 - r_P} \leq r_F$ (and $r_F < 2 - r_P$). If, moreover $2\frac{r_P}{4 - r_P} < r_F$ ($< 2 - r_P$), it is (strictly) so in any equilibrium. In Case 5, consumers are (strictly) better off under the dual role when $\max\left\{r_P, \frac{(2 - r_P)^2}{4 - r_P}\right\} \leq r_F$ ($< 2 - r_P$). In Case 6.1, i.e. $1 > r_P > r_F \geq \max\left\{2 - \sqrt{2r_P}\sqrt{2 - r_P}, \frac{(2 - r_P)^2}{2}\right\}$, the dual role is always strictly beneficial to consumers. Case 6.2 is (strictly) consumer-welfare-improving whenever its conditions hold and $\frac{(2 - r_P)^2}{4 - r_P} \leq r_F$ ($< 2 - r_P$), where this lower-bound on r_F is binding. Finally, Case 6.3 — $\min\left\{2\frac{r_P}{4 - r_P}, 2 - \sqrt{2r_P}\sqrt{2 - r_P}\right\} \geq r_F > 0$ — has the dual role being always strictly detrimental to consumers.

These comparisons are displayed graphically in Figure C1a and Figure C1b. Due to the multiplicity of equilibria, we have to consider some equilibrium selection criteria in order to be able to compare the cases. We compare the two extreme selection cases and find that there are no significant qualitative differences. In Figures C1a and C1b we consistently select the equilibrium investment levels in the dual role case which induce minimal and maximal welfare, respectively.

C.3.3 Unbiased Recommendation

Now we examine the case of simultaneous investments with unbiased recommendations.

Equilibrium Characterization: The payoff functions for the platform and the independent firm are similar to the main text:

$$\pi_P(q_P, q_F) := r_P \frac{1}{2}(1 - (q_F - q_P))(q_F + (1 - q_F)q_P) - q_P^2;$$
$$\pi_F(q_F, q_P) := r_P \frac{1}{2}(1 - (q_P - q_F))(q_F + (1 - q_F)q_P) + r_F q_F - q_F^2.$$

As $\frac{\partial^2}{(\partial q_F)^2}\pi_F(q_F, q_P) = -2 + (1 - q_P)r_P$ and $\frac{\partial^2}{(\partial q_P)^2}\pi_P(q_P, q_F) = -2 + (1 - q_F)r_P$, and as

$$\frac{\partial}{\partial q_F}\pi_F(q_F, q_P)|_{q_F=0}\frac{\partial}{\partial q_P}\pi_P(q_P, q_F)|_{q_P=0} > 0,$$

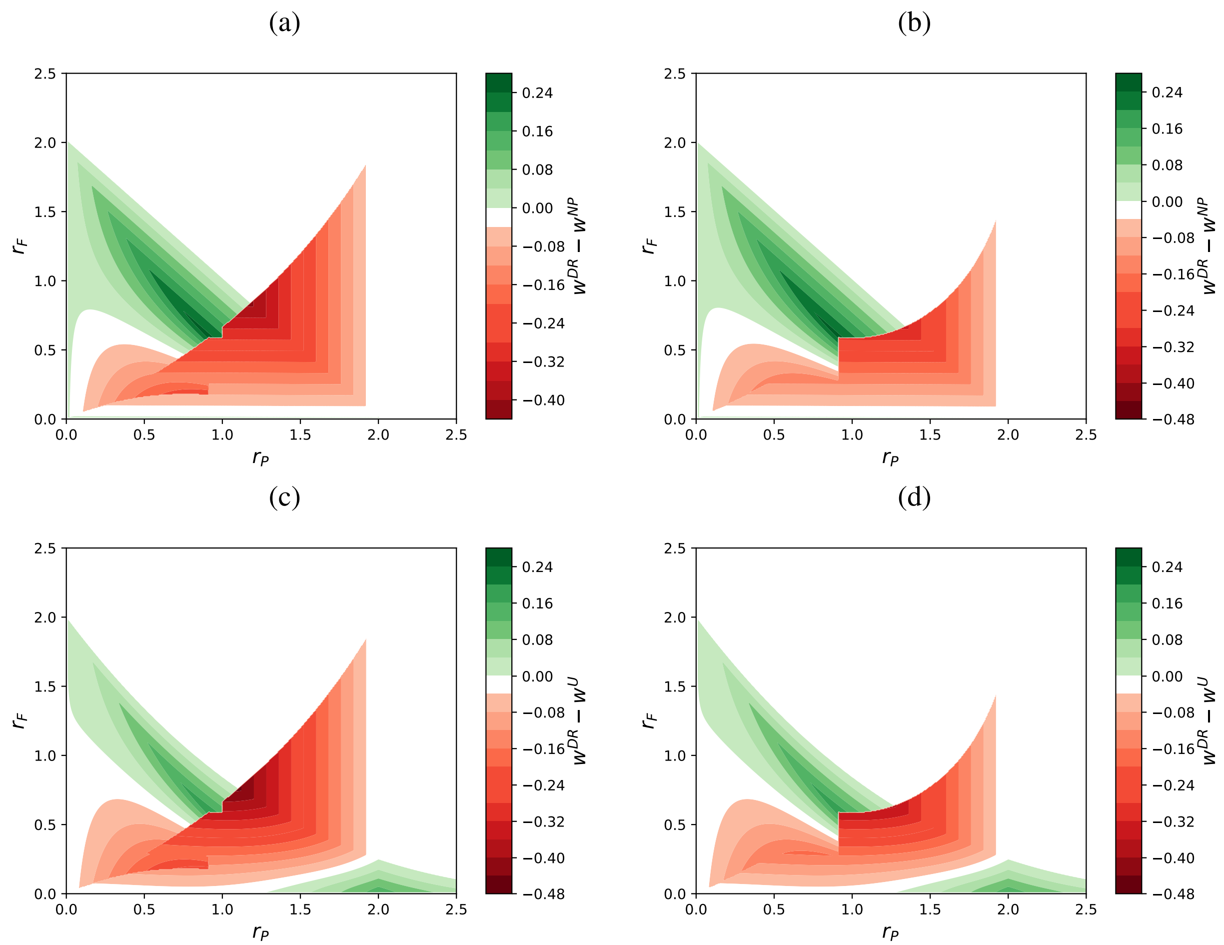
we know that their best-responses are uniquely defined:

$$q_P^*(q_F) := \arg\max_{q_P \in [0,1]} \pi_P(q_P, q_F) = \begin{cases} 1 & , \text{ if } q_F \leq \frac{r_P - 2}{r_P} \\ \min\left\{1, \frac{r_P}{2}\frac{1-q_F(1-q_F)}{2-r_P(1-q_F)}\right\} & , \text{ if } q_F > \frac{r_P - 2}{r_P} \end{cases}$$
$$q_F^*(q_P) := \arg\max_{q_F \in [0,1]} \pi_F(q_F, q_P) = \begin{cases} 1 & , \text{ if } q_P \leq \frac{r_P - 2}{r_P} \\ \min\left\{1, r_F\frac{1}{2-r_P(1-q_P)} + \frac{r_P}{2}\frac{1-q_P(1-q_P)}{2-r_P(1-q_P)}\right\} & , \text{ if } q_P > \frac{r_P - 2}{r_P} \end{cases}.$$

Moreover, algebraic manipulations show that $-2+(1-q_P^*(q_F))r_P, -2+(1-q_F^*(q_P))r_P \leq 0$ for any $q_F, q_P \in [0, 1]$, which implies that, at any equilibrium we have $q_P, q_F \geq \frac{r_P-2}{r_P}$ (trivially satisfied if $r_P \leq 2$).

For any $q_F > \frac{r_P-2}{r_P}$, $\frac{\partial^2}{(\partial q_F)^2}q_P^*(q_F) < 0$. For simplicity, we restrict ourselves to the case where $r_F \leq 3$ as $\forall r_F > 3$ we already know that consumer welfare is maximal under the dual role. Under such condition, we also have that $q_P > \frac{r_P-2}{r_P}$, $\frac{\partial^2}{(\partial q_P)^2}q_F^*(q_P) < 0$. Then, we can further verify algebraically that $q_P^*\left(\min_{1 \geq q_P \geq (r_P-2)/r_P} q_F^*(q_P)\right) \leq \arg\min_{1 \geq q_P \geq (r_P-2)/r_P} q_F^*(q_P)$, which delivers the uniqueness of an equilibrium.

Figure C1: Average Welfare Comparisons under Simultaneous Investment



Notes: This figure displays the difference in expected good quality, which corresponds to average consumer welfare on the platform, across the no platform production, dual role, and unbiased recommendation cases. Panels (a) and (b) compare welfare between the dual role and no platform production cases. Panels (c) and (d) compare welfare between the dual role and unbiased recommendation cases. Panels (a) and (c) display the comparisons where we select the equilibria in the dual role case with minimal welfare across the possible equilibria. Panels (b) and (d) display the comparisons where we select the equilibria in the dual role case with maximal welfare across the possible equilibria.

Welfare Comparisons We display the welfare comparisons between the dual role and unbiased recommendation case in Figures C1c and C1d. Since we only characterize the unique best-response functions, we compute the equilibrium for parameter values numerically. Similarly to the previous case, we plot the comparison between the equilibrium selection that induces minimal and maximal welfare in the dual role case in Figure C1c and C1d respectively. We find comparable results to the welfare comparisons in the sequential investment case.