# Serialized Content with "Wait-for-Free" Option: Access Delay as a Monetization Lever

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**Abstract**

Serialization whereby a unified content is partitioned into smaller installments has become a prevalent form of publication for digital platforms offering information goods. Recently, these platforms have begun to adopt a novel monetization strategy called "wait-for-free" (WFF) that leverages the trade-off between time and money by allowing consumers the option of free, but delayed access. This paper investigates the impact of shortening wait-times on the profitability of serialized information goods, a counter-intuitive approach that would encourage free consumption over purchases. We highlight three conceptual features unique to serialized information goods: defined sequence of consumption, complementarity between episodes and diminishing value of complementarity over time. Based on these features, we hypothesize that reducing wait-times may actually lead to higher revenues by increasing the expected valuation of episodes under uncertainty of consumer preferences. We leverage a natural experiment from a serial fiction platform to identify whether the wait-time reduction stimulates or hurts demand for paid consumption. We estimate the effect under a difference-in-difference framework, while addressing potential selection issues using a matching-based approach. We find that reduction in wait-time increases both free and paid consumption by 115% and 23%, respectively. We then conduct a battery of robustness checks rule out spurious correlations. Finally, we propose and conduct three empirical tests to validate our hypothesis and find that more consumers progress further in the series at a faster rate, which together explains the increase in revenues.

# 1 Introduction

> "In a highly competitive environment – fighting to occupy users' free time – producers and distributors of content depend more than ever on the engagement of the services they offer. The sustained, consistent and recurring consumption of narrative series has demonstrated its effectiveness above any other format in building up that engagement."
>
> (*Storytel 2018 report*, Link)

Serialized media refer to information goods such as books, TV shows or educational courses that consist of multiple episodes with a continuous plot under a single title. Modern advancements in digital media, such as the emergence of ebook and streaming platforms, have paved way for serialization as the dominant mode of publication across product types. Partitioning a unified content in short episodes is also well received as an effective format that fits the behavioral consumer trends such as growing media consumption on mobile devices and the diminishing attention span of the users (Marketing Charts, 2019; Speaking of Psychology, 2023). Such popularity amongst users and publishers alike has led to a remarkable growth of the industry. The largest platform for serialized fiction novels, Wattpad, is reported to have over 80 million readers, as well as the leading serialized comics platform, Webtoon, boasting over 85 million users. The video streaming giants Netflix, Amazon Prime Video and Disney+ together serve more than 600 million subscribers.

Against this backdrop, a personalized monetization strategy that leverages the trade-off between time and money has grown increasingly popular. In particular, firms are beginning to adopt *versioning through wait-times* or "wait-for-free" (WFF) as a business model. Under the WFF policy, consumers make consumption decisions for each episode in an à la carte fashion: they can access an episode immediately for a fixed price or for free after waiting for a pre-specified wait-time since accessing the prior episode of the same series. Hence, the access timing is personalized based on individual consumption, unlike sequential but uniform release schedules commonly observed.

To rationalize the growing adoption of the WFF policy, we must take stock of the unique structural features of *serialized* media that affect people's consumption decisions as highlighted by scholars in literary studies and semiology. First, consumers engage with each episode according to a predetermined sequence, essential for comprehending the overarching plot. Mittell (2006) calls out the narrative complexity of modern TV series, where devices such as analepses or alterations in chronology are implemented to allow the audience to slowly build their understanding of and become hooked to the storyline that gradually unfolds. This sequential engagement is not just a narrative device but also influences economic decisions, as the consumption of each episode informs the decision to proceed to the next. Second, episodes exhibit directed complementarities: consuming one episode enhances the value of the next. Each episode in a series strikes a balance between repetition and variation, diluting the idea of the ending – while adjacent episodes share structural and content similarities, they also introduce new elements that expand on the earlier episode (Eco, 1990; Kermode, 2000). This interconnectedness, in conjunction with narrative devices such as cliff-hangers and cutting-off techniques, ensures that earlier episodes increase the appeal and valuation of later ones, creating a stronger incentive for continuous consumption (Linkis, 2021). Third, the value of these complementarities diminishes over the interval since the last consumption, aligning with the notion that the consumption utility of subsequent episodes decreases as consumption capital dissipates over time (Becker and Murphy, 1988; Heather and Vuchinich, 2003). This time-sensitive aspect of valuation underscores the importance of timing in release strategies and the phenomenon of binge consumption (Schweidel and Moe, 2016; Lu et al., 2019, 2023; Godinho de Matos and Ferreira, 2020; Zhao et al., 2022), where the desire to maintain continuity leads consumers to consume episodes in close succession.

By taking advantage of such complementarity properties of serialized media, the WFF policy uses time as a discrimination device to induce consumers with high willingness-to-pay (WTP) to pay for immediate consumption, while preserving the opportunity of free but delayed consumption for consumers with lower WTP. This policy has been embraced by platforms across domains, including Webtoon (comics), Radish Fiction (books), ReelShort (videos) and Real Racing (games).

The goal of this paper is to study how changing wait-times affect consumption decisions and monetization of serialized media. Existing literature on vertically differentiated product design explore various mechanisms through which the relative positioning of the products affect consumption and revenues. Examples include product adoption through referrals (Lee et al., 2019; Kamada and Öry, 2020), network externalities (Shi et al., 2019), advertising revenues (Chiou and Tucker, 2013; Lambrecht and Misra, 2017) and consumer learning (Li et al., 2019; Deng et al., 2022). At the crux of these studies is the trade-off between new user acquisition and cannibalization. Increasing the value of the free option dissuades existing users from paying for the premium version, but at the same time attracts new users who may choose to purchase leading to increased extensive margins.

For serialized media, however, its complementarity properties lead to a richer set of consumption dynamics resulting in unique mechanisms that have a strong impact on monetization. In particular, we examine the role of *intensive margin*, a mechanism that has not been proposed or examined in this line of research. Adapting the terminology commonly used in the economics literature, we use intensive margin to capture how much a consumer consumes a *given series*, as opposed to extensive margin that captures how many consumers consume the series. If the required wait-time is reduced, a consumer's valuation for waited consumption increases, which may cause her to switch from purchasing to waiting at certain episodes (cannibalization effect). However, the complementarity properties give rise to positive across-episode spillovers within a consumer that counteract cannibalization through two channels. First, on episodes where complementarities are realized from consuming the previous episode, the consumer may switch from no consumption (outside option) to waiting or purchasing as both the realized value and the expected complementarity values on future episodes are higher. Second, this in turn allows her to realize complementarity on subsequent episodes, inducing her to make purchases at episodes where the realized value is sufficiently high. In essence, by increasing both free and paid consumption within a series, the firm is able to retain consumers over a broader product set and "harvest the acquisition" at times when they realize a high complementarity value, resulting in greater net monetization. At the same time, the well-understood aspect of extensive margin is still in play as more consumers are incentivized to start consuming the series for the first time under reduced wait-times. We aim to separate out and evaluate both the intensive and extensive margin impact, as well as their ensuing impact on aggregate consumption and revenues.

Our empirical setting involves a major U.S.-based serialized fiction novel platform that offers series under the WFF policy. We use a rich panel data of over a million users and 20,000 series that covers consumption records on the platform over a 15-month period, ranging from October 2020 to December 2021. The dataset details when and how the user accessed an episode (i.e., waiting, purchasing or using promotional gifts). We augment this dataset with various metadata on series and episodes such as publication date and the wait-time required for a series on any given day. We also leverage a panel data of in-app currency purchases to explore consumer heterogeneity based on platform spending.

We identify the causal effects of changes in wait-time on user consumption and purchase behaviors by exploiting a natural experiment where the platform unilaterally changed the wait-times for a subset of the series during our sample period. Specifically, the platform selected 191 series whose wait-times ranged from 3 to 72 hours and reduced them to 1 to 24 hours. The wait-times pre- and post-reduction as well as the magnitude of reduction vary across the selected series. The change was implemented without prior announcement and aimed to increase user engagement by making it easier for new and existing readers alike to consume episodes for free by waiting. We estimate the average treatment effect of wait-time reduction via a difference-in-differences (DiD) approach by comparing outcomes of interest within a tight time frame around the reduction.

Two empirical challenges remain: the series for which wait-times were reduced were decided by the platform, which may lead to selection bias, and the reduction was implemented in a staggered manner, which may cause a standard two-way fixed effects model to yield biased estimates. We first address selection bias using a panel matching approach (Imai et al., 2021). We compute the likelihood of a series receiving treatment (i.e., wait-time reduction) at a given time based on observed pre-treatment covariates. By constructing a control group of series whose wait-times remain unchanged but have comparable probability of being selected for reduced wait-times, we allow the treatment to be random conditional on observable covariates, satisfying conditional ignorability. We then mitigate potential concerns around staggered adoption by using a stacked DiD approach. The stacked DiD creates event-specific datasets for each of the treated series and its matched control series. By stacking the datasets based on relative periods around the reduction timing and saturating the model with cohort specific fixed effects, we circumvent the issues of biased estimates under staggered treatment adoption raised in Goodman-Bacon (2021).

Our empirical analysis proceeds in the following sequence. First, to explore the impact of changing wait-times on the intensive margin, we examine the total number of episodes consumed and purchased in a series by a consumer. For clarity, consumption encompasses all modes of accessing the episode – i.e., waited, purchased and gifted. By comparing the readers who started the series within 15 days before and after the reduction, we find a 38% increase in the total number of episodes consumed per consumer. The key estimate of interest is the impact of wait-time reduction on total purchases, as the increased consumption would only hurt platform revenues if it came at the expense of lower purchases. Our results report no significant change in the total number of episodes purchased per consumer, indicating that the cannibalization effect from increased incentives to wait for each episode is offset by the positive across-episode spillovers. Moreover, by exploring heterogeneous treatment effects based on consumer expenditure on the platform, we find that users who have spent any money on the platform in the past in fact purchase more episodes of the series post-reduction. Thus, we highlight the weakly positive increase in intensive margins – the same consumer who switches from purchasing to waiting for an episode may actually be monetized more in aggregate.

Second, we measure the change in consumption pace in response to reduced wait-times. Given the time-dependent complementarity value, shorter wait-times may endogenously increase the consumer's incentives to access the waited episode shortly after the wait-time elapses. By comparing the pace of waited consumption during the 15 days before and after the reduction, we find a 27% decrease in the time consumers waited in excess of what is required. This acceleration effect allows the consumers to progress through the series at a faster rate, leading to quicker decisions to purchase subsequent episodes. Third, we explore the impact of reduced wait-times on the extensive margin. We find that the wait-time reduction leads to a 35% increase in the inflow of new readers to the series.

Finally, to understand the net effect on consumption and revenues of the series, we measure the impact of reduced wait-times on daily aggregate consumption and purchases. We find that the reduction on average leads to an 86% and a 20% increase, respectively. This shows that despite the risk of cannibalization, the shorter wait-times actually uplift platform revenues through both expanded intensive and extensive margins. We also report the elasticity of consumption and purchases with respect to wait-times at 0.25% and 0.07%, respectively.

We then conduct a battery of robustness checks. To show that our estimated impact of wait-time reduction is indeed causal and unbiased, we conduct formal tests to confirm the parallel trends and SUTVA assumptions. We also supplement the tests with a sensitivity analysis that explains how strongly potential unobserved confounders would need to be associated with both the outcome and the treatment in order to explain away the estimated treatment effect. Moreover, to rule out spurious correlations that might drive our results, we conduct falsification tests using pseudo treatment series and dates. In addition, analyses using different subsamples of the data and model specifications show broad agreement with our main results.

The rest of the article is organized as follows. First, we discuss how this research is related to previous literature. Second, we describe institutional details and data. Third, we describe the empirical strategy and the econometric model. Fourth, we present the results and discuss the key findings. Finally, we conclude and provide future research directions.

## 2 Relationship to the Literature

Our paper contributes to the well-established literature on versioning. Versioning is a widely practiced and studied price discrimination strategy where the firm offers vertically differentiated products at different prices such that consumers with heterogeneous preferences self-select in to the the version-price pair that is targeted to them. Building on the theoretical works on product differentiation (Mussa and Rosen, 1978; Deneckere and McAfee, 1996), Shapiro and Varian (1998) and Varian (2000) laid the groundwork for versioning information goods, pointing out the economic feasibility of manipulating product quality at negligible marginal production costs.

A set of studies have investigated the economic viability of versioning information goods with an emphasis on freemium strategy. Kamada and Öry (2020) models consumers' referral behaviors to show that referral rewards and freemium contracts can be adopted to encourage word-of-mouth. Shi et al. (2019) shows that in the absence of such diffusion dynamics, the freemium model can be optimal when the two products provide asymmetric network externalities. Existing empirical research probes into the impact of introducing the free version on demand for the premium version (Gu et al., 2018; Li et al., 2019; Deng et al., 2022), when firms should charge for content (Lambrecht and Misra, 2017) and how much should be provided for free (Lee et al., 2019). In many of these cases, the key determinant of the firm's decision is the trade-off between extensive margin and cannibalization: an attractive free offering expands the consumer base, but does so at the cost of cannibalizing existing paid consumption. Our study enriches this dynamic by focusing on the intensive margin that has not been examined in the literature. Complementarity between episodes lead to increased consumption in a series per consumer, and the additional purchases made on subsequent episodes offset or even dominate the negative cannibalization effect, leading to greater monetization per consumer.

Moreover, there is limited research that exploits exogenous changes in version quality to empirically investigate the causal implications. Product quality is hard to quantify and empirical settings involving a discrete change in version quality are uncommon. An exception is Li et al. (2019), where they exogenously vary the resolution of free ebook samples. The authors distinguish between "sample quality" and "functional equivalence," where the former is the degree to which the free sample reveals the quality of the premium product (e.g., textual content, image resolution) and the latter is the degree to which the utility derived from the premium product can be obtained from the free product (e.g., limited usage time, hardcover vs. softcover). By varying sample quality, the authors show that under low functional equivalence, it may be profitable for the firm to provide high quality free samples as they serve as poor substitutes. In our study, we exploit an exogenous change in functional equivalence – consumers get full access to the exact same episode by waiting but receive lower utility from delayed consumption. Our results demonstrate that owing to the unique features of serialized media, closing the gap between the two versions in terms of functional equivalence can increase firm revenues.

Our work also relates to the literature on sequential product release or using time as a discrimination device. Firms often start with limited distribution through their primary channel and after some time release a secondary channel for mass distribution that sell at a lower margin (e.g., movie theater vs. DVDs, hardcover vs. softcover). If the inter-release timing is too short, forward-looking consumers might hold off on their purchases through the first channel, and if too long, consumers gradually lose interest (buzz decay) and decide to exit the market by the time the second channel opens (Calzada and Valletti, 2012; August et al., 2015; Luan and Sudhir, 2022). While the wait-for-free policy shares commonalities, the release timing is personalized based on the user's consumption timing and is applied to a series of products, giving way to distinct consumer dynamics.

With the proliferation of serialized media content, a growing stream of literature is giving attention to their monetization strategies. Zhao et al. (2022) investigates the connection between episode release timing and a rich set of behaviors such as binge consumption, rationing and platform visits to study the platform's optimal release schedule. The authors find that a hybrid strategy of simultaneous and sequential release strategies yield highest platform profits. Using data from a comics platform that allows early access for a fee, Choi et al. (2023) finds that habit formation gradually increases consumers' valuation and prompts them to pay for early access. To our knowledge, there are no papers that study the WFF policy with the exception of Choi et al. (2022) due to its novelty. The paper explores a setting where a comics platform adopted WFF policy on a subset of its comics for the first time and finds that the introduction resulted in a boost in free and paid viewership. Our work complements these results by exploiting an exogenous variation in wait-times to identify the causal effect on consumption dynamics and aggregate revenues.

Finally, another related stream focuses on consumption behaviors of serialized media. Zhang et al. (2022) provides evidence of time-inconsistent preferences, where consumers intentionally choose to overpay for content in order to curb future consumption (strategic self-control). Several works study the phenomenon of binge consumption, exploring the implications on downstream behaviors such as responsiveness to advertisements, series completion and spillovers to other content on the platform (Schweidel and Moe, 2016; Lu et al., 2019, 2023; Godinho de Matos and Ferreira, 2020). These papers focus on providing empirical evidence of binge consumption and abstract away from the mechanism that drives binge consumption. We add to this stream of literature by shedding light on the role of complementarity properties of serialized media that affect consumption decisions.

# 3 Institutional Details and Data

In this section, we describe the institutional details of our empirical setting and subsequently explain the data used in our empirical analysis.

## 3.1 Institutional Details

The serialized fiction market consists of three players: authors, readers and the two-sided platform. Independent authors publish their series that are comprised of multiple episodes on the platform, and the readers access each episode through a mobile application following various payment schemes. The serial fiction market has seen rapid growth globally, with notable platforms such as Wattpad and Kindle Vella.

We leverage data from a leading U.S.-based serial fiction platform specializing in the romance genre that hosts over 20,000 series and has over a million active users. The platform generates revenues through users' episode purchases. Specifically, each series belongs to one of three categories depending on the monetization type: free, premium and "wait-for-free" (WFF). Free series allow immediate access to all episodes at no cost. Premium series follow a pay-per-episode model where first several episodes are free, and users must pay using an in-app currency ("Coins") to "unlock" each subsequent episode. The only difference between WFF and premium series is that WFF allows users to unlock an episode for free once a pre-specified wait-time has elapsed after the last episode of the same series was unlocked. Like premium series, readers may alternatively pay to unlock the episode immediately. Coins can be bought with real money, and each episode costs 3 Coins regardless of wait-times, roughly equivalent to 50 cents. Users can earn Coins through other ways such as watching ads or inviting friends, but those make up a negligible portion compared to direct purchases.

The wait-time varies across series, ranging from 1 to 72 hours, and the same wait-time applies to all episodes and readers within a series. If the consumer becomes eligible to unlock an episode after the wait-time has elapsed, she must actively unlock an episode to "reset the clock" for the next free episode. Take for example a series that requires a 3-hour wait-time. A user may consume the entire series for free as long as she is willing to wait at least three hours *between each episode*. The consumer returning in 12 hours will only have a single free episode available rather than four. Hence, the frequency of visits matters, and one cannot "wait-and-binge," a behavior often observed for series with fixed release schedules. Also, note in this context that there is no incentive for a user to purchase an episode now to read later (i.e., stockpile), since it will eventually become free after waiting. To be clear, although firms have previously discriminated using time in contexts such as hardcover versus softcover books, the application to serialized media, where there are complementarities across episodes, has not been previously observed. Moreover, the release timing in WFF is personalized based on the user's consumption of the previous episode and is applied separately for each episode.

Figure 1 illustrates the distribution of series and consumption across the three sales types. Although WFF series constitute a third of all series on the platform, more than 85% of episode consumption in our dataset is generated by the WFF series. Given this pattern and our research objective, we focus only on the WFF series within the data.

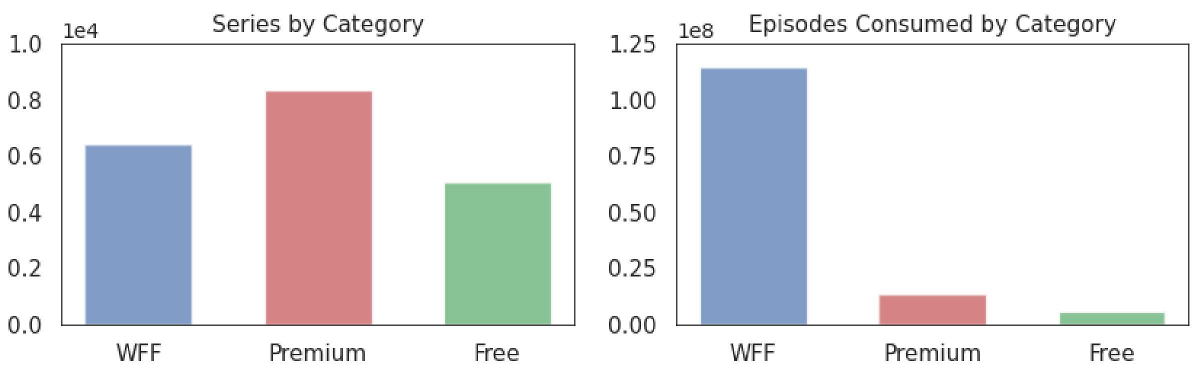


Figure 1: Distribution of series and episodes consumed across categories

Figure 2 illustrates the user experience on the app. The user can scroll through series available on the platform, and once she clicks on a series, additional relevant information is displayed, such as the wait-time, genre and a short description. In this example, "God of Wolves" is a paranormal romance series that requires a 3-hour wait-time and offers the first five episodes for free. The hourglass icon and "3 Coins" indicate that beginning with the sixth episode, the user may either wait 3 hours or pay 3 Coins to unlock.

On the left panel of Figure 3 is a sample episode. A typical episode is around 1,500 words, and the vast

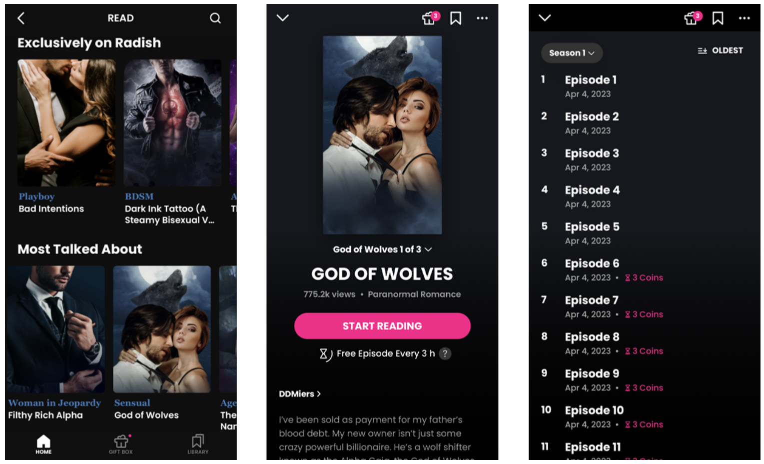


Figure 2: App User Interface

majority of the readers finish an episode within 15 minutes. At the end of the last unlocked episode, the user is presented with an option to pay to read now or wait to read for free, as shown in the right panel. The pink text on the bottom shows the wait clock ticking down.

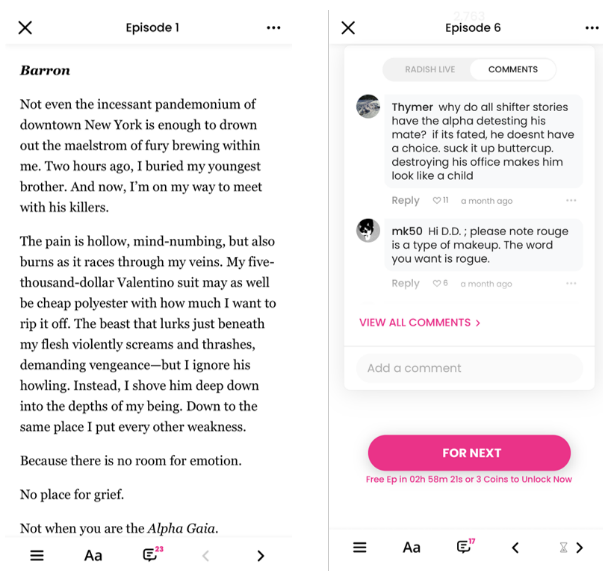


Figure 3: Wait-clock starts ticking down upon accessing the last available episode

## 3.2 Data

We leverage multiple datasets that cover series and episode metadata as well as user consumption. Series metadata include title, genre, author, sales type, date of first publication and the required wait-time for an episode. Episode metadata include series ID, sequence in the series, whether the episode is free, episode publication date and word count. The consumption panel data, which covers 15 months from October 1, 2020 to December 31, 2021, matches every user with every episode that she has read, including the exact time as well as how the episode was unlocked: "waited" if she waited for the required wait-time to elapse; "purchased" if she paid to unlock; "gifted" if she used coupons gifted by the platform (the platform occasionally offers coupons for specific series that can be used to unlock an episode). We also have access to panel data on Coin purchases from January 1, 2019 to October 30, 2022, which we leverage to explore heterogeneity across users based on historical spending behavior. Our expansive dataset comprised of detailed access timing and method over an extended time window presents a unique opportunity to delve into the consumption dynamics of serialized media.

To isolate the effect of wait-time reduction on the *existing reader base* of the platform, we filter the panel data to the readers that joined the platform before October 1, 2020, the beginning of our consumption panel data. This ensures that there are no compositional changes to the platform users that might otherwise introduce a form of selection bias. Moreover, to reduce noise from tail end series that are rarely read, we filter for series with at least non-free 1,000 episodes read during over the entire observation period. Our resulting dataset covers 1,940 WFF series and 308,681 users, basic summary statistics provided in Table 1. The median series contains 44 episodes from a single season, and the median user has read two series and 44 episodes during our observation period. Table 4 shows the distribution of the WFF series across wait-times.

| | **Mean** | **SD** | **25%** | **50%** | **75%** |
|---|---|---|---|---|---|
| Episodes per series | 79.1 | 163.2 | 31.0 | 44.0 | 83.0 |
| Series consumed per user | 14.1 | 40.2 | 1.0 | 2.0 | 8.0 |
| Episodes consumed per user | 367.0 | 1016.0 | 7.0 | 44.0 | 233.0 |
| Episodes waited per user | 312.8 | 1233.7 | 2.0 | 18.0 | 141.0 |
| Episodes purchased per user | 132.4 | 392.3 | 1.0 | 16.0 | 87.0 |

Table 1: Summary statistics for the main dataset

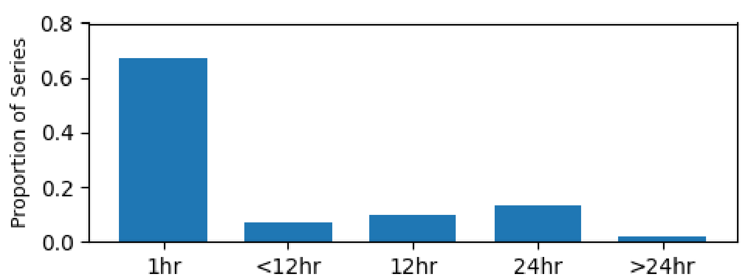


Figure 4: Distribution of series by required wait-time

We next provide a set of descriptive statistics. The left panel of Figure 5 illustrates the distribution of series by the size of their reader base, with the x-axis indicating unique reader count (log-transformed) and the y-axis indicating the number of series. The normally distributed histogram shows a heavy concentration of readers on the most popular series. The right panel of the figure illustrates the distribution of users by their purchase propensities. Users are heterogeneous in their tendency to purchase versus wait with a bimodal distribution of those that rarely and very frequently purchase.

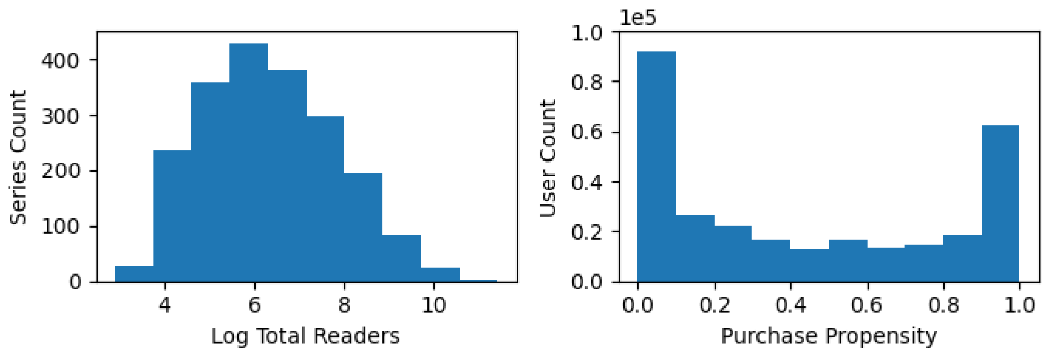


Figure 5: Distribution of readers across series and purchase propensity

Next, to provide empirical evidence in suppor we sample the consumption panel data for 10,000 randomly selected users. We find that 99% of the episodes are read along with the immediately preceding episode, and 90% of the episodes are read in sequential order. A logistic regression analysis reveals that reading an episode significantly increases the likelihood of reading the immediately subsequent episode ($p < 0.001$). In other words, the vast majority of readers read the series in the specified order of episodes and read an episode only if they have read the preceding episode.

Together with the gradually declining retention plot in Figure 8, this provides patterns consistent with the unique features of serialized media: the defined sequence of consumption and the directed complementarity between episodes that diminishes over time.

We conclude this section by exploring consumption patterns across reading sessions. Because the dataset captures when and for how long each user reads an episode, we are able to define reading sessions at a granular level. For each user, we define a reading session to be one where the interval between finishing an episode and starting the next episode is less than 30 minutes. Table 2 shows the proportion of reading sessions by the number of episodes read and the number of unique series read during the session. Given that 60% of reading sessions consist of a single episode and a median user reads four episodes per day (Table 1), we can infer that users visit the platform multiple times throughout the day mostly just to access the waited episodes and occasionally end up purchasing another episode. Moreover, the table also suggests patterns of binge consumption. 40% of reading sessions involve two or more episodes, and irrespective of session length, at least 25% of sessions involved reading episodes from a single series.

| Session Length (eps) | % Sessions | mean | 25% | 75% |
|---|---|---|---|---|
| 1 | 58.6 | 1.0 | 1 | 1 |
| 2 | 16.2 | 1.5 | 1 | 2 |
| 3 | 7.9 | 1.8 | 1 | 3 |
| 4 | 4.6 | 2.0 | 1 | 3 |
| 5 | 2.9 | 2.2 | 1 | 3 |
| 6 | 2.0 | 2.3 | 1 | 3 |
| 7 | 1.5 | 2.4 | 1 | 3 |
| 8 | 1.1 | 2.4 | 1 | 3 |
| 9 | 0.9 | 2.5 | 1 | 3 |
| 10+ | 4.3 | 2.7 | 1 | 3 |

Table 2: Unique series read, by number of episodes in the session

# 4 Empirical Strategy and Model

## 4.1 Empirical Strategy

In this section, we discuss our empirical strategy to identify the causal effect of wait-time reduction on reader consumption and purchase decisions. An ideal experiment would randomly assign different readers to different wait-times for a given series and compare outcomes between experimental designs. However, our setting features the same wait-time for all readers for a given series. Thus, we instead leverage exogenous policy changes implemented by the platform. Specifically, the platform unilaterally reduced wait-times for a certain set of series in varying degrees in a staggered manner within our observation period. For example, users that had to wait 24 hours to unlock an episode would now be able to unlock an episode every hour after the reduction. The platform indicated that the objective of the policy change was to increase overall reader engagement, but the process was rather informal without analytical evidence that called for the change or a systematic criteria. The wait-times before and after the reduction, as well as its timing varies across series, but importantly, there were no prior announcements by the platform regarding the reduction. Hence, readers could not have expected any changes to the wait-time in advance, making it exogenous to them. Similar instances can be found on other platforms where they unexpectedly implemented changes on the terms of the WFF policy such as raising the price of purchased episodes, extending the wait-time for free episodes, or making the waited episode accessible only for a limited duration (Webtoon 2022; Tapas Forum 2022).

We identified 191 series that had wait-times reduced in our dataset, which we call the *treated series*. The rest of the 1,749 series did not have any changes to their wait-times, which we call *non-treated series*. Table 3 lists the number of series from our dataset based on pre- and post-wait-time changes. The diagonal figures represent the series that did not experience changes, and the off-diagonal figures represent the treated series. Note that the treated series are all below the diagonal since the wait-times were reduced. Figure 6 illustrates the distribution of reduction dates and the magnitudes.

| pre/post | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 10 | 12 | 24 | 48 | 72 | All |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 1304 | - | - | - | - | - | - | - | - | - | - | - | - | 1304 |
| 2 | - | 40 | - | - | - | - | - | - | - | - | - | - | - | 40 |
| 3 | 12 | - | 20 | - | - | - | - | - | - | - | - | - | - | 32 |
| 4 | 6 | - | - | 38 | - | - | - | - | - | - | - | - | - | 44 |
| 5 | 1 | - | - | - | 4 | - | - | - | - | - | - | - | - | 5 |
| 6 | 6 | - | - | - | - | 7 | - | - | - | - | - | - | - | 13 |
| 7 | 1 | - | - | - | - | - | 1 | - | - | - | - | - | - | 2 |
| 8 | 1 | - | - | - | - | - | - | 6 | - | - | - | - | - | 7 |
| 10 | 1 | - | - | - | - | - | - | - | 1 | - | - | - | - | 2 |
| 12 | 45 | - | 3 | 1 | - | - | - | - | - | 139 | - | - | - | 188 |
| 24 | 62 | - | 21 | - | - | - | - | 2 | - | 8 | 165 | - | - | 258 |
| 48 | 16 | - | - | - | - | - | - | - | - | 1 | 3 | 23 | - | 43 |
| 72 | 1 | - | - | - | - | - | - | - | - | - | - | - | 1 | 2 |
| All | 1456 | 40 | 44 | 39 | 4 | 7 | 1 | 8 | 1 | 148 | 168 | 23 | 1 | 1940 |

Table 3: Number of series by wait-time (hrs) for pre- and post- change

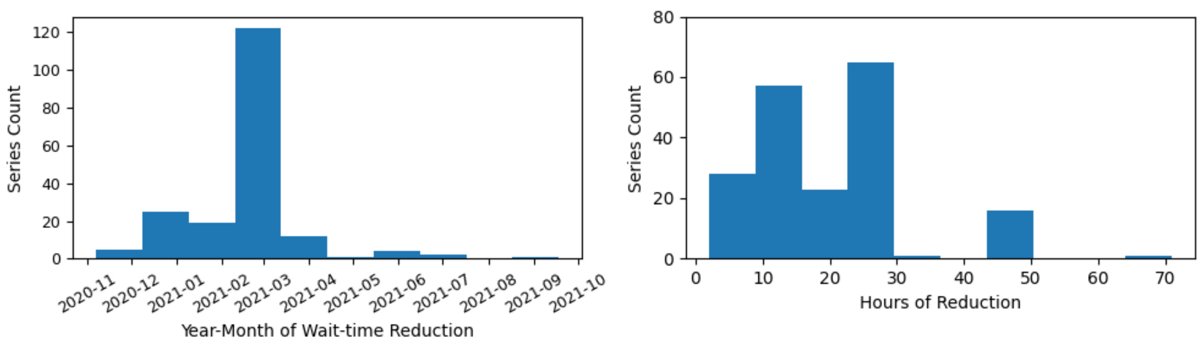


Figure 6: Distribution of treated series by treatment timing and reduction magnitude

Figure 7 compares the purchase probability for each of the 191 treated series, computed as the ratio of episodes purchased to all episodes consumed, before and after the wait-time change. Since the wait-times were reduced, the probabilities before (blue circle) and after (green square) the change are located at the right- and left-end of the connecting lines, respectively. Unsurprisingly, the purchase probabilities decline for most series, as noted from the downward sloping lines from right to left. Since the option of waiting for free has become more attractive, it intuitively leads to an increase in waited consumption. Having said that, the ultimate impact of the wait-time reduction on platform revenues remains an empirical question that requires us to go beyond purchase probabilities to carefully consider individual consumption and purchase dynamics.

To motivate the need to consider the impact on intensive margins, we visualize how retention across episodes in a given series changes upon the wait-time reduction. Figure 8 illustrates the average proportion of readers that consume the first non-free episode that proceed to consume subsequent episodes, separately across non-treated series and treated series before and after the reduction. The plots overall indicate high churn especially in the early episodes, and less than 40% of the readers remain by the fiftieth episode.

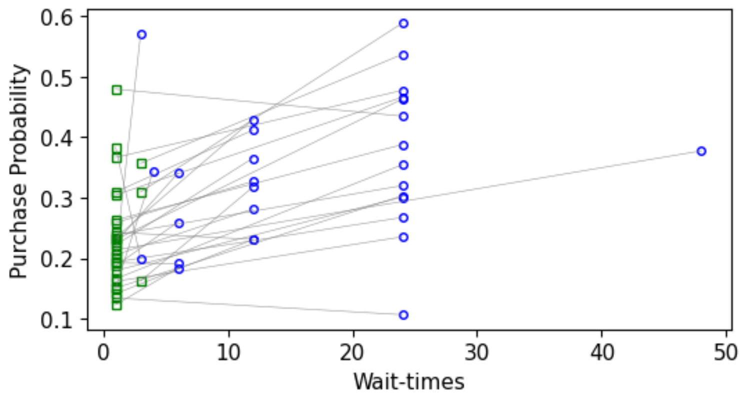


Figure 7: Distribution of treated series by treatment timing and reduction magnitude

Importantly, we see a noticeable increase in retention for the treated series after the reduction. This lends empirical support to the notion of the complementarity value diminishing over time: as the reader waits less for free access, she receives a higher complementarity value and thus decides to consume the episode rather than churn, which in turn allows her to benefit from complementarity with the next episode. Such increase in intensive margins may result in increased purchases despite the higher incentives to wait for free.

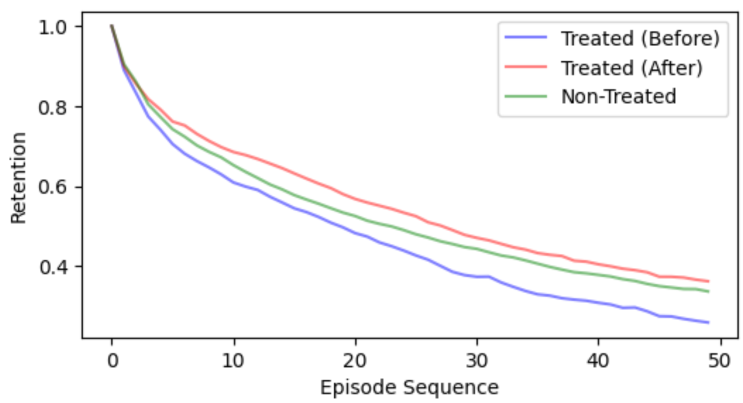


Figure 8: Retention of readers across episodes

To identify the effect of shortening the wait-time on individual consumption and aggregate demand, we focus our analysis on a tight window around the reduction. The assumption is that any changes within this brief time period can be attributed only to the wait-time reduction, controlling for a comprehensive set of features. Comparing the treated series to a set of appropriately constructed set of control series with no wait-time changes, we can estimate the average effect of wait-time reduction using a difference-in-difference (DiD) framework.

#### 4.1.1 Empirical Challenges

Our empirical context poses two main challenges. The first challenge is the selection into treatment. Although the platform confirmed that they did not have a specific selection criteria for the treated series, the selection of treated series could be endogenous, which can potentially lead to biased results when naively comparing series that are systematically different. For example, if the treated series have previously been more widely read, then the estimated treatment effect will be downward biased, as the reduction will have no effect on users that have already read the series. If the platform selected series that were recently updated with new episodes, then the estimate will be upward biased, as it is confounded with by the renewed attention from updated episodes.

The second challenge is that we have an unbalanced panel data with variation in treatment timing. Since series are published on or removed from the platform at different points in time, the observed time window varies across series (only about 6% of the series are removed during the observation period). The missing observations can lead to differences in trends before treatment, making the parallel trends assumption difficult to assess and justify. Moreover, recent econometrics literature has shown that variation in treatment timing can lead to biased average treatment effect (ATE) estimates in a two-way fixed effects (TWFE) model, especially in the presence of heterogeneous treatment effects (Borusyak and Jaravel, 2018; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). Specifically, the "forbidden comparison" of later treated units to already treated units may assign negative weights to certain sample treatment effects, thereby making the estimated ATE markedly different from the rest of the sample treatment effects.

We address these challenges by using panel-matching approach (Imai et al., 2021) and a stacked DiD model (Cengiz et al., 2019; Deshpande and Li, 2019; Baker et al., 2022; Deng et al., 2022). We first match each of the 191 treated series to a *matched control set* that consists of non-treated series that are fully observed around the treatment timing and have similar propensity score, which is the conditional probability of receiving the treatment given pre-treatment time-varying characteristics. Hence, treatment assignment is independent of potential outcomes conditional on potential outcomes, satisfying the conditional independence assumption. Any difference in observed outcomes between the matched treated and control units can be attributed to the treatment effect, thereby approximating a randomized experimental design.

We then estimate the treatment effect using the stacked DiD method, which focuses on a fixed time window around the treatment event for each treated series, effectively creating a series of "mini" DiD analyses centered on the point of treatment adoption. This approach stacks these fixed time windows to form a consolidated dataset, within which the treatment effect is estimated using a DiD model that incorporates group-specific fixed effects. By doing so, the stacked DiD model ensures that the estimation of treatment effects is grounded in a comparison of treated and control units within narrowly defined temporal contexts, thereby restoring the validity of the parallel trends assumption and reducing the risk of biased estimates arising from heterogeneous treatment effects over time. This methodological refinement allows for a more precise estimation of the treatment effect, accounting for the nuanced dynamics of staggered treatment adoption. We provide additional details on how we address the empirical challenges in the following section.

## 4.2 Constructing a Matched Control Set via Panel Matching

We first explore whether the treated and non-treated series are indeed systematically different prior to the reduction. Table 4 presents time-varying features across the two groups, as well as the p-values to compare the distributions. For each series, we observe the daily average count of episodes consumed by waiting, purchasing, and unique readers, as well as the presence of any gifted episodes or new episodes published. For the treated series, features are measured over the four weeks before their respective treatment dates, and for the non-treated series, four weeks before the earliest treatment date (or the first four weeks of publication if the former time frame is less than four weeks). We see that the two groups are significantly different across almost all measures, which suggests a simple comparison between the two groups can lead to confounding and motivates the need for a matched control group to satisfy the conditional independence assumption, $(Y(0), Y(1)) \perp\!\!\!\perp T|X$.

| | Mean | | Median | | |
|---|---|---|---|---|---|
| | Non-treated | Treated | Non-treated | Treated | p-value |
| *episodes waited* | 317.8 | 104.9 | 33.1 | 30.4 | 0.003 |
| *episodes purchased* | 83.2 | 53.8 | 12.3 | 17.6 | 0.002 |
| *readers* | 107.3 | 90.4 | 16.7 | 29.1 | 0.000 |
| $\mathbf{1}$(*gifted episodes*) | 0.11 | 0.06 | 0 | 0 | 0.075 |
| $\mathbf{1}$(*new episodes*) | 0.28 | 0.19 | 0 | 0 | 0.016 |

Table 4: Comparison of treated and untreated series

In order to address the systematic differences, we create a control group for each treated series by matching it with non-treated series that have similar probability of being treated. By making treatment independent of observed potential confounders, we are able to draw causal conclusions about the impact of reduced wait-time by comparing the two groups. We utilize the propensity score matching procedure for time-series cross-section data (panel-matching) developed in Imai et al. (2021). Despite the popularity of matching methods, almost all of the existing methods assume a cross sectional dataset using static features measured at a point in time (Abadie and Imbens, 2011; Diamond and Sekhon, 2013; Hansen, 2004). Studies involving a panel dataset compute the average of time-varying covariates over a static time-frame (Datta et al., 2018; Deng et al., 2022; Narang and Shankar, 2019), which can miss out on important time-varying factors such as demand trends leading up to treatment that affect selection into treatment.

In our setting however, the potential demand-related confounders (e.g., waited/purchased consumption, number of readers) are time-varying, and the variation in treatment timing makes it difficult to define a single pre-treatment period for the non-treated series. Moreover, matching on the average of time-varying covariates might match series whose covariates are similar on average but exhibit very different temporal trajectories. For example, a series that is gaining traction among readers and one that is becoming increasingly unpopular prior to treatment will clearly experience different effects from reduction in wait-time. Furthermore, since we have an unbalanced panel data with staggered treatment adoption, we must match each treated series to non-treated series that are observed in the same time window. As an illustrative example, Figure 9 is a treatment variation heatmap from a random sample of series. Each row represents a series, and each column represents a week from our dataset. The red (blue) areas represent treated (non-treated) series-week observations, and white areas indicate no observation (weeks when the series was not on the platform). We want to match each treated series to non-treated series that are fully observed (blue areas) around the treatment timing and are comparable in covariate values leading up to treatment.

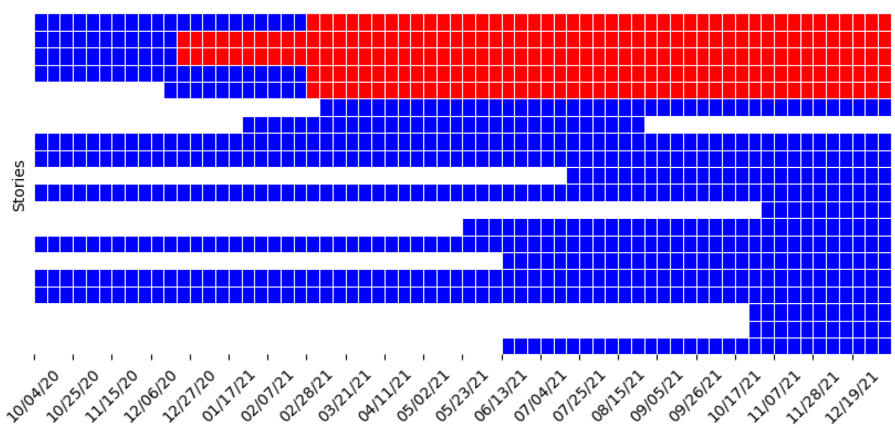


Figure 9: Treatment Variation Plot

**Constructing the Matched Control Set** We now describe the matching procedure in detail. Let us denote a treated series $s$ that receives treatment for the first time in period $t$ as observation $(s, t)$. For each treated observation $(s, t)$, we construct the matched set of never-treated units that are fully observed from time $t - L$ to $t - 1$. Imai et al. (2021) allows the matched set to include not-yet treated units, but we only allow for never treated units for a more robust comparison. Figure 10 illustrates an example of how matched sets are created when $L = 2$, indicated by the color of the boxes around the observations. In this example, treated series $s = 0$ is matched to non-treated series $s \in \{3, 4\}$ over weeks $t \in \{0, 1\}$ (blue color). Note that those non-treated series are fully observed in the two weeks prior to the treatment timing of series $s = 0$.

Series $s \in \{1, 2, 5\}$ are not included in the matched set because they are either eventually treated at a later date or are not fully observed. Similarly, series $s = 1$ is matched to series $s \in \{4, 5\}$ (red color), and series $s = 2$ is matched to series $s \in \{3, 4, 5\}$ (green color), each in their respective time window. In our case, we set $L = 4$, which assumes that adjusting for covariate trends up to previous four weeks removes most of the possible confounding. Formally, the matched set for observation $(s, t)$ is defined as

$$M_{st} = \{s' : s' \neq s,\ D_{s't'} = 0\ \forall\ t' = t, t-1, \ldots, t-L\} \tag{1}$$

where $D_{st}$ is an indicator equal to 1 if series $s$ is treated at time $t$ and 0 if not.


Figure 10: Illustrative example of constructing the matched control set

**Refining the Matched Sets** The previous matched sets only account for treatment and observation history. Next, we refine the matched sets based on propensity scores, the conditional probability of treatment assignment given observed pre-treatment covariates (Rosenbaum and Rubin, 1983). The propensity score is computed using a logistic regression based on a rich set of observed time-varying covariates prior to treatment that can reasonably discriminate the treated and non-treated series:

$$e_{st}(\{V_{s,t-l}\}_{l=1}^{L}) = Pr(D_{st} = 1 | V_{s,t-1}, \ldots, V_{s,t-L}) = \frac{1}{1 + exp(-\sum_{l=1}^{L} \beta_l^T V_{s,t-l})} \tag{2}$$

where $V_{s,t}$ is a matrix of observed time-varying covariates for series $s$ in week $t$. The covariates used in the logistic regression include weekly count of waited/purchased episodes, unique readers, and binary indicators for gifted and new published episodes over the four weeks leading up to treatment. The use of endogenous pre-treatment variables such as lagged outcomes, consumer spending and income to model propensity score is consisting with the existing research (Heckman et al., 1998; Dehejia and Wahba, 2002). These covariates serve as critical proxies for latent variables that might influence both the selection into treatment and the post-treatment outcomes of interest. By incorporating these variables, we are able to indirectly adjust for unobservable confounders and satisfy the parallel trends assumption.

Given the fitted model, we compute the estimated propensity score $\hat{e}_{st}$ for all treated observations and their matched sets. Among the series in the matched set whose propensity score distance to the treated unit is less than a defined caliper ($C = 0.1$), we select up to $N$ series (or all units if fewer than $N$ satisfy the criterion) with replacement. Formally, the refined matched set for the treated observation $(s,t)$ is given by:

$$M^*_{st} = \{s' : s' \in M_{st},\ |\hat{e}_{st} - \hat{e}_{s't}| < C,\ |\hat{e}_{st} - \hat{e}_{s't}| \leq (|\hat{e}_{st} - \hat{e}_{s''t}|)^{(N)}\} \tag{3}$$

where $(|\hat{e}_{st} - \hat{e}_{s''t}|)^{(N)}$ is the $N^{th}$ order statistic of the propensity score distance to the treated unit among the units in the original matched set.

**Covariate Balance Diagnostics** The number of matches, $N$, is set such that the best covariate balance between the treated series and their matched counterpart is achieved. Figure 11 compares the quality of covariate balance across $N = 1, \ldots, 10$. On the left panel, the x-axis indicates the number of matches, and the y-axis indicates the number of covariates that are diagnosed to be balanced based on standardized mean differences (SMD) and p-values. Lower SMDs indicate stronger balance, with literature suggesting 0.1-0.15 as the cutoff for indication of good balance (Stuart et al., 2013; Zhang et al., 2019). The plots show good balance for $N \geq 3$. The right panel of Figure 11 plots the Kullback–Leibler divergence (KLD) for the propensity scores between the treated and matched control series by the number of matches. KLD closer to zero indicate greater similarity between two distributions. We see that the similarity between the two groups are stable across $N$.

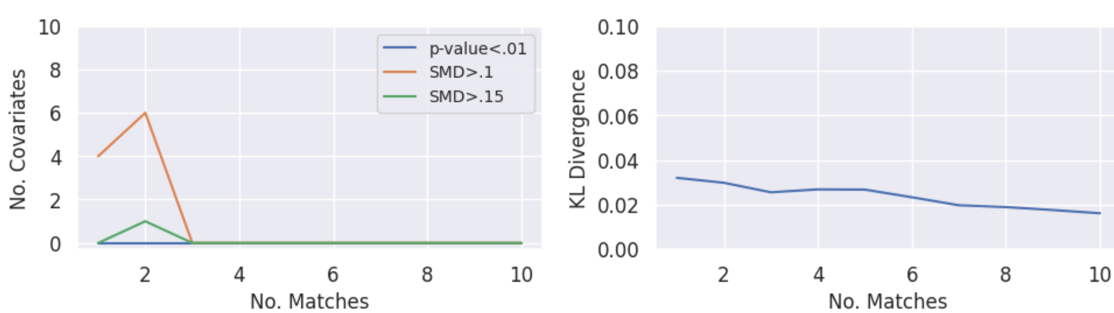


Figure 11: Assessment of match quality by the number of matches

Matching a treated unit to a single or multiple control units are both widely used in practice, each with its tradeoffs. First is the precision-bias tradeoff. One-to-one matching tends to produce less biased estimates because each treated unit is matched with its closest match, but it may yield fewer matches, resulting in less precision. On the other hand, 1:N matching could increase precision by utilizing more data (i.e., more comparisons), but may introduce bias if the matched units are not as similar to the treated unit. Since we set the difference in propensity scores of matched units to be less than a caliper $C = 0.1$, potential bias issues are limited. In terms of sample size, 1:N matching can potentially utilize all available control units, thereby maintaining a larger sample size and consequently reducing variance. At the same time, 1:N matching runs a risk of overfitting if one uses too many matches for each treated unit. In order to secure a large enough sample while against potential overfitting, we proceed with $N = 6$, beyond which some treated series start to have less eligible matched series. We conduct robustness checks to show that the results of the analysis remain unchanged for $N \geq 5$.

To evaluate the quality of propensity score matching, we check for covariate balance and overall distributions of propensity scores before and after matching (Caliendo and Kopeinig, 2008; Haviland et al., 2007). The results in Table 5 show good balance across every covariate and significant improvements in SMD before and after matching. Figure 12 is a density plot of propensity scores before and after matching. Before matching, we see a greater density of control units with low probability of treatment as expected. After matching, treated and control groups are indistinguishable in terms of their treatment propensities, indicating a strong match.

| | Treated | Control (Before Matching) | | Control (After Matching) | | % Reduction |
|---|---|---|---|---|---|---|
| | Mean | Mean | p-value | Mean | p-value | in SMD |
| Propensity Score | 0.595 | 0.405 | 0.000 | 0.594 | 0.898 | 99.106 |
| $\log(\textit{T1 waited} + 1)$ | 5.305 | 4.989 | 0.000 | 5.373 | 0.582 | 78.572 |
| $\log(\textit{T2 waited} + 1)$ | 5.267 | 5.003 | 0.000 | 5.243 | 0.845 | 90.702 |
| $\log(\textit{T3 waited} + 1)$ | 5.258 | 5.000 | 0.001 | 5.225 | 0.791 | 86.853 |
| $\log(\textit{T4 waited} + 1)$ | 5.222 | 4.987 | 0.015 | 5.159 | 0.635 | 73.625 |
| $\log(\textit{T1 purchased}+1)$ | 4.594 | 4.006 | 0.000 | 4.713 | 0.391 | 78.517 |
| $\log(\textit{T2 purchased}+1)$ | 4.655 | 4.020 | 0.000 | 4.758 | 0.429 | 82.071 |
| $\log(\textit{T3 purchased}+1)$ | 4.511 | 4.015 | 0.000 | 4.555 | 0.760 | 90.724 |
| $\log(\textit{T4 purchased}+1)$ | 4.455 | 4.020 | 0.000 | 4.528 | 0.616 | 82.354 |
| $\log(\textit{T1 readers} + 1)$ | 4.177 | 3.643 | 0.000 | 4.201 | 0.827 | 95.414 |
| $\log(\textit{T2 readers} + 1)$ | 4.141 | 3.655 | 0.000 | 4.127 | 0.900 | 97.056 |
| $\log(\textit{T3 readers} + 1)$ | 4.131 | 3.657 | 0.000 | 4.097 | 0.765 | 92.711 |
| $\log(\textit{T4 readers} + 1)$ | 4.096 | 3.659 | 0.000 | 4.053 | 0.703 | 90.081 |
| $\mathbf{1}(\textit{T1 gifted episodes})$ | 0.026 | 0.044 | 0.005 | 0.027 | 1.000 | 94.252 |
| $\mathbf{1}(\textit{T2 gifted episodes})$ | 0.031 | 0.044 | 0.005 | 0.027 | 0.640 | 60.792 |
| $\mathbf{1}(\textit{T3 gifted episodes})$ | 0.016 | 0.043 | 0.000 | 0.017 | 1.000 | 95.689 |
| $\mathbf{1}(\textit{T4 gifted episodes})$ | 0.010 | 0.045 | 0.000 | 0.014 | 1.000 | 84.860 |
| $\mathbf{1}(\textit{new episodes})$ | 0.188 | 0.139 | 0.012 | 0.167 | 0.467 | 57.364 |

Table 5: Similarity between treated and control groups before and after matching

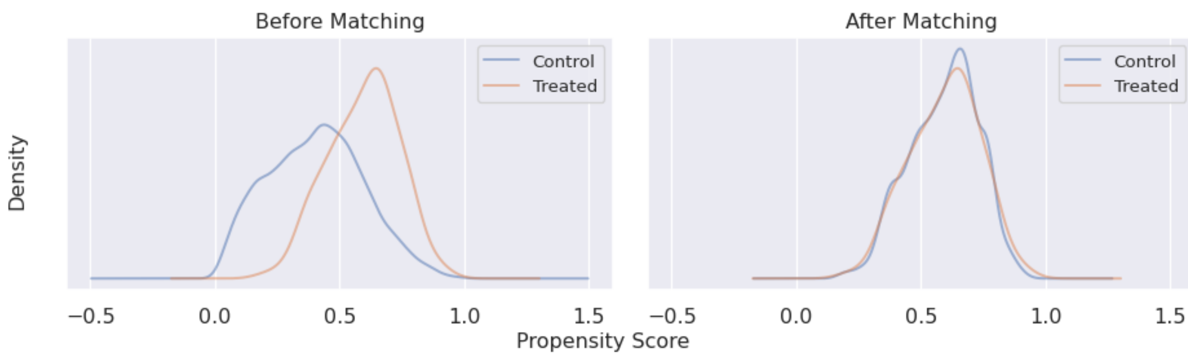


Figure 12: Propensity score distribution before and after matching

### 4.3 Econometric Model

We now turn to the econometric model to estimate the causal effect of the reduction in wait-times. To control for unobservable time-trends, we estimate the treatment effect in a difference-in-difference framework controlling for a set of fixed effects and covariates. To retrieve an unbiased measure of ATE, we must address the issue of variation in treatment timing, which can be problematic for standard TWFE models. When treatment effects vary across both time and units, then the TWFE estimand of ATE may correspond to a non-convex weighted average of individual treatment effects. Hence, we utilize a stacked difference-in-difference model. Stacked DiD has been suggested and widely used in the marketing and economics literature as a way to analyze data from a staggered treatment adoption design (Cengiz et al., 2019; Deshpande and Li, 2019; Baker et al., 2022; Deng et al., 2022).

We start by constructing event-specific datasets of equal length for each of the 191 treated series. The dataset includes the outcome and control variables of the treated series and its six matched control series, which we denote as a *series group* (or cohort) consistent with the notation from Deng et al. (2022). Note that although a non-treated series may appear in multiple series groups, the corresponding data will vary depending on the reduction timing of respective series groups. We then stack these datasets together and estimate a TWFE DiD regression with series group-specific series and period fixed effects, which fully controls for self-selection on unobserved time-invariant factors. Gardner (2022) shows that this approach estimates a convex weighted average of the individual treatment effects under parallel trends and no anticipation.

The stacked DiD model takes the following form:

$$Y_{sgp} = \beta^{DD}(after_p \times treated_s) + X_{sgp}\gamma + \delta_{sg} + \nu_{gp} + \epsilon_{sgp} \tag{4}$$

where $s$ denotes series, $g$ denotes series group, $p$ denotes period (day), and $Y_{sgp}$ denotes the main dependent variable measured for series $s$ of series group $g$ in period $p$. $treated_s$ is a binary treatment indicator for series

$s$, and $after_p$ is a post-treatment dummy for period $p$. The main coefficient of interest is $\beta^{DD}$, the average treatment effect of wait-time reduction. $X_{sgp}$ is a matrix of observable control covariates; $\delta_{sg}$ is a fixed effect specific to series $s$ in series group $g$ that captures time-invariant unobservable characteristics (referred to as *Group-Series FE*); $\nu_{gp}$ is a fixed effect specific to group $g$ in period $p$, which captures unobservable time trends such as day-of-week (referred to as *Group-Period FE*). By including group specific fixed effects, the model essentially estimates the DiD from each series group and then applies variance weighting to aggregate the treatment effects (Baker et al., 2022). Finally, $\epsilon_{sgp}$ is the error term, which are clustered at the series level.

# 5 Main Analysis and Results

The objective our paper is to analyze the impact of changing wait-times on consumption decisions and monetization of serialized media. Our theoretical framework leverages the complementarity properties unique to serialized media. We posit that under reduced wait-times, the increased complementarity value may cause the consumer to switch from no consumption to consumption, which in turn sets off a chain of events where she can benefit from complementarity on subsequent episodes from increased progress within the series. The resulting additional purchases counteracts the potential cannibalization effect, calling for the need to study the intensive margins. At the same time, we expect to see a greater inflow of new readers in to the series as the cost of waiting has become cheaper, leading to an increase in extensive margins.

The empirical analysis proceeds in four steps. First, we examine the effect of wait-time reduction on the consumer's total number of episodes purchased. This enables us to measure the net impact of positive across-episode spillover and the negative cannibalization effect on the intensive margin. Second, we examine the change in waited consumption in response to the reduction. Third, we examine the effect on the extensive margin or new reader inflow. Finally, we measure the effect of reduction on aggregate consumption and revenues, complemented with a battery of robustness checks.

## 5.1 Impact on Intensive Margins

The consumer's decision on whether and how to consume an episode depends on her net utility (valuation for the episode minus cost of time, effort, money, etc.). For serialized media, the complementarity value from having consumed the previous episode represents a significant portion of the valuation. Data patterns in Section 3.2 showed that most users typically read an episode only if they have read the preceding episode, providing supporting evidence. Conversely, if a consumer decides not to consume an episode and thus is unable to benefit from complementarity, she will likely not consume any of the subsequent episodes, including episodes that she would have otherwise purchased. In aggregate, the platform loses out on the sales of not only that episode, but also all following episodes.

Reducing wait-times runs the risk of cannibalizing purchases because the increase in the value of free consumption is more pronounced relative to paid consumption, as widely documented in the existing freemium literature (Lee et al., 2019; Li et al., 2019; Cao et al., 2023). Nonetheless, it can serve as a solution to this problem by allowing consumers to realize the complementarity on more episodes. As an illustrative example, take a consumer that consumed Episode 1 and decided to not consume Episode 2. She does not consume any episode beyond the second since she cannot benefit from complementarities. Under reduced wait-times, she may switch to waiting for or purchasing Episode 2 for two reasons. Her valuation of waited consumption increases because the realized complementarity value on that episode is higher (complementarity value diminishes over a shorter interval), and consuming Episode 2 yields complementarities on subsequent episodes whose *expected value* is also higher. There is uncertainty around the exact complementarity value on the subsequent episodes until it is realized by reading the preceding episode, but the expected value increases because it diminishes over a shorter waiting period and it is more likely that she will actually benefit from it. By consuming Episode 2, she realizes the complementarity value on Episode 3, and in the case that it is sufficiently high, she may choose to purchase. Essentially, the firm gains greater option value by retaining consumers over a longer period that can potentially generate future cash flow. Nonetheless, this is an empirical question as the consumer is relatively more incentivized to wait for than purchase each episode.

We begin the empirical analysis by sampling two groups of readers for every treated series and its matched control series. The groups include users who first started reading the series within 15 days before and after treatment. For every reader, we measure the total number of episodes consumed and purchased in a given series. The assumption is that given the tight 30-day time frame, any differences in individual consumption patterns for the treated series relative to the control series can be attributed to the wait-time reduction.

We first investigate the impact of wait-time reduction on total consumption by running the stacked DiD regression from Equation 4 with two periods. The outcome variable is the total number of episodes consumed by a reader for a given series, log-transformed to address the skewed distribution of the data. The model controls for observable characteristics that may affect consumption, including the number of published episodes, days since first and last publication and the number of episodes gifted. In addition to the series group specific series- and period-fixed effects, we also include individual-fixed effects based on pre-treatment spending behavior. Specifically, we categorize the readers into two types based on whether they have ever purchased Coins on the platform prior to the earliest treatment date. The stacked dataset consists of 25,026 spenders (44%) and 32,302 non-spenders (56%).

Estimation results are presented in column (1) of Table 6. We find that the reduction of wait-times lead to a significant increase in episodes consumed, meaning readers on average progress further in the series. The direction of the result is expected as shorter wait-times encourage readers to consume episodes who would have otherwise chosen not to. To assess the magnitude of the treatment effect, denote the consumption count before and after the reduction as $N_0$ and $N_1$, respectively. Using $\hat{\beta}$, the estimated coefficient of $after \times treated$, we can compute $N_1 = e^{\hat{\beta}}(N_0 + 1)$ and the percentage change in the dependent variable as $(N_1 - N_0)/N_0$. The estimate 0.451 in column (1) suggests that if the episodes consumed per series per reader before the reduction is at the mean (24.3), it would increase to 33.5, a 38% increase. Column (2) investigates the heterogeneous treatment effect based on pre-treatment spending behavior. The coefficient on $after \times treated \times spender$ is the incremental effect on readers who have spent any money on the platform prior to treatment. We find that the effect on the spenders in terms of total consumption is 29% greater compared to non-spenders.

| | log(Consumed + 1) | |
|---|---|---|
| | (1) | (2) |
| $after \times treated$ | 0.310*** | 0.270*** |
| | (0.084) | (0.081) |
| $after \times treated \times spender$ | | 0.077*** |
| | | (0.029) |
| log(*no. episodes*) | -0.178 | -0.178 |
| | (0.257) | (0.258) |
| log(*days since first pub.*) | -0.384** | -0.384** |
| | (0.177) | (0.177) |
| log(*days since last pub.*) | 0.018 | 0.018 |
| | (0.027) | (0.027) |
| log(*gifted episodes*) | 0.522*** | 0.522*** |
| | (0.065) | (0.065) |
| Group-Series FE | Y | Y |
| Group-Period FE | Y | Y |
| Individual FE | Y | Y |
| N Obs | 382995 | 382995 |
| N Series Groups | 188 | 188 |
| R-squared Adj. | 0.043 | 0.043 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 6: Treatment Effect on Total Consumption per Series per Reader

Next, we estimate the impact of wait-time reduction on the total number of episode purchases made by a reader for a given series. Increased consumption would only be detrimental to revenues if it came at the expense of less purchases. Reduced wait-times lower the incentives for a reader to purchase each episode, but also increases the WTP for subsequent episodes due to complementarity, which may result in incremental purchases. Results using the same model specification as above are presented in column (1) of Table 7. The estimated coefficient of $after \times treated$ is not statistically significant, and we are unable to reject the null hypothesis that total purchases per reader per series remain unchanged from the wait-time reduction.

Moreover, this overall null effect masks the both the positive and negative heterogeneous treatment effects that have critical implications on platform revenues. That is, there are price sensitive readers unwilling to spend money on the platform irrespective of wait-times. However, since those readers do not contribute to revenues in the first place, the priority of the platform is how the purchase behaviors of spenders change. Column (2) reports the heterogeneous treatment effects for spenders and non-spenders. Although total purchases for non-spenders significantly decrease (-0.201), the incremental effect on spenders is significantly higher (0.292), leading to a slight increase in total purchases. At the pre-treatment mean of total purchases for non-spenders (1.6), it would merely decrease to 1.2, whereas for the spenders, it would increase from 6.7 to 7.4, a 11% increase. Note that non-spenders have positive purchases because there are alternative ways of obtaining Coins other than purchasing with real money, such as referrals, watching ads or giveaway events.

| | log(Purchased + 1) | |
|---|---|---|
| | (1) | (2) |
| $after \times treated$ | -0.049 | -0.201*** |
| | (0.054) | (0.053) |
| $after \times treated \times spender$ | | 0.292*** |
| | | (0.060) |
| log(*no. episodes*) | -0.001 | -0.002 |
| | (0.349) | (0.349) |
| log(*days since first pub.*) | -0.131 | -0.131 |
| | (0.241) | (0.241) |
| log(*days since last pub.*) | 0.056 | 0.056 |
| | (0.040) | (0.040) |
| log(*gifted episodes*) | -0.095** | -0.095** |
| | (0.038) | (0.038) |
| Group-Series FE | Y | Y |
| Group-Period FE | Y | Y |
| Individual FE | Y | Y |
| N Obs | 382995 | 382995 |
| N Series Groups | 188 | 188 |
| R-squared Adj. | 0.046 | 0.047 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 7: Treatment Effect on Intensive Margins

For robustness, we conduct the analysis using an extended window of 30 days before and after the reduction. Alternatively, to compare consumption explicitly under different wait-time regimes, we drop readers that are observed to continue reading the series either after the treatment date (*before group*) or 15 days after treatment date (*after group*). Hence, we have a clean comparison of readers that started and churned from a series just before and after the wait-time reduction. In both cases, our results remain qualitatively unchanged. In sum, we find that providing more for free may be a transient, rather than an unequivocal, cannibalization, which is completely offset and in some cases dominated by the positive across-episode spillovers due to complementarity.

## 5.2 Impact on Pace of Waited Consumption

Platforms focus on maintaining regular and frequent user engagement with the product as it is a critical driver of customer lifetime value (Fader et al., 2005). The WFF policy encourages such behavior by setting wait-times conditional on the user's consumption timing of the last waited episode of the series. This unique feature prevents irregular binge behavior and ensures that the reader periodically visits the series to (1) claim the free episode and (2) reset the clock for another free episode by reading. In order to establish frequent engagement, it is important that the reader accesses her free episode shortly after she becomes eligible. By reducing wait-times, the platform endogenously strengthens the reader's incentive to quickly access the free episode. Again due to the diminishing complementarity over time, her valuation for waited consumption of the current and subsequent episodes, as well as the probability of waiting for the subsequent episode are higher. This acceleration (time-shifting) effect prompts readers to make consumption decisions at a faster pace, in some cases leading to greater purchases.

To empirically explore this dynamic, we investigate how wait-times affect *excess wait-time*. We define excess wait-time to be the time interval between when an episode becomes eligible to be consumed for free for a given reader and when she actually consumes it. Low excess wait-time would indicate that the reader consumed the free episode as soon as the free version became available. Using all waited consumption during 15 days before and after the treatment date for each treated series and its matched control series, we run the stacked DiD regression from Equation 4 with two periods, with excess wait-time (log-transformed) as the dependent variable. The model controls for observable characteristics that may affect consumption pace, including the number of published episodes, relative position of the episode, computed as the ratio of episode number divided by the number of published episodes, and days since first and last publication. Also included are series group specific series- and period-fixed effects, as well as individual-fixed effects based on pre-treatment spending. The stacked dataset consists of 24,248 spenders (43%) and 31,867 non-spenders (57%).

Table 8 reports the estimates. The coefficient on $after \times treated$ in column (1) reports a significant decrease in excess wait-time. If the excess wait-time before the reduction is at the mean (3.6 hours), it would decrease to 2.6 hours, a 27% decrease. The heterogeneous treatment effects estimated in column (2) report that the effect is higher for spenders by 12%. We confirm the robustness of our findings to a wider window of 30 days before and after the treatment date.

| | log(Excess Wait-time + 1) | |
|---|---|---|
| | (1) | (2) |
| $after \times treated$ | -0.238*** | -0.225*** |
| | (0.052) | (0.050) |
| $after \times treated \times spender$ | | -0.026* |
| | | (0.015) |
| *episode position* | 0.425* | 0.425* |
| | (0.242) | (0.242) |
| log(*no. episodes*) | 0.351 | 0.352 |
| | (0.383) | (0.383) |
| log(*days since first pub.*) | 2.368*** | 2.368*** |
| | (0.290) | (0.290) |
| log(*days since last pub.*) | -0.169*** | -0.169*** |
| | (0.036) | (0.036) |
| Group-Series FE | Y | Y |
| Group-Period FE | Y | Y |
| Individual FE | Y | Y |
| N Obs | 7887774 | 7887774 |
| N Series Groups | 191 | 191 |
| R-squared Adj. | 0.018 | 0.018 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 8: Treatment Effect on Excess Wait-time

### 5.3 Impact on Extensive Margins

Next, we investigate the impact of wait-times on the extensive margins or the inflow of new readers to the series. The expansion of extensive margins (more users) from increased value of the free option is well-understood from the freemium literature. In the case of serialized media, a user decides to start a series if the aggregate expected utility from the episodes outweighs the start-up cost. The aggregate expected utility increases under reduced wait-times due to higher complementarity, and more consumers will now choose to start the series, expanding the total size of the pie. Analogous to retailers incentivizing store visits through loss leaders (Hess and Gerstner, 1987), the series lures in a larger traffic of new readers in the hopes that they will purchase episodes conditional on starting the series.

To measure the impact on new reader acquisition at the series level, we estimate Equation 4 with two periods, using the number of new readers (log-transformed) for a given series during the 15 days before and after the reduction as the DV. The control variables include the number of published episodes, days since first and last publication, and an indicator for whether any episode gifts were given during the period. As the analysis is on a series level, we include series group specific series- and period-fixed effects.

Results in column (1) of Table 9 demonstrate that the wait-time reduction leads to a significant increase in new readers. At the pre-treatment mean of 96, the wait-time reduction increases the number of new readers to 130, a 35% increase. Columns (2) and (3) report regression results separately for spenders and non-spenders. The treatment effects are nearly identical for both groups – at the pre-treatment mean, the number of new spenders increase from 47 to 64 (36% increase), and the number of new non-spenders increase from 49 to 66 (36% increase). The results are robust to a wider time window of 30 days before and after treatment.

| | log(New Readers + 1) | | |
|---|---|---|---|
| | (1) All Readers | (2) Spenders | (3) Non-spenders |
| $after \times treated$ | 0.297*** | 0.303*** | 0.304*** |
| | (0.080) | (0.081) | (0.084) |
| log(*no. episodes*) | 0.434 | 0.953 | 0.183 |
| | (0.697) | (0.752) | (0.769) |
| log(*days since first pub.*) | -1.517** | -1.373** | -1.704*** |
| | (0.590) | (0.579) | (0.616) |
| log(*days since last pub.*) | 0.060 | 0.053 | 0.070 |
| | (0.060) | (0.066) | (0.061) |
| $\mathbf{1}$(*gifted episodes*) | 1.127*** | 1.166*** | 1.100*** |
| | (0.258) | (0.261) | (0.260) |
| Group-Series FE | Y | Y | Y |
| Group-Period FE | Y | Y | Y |
| N Obs | 2670 | 2670 | 2670 |
| N Series Groups | 191 | 191 | 191 |
| R-squared Adj. | 0.088 | 0.087 | 0.082 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 9: Treatment Effect on Extensive Margins

## 5.4 Impact on Aggregate Consumption and Revenues

Thus far, we have found empirical evidence of rich consumption dynamics in connection with changing wait-times that arise due to the complementarity properties. Under shorter wait-times, readers are likely to receive higher complementarity value, and as a result, consume more episodes in a series. Although they face lower incentives to purchase each episode, the downward shift in total purchases is offset by additional purchases from the incremental consumption. For readers who have spent any money on the platform, the intensive margins are left unchanged. The higher complementarity value also accelerates the pace of waited consumption, allowing potential purchase decisions to be made quicker. Because readers perceive higher a priori value of the series from increased complementarity, it prompts more readers to start consuming the series, leading to an expansion of extensive margins. However, the central question from the perspective of the firm remains: What is the net effect of the wait-time reduction on aggregate consumption and platform revenues?

To answer this, we analyze the change in aggregate consumption and purchases at the series-day level over the 15 days before and after the wait-time reduction. We use four dependent variables: the daily number of episodes consumed and purchased for a given series (log-transformed), and both on a per reader level, divided by the number of daily unique readers. We control for a host of observable characteristics that may affect aggregate demand, including the number of published episodes, days since first and last publication, the number of episodes gifted, as well as its 1-week lag to account for potential carry-over effects. Again, we include series group specific series- and period-fixed effects.

The results of our main analysis presented in Table 10 demonstrate a significant positive effect of the wait-time reduction on daily aggregate consumption and purchases. Column (1) shows the impact on total daily consumption, $\log(Consumed + 1)$. The estimate 0.616 in column (1) suggests that if the daily episodes consumed before the reduction is at the mean (181), holding all else equal, it would increase to 336, a 86% increase. The daily aggregate purchases is the platform's primary concern as it is directly linked to revenues, and it would be troubling if consumption increased at the expense of purchases. Column (2) shows that those concerns are unwarranted and rather shows that the positive impact from increased complementarity exceeds the negative cannibalization effect. It suggests that if the daily purchased episodes before the reduction is at the mean (58), it would increase to 69, a 20% increase. The estimates of the effect on daily consumption and purchases per reader shown in columns (3) and (4) are also consistent, indicating an increase of 1.56 and 0.14 episodes, respectively.

To measure the elasticity of consumption and purchases with respect to wait-times, we conduct the analyses in columns (1) and (2) using the change in wait-times. Specifically, we replace $after \times treated$ with $after \times treatment\ intensity$, where treatment intensity is defined as the log-difference in wait-times pre- and post-treatment. The estimated coefficient of the log-log specification yields the elasticity. The signs and significance remain unchanged, showing robustness of our results to using the intensity of treatment rather than a binary indicator. We find that a 1% reduction in wait-times lead to 0.25% ($^{***}p < .01$) and 0.07% ($^{***}p < .01$) increase in daily aggregate consumption and purchases, respectively. We also check for non-linearity by including an interaction term with treatment intensity squared, but the estimated coefficients are not statistically significant.

## 5.5 Robustness Checks

### 5.5.1 Identifying Assumptions

Causal identification of the DiD estimate holds under the assumptions of parallel trends, no anticipation and the stable unit treatment value assumption (SUTVA). The parallel trends assumption requires that the treatment group would have had an identical trend to the control group had the treatment not been

| | (1) log(Consumed+1) | (2) log(Purchased+1) | (3) Consumed PR | (4) Purchased PR |
|---|---|---|---|---|
| $after \times treated$ | 0.616*** | 0.180*** | 1.564*** | 0.135*** |
| | (0.051) | (0.053) | (0.070) | (0.032) |
| log(*no. episodes*) | 1.140*** | 0.455 | 0.669** | -0.223 |
| | (0.406) | (0.374) | (0.310) | (0.212) |
| log(*days since first pub.*) | -0.004 | -0.081 | -0.461 | -0.154 |
| | (0.437) | (0.376) | (0.300) | (0.139) |
| log(*days since last pub.*) | -0.163*** | -0.213*** | 0.118*** | -0.008 |
| | (0.040) | (0.044) | (0.043) | (0.017) |
| log(*gifted episodes*) | 0.204*** | 0.101*** | 0.176*** | -0.036*** |
| | (0.016) | (0.016) | (0.022) | (0.012) |
| log(*T7 gifted episodes*) | 0.093*** | 0.106*** | -0.003 | 0.006 |
| | (0.016) | (0.015) | (0.013) | (0.007) |
| Group-Series FE | Y | Y | Y | Y |
| Group-Period FE | Y | Y | Y | Y |
| N Obs | 40050 | 40050 | 40050 | 40050 |
| N Series Groups | 191 | 191 | 191 | 191 |
| R-squared Adj. | 0.097 | 0.032 | 0.099 | 0.002 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 10: Treatment Effect on Daily Aggregate Consumption and Purchases

implemented, and the no anticipation assumption requires that in periods prior to treatment, the outcome variable was not affected by the upcoming treatment. If these two assumptions hold, then any time-varying unobservables will be absorbed by the trends in the control group and the model would yield unbiased estimates. In our empirical setting, the platform unilaterally implemented the reduction without notifying the users in advance, which prevents any strategic action from the users such as delaying consumption or purchase. To formally test the parallel trends assumption, we follow the approach commonly used in the literature that exploits the pre-treatment time series in the panel data (Angrist and Krueger, 1999; Bronnenberg et al., 2020). Using only the pre-treatment periods, we run the analysis from Section 5.4 replacing $after \times treated$ with $period \times treated$. As shown in Table 11, the deviation from the common trend for the treatment series is very small and not statistically significant. Thus, we fail to reject the null hypothesis that the trend of the treated series is not significantly different from the control series, providing support of the parallel trends assumption.

We provide further evidence that parallel trends and no anticipation assumptions are likely to hold by estimating a dynamic specification of Equation 4 using a period indicator $I_p$ that allows the treatment effect to vary by period. Figure 13 plots the estimated treatment effect by period. The estimated treatment effects are not significant prior to treatment, further indicating that the common trends assumption is likely to hold.

| | (1) log(Consumed+1) | (2) log(Purchased+1) | (3) Consumed PR | (4) Purchased PR |
|---|---|---|---|---|
| $period \times treated$ | -0.002 | -0.010 | 0.000 | -0.006 |
| | (0.005) | (0.007) | (0.005) | (0.005) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 20025 | 20025 | 20025 | 20025 |
| N Series Groups | 191 | 191 | 191 | 191 |
| R-squared Adj. | 0.080 | 0.032 | 0.009 | 0.000 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 11: Parallel Trends

$$Y_{sgp} = \sum \beta_p^{DD}(I_p \times treated_s) + X_{sgp}\gamma + \delta_{sg} + \nu_{gp} + \epsilon_{sgp} \tag{5}$$

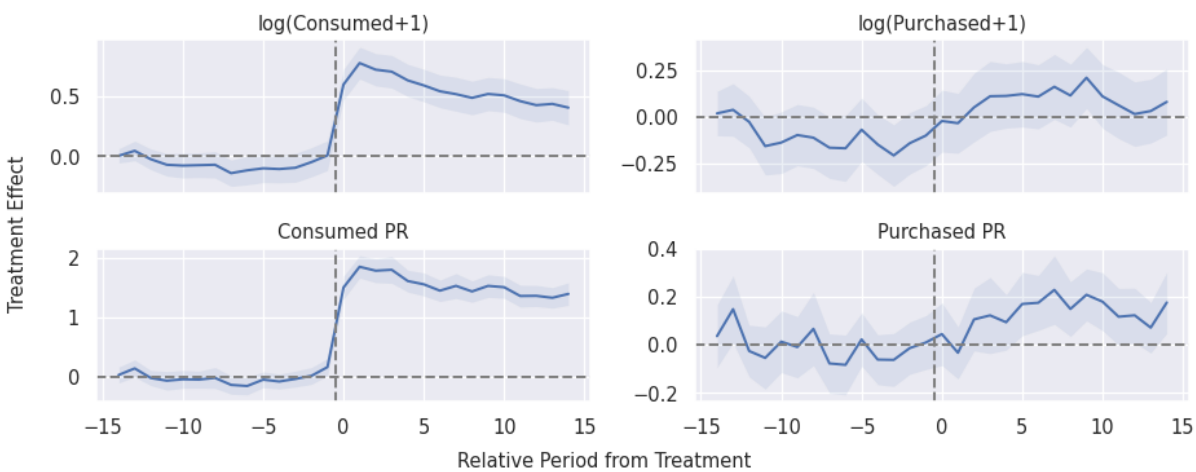


Figure 13: Estimated Treatmenent Effect by Period

In addition, we conduct a sensitivity test following Cinelli and Hazlett (2020) and Gibson and Zimmerman (2021) to understand how much deviation from parallel trends would be required to undermine the conclusions of our analysis, which assumes the parallel trends assumption is true. Cinelli and Hazlett (2020) reformulates the classical omitted variables framework to develop a sensitivity analysis that provides, relative to an observed covariate benchmark, how strongly unobserved confounders would need to be associated with both the outcome and treatment variables (in terms of partial $R^2$) to explain away the estimated treatment effect. The key advantage of using $R^2$ is that it is scale-free and does not require distributional assumptions of unobserved confounders as well as on the treatment assignment mechanism. As the benchmark observed covariate, we do not rely on a single variable, but rather include all observed covariates from Table 10 to be more conservative. The results presented in Figure 14 show that even if the unobserved confounders are twice as strong as the combined explanatory power of the benchmark covariates, the effects remain consistent.

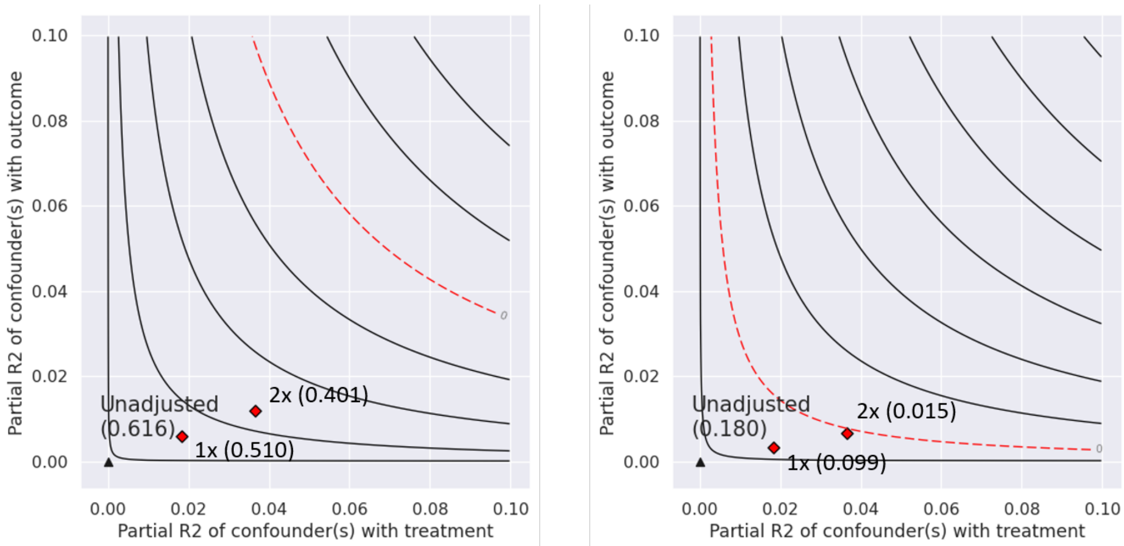


Figure 14: Sensitivity contour plots of estimated treatment effect on daily consumption (left) and purchases (right)

SUTVA states that the potential outcomes of each unit are not influenced by the treatment assignment or outcomes of other units (i.e., no spillover effect between units). For example, there may be potential substitution where readers move away from non-treated series to spend more time reading stories with reduced wait-times. There also may be complementary effects where readers consume episodes of the treated series more frequently and in doing so also read the untreated series more frequently. In our empirical context, the platform hosts over 10,000 series on the platform, and treatment was implemented at various times across only 191 series. Hence, it is unlikely that a reduction in wait-time for any series had a meaningful spillover effect on other series on the platform.

Nonetheless, to mitigate the bias from the potential violation of SUTVA, we run a subsample analysis using control series with minimal overlap in the reader base with the treated series. The idea is that if there are any substitution or complementary spillover effects, the untreated series who share more readers in common with the treated series should be affected more. Specifically, we define $overlap_{sg}$ as the proportion of readers of series $s$ who have also read an episode of the treated series in group $g$ during the 15 days prior to treatment. Then, we conduct the analysis from Section 5.4 on a subsample dropping all control series with an overlap greater than 10%. Alternatively, we estimate the treatment effects by explicitly controlling for potential interference between series, motivated by the approach from Clarke (2017) and Jo et al. (2020). By including an interaction term $after \times overlap$, we run the following regression which ensures that the treatment effect is isolated from any spillover effects:

$$Y_{sgp} = \beta^{DD}(after_p \times treated_s) + \rho(overlap_{sg} \times treated_p) + X_{sgp}\gamma + \delta_{sg} + \nu_{gp} + \epsilon_{sgp} \tag{6}$$

where $\rho$ represents the spillover effect from the treated to the untreated series. The results shown in Table 12 provides empirical support for SUTVA. The treatment effect estimates from the subsample analysis (top-half) and explicitly controlling for influence of reader base overlap (bottom-half) remain similar to the main analysis.

| | (1) log(Consumed+1) | (2) log(Purchased+1) | (3) Consumed PR | (4) Purchased PR |
|---|---|---|---|---|
| $after \times treated$ | 0.617*** | 0.188*** | 1.540*** | 0.130*** |
| (Subsample Analysis) | (0.053) | (0.054) | (0.073) | (0.034) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 37020 | 37020 | 37020 | 37020 |
| N Series Groups | 188 | 188 | 188 | 188 |
| R-squared Adj. | 0.096 | 0.031 | 0.093 | 0.002 |
| $after \times treated$ | 0.605*** | 0.173*** | 1.537*** | 0.119*** |
| | (0.054) | (0.055) | (0.073) | (0.034) |
| $after \times overlap$ | -0.430 | -0.243 | -0.859** | -0.445* |
| | (0.360) | (0.323) | (0.384) | (0.227) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 40050 | 40050 | 40050 | 40050 |
| N Series Groups | 191 | 191 | 191 | 191 |
| R-squared Adj. | 0.093 | 0.030 | 0.091 | 0.002 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 12: Check for potential violation of SUTVA using reader base overlap

### 5.5.2 Falsification Tests

We test the possibility that the estimates in Table 10 are coincidentally picking up spurious effects by using pseudo treatment indicators and dates. For pseudo treatment indicators, we randomly assign a control series as treated for each matched series group and estimate the same model without the actual treated series. Under this falsification test, since the pseudo treatment indicator does not reflect the true information of whether the wait-time of the series is reduced, the estimated treatment effects should be insignificant (Ghose and Todri-Adamopoulos, 2016; Jo et al., 2020). For the pseudo treatment dates, we manipulate the treatment date to be 15 days prior to the actual date of reduction. Since the modified time frame does not include the actual treatment date, the estimates should again be insignificant. Table 13 shows that the coefficients on $after \times treated$ are indeed statistically insignificant for both specifications, indicating that our findings are not a statistical artifact of our specification.

|  | (1) log(Consumed+1) | (2) log(Purchased+1) | (3) Consumed PR | (4) Purchased PR |
|---|---|---|---|---|
| Pseudo Treatment Indicator |  |  |  |  |
| $after \times treated$ | -0.026 | 0.018 | -0.055 | -0.018 |
|  | (0.037) | (0.037) | (0.036) | (0.023) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 34320 | 34320 | 34320 | 34320 |
| N Series Groups | 191 | 191 | 191 | 191 |
| R-squared Adj. | 0.070 | 0.030 | 0.010 | 0.001 |
| Pseudo Treatment Date |  |  |  |  |
| $after \times treated$ | -0.044 | -0.017 | -0.038 | -0.023 |
|  | (0.055) | (0.064) | (0.031) | (0.035) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 39870 | 39870 | 39870 | 39870 |
| N Series Groups | 191 | 191 | 191 | 191 |
| R-squared Adj. | 0.056 | 0.026 | 0.017 | 0.000 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 13: Results of Falsification Tests using Pseudo Treatment Indicators and Dates

### 5.5.3 Alternative Explanations and Model Specifications

Next, we explore whether the estimated treatment effect could arise from the platform strategically timing the wait-time reduction. One alternative explanation for the positive treatment effect is that the platform sends episode gifts in anticipation of or simultaneously with the wait-time reduction to draw reader attention. If this were the case, our estimates might reflect the effect of those marketing activities rather than the sole effect from shorter wait-times. Another explanation is that the timing of the reduction is coordinated with new episode releases, in which case the estimated effect would again be confounded by renewed reader interest from the new episode. Although we control for these factors in our main analysis, we re-estimate the model by entirely removing treated and non-treated series that sent any gifts or had any new episode releases within our 30-day timeframe. Results are presented in Table 14, and for both specifications, the effects remain qualitatively unchanged.

We also confirm the robustness of our results using a subsample of the treated series with the same post-reduction wait-time. As show in Table 3, there is variation in wait-times before and after the reduction among the 191 treated series. Our main analysis investigates the effect of the reduction irrespective of the starting and ending points, as our goal is to explore the consumption dynamics that arise due to the unique complementarity properties rather than pinpoint an optimal wait-time. Nevertheless, there may be non-uniform effects based on the absolute length of wait-times that may bias our results. Our findings are robust to a specification using treatment intensity as shown in 5.4, but to further alleviate the concerns, we select

| | (1) log(Consumed+1) | (2) log(Purchased+1) | (3) Consumed PR | (4) Purchased PR |
|---|---|---|---|---|
| $after \times treated$ | 0.597*** | 0.179*** | 1.570*** | 0.158*** |
| (excl. gifted episodes) | (0.054) | (0.057) | (0.077) | (0.036) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 34080 | 34080 | 34080 | 34080 |
| N Series Groups | 175 | 175 | 175 | 175 |
| R-squared Adj. | 0.044 | 0.014 | 0.086 | 0.002 |
| $after \times treated$ | 0.613*** | 0.173*** | 1.628*** | 0.154*** |
| (excl. new episodes) | (0.060) | (0.063) | (0.082) | (0.041) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 26850 | 26850 | 26850 | 26850 |
| N Series Groups | 146 | 146 | 146 | 146 |
| R-squared Adj. | 0.076 | 0.018 | 0.097 | 0.001 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 14: Robustness Checks on Marketing Activities and New Episode Releases

a subsample of 152 treated series for which wait-times were reduced to one hour. We confirm in Table 15 that the previous results of the effect on daily consumption and purchases hold for the subsample.

| | (1) log(Consumed+1) | (2) log(Purchased+1) | (3) Consumed PR | (4) Purchased PR |
|---|---|---|---|---|
| $after \times treated$ | 0.688*** | 0.202*** | 1.760*** | 0.145*** |
| | (0.059) | (0.062) | (0.079) | (0.036) |
| Other control variables | Y | Y | Y | Y |
| Fixed Effects | Y | Y | Y | Y |
| N Obs | 31860 | 31860 | 31860 | 31860 |
| N Series Groups | 152 | 152 | 152 | 152 |
| R-squared Adj. | 0.109 | 0.033 | 0.115 | 0.002 |

Note: Robust standard errors clustered at series level in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 15: Subsample analysis using treated series with 1-hour wait-time post-reduction

We also estimate the model with alternative dependent variable transformations (Box-Cox), time periods (14 and 60 days around treatment) and the number of matched controls on propensity scores (1-10 matches). The results from these models replicate our findings.

# 6 Conclusion

Digital content platforms today are not only competing among peers of the same medium but also across medium – content produced in text, video and audio forms are all fighting to occupy consumers' free time such that they will be the first in line when consumers open up their smartphones. In such a competitive environment, serialization has become the prevalent mode of publication due to its effectiveness in encouraging consistent and recurring consumption. Moreover, the ability of digital platforms to track individual consumption has allowed them to exploit time as a monetization lever, leading to the adoption of "wait-for-free" (WFF). In this research, we delve into the impact of wait-times on individual consumption decisions and monetization of serialized media. We begin by highlighting the conceptual characteristics unique to a series, namely the defined sequence of consumption and directed complementarities that diminish over the consumption interval. These unique characteristics motivate us to consider the role of intensive margins – the degree to which a given consumer is monetized – a dimension that has been overlooked in the versioning literature. Changing wait-times affect whether consumers can realize the complementarity as well as the magnitude of its ex ante expected value, which together determine the shift in intensive margins. Moreover, we expect changing wait-times to also affect extenive margins – the consumers' decision to participate in the series.

Using data from a platform serving serialized books, we leverage a natural shift in policy where the platform reduced the wait-times for a set of series. We estimate using a difference-in-difference framework how the reduction impacted user consumption and purchases. We provide evidence that the positive across-episode spillovers from complementarity counteract the negative cannibalization effect. In addition, we find evidence of consumption acceleration and the expansion of extensive margins. The net impact is an 86% and 20% increase in daily aggregate consumption and purchases, respectively. We conduct a battery of robustness checks to support the identification assumptions and rule out any spurious correlations.

By illustrating the multifaceted consumption dynamics of the WFF policy, which is gaining popularity, our research equips firms with a comprehensive understanding of strategic levers for policy design. This goes beyond the traditional focus on the acquisition-cannibalization trade-off, emphasizing the importance of evaluating the extent of consumer monetization. We demonstrate that firms may exploit complementarities across episodes to potentially boost long-term consumer spending. Additionally, our research underscores the significance of recognizing consumer heterogeneity. For instance, a segment of price-sensitive consumers may not respond positively to more lenient policies on free consumption. This suggests that platforms could gain more from implementing targeted strategies, tailoring policies to different consumer groups to maximize overall revenues.

Although the present study is one of the first to investigate the novel WFF policy and the economics of serialized media, it is not without its limits. First, our research relies on non-experimental variation in the data. Although we leverage various identification strategies to support causality, a randomized field experiment that manipulates wait-times across consumers for a given series could further strengthen our findings. Second, our analysis demonstrates the causal effect of changing wait-times, but cannot comment on the optimal wait-time. This would require estimating a structural model of consumers' episode consumption, which would also allow one to investigate insightful counterfactual policies such as charging a positive price on waited episodes or targeted wait-times. Estimating a rich state-dependent utility model incorporating inherent individual characteristics, consumption context and episode content to explore optimal policies would be a very interesting avenue. Given the pivotal role of episode complementarities, leveraging recent advancements in text analysis to understand how the episode content such as the strength of cliffhangers, level of suspense and sentiment affects consumption decisions may be a fruitful area of future research. Third, although we provide ample evidence that correlated marketing activities are unlikely the driver of our results, we cannot fully rule out that in some instances firms may still have tried to support the treated series with alternative methods such as curation or recommendation algorithms, which might have had some effect on our estimates. Fourth, we focus on the short-term effect of the wait-time reduction, as identification of a long-term effect is more difficult. It would be interesting to explore the long-term effect of varying wait-times on platform-wide consumption and series that they later consume.

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