Measuring Markets for Network Goods*

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Abstract

Market definition is challenging in settings with network effects, where substitution patterns depend on changes in network size. We study these effects in the context of social media. We conduct an incentivized experiment comparing substitution in response to a proposed U.S. TikTok ban, in which all users simultaneously leave the app, with substitution when only a single user deactivates. Consistent with a simple network model, we find substantially higher valuations of alternative social apps under a collective TikTok ban than under an individual TikTok deactivation. We then show that a collective time limit challenge, where peers jointly reduce TikTok or Instagram use, leads to more time spent on alternative social apps than has been observed in prior individual deactivation experiments. Together, our results suggest that individual-level substitution estimates can be an unreliable guide to market definition for network goods.

Keywords: Market Definition, Network Goods, Coordination, Substitution, Social Media.

JEL Classification: D85, L00, L40

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1 Introduction

Market definition is central to antitrust analysis, guiding assessments of market power, competition, and consumer harm. Consider the recent U.S. antitrust case against Meta, which hinges critically on defining the "relevant market" in which Meta's platforms compete. The Federal Trade Commission (FTC) argues that the market only comprises "personal social networking services," focusing on platforms like Facebook and Instagram that connect users with friends and family, while excluding other entertainment-based social apps such as YouTube and TikTok. Meta counters that the market should be broader, including all platforms competing for user attention and advertising revenue.[1]

A first step in market definition assessments is determining which products are substitutes. Empirical estimates of substitution patterns often capture how the unavailability of a given product affects consumer demand for alternative products—for example, through deactivation studies in the case of digital products (Allcott et al., 2020; Aridor, 2025). Such evidence primarily relies on individual-level interventions, which evaluate changes in demand while holding others' consumption fixed. Yet, in real-world markets, network effects—which arise when demand depends on network size or others' consumption—can play an important role in determining the equilibrium level of demand for alternative products. Obtaining credible estimates that account for network effects is challenging: experiments typically hold network size constant, and natural experiments that provide the necessary variation in network size are uncommon and lack individual-level counterfactuals.

In this paper, we introduce new evidence on the gap between substitution patterns that account for network effects and those that do not. We first show, using a simple conceptual framework, that cross-price derivatives estimated while holding network size fixed generally fail to reflect the substitution that would result from market-wide price changes—potentially even resulting in a different sign. Such estimates reflect the direct effect of a change in a product's price on another product's demand, but ignore that the resulting changes in the network sizes will trigger feedback effects on demand that amplify or dampen the initial cross-price response. Therefore, collective interventions, which evaluate the responses of multiple consumers simultaneously, may provide a more accurate picture of market-level substitution patterns in such settings.

To study how network effects influence substitution patterns, we conduct a pre-registered

[1] See Federal Trade Commission (2021). For popular press coverage, see "Meta faces April trial in FTC case seeking to unwind Instagram merger" (Reuters, 2024).

online experiment with 900 active U.S. TikTok users aged between 18 and 27. Participants are recruited from Prolific, a widely used online survey provider. Our experimental design leverages a moment of increased policy uncertainty surrounding a potential U.S. ban of TikTok—one of the most widely used social media platforms at the time, with over 170 million U.S. users.[2] After several months during which a nationwide ban on TikTok seemed increasingly likely, the U.S. government implemented the ban on January 19, 2025, prompting a temporary shutdown of the platform.[3] The uncertainty in the period leading up to the ban allows us to credibly elicit individuals' willingness to accept (WTA) to deactivate various platforms under different potential TikTok ban scenarios. These scenarios isolate the role of network effects and provide insights into the substitution patterns between TikTok and other platforms.

In particular, we examine respondents' incentivized valuations of other social apps using a simple Becker-DeGroot-Marschak (BDM) mechanism (Becker et al., 1964). We focus on three other social apps: YouTube, Instagram, and Snapchat, which are also popular among young adults (Pew Research Center, 2024). Like TikTok, Instagram and YouTube center on algorithmically curated, short-form, visually engaging public content aimed at broad audiences. Snapchat's primary focus is on ephemeral messaging and personal interactions rather than public content sharing and consumption. We randomly assign each participant one of these three other social apps, which we refer to as their *focal app*.

Respondents complete three scenarios for their focal app. In the *no TikTok ban* scenario, participants are asked how much compensation they would require to individually deactivate their focal app for four weeks if the TikTok ban does not take place. We then elicit respondents' required compensation to deactivate their focal app under two additional, randomly ordered, scenarios: 1) the *TikTok ban* scenario, in which the nationwide TikTok ban is implemented, and 2) the *individual TikTok deactivation* scenario, in which the ban does not happen but the respondent is required to individually deactivate TikTok in exchange for monetary compensation.[4]

[2] On TikTok, network effects could arise through content generation: as more users join and engage with the platform, the volume and diversity of user-generated videos increases, which enhances the experience for others. Network effects could also arise through content sharing between individuals: users might enjoy a video more when they can discuss it with a larger fraction of their friends. Bursztyn et al. (2023) provide evidence of network effects on TikTok between college students.

[3] Anticipating the nationwide ban, TikTok voluntarily suspended its U.S. services on January 18, resulting in a roughly 14-hour shutdown. On January 20, President Donald Trump reversed the ban by issuing an executive order postponing enforcement for 75 days to allow for negotiations over the app's ownership and to address national security concerns (Associated Press, 2025).

[4] Respondents estimated a 46% likelihood that the TikTok ban would take effect on January 19, 2025,

We begin by comparing participant valuations across the *individual TikTok deactivation* and the *no TikTok ban* scenarios, holding network size constant.[5] For Instagram, 37.7% of participants value the platform more under an individual TikTok deactivation compared to the no ban scenario. Conversely, 23.7% of participants assign a higher valuation to Instagram when TikTok remains available relative to when it is individually deactivated. Thus, a substantial positive *net fraction* (13.9 percentage points) of participants value Instagram more under the *individual TikTok deactivation* scenario compared to the *no TikTok ban* scenario. YouTube exhibits similar valuation patterns, with a net fraction of 24.4 percentage points. In contrast, Snapchat's net fraction is negative and near zero, suggesting that when the network size remains fixed, a similar fraction of our participants consider Snapchat to be a complement to TikTok as those who consider it a substitute.[6]

Next, we compare valuations between the *TikTok ban* and the *no TikTok ban* scenarios. The net fractions of participants with a higher valuation under a collective ban compared to no ban are 48.1, 41.8, and 14.8 percentage points for Instagram, YouTube, and Snapchat, respectively ($p < 0.01$ for all). These results imply that the fraction of people who view these three platforms as substitutes for TikTok is larger than the fraction who view them as complements in a collective deactivation scenario.

Lastly, we turn to the role of network effects by comparing valuations between the collective *TikTok ban* and *individual TikTok deactivation* scenarios. For Instagram, 44.9% of participants report a higher valuation under the collective TikTok ban than under the individual TikTok deactivation, while 19.9% indicate the reverse. Instagram thus exhibits a positive net fraction (25.0 percentage points) of participants who value it more under the collective compared to the individual TikTok deactivation. Similar results emerge for YouTube and Snapchat, with net fractions of 16.0 and 15.5 percentage points, respectively ($p < 0.01$ in all cases). Our findings on Snapchat are particularly noteworthy, revealing

underscoring that they perceived this scenario as quite likely at the time of our experiment. Reassuringly, this number is close to the 42% average perceived likelihood observed on Polymarket, an online betting platform, reflecting the general market sentiment at the time of our experiment.

[5] We focus on within-subject comparisons, as these increase statistical precision and offer more interpretable insights than absolute valuations, which may lack coherence (Ariely et al., 2003).

[6] While our net fraction measure does not directly correspond to substitution in a traditional Hicksian sense, under quasilinear utility, it represents a discrete-choice analogue of money-metric substitutability as described in Samuelson (1974). It effectively captures the share of users who view each platform as a money-metric substitute rather than complement when TikTok is removed from the choice set. As we show in Section 3.7, our main implications remain unchanged when, instead of net fractions, we consider a parameter more directly related to diversion ratios: second-choice Wald estimates (Conlon and Mortimer, 2021), given by the gain in users of a focal app divided by the number of lost TikTok users in response to a TikTok deactivation or ban.

qualitative differences in substitution patterns due to network effects. On average, this platform does not appear to be a substitute for TikTok when TikTok is individually deactivated (network size constant), but it emerges as one under collective TikTok deactivation for a substantial share of users, albeit less strongly than Instagram or YouTube. Given that Snapchat is a messaging-oriented app, this difference highlights the critical role coordination plays in shaping perceptions of platform substitutability, and how the choice between individual-level versus collective interventions matters for the measurement of substitution patterns in the presence of network effects.

We also ask participants directly how they expected their own and others' time use to change in response to the possible scenarios. These results are consistent with the findings above. First, respondents' expectations about changes in others' time use on Instagram, YouTube, and Snapchat align with their substitution patterns. In particular, individuals who expect an above-median increase in the time their friends will spend on their focal app exhibit a significantly larger gap in valuation between the *TikTok ban* and *individual TikTok deactivation scenarios.* This finding further provides evidence that network effects are important determinants of substitution patterns. Second, individuals' own expected time changes are consistent with the patterns observed in the elicitation exercise. We find that a net positive fraction of respondents expect to spend more time on other social apps—namely, Instagram, YouTube, and Snapchat—under the *TikTok ban* compared to the *individual TikTok deactivation.* Conversely, intended substitution toward non-social activities, such as playing phone games or meditating, is weaker under the *TikTok ban* than under the *individual TikTok deactivation.*

One limitation of our evidence is that it is unclear how changes in valuations, which capture substitution patterns at the extensive margin (usage vs. no usage), map to changes in substitution patterns that include intensive-margin responses (changes in time spent). Another limitation is that our elicitation requires respondents to accurately predict the general equilibrium effects of collective interventions.

To address these limitations, we provide supplemental evidence from a collective, incentivized time limit challenge launched by the social coordination app NOMO (No Missing Out). The collective challenge limited the use of Instagram and TikTok during two weeks at the University of Chicago. Participants were asked to adhere to a one-hour daily time limit between October 20th and November 3rd, 2024, and to verify compliance by uploading screenshots documenting their app usage. More than 800 undergraduate students, almost 11% of the undergraduate student population, participated.

Our estimates from this collective challenge reveal substantial substitution to other social apps: A 10-minute reduction of TikTok and Instagram is associated with an increase in the consumption of other social apps by 8.6 minutes ($p < 0.05$), implying a rate of substitution of 86%. Consistent with the idea that coordination on a new outside option takes time to materialize, we document larger substitution toward other social apps over time: while the rate of substitution in week one of the challenge is 79% of the reduction in TikTok and Instagram, it is 94% in week two. The extent of time substitution we observe is larger than what is reported in some prior individual-level deactivation estimates in the literature, which range between 9% and 41% (Aridor, 2025; Allcott et al., 2025b). However, we emphasize that this evidence should be interpreted with caution given the lack of a randomized control group.

Another key limitation of our findings stems from the self-selected nature of our samples both in the experiment and in the field study. In our experiment, around 82% of respondents who initially started our survey chose to participate in the deactivation study.[7] Finally, our estimates ignore other equilibrium responses besides direct network effects, such as changes in advertising prices (Donati and Fong, 2025).

Notwithstanding these limitations, both our experimental estimates and descriptive field evidence highlight the importance of accounting for changes in network size through collective interventions when defining the relevant market for social media platforms. Our results showcase that fixed-network interventions can underestimate the degree of substitutability between social products and overestimate the substitutability between social and non-social products. This effect could also spill over to other non-digital social activities, such as eating out with friends, where collective treatments may facilitate coordination among individuals. Beyond social media, these findings have broader implications for competition policy in markets with network effects.

Our paper speaks to a growing literature on the economics of social media (Aridor et al., 2024). Our study builds on previous research examining the effects of individual-level social media deactivation, with a particular focus on substitution patterns (Mosquera et al., 2020; Brynjolfsson et al., 2023a,b; Allcott et al., 2020, 2022, 2024; Collis and Eggers, 2022; Katz and Allcott, 2025; Aridor, 2025). The most closely related study is Aridor (2025), who estimates substitution patterns for YouTube and Instagram based on an individual-level deactivation study and finds cross-category substitution to other social apps but also substantial substitution rates to non-digital activities. Rehse and Valet (2025) find

[7] This fraction is relatively high compared to other deactivation studies (Allcott et al., 2024).

quantitatively similar substitution patterns to Aridor (2025) among US users in response to a 6-hour Meta platform outage.[8] We differ from this literature in our focus on explicitly accounting for network effects in this market. Further, in comparison to existing estimates from individual-level interventions, our data from a two-week collective social media time limit yields a larger magnitude of substitution to other social apps.

We also contribute to a longstanding literature in industrial organization that examines consumer choice in the presence of network effects (Rohlfs, 1974; Katz and Shapiro, 1985; Farrell and Saloner, 1985; Rochet and Tirole, 2003; Rysman, 2004). More recently, the literature has theoretically and empirically studied product market traps—situations where a large fraction of active users derive negative welfare from the product—in settings with network effects (Bursztyn et al., 2023; Hagiu and Wright, 2025). Despite their importance, network effects have proven challenging to account for. Related to the literature on contingent valuation (Landry and List, 2007), we provide an empirical methodology to credibly measure valuations of social media apps for the scenario of a collective deactivation. Building on Bursztyn et al. (2023), who demonstrate that considering the collective nature of the outside option is crucial for accurate welfare measurement, we show that accounting for the collective nature is also essential for correctly identifying the direction and magnitude of substitution patterns.

Finally, we contribute to a literature examining market power and market definition, particularly in the context of digital platforms (Franck and Peitz, 2019; Calvano and Polo, 2021; Scott Morton et al., 2019; Allcott et al., 2025a), and a literature studying competition in media markets (Anderson and Coate, 2005; Bergemann and Bonatti, 2011; Anderson and De Palma, 2012; Athey et al., 2018; Prat and Valletti, 2022; Anderson and Peitz, 2023).[9] This literature recognizes that direct and indirect network effects (Filistrucchi et al., 2014) affect market definitions; we contribute by providing both experimental and descriptive empirical evidence on substitution patterns after accounting for direct network effects.

[8] Rehse and Valet (2025) find that a 100% reduction in Meta's services leads to an 18.4% increase in non-Meta social media usage, while Aridor (2022) finds that a 100% restriction of Instagram usage leads to a 22.7% increase in the time spent on non-Instagram social applications. A limited network response could explain this similarity. While platform outages can, in principle, capture network effects and the coordination of users on different platforms, the short-lived duration of the 2021 Meta outages (6 hours) studied by Rehse and Valet (2025) restricts this possibility.

[9] Recent work also studies how social forces affect market power (Bursztyn et al., 2025).

2 Conceptual Framework

Suppose there are J products. The aggregate demand for product j in a model with network effects is given by $Q_j(p, q)$, where $p = (p_1, p_2, \ldots, p_J)$ is the vector of prices and $q = (q_1, q_2, \ldots, q_J)$ is the vector of quantities. Prices could take the form of monetary prices or advertising loads (Anderson and Coate, 2005). Quantities can represent different units of demand—such as the number of consumers, total time spent, or total amount consumed—depending on the application. Demands are allowed to exhibit not just own network effects (to depend on q_j) but also cross-product network effects (to depend on q_k for $k \neq j$).[10] We assume that demands are smooth, non-negative, and bounded.

Let $q_j(p)$ denote the equilibrium quantities that result from taking into account network effects. These (possibly non-unique) quantities solve the following fixed-point problem which imposes rational expectations:

$$q = Q(p, q).$$

Consider the case of a small change in the price of product 1. We are interested in the cross-price derivative that accounts for adjustments in the network structure, $\frac{\partial q_j}{\partial p_1}$.[11] This parameter is a crucial input for computing diversion ratios (Conlon and Mortimer, 2021) and, hence, for market definition exercises such as critical-loss analysis (Katz and Shapiro, 2002). To understand how network effects change measured substitution patterns, we compare this derivative to the "fixed-network" derivative $\frac{\partial Q_j}{\partial p_1}$ which is computed holding the network sizes fixed.

To fix ideas, consider a canonical model of network effects à la Katz and Shapiro (1985), with a continuum of individuals who must choose one of two products. Individual i's utility

[10] Cross-product network effects can arise even when the utility from each product depends only on its own user base. For example, with positive own-network effects, an increase in the size of product k raises the utility of choosing that product, which in turn reduces the equilibrium share of users selecting a competing product j.

[11] We focus on small price changes for analytical convenience, although our empirical estimates use platform deactivations or bans, effectively corresponding to infinite price increases (or increases above the "choke" point) and similar to second-choice estimates. These estimates are informative for antitrust investigations but in general differ from those based on small price changes which are used in market definition tests (Reynolds and Walters, 2008; Conlon and Mortimer, 2021). For example, the standard small but significant and non-transitory increase in price (SSNIP) analysis measures whether a 5% price rise diverts enough users to render the increase unprofitable. The network adjustment associated with such a price increase is likely much smaller than that of a full-scale ban. Note also that these derivatives might not be well-defined if the fixed point problem has multiple solutions.

from choosing j is quasilinear in money and is increasing in the size of the network, q_j:

$$u(q_j) + \gamma_j^i - p_j,$$

where u is a smooth function and γ_j^i is the heterogeneous "membership" benefit from joining network j. We assume that u is increasing, to capture positive network effects. We also assume that the net benefit from joining network 1, $\gamma^i := \gamma_1^i - \gamma_2^i$, is distributed according to a smooth distribution with density f with full support and that network effects are "small enough"—which we formalize by imposing the following bound: $u'(q_j) < (2\|f\|_\infty)^{-1}$.

In this case, there is a unique equilibrium and the difference $\frac{\partial q_2}{\partial p_1} - \frac{\partial Q_2}{\partial p_1}$ equals $\frac{2f^2}{1-2fu'(q_j)} \times u'(q_j)$, which is proportional to $u'(q_j)$, a positive number.[12] In other words, the fixed-network derivative will underestimate the degree of substitution between both products. Intuitively, when the price of 1 increases, there is a direct increase in the demand for product 2—and a corresponding decrease in the demand for 1—holding network effects constant, which is captured in the fixed-network derivative. However, this derivative ignores the subsequent impact on the demand for 2 due to the change in the network of both products. The partial increase in the demand for 2 will further increase the demand for 2 due to own-network effects. Additionally, the partial decrease in the demand for 1 will reinforce this effect due to cross-product network effects—product 2 becomes relatively more attractive since fewer people choose 1. Therefore, both own-network and cross-product network effects contribute to the bias of the fixed-network derivative.

More generally, network effects cause the fixed-network derivatives to differ from the relevant cross-price derivatives, sometimes even resulting in opposite signs. Focusing on locally-stable equilibria (where the matrix $I - \frac{\partial Q}{\partial q}$ is invertible), the cross-price derivatives that account for network effects are:

$$\frac{\partial q}{\partial p_1} = \left(I - \frac{\partial Q}{\partial q}\right)^{-1} \frac{\partial Q}{\partial p_1},$$

which in general differ from the fixed-network derivatives $\frac{\partial Q}{\partial p_1}$ unless there are no network effects, $\frac{\partial Q}{\partial q} = 0$.

[12] To show uniqueness, note that the equilibrium is given by the fixed point problem $q_2^* = F(u(q_2^*) - u(1-q_2^*) + p_1 - p_2) := \phi(q_2^*)$, since $q_1^* = 1 - q_2^*$. Given that $|\phi'| = 2fu' < 1$ by our bound on u', there is a unique equilibrium by the Banach Fixed Point Theorem.

To understand the magnitude and sign of the gap, we focus on the two-product case:

$$\frac{\partial q_2}{\partial p_1} = \frac{\frac{\partial Q_2}{\partial q_1}\frac{\partial Q_1}{\partial p_1} + \left(1 - \frac{\partial Q_1}{\partial q_1}\right)\frac{\partial Q_2}{\partial p_1}}{\left(1 - \frac{\partial Q_1}{\partial q_1}\right)\left(1 - \frac{\partial Q_2}{\partial q_2}\right) - \frac{\partial Q_1}{\partial q_2}\frac{\partial Q_2}{\partial q_1}}. \tag{1}$$

Consider a scenario when two products are substitutes based on the fixed-network derivatives, $\frac{\partial Q_2}{\partial p_1} > 0$. We focus on the commonly-studied case of locally-stable equilibria with positive own-network effects, assuming that the network effects are small enough such that the denominator is positive.[13] In this case, the sign of the difference $\frac{\partial q_2}{\partial p_1} - \frac{\partial Q_2}{\partial p_1}$ will largely depend on the sign of the cross-product network effects, $\frac{\partial Q_j}{\partial q_k}$. When cross-product network effects are zero, the fixed-network derivatives will *underestimate* the strength of substitution to product 2: they ignore that an initial increase in the demand for product 2 will be further amplified by positive own-network effects. A similar underestimation occurs when cross-product network effects are negative: fixed-network estimates ignore the decrease in the demand for product 1 which further increases the demand for product 2. On the other hand, when cross-product network effects are positive and large enough (and demand for 1 is sufficiently elastic),[14] the fixed-network derivatives will *overestimate* the strength of substitution to product 2. Intuitively, fixed-network estimates ignore that the increase in p_1 will decrease the demand for 1, which further decreases the demand for 2. In this case, there can even be a qualitative difference—a change in sign—between the substitution patterns inferred from fixed-network derivatives and the relevant cross-price derivatives.

3 Collective versus Individual Valuations

To quantify the role of network effects in shaping substitution patterns, we conducted an experiment shortly before the Supreme Court ruling on the TikTok ban in the United States. The uncertainty surrounding this decision enables us to compare valuations of various social media apps across three plausible scenarios for TikTok's future: 1) a status quo scenario where TikTok is not banned, 2) a scenario where TikTok is not banned and

[13]Concretely, assume that $\frac{\partial Q_j}{\partial q_j} < 1$, that $\frac{\partial Q_j}{\partial q_k}$ and $\frac{\partial Q_k}{\partial q_j}$ have the same sign, and that the denominator in (1) is positive.

[14]This case requires: $\frac{\partial Q_2}{\partial q_1}\left(\left|\frac{\partial Q_1}{\partial p_1}\right| - \frac{\partial Q_1}{\partial q_2}\frac{\partial Q_2}{\partial p_1}\right) > \frac{\partial Q_2}{\partial p_1}\left(1 - \frac{\partial Q_1}{\partial q_1}\right)\frac{\partial Q_2}{\partial q_2}$. The right-hand side of this expression is positive. For the inequality to hold, $\frac{\partial Q_2}{\partial q_1}$ need to be positive and large, and the demand for 1 has to be sufficiently elastic with respect to its own price.

users individually deactivate their TikTok accounts, and 3) a scenario in which TikTok is banned for all users.

3.1 Study context: TikTok ban in January 2025

Over the past years, U.S. officials have warned that TikTok could be used by the Chinese government to collect sensitive information or influence public opinion. These national security concerns over foreign access to Americans' personal data prompted Congress to pass a "sell-or-ban" law against TikTok in April 2024. The law required ByteDance, TikTok's parent company, to sell its U.S. operations within nine months or face a nationwide ban starting January 19, 2025.

TikTok challenged the law in court, culminating in a critical Supreme Court hearing on January 10th, 2025. Nevertheless, the Supreme Court upheld the law on January 17, 2025, affirming the government's authority to act on national security grounds. A shutdown was widely expected, and TikTok suspended its U.S. operations on January 18, 2025. Two days later, President Trump signed an executive order delaying enforcement for 75 days to allow for TikTok to negotiate with potential American buyers.

As a result, leading up to the ban, American TikTok users were plausibly uncertain about their ability to use TikTok after January 19, providing us an opportunity to leverage this policy uncertainty for our experiment in early January 2025.[15]

3.2 Sample

Sample characteristics We recruited 900 respondents from the online survey provider Prolific between January 6 and January 9, 2025, prior to the Supreme Court appeal on January 10.[16] Our sample consists of participants from the U.S. aged between 18 and 27 who own iPhones and are active TikTok users.[17] We focus on this demographic as young adults are among the most active on social media platforms, and especially on TikTok. Indeed, as of 2022, approximately 54% of U.S. adults aged 18-29 use TikTok, compared to 25% for all other age groups (Pew Research Center, 2022). Among participants who began our survey, 81% were active TikTok users. From these participants, 82% agreed

[15] For press coverage, see "TikTok starts restoring service in the U.S. after shutting down over ban concerns" (CBS News, 2025).

[16] For popular press coverage, see "Supreme Court appears inclined to uphold TikTok ban in U.S." (Reuters, 2025).

[17] We recruit iPhone users as we require screenshots from Screen Time usage to monitor phone app deactivation, which is simplified on iOS devices.

to participate in the four-week deactivation study, which would require them, if selected, to upload screenshots of their iPhone screen time usage to verify deactivation compliance. While this restriction implies sample selection, the degree of selection is smaller than in existing deactivation studies.[18] After the consent process, the survey includes two comprehension checks on the method of compliance and the length of the deactivation—correctly answered by 94% and 84% of participants, respectively.[19]

Summary statistics Our sample includes 67% female participants, similar to the proportion of U.S. TikTok users aged 18-29 who are female (60.0%; Pew Research Center 2022). The average age is 23.5 years old. Additionally, 49% of participants are students and 46% are single. At baseline, respondents self-report spending an average of 103 minutes per day on TikTok, with 74% using the platform daily. On average, participants also self-report spending an average of 80 minutes per day on YouTube, 52 minutes on Instagram, and 31 minutes on Snapchat.[20] Based on these figures, 94%, 96%, and 74% of our sample are multi-homers on TikTok and Instagram, TikTok and YouTube, and TikTok and Snapchat, respectively.[21]

Pre-registration The pre-registration for the data collection can be found on AsPredicted #206616.[22] It provides information on the study design, hypotheses, primary and secondary outcomes, sample size, and criteria for excluding participants from the sample.

3.3 Design

Our design aims to measure participants' valuation of their focal app that could be a substitute for TikTok. In particular, it allows us to evaluate how the valuations of these focal apps depend on whether TikTok consumption is reduced individually or collectively.

[18] We chose not to pre-specify the apps that participants may be asked to deactivate prior to consent to minimize concerns over differential attrition. Indeed, we find that the attrition rate at the consent stage is 18.0%, 17.5%, and 18.6% for Instagram, YouTube, and Snapchat, respectively. These differences are not statistically significant.

[19] We do not collect data for participants who fail either of these questions, as pre-specified.

[20] However, we note that self-reported usage measures may be unreliable in general and thus these values should be interpreted with caution.

[21] These values are fairly close to what is observed among active TikTok users in the American Trends Panel survey (Pew Research Center, 2024), where there are multi-homing rates of 88%, 96%, and 79% on TikTok with Instagram, YouTube, and Snapchat, respectively.

[22] For details, see `https://aspredicted.org/d55q-yw33.pdf`. There were no deviations from what was pre-registered.

Figure 1 presents an overview of the experimental design. Details on the experimental instructions are available here.

Background Information on the Ban We begin the experiment by providing all respondents with information about the potential TikTok ban in the U.S.:

> Over the past year, U.S. lawmakers and officials have expressed concerns about data privacy and misinformation on TikTok, which is owned by the Chinese company ByteDance.
>
> In April, the U.S. government enacted a law requiring TikTok to be sold to another company or face a ban on operating in the United States.
>
> The ban is scheduled to take effect on January 19th, 2025. However, the Supreme Court has agreed to hear TikTok's appeal on January 10th. As a result, it is possible that TikTok will be banned for all users in the United States on January 19th.

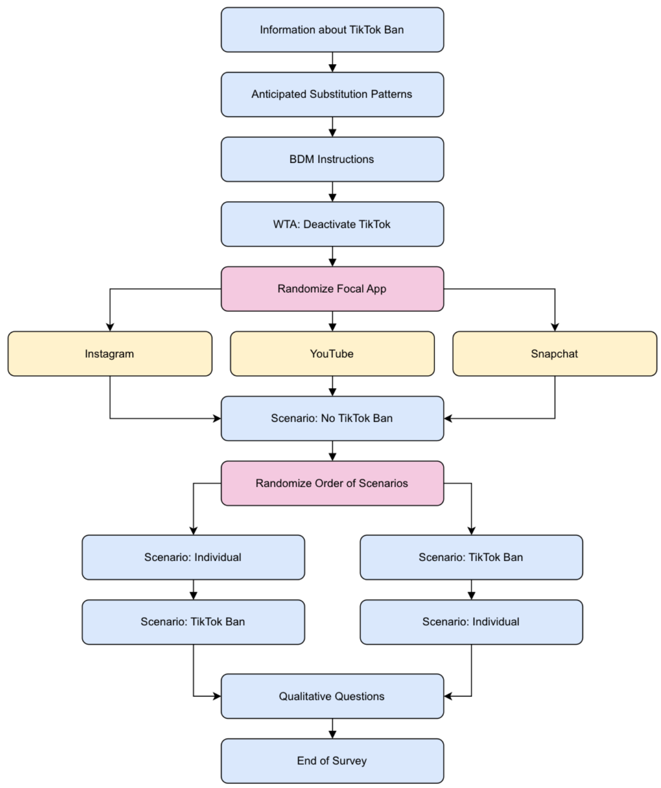
WTA Elicitation Instructions Next, we explain our WTA elicitation method to respondents, designed to measure their valuation of their focal app. We employ a BDM elicitation method, which is explained to respondents in simple terms. Specifically, we ask participants to indicate the minimum amount of money they would require to deactivate their focal app for four weeks under each scenario. We allow for an upper limit of $500 and a lower limit of $0.[23] A series of best practices are implemented in our elicitation process. First, we include a practice app (Facebook) to familiarize respondents with the BDM elicitation when presenting the instructions. Second, we ensure high data quality by only allowing respondents who pass a comprehension question on the BDM elicitation to participate in the experiment.[24] Third, we ask respondents whether they agree with the valuation implied by their responses. If respondents disagree with their initial valuation, they are given the opportunity to retake the question once.[25] We incentivize our experiment by informing participants that 1 in 10 respondents will be randomly selected to take

[23] We have minimal top or bottom coding issues as we find that only 7.78% of respondents enter $500 and only 3.22% enter $0.

[24] As pre-specified, we do not collect data for participants who fail the BDM comprehension check. 15% of participants fail this check.

[25] If respondents disagree a second time, they proceed with the survey, and their second attempt is recorded as their final response. As pre-specified, we exclude them from our analysis. Reassuringly, across all elicitations, we find that only 1.6% of first choices are regretted and only one respondent regrets their choice twice.

Figure 1: Structure of the experiment: TikTok Ban Study



Notes: Figure 1 displays the structure of our experiment. Participants begin by receiving information about the upcoming TikTok ban and subsequently answer questions regarding their anticipated time substitution patterns to social apps. Next, the survey provides instructions for the BDM mechanism, followed by the elicitation of participants' WTA for individually deactivating TikTok in the absence of a ban. Participants are then randomly assigned one of three focal apps (Instagram, YouTube, or Snapchat), after which their WTA is elicited under three distinct scenarios. Initially, participants indicate their WTA for deactivating their focal app assuming that no TikTok ban occurs. Subsequently, the individual TikTok deactivation scenario (participants are asked to individually deactivate TikTok when no TikTok ban occurs) and the TikTok ban scenario (TikTok is banned in the U.S.) are presented in random order. In each scenario, participants specify their WTA to deactivate the focal app. The study concludes with participants providing qualitative responses on anticipated substitution to non-social activities, network effects, and social media use, and demographic questions. In the schematic diagram, yellow boxes denote embedded data, blue boxes indicate question sections, and pink boxes highlight randomization points.

part in the study, for the scenario based on whether the TikTok ban is implemented on January 19th, 2025. Each selected respondent is invited to participate in the deactivation if their randomized BDM draw exceeds their stated WTA for that scenario. Respondents receive the randomized BDM draw as compensation upon successfully complying with the deactivation.[26]

3.3.1 Deactivation Scenarios

Our experiment then examines how people value their focal app under three different scenarios. Each participant is randomly assigned one of either Instagram, YouTube, or Snapchat as their focal app.

No TikTok ban scenario We start with the *no TikTok ban* scenario, which serves as our baseline, where TikTok remains fully available. Participants are asked how much compensation they would require to deactivate their focal app for four weeks. Specifically, respondents are provided with the following instructions:

> Assume that TikTok wins the appeal and remains available to all users in the U.S. after January 19th.
>
> In this scenario, how much would we need to pay you (in U.S. dollars) to deactivate your [focal app] account for four weeks?

Next, we elicit respondents' valuations of the focal app under two additional scenarios, presented in random order.

Individual TikTok deactivation scenario The *individual TikTok deactivation* scenario enables us to measure how a respondent's valuation of a focal app changes when they personally lose access to TikTok, holding others' consumption fixed. Here, TikTok is not banned for the general public, but the respondent is asked to deactivate their personal TikTok account for four weeks in exchange for a monetary payment exceeding their previously stated valuation.[27] We then ask how much additional compensation they would require to

[26] Our methodology therefore also relates to the literature on contingent valuation in economics that measures the value of non-market goods through hypothetical surveys but has been shown to be subject to hypothetical bias (Landry and List, 2007; List, 2001). We address this bias by exploiting real policy uncertainty surrounding a potential TikTok ban to incentivize our experiment.

[27] Before measuring their valuation of the focal app in the three scenarios, we elicit how much compensation respondents require for an individual TikTok deactivation in an incentivized manner. This allows us to credibly identify valuations of focal apps for the scenario of an individual TikTok deactivation.

also deactivate their focal app. Participants receive the following instructions:

> Assume that TikTok wins the appeal and remains available to all users in the U.S. after January 19th. This means the general public in the U.S. can continue using TikTok as usual.
>
> Additionally, assume the random draw exceeds the valuation you provided to deactivate TikTok for four weeks in a previous question, and we ask you to deactivate your TikTok in exchange for this payment.
>
> In this scenario, how much additional money would we need to pay you (in U.S. dollars) to also deactivate your [focal platform] account for four weeks?[28]

TikTok ban scenario Finally, the *TikTok ban* scenario entails a situation in which TikTok becomes unavailable to all U.S. users. This scenario allows us to examine how focal app valuations shift when there is a collective TikTok ban, which allows us to isolate the role of network effects on respondents' valuations, when compared to the individual scenario. Participants in this condition are told:

> Assume that TikTok loses the appeal and is banned in the U.S. on January 19th. The TikTok ban would apply to everyone in the U.S., including you.
>
> In this scenario, how much would we need to pay you (in U.S. dollars) to deactivate your [focal app] account for four weeks?

Design Discussion The key advantage of our approach is that it measures participants' incentivized—rather than hypothetical—valuations in a scenario where both an individual and collective deactivation are plausible outcomes, due to the substantial legal uncertainty. This uncertainty is reflected in respondents' perceived likelihood of the ban occurring, as well as in predictions from Polymarket, one of the world's largest live prediction markets, at the time of our experiment. In particular, we find that participants, on average, assign a 46% likelihood to the TikTok ban taking place, closely aligned to the average perceived likelihood of 42% on Polymarket at that time, as seen in Appendix Figure A1.

[28]In the individual TikTok deactivation scenario, participants are paid to deactivate their personal TikTok accounts, ensuring that the focal app deactivation is incentivized. As a result, there is a potential income effect for those in the individual TikTok deactivation group. Consistent with the previous literature, we find it plausible that income effects are small. Moreover, our self-reported time-use intentions are immune to income effects, yet they exhibit the same qualitative patterns as our incentivized measures. This suggests that income effects are unlikely to be quantitatively large in our experiment.

An important feature of our experimental design is its ability to facilitate within-subject comparisons. Specifically, the design allows us to observe how valuations change across three distinct scenarios: (1) no deactivation, (2) individual TikTok deactivation, and (3) collective deactivation. Additionally, employing a within-subject comparison enhances our statistical power, especially since we aimed to elicit valuations for multiple platforms but faced limitations due to Prolific's sample-size constraints for our target demographic.

3.3.2 Focal Apps

We consider three popular focal apps in the experiment: Instagram, YouTube, and Snapchat. These platforms were chosen since network effects may play an important role for substitution patterns given their established presence as content-sharing platforms that, to varying degrees, share some functional similarities with TikTok.

Instagram Instagram shares numerous relevant characteristics with TikTok. Both platforms contain visually engaging, short-form content and encourage user interaction through algorithmically curated feeds. TikTok's "For You" page provides highly personalized content discovery, while Instagram's discovery features, such as Reels and hashtag-based browsing, fulfill a similar function. Both platforms prominently feature creator-driven trends and influencer engagement. Additionally, both Instagram and TikTok users maintain personal profiles to post content for their followers. Instagram also allows for interactions within users' social networks through direct messaging, story responses, and interactive features such as polls and Q&A sessions, facilitating meaningful social engagement among friends and followers.

YouTube YouTube also shares key similarities with TikTok, with both platforms centering on user-generated video content, employing algorithmic feeds to drive engagement, and offering monetization tools to attract and retain creators. Furthermore, YouTube Shorts—launched in 2020 following TikTok's ban in India[29]—significantly enhanced YouTube's competitive position in the short-form video segment. Although YouTube offers social engagement features such as comments, channel subscriptions, community posts, and live chats, these interactions typically occur within broader, interest-based communities rather than strongly emphasizing direct interactions within users' local friendship networks.

[29] "YouTube Shorts launches in India after Delhi TikTok ban" (The Guardian, 2020).

Snapchat Snapchat is known for its ephemeral messaging and highly personal interactions within users' own social networks, differing from the previously mentioned platforms. In particular, Snapchat's core functionality revolves around immediate, direct communication with friends through snaps, private messaging, stories targeted at specific social circles, and group interactions. Snapchat also has Spotlight, which is used to promote viral Snapchat videos from creators. However, while the format is similar to TikTok's "For You" page, the social environment differs as there are limited interactions (e.g., no comments section).

3.4 Results

3.4.1 Incentivized Valuation of Other Platforms

As pre-registered, our main analysis focuses on the proportion of respondents with higher, equal, or lower valuations across the different scenarios, as these measures are robust to concerns about measurement error in continuous WTA elicitations. For ease of exposition, Figure 2 displays the differences in valuations across three platforms when TikTok is individually deactivated or collectively banned compared to the no ban scenario. Each color reports the fraction of individuals whose WTA for a focal app is higher or lower under one treatment scenario relative to another. The values above the bars report the difference between the two bars, indicating the net fraction of responses with a higher valuation. Positive net values indicate that, on net, more individuals place higher value on the focal app under one scenario compared to another, suggesting stronger substitutability between that platform and TikTok. We present three sets of comparisons. The two light blue bars per platform indicate the fraction of participants whose willingness to accept (WTA) for the focal app differs under the *individual TikTok deactivation* scenario relative to the *no TikTok ban* scenario.[30] The green bars present comparisons between the *TikTok ban* scenario and the baseline *no TikTok ban* scenario. Lastly, the dark blue bars show analogous comparisons between the *TikTok ban* and the *individual TikTok deactivation* scenarios.

We begin by examining valuations under individual TikTok deactivation compared to the no TikTok ban scenario. Figure 2 displays, for each focal app, the net difference between the proportions of participants who exhibit higher versus lower WTA under individual deactivation compared to no ban. We observe substantial positive net effects for Instagram (13.9 p.p., $p < 0.01$) and YouTube (24.4 p.p., $p < 0.01$), indicating that TikTok tends to be

[30] The remaining fraction indicates equal WTA across scenarios.

a substitute for these platforms absent network considerations. Conversely, Snapchat shows no significant net effect, suggesting it is equally perceived as a substitute and complement among participants.[31]

Next, we analyze overall valuation changes by comparing the TikTok ban scenario to the no TikTok ban baseline. Instagram, YouTube, and Snapchat all exhibit large and positive net valuation increases (48.1 p.p., 41.8 p.p., and 14.8 p.p., respectively). This indicates that banning TikTok substantially enhances valuations for these focal apps relative to a scenario without a ban.

Finally, we turn to our core interest—network effects. The dark blue bars show significant positive differences for Instagram (25.0 p.p.) and YouTube (16.0 p.p.), indicating that valuations rise substantially when TikTok deactivation occurs collectively rather than individually. The distinguishing factor between these scenarios is the change in participants' network sizes on TikTok and the focal apps, highlighting the significant role of network effects in determining app valuation.

Notably, Snapchat exhibits a somewhat different pattern. While its net share under individual TikTok deactivation is negligible (-0.7 p.p.), valuations become significantly higher under a collective TikTok ban, both compared to individual deactivation (15.5 p.p.) and relative to the no TikTok ban scenario (14.8 p.p.). This shift underscores that coordinated user movements due to collective deactivation transform Snapchat into a stronger substitute for TikTok. [32]

Collectively, our findings suggest that coordinated TikTok deactivation leads significantly more users to substitute toward alternative platforms, emphasizing network effects' role in broadening market boundaries within social apps. The difference between the collective and individual treatments is particularly stark in the case of Snapchat.

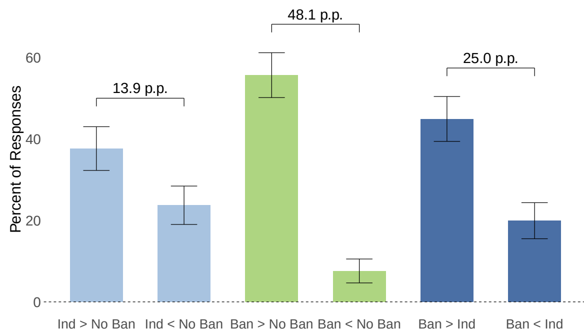
Average Willingness to Accept (WTA). Next, we present pre-registered analyses of average differences in valuations of the focal apps, which measure the overall intensity of

[31] For some users, TikTok and the focal platforms may function as complements across our pairwise scenario comparisons, due to cross-platform content sharing and complementarities in content creation and consumption.

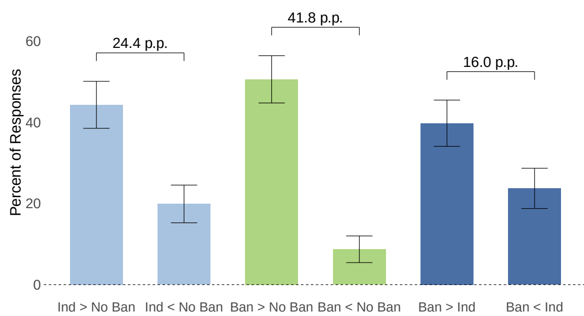
[32] We further explore treatment effect heterogeneity for Snapchat by comparing multi-homers (73% of the sample), who use both Snapchat and TikTok at baseline, with TikTok-only users. We find the net fraction of multi-homers reporting higher valuations under collective versus individual deactivation is significantly larger (19.6 p.p.) than among TikTok-only users (2.8 p.p.). This could indicate that network effects primarily influence Snapchat on the intensive margin. We do not examine this heterogeneity for Instagram and YouTube, as the vast majority of participants are already multi-homers.

Figure 2: Fraction with Higher or Lower Valuation By Scenario

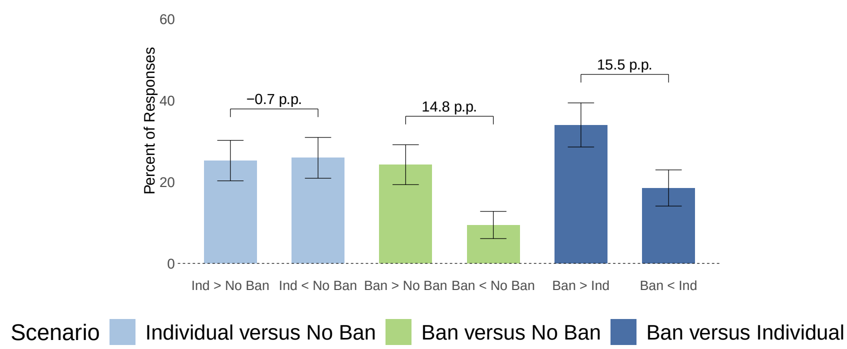
(a) Instagram



(b) YouTube



(c) Snapchat



Notes: By platform, Figure 2 illustrates differences in the valuation of focal apps across three scenarios: no TikTok ban, individual TikTok deactivation, and a TikTok ban. Panel a) is for Instagram, b) for YouTube, and c) for Snapchat. For each platform, the light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their focal app during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

respondents' preferences. Figure 3 summarizes how average valuations differ across three key scenarios: individual TikTok deactivation, a complete TikTok ban, and the no TikTok ban baseline.[33]

The light blue bars illustrate relatively modest valuation differences between the individual TikTok deactivation and the no TikTok ban scenarios. Specifically, these differences amount to \$7.48 ($p = 0.051$) for Instagram, \$10.59 ($p < 0.01$) for YouTube, and -\$0.12 ($p = 0.968$) for Snapchat.

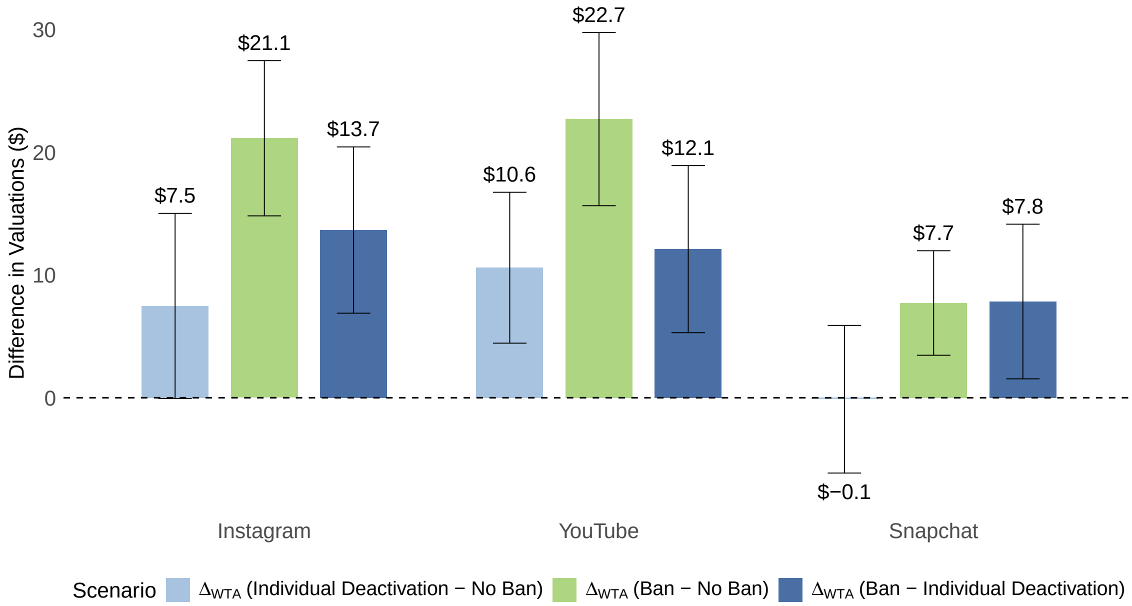
In contrast, the light green bars indicate considerably larger differences when comparing the TikTok ban scenario with the no ban baseline. Respondents' WTA to deactivate each focal app under a TikTok ban increases significantly: by \$21.13 ($p < 0.01$) for Instagram, \$22.69 ($p < 0.01$) for YouTube, and \$7.72 ($p < 0.01$) for Snapchat.

Finally, the dark blue bars isolate the impact of network effects by comparing the complete TikTok ban to individual TikTok deactivation. Collective deactivation increases respondents' WTA by \$13.66 ($p < 0.01$) for Instagram, \$12.10 ($p < 0.01$) for YouTube, and \$7.84 ($p < 0.05$) for Snapchat. These network-induced valuation increases correspond to 16.4%, 14.3%, and 10.6%, respectively, relative to baseline valuations in the no TikTok ban scenario. Taken together, we find that ignoring network effects leads to an underestimation of substitutability with social apps and even produces qualitatively different conclusions about whether Snapchat is a net substitute for TikTok.

We next interpret the effect sizes comparing the difference between collective and individual TikTok deactivation to the difference between the collective deactivation and the no TikTok ban scenario. For Instagram, 65% of the valuation increase for the focal app under a TikTok ban can be attributed to the collective component of deactivation. For Snapchat and YouTube, these shares are 100% and 53%, respectively. The relative importance of network effects aligns closely with the role of non-anonymous interactions on each platform: personal social networks play a more limited role on YouTube, are more significant on Instagram, and are essential for Snapchat. Taken together, these patterns underscore that the collective component, which accounts for network effects, represents over half of the total increase in valuation of the focal apps under the TikTok ban.

[33] Appendix Figures A4 through A5 provide inverse demand curves, both pooled and disaggregated by platform, as an alternative visualization.

Figure 3: Average Difference in Valuations Across Scenarios by Platform



Notes: Figure 3 illustrates the differences in continuous valuations of the focal app across our three scenarios. The light blue bars depict the average difference between valuations under the individual TikTok deactivation scenario and the no TikTok ban scenario. The green bars represent the average difference in respondents' valuations between the TikTok Ban scenario and the no TikTok ban scenario. The dark blue bars show the difference in average valuation between the TikTok ban and the individual TikTok deactivation scenario. The error bars indicate 95% confidence intervals.

3.4.2 Self-reported Substitution Intentions

While the previous results provide incentivized estimates on substitution patterns in terms of platform valuation, they do not directly speak to changes in time use. Given that advertising is the primary revenue source for most social media platforms, it is natural to consider a more direct measure of quantity: the time users spend on the platform. We therefore examine respondents' self-reported substitution intentions.

Figure 4 shows the proportions of respondents who expected to spend more or less time on a given activity under collective versus individual TikTok deactivation.[34] First, we find that people predict spending more time on other social apps. In particular, we find a net positive difference of 4.44 p.p. ($p < 0.05$), 6.44 p.p. ($p < 0.01$), and 3.22 p.p. ($p < 0.05$)

[34]We define the net substitution as the percentage of respondents intending to spend more time on a given activity under collective TikTok deactivation minus the percentage intending to do so under individual TikTok deactivation.

of respondents who expect to spend more time on Instagram, YouTube, and Snapchat, respectively, under a ban relative to an individual TikTok deactivation of TikTok.[35] In contrast, we find evidence that people predict spending more time on non-social activities under the individual TikTok deactivation, such as playing phone games or meditating, where we find a net difference of -4.44 p.p. ($p < 0.05$) and -3.67 p.p., respectively ($p = 0.056$). We also find that people plan to spend somewhat less time on their laptop in the individual TikTok deactivation scenario, but this effect is not statistically significant ($p = 0.263$).

Our estimates suggest that digital social platforms, broadly defined, become closer substitutes to TikTok once network effects are considered, increasing the likelihood that they belong in the relevant market. Individual-level interventions thus underestimate substitution toward other social apps. At the same time, non-social digital activities appear to be less close substitutes for TikTok after accounting for network effects.

Anticipated time substitution patterns To validate the incentivized WTA measure, we collect data on how participants expect their time spent on various social apps to change under an individual TikTok deactivation and a TikTok ban. As shown in Appendix Figure A6, participants anticipate increasing their time spent on other social media platforms in both the individual and collective treatment scenarios. Note that, while qualitatively similar, the estimates in Figure A6 differ from those presented in Figure 4: the latter displays data based on a question asking respondents to evaluate their likely time spent under a collective versus an individual TikTok deactivation, while Figure A6 relies on a question where respondents are asked to evaluate the likely time spent under a collective and an individual TikTok deactivation compared to the no-ban scenario.

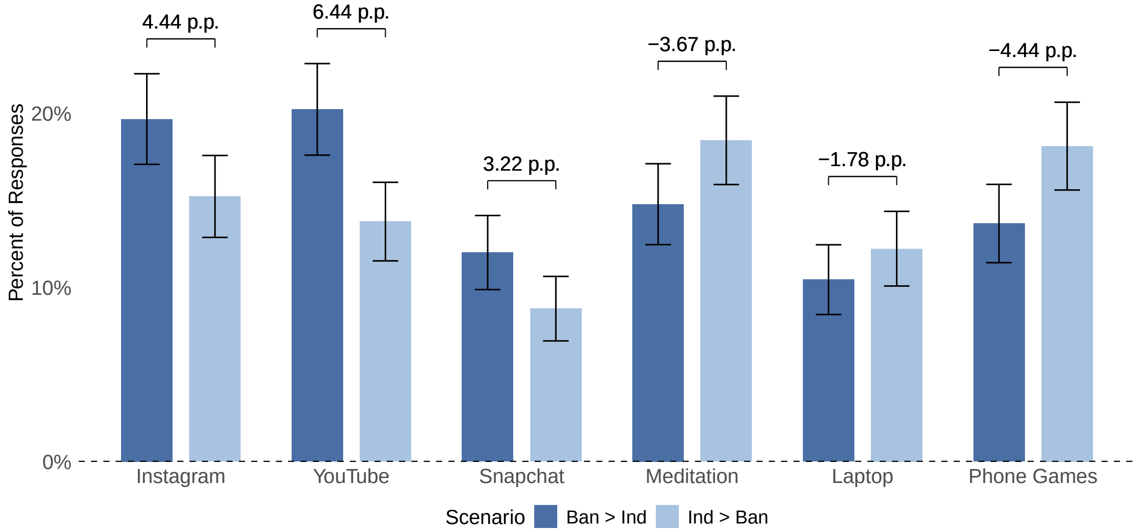
As shown in Appendix Figure A7, we find that participants who predicted above-median increases in time on their focal app exhibit a higher WTA for deactivating TikTok, compared to the no TikTok ban scenario, in both the collective ($p < 0.01$) and individual ($p < 0.01$) treatment conditions.

3.5 Anticipated network effects

To more directly speak to the role of network effects in explaining differences between our individual and collective treatments, we also collect data on participants' expectations

[35] Note that differences in time use need not align with differences in valuation, as shown in Beknazar-Yuzbashev et al. (2024).

Figure 4: Fraction with Higher or Lower Predicted Time Spent Under Collective vs. Individual Deactivation



Notes: Figure 4 illustrates how respondents' predicted time spent using alternative platforms and on activities differs between the TikTok ban (collective deactivation) and individual TikTok deactivation scenarios. Dark blue bars represent the percentage of respondents who intend to spend more time on a given activity under the TikTok ban scenario compared to the individual TikTok deactivation scenario, while light blue bars represent the percentage who intend to spend more time on the same activity under individual TikTok deactivation. We define net substitution as the difference between these two values. Positive values indicate a net shift toward the activity under the collective TikTok ban scenario, while negative values indicate a shift toward the activity under individual TikTok deactivation. The error bars represent 95% confidence intervals.

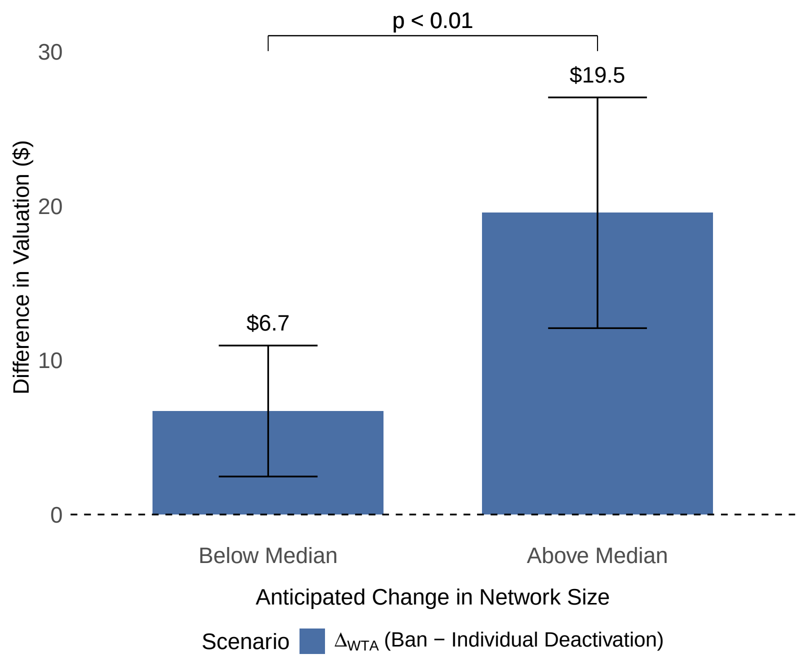
about how their friends would substitute toward other platforms if TikTok were banned. Through the lens of our conceptual framework, these anticipated changes in the network sizes of focal apps following a TikTok ban reflect shifts in both own-platform and cross-platform network effects—the two key mechanisms driving differences in substitution patterns between individual and collective interventions.[36] As shown in Figure A8, 93%, 86%, and 66% of respondents expect their friends to increase time spent on Instagram, YouTube, and Snapchat, respectively, under a TikTok ban compared to current usage levels. These patterns broadly reflect respondents' expectations of substantial changes in network size of other social apps resulting from collective interventions.

Moreover, as shown in Figure 5, we compare average valuation differences across sce-

[36] Note, due to a coding error we only collected this data for YouTube for 57% of participants.

narios based on anticipated change in network size. Respondents who anticipated above-median changes in their focal app's network size due to the TikTok ban exhibited significantly larger shifts in valuations between the TikTok ban and individual TikTok deactivation scenarios than respondents who anticipated below-median changes ($p < 0.01$). These patterns are consistent with network effects playing an important role in defining markets for network goods.[37]

Figure 5: Individual versus Collective Treatment Effect and Anticipated Network Change (Pooled Across Platforms)



Notes: We ask respondents a question on their anticipated network change: "If the TikTok ban happens for everyone in the U.S., the amount of time I would expect my friends to spend on [platform] would..." with answers being on a 7-point likert scale ("Strongly decrease", "Decrease", "Slightly decrease", "Not change", "Slightly increase", "Increase", "Strongly increase"). The figure displays the average change in WTA between the ban scenario and the individual TikTok deactivation separately for respondents with below- and above-median anticipated changes in their network size. The error bars represent 95% confidence intervals

3.6 Robustness

Perceived Probability In normal times, studying incentivized valuations under collective deactivation is difficult because the deactivation may be perceived as having a low

[37]This pattern also holds when looking at the individual platforms (see Appendix Figure A9).

probability of occurring. Given the large amount of uncertainty about the TikTok ban, we found it ex ante likely that respondents would perceive the TikTok ban to be relatively plausible. To quantify the perceived credibility of the ban, we directly elicit participants' beliefs about the probability of the TikTok ban occurring on January 19, 2025. On average, respondents report a perceived likelihood of 46%. Additionally, this perceived likelihood is similar in magnitude to respondents' perceived probability (52%) of being asked to deactivate their TikTok accounts if the ban does not occur and they are selected for the deactivation stage. We show in Appendix Table A2, Figure A10, and Figure A11 that our results are robust to focusing on participants with either an above or below median perceived likelihood for either event.

Dropping regretters Next, we examine the robustness of our findings depending on whether respondents agree with the valuation implied by their responses. In Appendix Table A3 and Figure A12, we show that our estimates are robust to dropping anyone who regrets at least one of their choices in any of the four WTA elicitations (5.6%).

Order of treatments Recall that we randomly varied the order in which we presented the TikTok ban and individual TikTok deactivation scenarios during the experiment. We find that our results remain consistent regardless of the order of elicitation in Appendix Table A4, Table A5, Table A6, Figure A13, Figure A14, and Figure A15.

Compliance We randomly selected 1 out of 10 participants for the deactivation study. After the random BDM draw, 55 participants were invited to deactivate their focal app based on their reported valuation. A majority (60%) of participants agreed to participate.[38] The compliance rate with the deactivation was 76%, which provides further support that our design was perceived as credible by participants.[39] Importantly, we find no differential

[38]Since we needed to re-contact participants through the Prolific platform, most of those who did not agree to participate simply did not respond to our message; it is therefore possible they did not see the message.

[39]We monitor compliance by tracking screen time on participants' iPhones, although we cannot rule out the possibility that participants accessed TikTok using alternative devices. This potential discrepancy represents a possible difference between our individual and collective treatments, as access from any device was fully restricted only during the TikTok ban. Nevertheless, in both treatments, participants could still theoretically access TikTok on laptops by employing VPNs—a common method for circumventing country-specific online restrictions.

compliance rate across platforms.[40] Our WTA measure captures the option value of deactivation and therefore there is a chance people do not comply with the TikTok deactivation in the individual deactivation scenario. In Appendix Figure A16, we also show that our results are robust to this possible concern regarding differences in compliance rates under the collective versus individual deactivation.[41]

3.7 Diversion ratios

The previous estimates provide evidence on how substitution patterns change after accounting for network effects, but they do not directly map onto parameters commonly used in antitrust analysis, such as diversion ratios.[42] To address this concern, we provide evidence of a related parameter, the second-choice Wald estimator (Conlon and Mortimer, 2021), used in practice by some antitrust authorities (Competition and Markets Authority, 2017). This parameter is given by the gain in users of a focal app (at a given price of this focal app) divided by the lost (original) TikTok users in response to a TikTok deactivation or ban. As Conlon and Mortimer (2021) show, the Wald estimator in general differs from the average diversion ratio and is equivalent, under LATE-like assumptions, to the average diversion ratio among "compliers" (TikTok users in our surveys who stop using TikTok).

Appendix Figure A17 (a) presents Wald estimates for Instagram, YouTube, and Snapchat, calculated at different levels of the WTA (around 0) to deactivate each of these platforms, as a proxy of their price.[43] That figure shows that the Wald estimates for the focal apps calculated under the collective ban are in general larger than those calculated under the individual deactivation. Indeed, Figure A17 (b) confirms that this difference is positive

[40] We have a 70% compliance rate for people in our deactivation group for the YouTube app (7 out of 10), 80% for people in our deactivation group for the Instagram app (8 out of 10) and 77% for people in our deactivation group for the Snapchat app (10 out of 13).

[41] In particular, we correct for this by assuming the chance of individual TikTok compliance is the same as the average compliance rate (76%), which is a conservative estimate as it assumes that each phone app compliance is independent. In particular, we adjust the WTA under the individual deactivation by assuming that: $WTP^{YouTube}_{ind,measured} = p \cdot WTP^{YouTube}_{ind,true} + (1-p) \cdot WTP^{YouTube}_{noban,true}$, where p is the compliance rate.

[42] The diversion ratio is defined in the U.S. 2010 Horizontal Merger Guidelines as "the fraction of unit sales lost by the first product due to an increase in its price that would be diverted to the second product" (U.S. Department of Justice and Federal Trade Commission, 2010).

[43] Diversion ratios and Wald estimators are computed holding the price of alternative products fixed; i.e., measuring the horizontal change in their demand curves at the current market price. In the case of social media, such prices are not available, so we use the WTA to approximate these changes. Given potential noise in the estimation of the WTA (e.g., due to the hassle costs of deactivation), we compute the Wald estimates on an interval around the "market price" of a zero WTA.

and statistically significant for some intervals of the WTA. Put differently, taking these estimates at face value, one might reach different conclusions about substitution patterns from TikTok to other platforms depending on whether this parameter is computed using the individual vs. the collective deactivation data. The Wald estimates computed using data from the individual deactivation suggest there is little substitution to the focal apps while the estimates computed using the ban suggest that these products are substitutes.[44]

4 Measuring Substitution Using Collective Time Limits

A limitation of our previous analysis is that valuations capture substitution patterns primarily at the extensive margin (usage vs. non-usage), leaving unresolved how these translate into intensive-margin adjustments, such as changes in time allocation. A further concern arises from our elicitation method, which relies on respondents' ability to accurately anticipate the network effects associated with collective deactivations.

To address these limitations, we examine detailed time-use data derived from a collective social media time-limit intervention by NOMO (No Missing Out), a technology startup. Distinct from prior studies that consider individual-level interventions, our evidence leverages an intervention explicitly collective in nature. This dataset is particularly valuable since collective interventions are challenging to implement, requiring coordinated participation across a large number of users in the same network simultaneously.

Our main outcome of interest is whether substitution patterns observed in this collective time-limit experiment differ from those documented in prior individual-level social media deactivations in the literature. The results reported here, however, are only descriptive and should be interpreted cautiously given the lack of a randomized counterfactual group.[45]

4.1 The Collective Time-Limit Challenge

Context and Goals In fall 2024 NOMO initiated a two-week-long time limit framed as a challenge at the University of Chicago. This challenge served as a first prototype for the future launch of the app. The primary goal was to curb student usage of Instagram and

[44] One of the assumptions required to interpret the Wald estimates as the average diversion ratio among compliers is that users single-home, which is clearly violated in our setting. These caveats aside, these calculations are in line with our evidence in the previous parts and suggest that these platforms become *closer* substitutes to TikTok under collective deactivation.

[45] Due to potential spillovers, data from University of Chicago students who did not comply with or participate in the challenge would not be an appropriate control group.

TikTok by imposing a combined daily usage cap of 60 minutes. The challenge explicitly targeted the university's undergraduate student body, numbering approximately 7,500 students, who were required to enroll via their institutional email addresses. This university setting provides an ideal environment for examining collective interventions, given the significant role campus-based social networks play in shaping social media consumption and the practical challenges of targeting entire networks in other contexts.

The Collective Challenge The challenge was inherently collective for several reasons. Recruitment was primarily conducted via word-of-mouth among friends and targeted classroom visits, making it likely that each participant's friends would also be recruited to the challenge. The deactivation therefore targeted a concentrated and ultimately sizeable share of the university undergraduate population. Additionally, the challenge was administered using a digital platform designed around community-driven challenges, thus making clear to each participant that their recruited friends were subject to the time limit as well.[46] A total of 808 undergraduates (approximately 11% of the total undergraduate population) enrolled in the time-limit challenge at the University of Chicago.

4.2 Summary Statistics

NOMO collected 246 submissions from the participants initially enrolled—65% of whom used iPhones and submitted screenshot data. After screening out invalid screenshots, we end up with valid screenshot data for 161 respondents. We focus our analysis on participants who used at least one of TikTok or Instagram during the pre-treatment week (70.7%), resulting in a final sample of N=106 users. The average age in the challenge sample is 19 years, with 54% females.[47]

Screen Time Measures We find that during the pre-treatment week, users spent an average of 258 minutes per day (4.30 hours) across all apps, with TikTok and Instagram—the apps targeted by the collective time limit challenge—accounting for 68 minutes (26%). In

[46] The challenge incentivized compliance through a structured reward system. Notably, a collective incentive was implemented: the residential house with the highest proportion of compliant participants received tickets to "Harry Potter and the Cursed Child." Other incentives included complimentary Starbucks beverages, charitable donations to local animal shelters, and access to Billie Eilish's sold-out Chicago concert.

[47] The sample consists of 32% first-year students, 36% second-year students, 23% third-year students, and 9% fourth year students.

the pre-treatment week, participants spent 43 and 25 minutes per day on Instagram and TikTok, respectively.

4.3 Results

4.3.1 Main Estimates

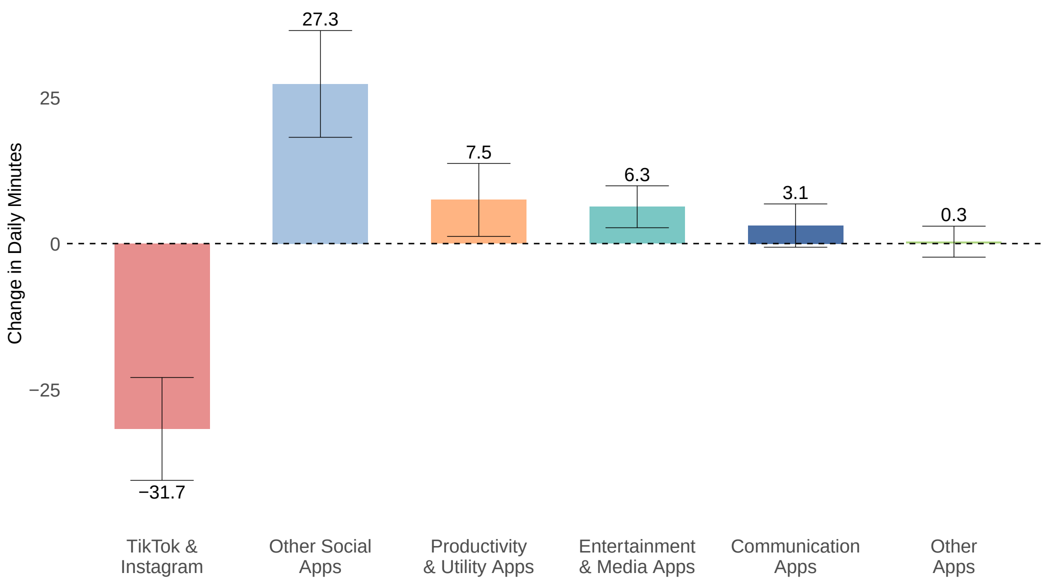
Figure 6 displays changes in time spent in different categories of apps during the two-week time limit challenge compared to the week before the challenge. The figure shows that challenge participants substantially reduced their daily scrolling time of TikTok and Instagram by 31.7 minutes (or 47.7%) compared to the baseline ($p < 0.01$). Notably, participants largely substituted this reduction towards increased daily usage of other social apps, broadly defined (e.g. YouTube, Snapchat, LinkedIn, Reddit, Pinterest, Facebook, and X), by approximately 27.3 minutes ($p < 0.01$). We see a modest increase of 7.5 and 6.3 minutes for productivity and utility apps (such as Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, and Duolingo) as well as entertainment and media apps (such as Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, and Candy Crush), respectively. We document relatively muted effects on communication apps (a 3.1 minute increase, $p = 0.102$) and other apps (a 0.3 minute increase, $p = 0.808$).

Further, Appendix A18 compares the cumulative distribution functions of time spent on each category from pre-treatment to post-treatment weeks. Overall daily screen time during the study period changes by an average of -1 minutes or -0.6% compared to the baseline week. These substitution patterns suggest that under a collective intervention, which generates changes in network size, students significantly shift their usage towards other social apps relative to other apps.

We can interpret our substitution patterns as Wald estimates following the product unavailability approach from Conlon and Mortimer (2021). As previously mentioned, Wald estimates are closely related to diversion ratios, a key parameter for antitrust analysis.[48] With time use data, the Wald estimate is given by the average share of baseline time that is diverted toward a given application during the challenge period. Thus, our substitution estimates directly imply the Wald estimate to other social apps is 86%. A comprehensive list of category definitions and classified apps can be found in Appendix Section B.3.

[48] Conlon and Mortimer (2021) show that the Wald estimator in general differs from the average diversion ratio and is equivalent, under LATE-like assumptions, to the average diversion ratio among "compliers." In our case, we include all participants in the challenge (both full and partial compliers); as a robustness check (see Appendix Section B.6), we re-estimate restricting to compliers only, with very similar results.

Figure 6: Substitution Patterns During the Two-Week Time Limit Challenge



Notes: This figure presents the average change in daily minutes spent on app categories during the two-week time limit challenge for participants compared to the previous week. We categorize apps into six groups: (1) "TikTok & Instagram," which includes the two apps affected by the 1-hour time limit; (2) "Other Social Apps," defined as broader social platforms built around user-generated or community-driven content (e.g., YouTube, Snapchat, LinkedIn, Reddit, Pinterest, Facebook, and X); (3) "Productivity & Utility Apps," defined as applications that support information access, organization, work, study, navigation, or everyday tasks, such as email clients, note-taking tools, browsers, and transport apps (e.g., Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, Duolingo); (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games (e.g., Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, Candy Crush, PUBG); (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds (most notably Messenger, Messages, WhatsApp, Discord, FaceTime, GroupMe, Slack, and BeReal); (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section B.3. Error bars represent 95% confidence intervals.

Since network effects might unfold gradually rather than instantaneously as users observe and respond to peer adoption decisions, the aggregate Wald estimates documented above might underestimate the longer-term substitution patterns. Consistent with this hypothesis and displayed in Figure A19, the substitution rate in week 1 is 79%, whereas in week 2 it increases to 94%. Although we lack statistical power to distinguish differences in Wald estimates across these two weeks, the observed increase highlights the importance of leveraging variation in collective time use sustained over longer durations to accurately measure substitution dynamics.

4.3.2 Benchmarking Substitution Patterns

Our main result from the collective time-limit challenge is that 86% of the decrease in time spent on TikTok and Instagram is substituted towards other social apps. To our knowledge, this is the first estimate of substitution patterns from a collective intervention that took place over a sustained period of time.[49] We benchmark our results against the literature on individual deactivation challenges. Two studies provide comparable estimates from individual-level interventions that hold network effects fixed.

Aridor (2025), the most closely related paper, finds an 18.5% (approximately 4 minutes) and a 9% (approximately 4 minutes) time substitution toward other social apps following an individual Instagram and YouTube deactivation, respectively. Allcott et al. (2025b) find that deactivating Instagram results in a 39% increase (approximately 8 minutes) in time spent on other social media apps, while deactivating Facebook leads to a 41% increase (approximately 15 minutes).[50] In summary, the estimates from both studies are substantially lower than those observed in our collective intervention.

4.3.3 Limitations

Robustness checks In Appendix B.6, we demonstrate that our estimates are robust across various sample inclusion criteria, different levels of winsorization, and focusing on users with perfect challenge compliance.

Limitations Our results are subject to several limitations. First, the evidence is descriptive in nature given the lack of a randomized control group that undergoes an individual-level deactivation. We also cannot rule out the possibility of underlying time trends during our study period.[51] Future work should analyze the effects of randomly assigned collec-

[49]Rehse and Valet (2025) find a 18.4% increase in non-Meta social usage after a six-hour Meta outage. This magnitude is similar to the 18.5% increase in the time spent on non-Instagram social applications following the Instagram restriction in Aridor (2022). Consistent with our findings on the dynamics of diversion, this similarity could be explained by the fact that coordination takes time to materialize.

[50]Notably, Allcott et al. (2025b) involved young to middle-aged adults, whereas our evidence from the collective challenge comes from U.S. undergraduate college students. Further, the respondents exhibited significantly lower average baseline usage of Instagram compared to our sample.

[51]Aridor et al. (2025) provides evidence that election content consumption on smartphones was limited and stable during the 2024 U.S. Presidential election, suggesting that election-related events are unlikely to contribute to an underlying time trend for our results.

tive versus individual interventions. Second, our analysis focuses on substitution patterns arising from a joint reduction in TikTok and Instagram usage; thus, we cannot separately identify how substitution might differ if the intervention targeted only one platform or involved complete deactivation. While our findings reflect substitution patterns influenced by network effects—given that recruitment heavily relied on peer networks, the short duration (two weeks) and limited penetration of the network (approximately 11% of the undergraduate population) imply that these estimates likely represent a lower bound on substitution responses driven by network dynamics. Finally, our sample comprises self-selected University of Chicago undergraduates who chose to join the challenge and provide screenshot data. Future research should gauge how generalizable these findings are to broader populations.

5 Conclusion

In this paper, we document a gap between substitution patterns that account for network effects and those that do not. Our framework and estimates highlight that individual and collective treatments can lead to qualitatively different conclusions about which alternative goods are substitutes or complements. Our incentivized experiment with young Americans reveals that valuations for other social apps increase more sharply in response to a collective TikTok ban compared to an individual TikTok deactivation. Conversely, intended substitution patterns toward non-social goods are stronger in the case of an individual TikTok deactivation. We additionally analyze actual time use data from a collective social media time limit challenge, where we find larger substitution to other social apps compared to prior individual deactivation estimates in the literature.

Our results suggest that the failure to account for network effects could result in mismeasuring a product's relevant market. For TikTok, accounting for network effects reveals that other social apps are closer substitutes than suggested by fixed-network estimates, making it more likely that they are part of the relevant market. At the same time, our estimates suggest that non-social activities—such as video gaming and meditation—are weaker substitutes for social media, making it less likely that they are part of the relevant market. Thus, network effects may make the market narrower—vis à vis non-social activities—yet broader within the set of social media apps. Beyond social media, our findings carry important implications for antitrust policy regarding network goods.

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Online Appendix: Not for publication

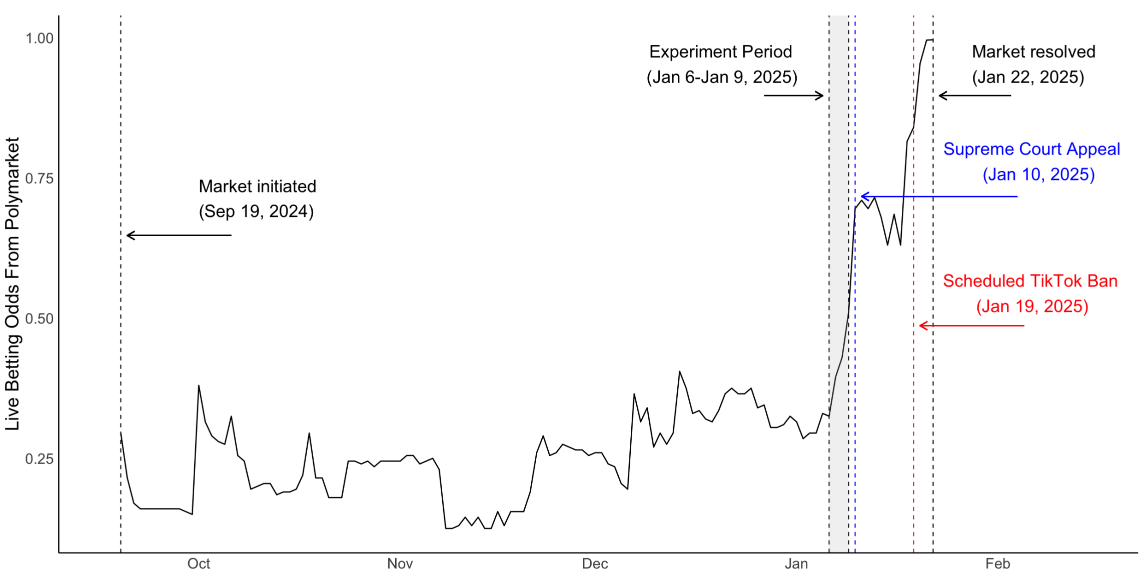
Our supplementary material is structured as follows. Appendix A includes additional tables and figures about the TikTok collective versus individual experiment. Appendix B includes additional tables and figures about the collective time limit challenge.

A Deactivation Experiment: Additional Tables and Figures

A.1 Polymarket Live Odds

Online Betting Market Data We collect data from Polymarket, one of the biggest live online betting markets in the world, which shows the live market-implied probability of the TikTok ban occurring over time. In Figure A1, we display the live odds on Polymarket from September 19, 2024—the date the market was created by platform market makers—through January 22, 2025 when it was resolved following the implementation of the TikTok ban. We implement the "TikTok ban" deactivation scenario for our randomly chosen individuals in the deactivation experiment from Section 3 based on this market resolution. The figure shows that our experiment was conducted during a period of time when the TikTok ban was highly uncertain and the probability of its implementation was volatile. This supports the credibility that both scenarios were taken seriously.

Figure A1: Implied Probability of TikTok Ban Implementation Over Time (Polymarket Betting Data)



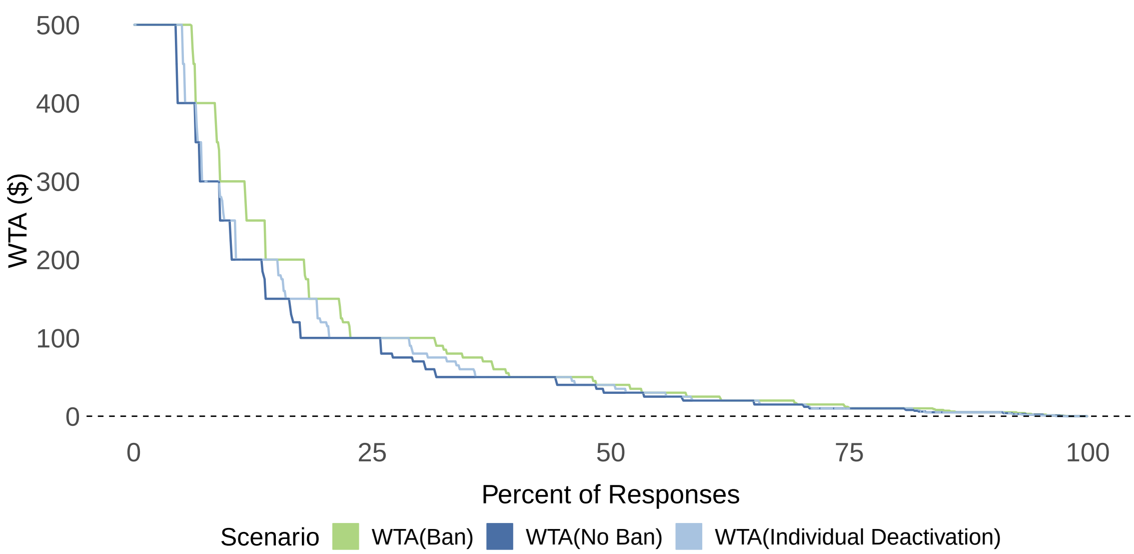
Notes: Figure A1 illustrates the evolution of market expectations regarding the probability of a TikTok ban, based on data extracted from Polymarket from September 19, 2024 when the market was initiated by the platform market makers until January 22, 2025 when the market was resolved after the TikTok Ban was implemented. The vertical dashed blue line marks the Supreme Court appeal hearing on January 10, a day after our data collection ended. The vertical dashed red line marks the implementation of the scheduled TikTok ban on January 19, approximately 10 days after our data collection ended.

A.2 Inverse Demand Functions

Figure A2 displays the inverse demand curve for respondents' WTA for deactivating their assigned alternative app for each scenario pooled across platforms. Each point on the curve reflects the share of individuals whose WTA for losing access exceeds a given dollar amount. Based on our elicitation method, the values are bounded between \$0 and \$500 dollars. The green curve represents valuations under a *TikTok ban*, the light blue curve corresponds to the *individual TikTok deactivation*, and the dark blue curve reflects valuations under the *no TikTok ban* scenario.

Notably, we find that WTA under the *TikTok ban* scenario results in a rightward shift of the inverse demand curve relative to the *no TikTok ban* scenario. The inverse demand curve corresponding to the *individual TikTok deactivation* scenario lies between the other two scenarios. As displayed in Figure A2, we see that the rightward shift for the *TikTok ban* scenario occurs almost exclusively in the first 50% of respondents. This suggests that cross-product network effects are larger in absolute magnitude (more negative) for individuals who already place an above average value on the alternative platform in the baseline scenario.

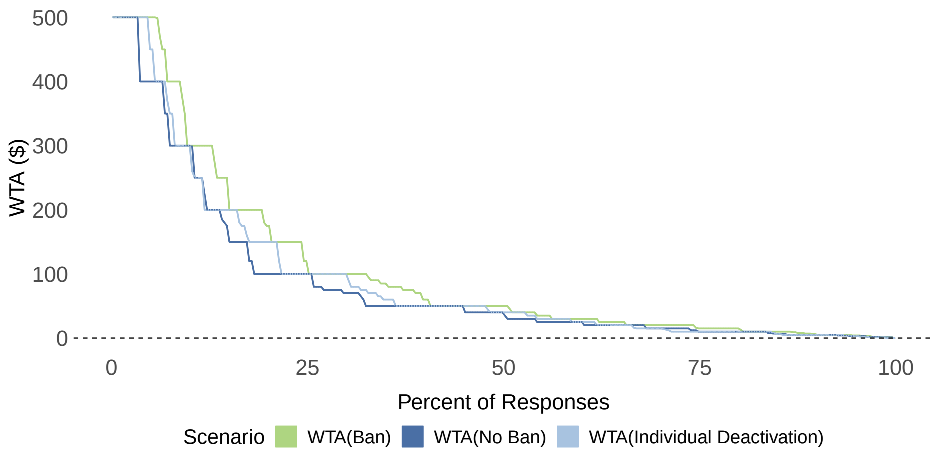
Figure A2: Inverse Demand Curves (Pooled)



Notes: Figure A2 displays the inverse demand curve for respondents' incentivized willingness-to-accept (WTA) for deactivating the social app (Instagram, YouTube or Snapchat) for four weeks under three scenarios. The green curve shows WTA under the TikTok ban scenario. The dark blue curve shows WTA under the status quo scenario, where no TikTok ban and no individual TikTok deactivation occur. Finally, the light blue curve shows WTA under the individual TikTok deactivation.

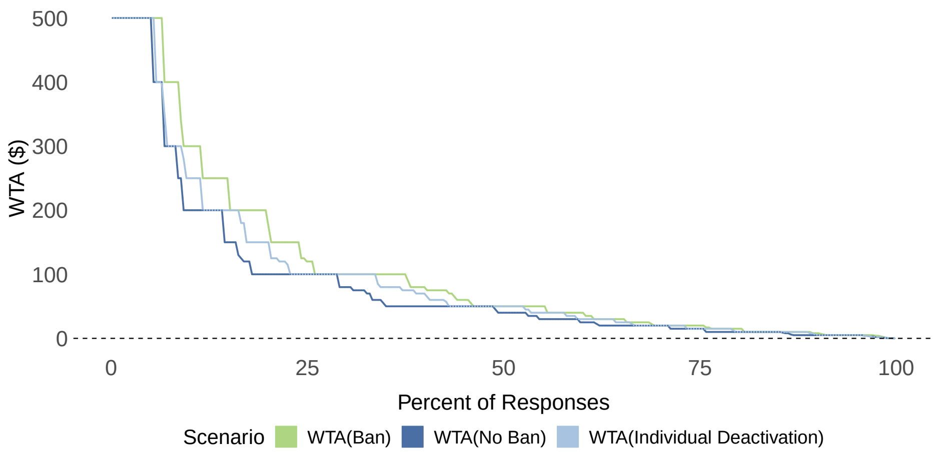
The figures below display the inverse demand functions for all three platforms separately across three scenarios: no TikTok ban, individual TikTok deactivation, and TikTok ban.

Figure A3: Inverse Demand Function for Instagram



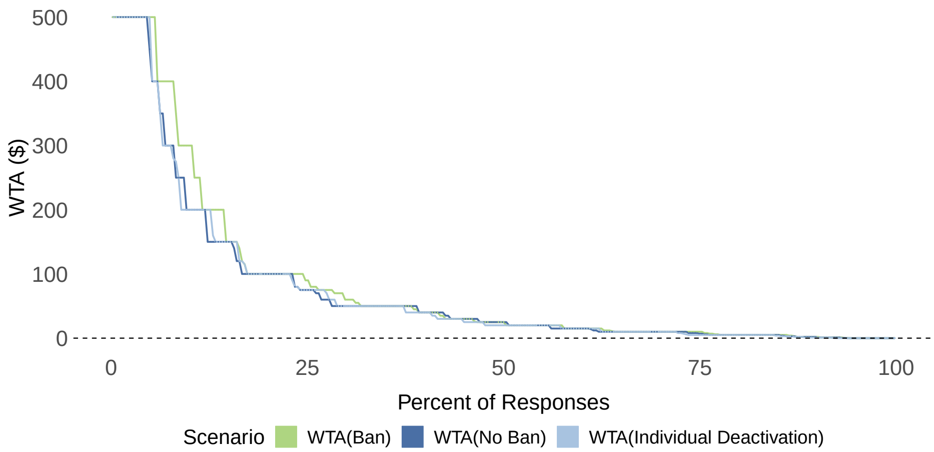
Notes: Figure A3 displays the inverse demand curve for respondents' incentivized willingness-to-accept (WTA) for deactivating the Instagram app for four weeks under three scenarios. The green curve shows WTA under the TikTok ban scenario. The dark blue curve shows WTA under the status quo scenario, where no TikTok ban and no individual TikTok deactivation occur. Finally, the light blue curve shows WTA under the individual TikTok deactivation.

Figure A4: Inverse Demand Function for YouTube



Notes: Figure A4 displays the inverse demand curve for respondents' incentivized willingness-to-accept (WTA) for deactivating the YouTube app for four weeks under three scenarios. The green curve shows WTA under the TikTok ban scenario. The dark blue curve shows WTA under the status quo scenario, where no TikTok ban and no individual TikTok deactivation occur. Finally, the light blue curve shows WTA under the individual TikTok deactivation.

Figure A5: Inverse Demand Function for Snapchat



Notes: Figure A5 displays the inverse demand curve for respondents' incentivized willingness-to-accept (WTA) for deactivating the Snapchat app for four weeks under three scenarios. The green curve shows WTA under the TikTok ban scenario. The dark blue curve shows WTA under the status quo scenario, where no TikTok ban and no individual TikTok deactivation occur. Finally, the light blue curve shows WTA under the individual TikTok deactivation.

A.3 Continuous WTA Results

Table A1: Regression Results: Continuous WTA by Platform

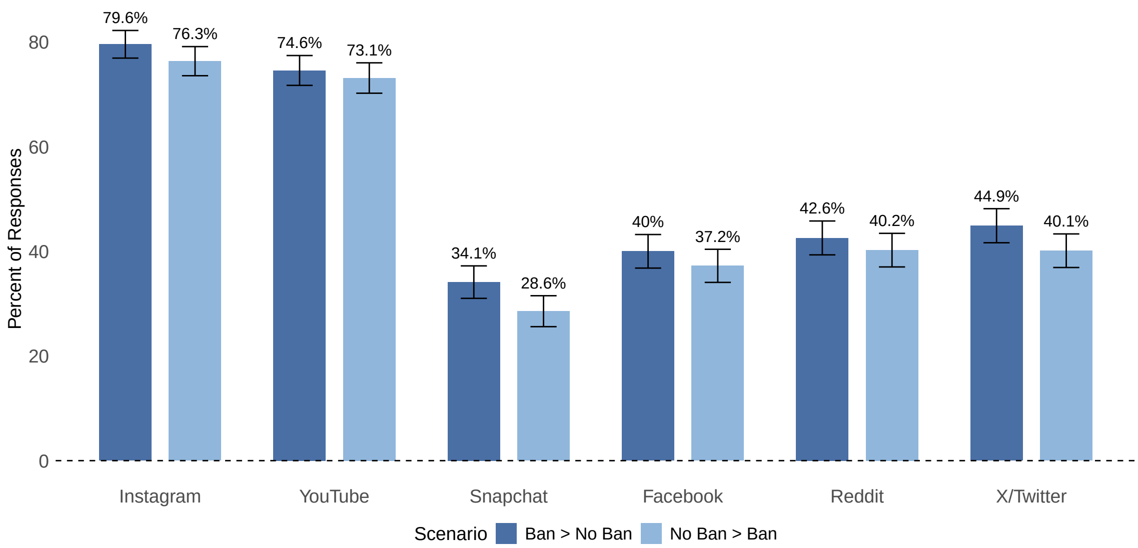
	Instagram	**YouTube**	**Snapchat**
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	13.658***	12.103***	7.837**
	(3.442)	(3.457)	(3.200)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	7.477*	10.589***	−0.121
	(3.831)	(3.122)	(3.055)
WTA: **No TikTok Ban**	83.372***	84.671***	73.761***
	(2.059)	(1.921)	(1.406)
R-squared	0.056	0.073	0.017
Number of Observations	316	287	297

Notes: Table A1 displays the regression results for our pre-registered specification for the continuous WTA measure. WTA: No TikTok Ban represents the average WTA to deactivate the platform when there is no TikTok ban. Δ_{WTA}: Individual TikTok Deactivation - No TikTok Ban is the change in WTA when going from no TikTok ban to individual TikTok deactivation. Δ_{WTA} TikTok Ban - Individual TikTok Deactivation is the change in WTA when going from individual TikTok deactivation to TikTok ban. These regressions were pre-specified. Robust standard errors are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Anticipated time substitution

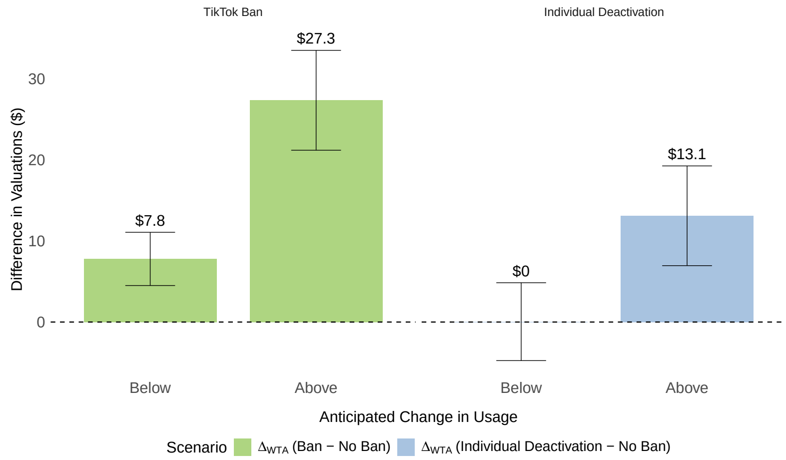
In Appendix Figure A6 below, we present results on how individuals expect to shift toward other social media platforms in response to both a TikTok ban and an individual TikTok deactivation. Our findings indicate that people anticipate significantly greater substitution toward YouTube and Instagram compared to other platforms, such as Snapchat, in both scenarios.

Figure A6: Proportion of Respondents Indicating an Increase in Time Spent on a Given Platform Under Individual TikTok Deactivation and TikTok Ban Scenarios



Notes: Figure A6 presents the fraction of respondents who expect to increase their usage of various social media platforms following either an individual TikTok deactivation of TikTok (light blue) or a TikTok ban (dark blue), with answers being rated on a 7-point Likert scale ("Strongly decrease", "Decrease", "Slightly decrease", "Not change", "Slightly increase", "Increase", "Strongly increase"). The error bars represent 95% confidence intervals.

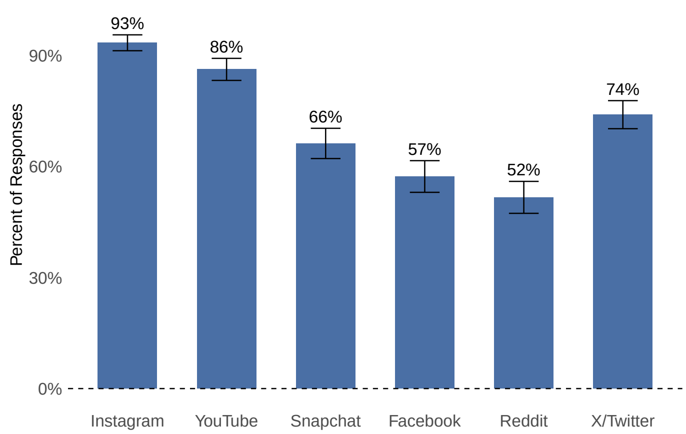
Figure A7: Treatment Effect and Anticipated Substitution Time Change



Notes: For both the TikTok ban and individual deactivation scenarios, Figure A7 displays the average change in WTA from the No Ban scenario separately for respondents with below- and above-median anticipated changes in their time use of their alternative platform under the given scenario. The error bars represent 95% confidence intervals.

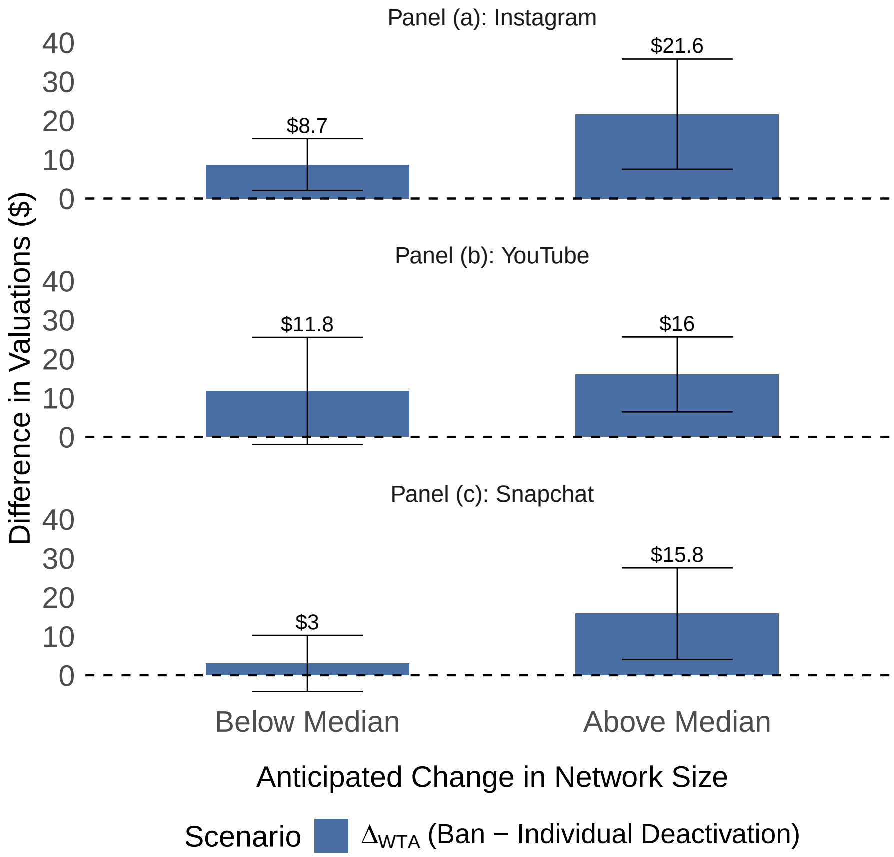
A.5 Anticipated network effects

Figure A8: Share of respondents expecting that their friends will spend more time on this platform if TikTok is banned



Notes: Figure A8 presents the fraction of respondents who expect their friends to increase their usage of various social media platforms following a TikTok ban, with answers being rated on a 7-point likert scale ("Strongly decrease", "Decrease", "Slightly decrease", "Not change", "Slightly increase", "Increase", "Strongly increase"). The error bars represent 95% confidence intervals.

Figure A9: Treatment Effect and Anticipated Network Change (Split by Platform)



Notes: We ask respondents a question on their anticipated network change: "If the TikTok ban happens for everyone in the U.S., the amount of time I would expect my friends to spend on [platform X]..." with answers being on a 7-point likert scale ("Strongly decrease", "Decrease", "Slightly decrease", "Not change", "Slightly increase", "Increase", "Strongly increase"). The figure displays the average change in WTA between the TikTok ban scenario and the individual TikTok deactivation separately for respondents with below- and above-median anticipated changes in their network size for their assigned platform. Panel (a) shows the difference in valuations for Instagram. Panel (b) shows the same for YouTube, and Panel (c) for Snapchat. The error bars represent 95% confidence intervals.

A.6 Robustness

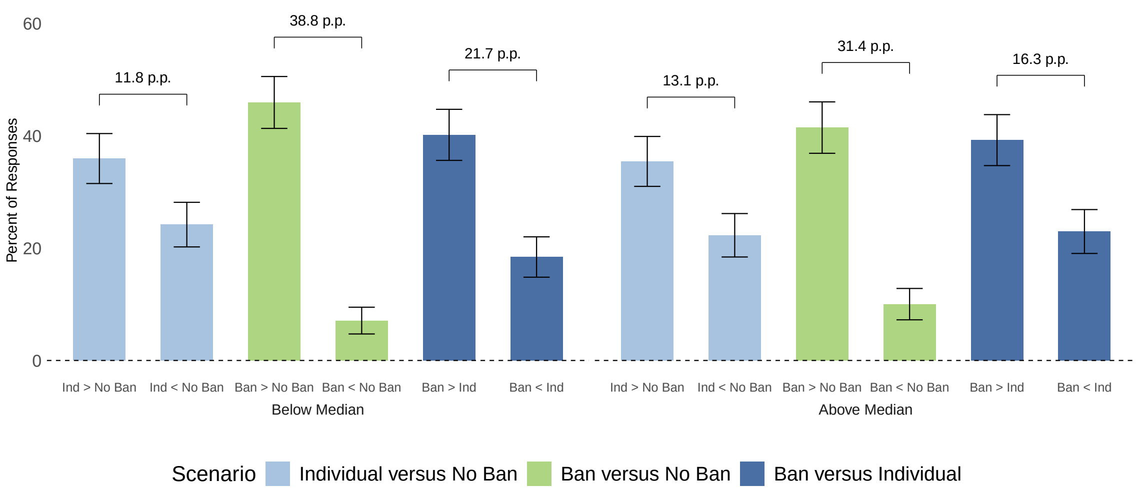
Perceived Likelihood In columns 1 and 2 of Table A2 below, we separately present the continuous WTA results for people with above and below median perceived likelihood of the TikTok ban occurring. In columns 3 and 4 we separately present the results for people with above and below median perceived likelihood of the individual TikTok deactivation occurring. We pool across outside options for ease of exposition. We also present these results for the fraction with higher or lower valuations in Figures A10 and A11.

Table A2: Continuous WTA by Median Perceived Likelihood Split

	TikTok Ban		Individual TikTok Deactivation	
	Below Median	**Above Median**	**Below Median**	**Above Median**
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	12.522***	9.955***	10.459***	11.875***
	(2.941)	(2.551)	(3.160)	(2.423)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	6.766**	5.155**	6.553*	5.482***
	(3.071)	(2.427)	(3.509)	(2.116)
WTA: **No TikTok Ban**	89.600***	71.589***	94.563***	69.304***
	(1.700)	(1.269)	(1.858)	(1.194)
R-squared	0.047	0.044	0.035	0.059
Number of Observations	451	449	403	497

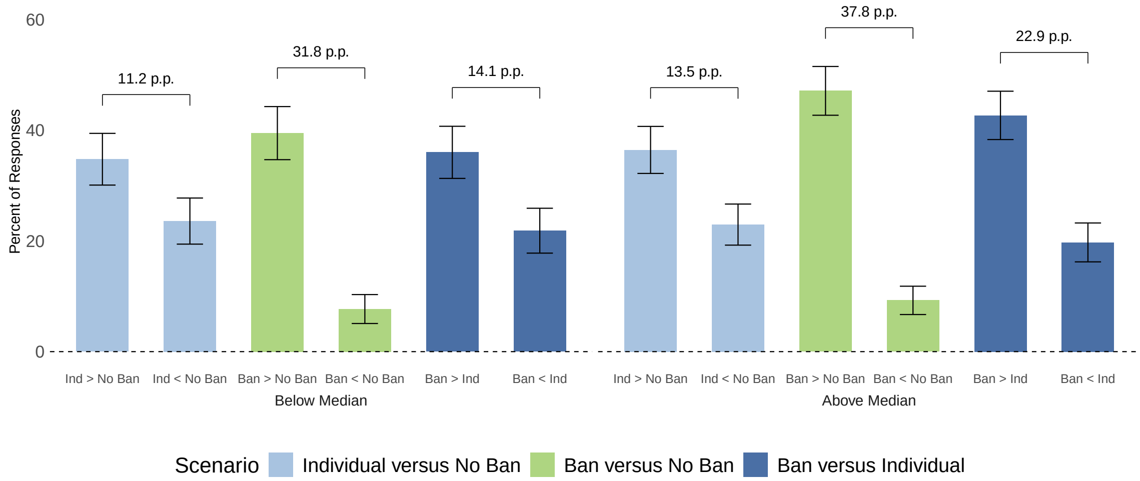
Notes: Table A2 displays the regression results for the continuous WTA for participants above and below the median perceived likelihood for both the TikTok ban and the individual TikTok deactivation. We find similar average differences in valuation between the three scenarios for those above or below the median of either perceived likelihood elicitation. Robust standard errors are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A10: Fraction with Higher or Lower Valuation By Median Perceived Likelihood Split of TikTok Ban



Notes: Figure A10 illustrates the fraction with higher or lower valuation by scenario for those above and below the median perceived likelihood of the TikTok ban. The light blue bars shows the proportion of individuals who have a higher or lower WTA to deactivate their alternative option platform during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

Figure A11: Fraction with Higher or Lower Valuation By Median Perceived Likelihood Split of Individual TikTok Deactivation



Notes: Figure A11 illustrates the fraction with higher or lower valuation by scenario for those above and below the median perceived likelihood of the individual TikTok deactivation. The light blue bars shows the proportion of individuals who have a higher or lower WTA to deactivate their alternative option platform during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

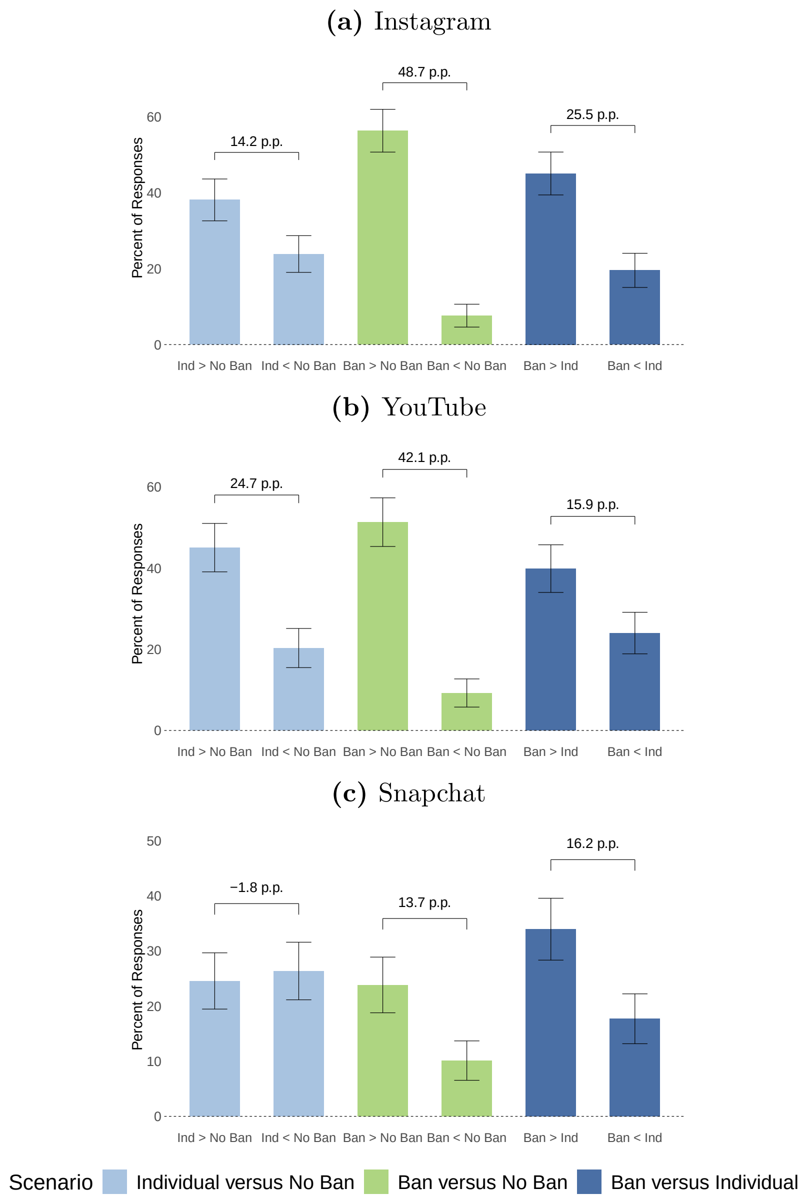
Regret We allow our respondents to regret their valuations to ensure accurate data quality. After entering their BDM, we ask them if they would agree to participate in the deactivation for their implied valuation. Specifically, we ask whether they agree with the valuation implied by their answer. For example, "You indicated that you would accept $X USD to deactivate your TikTok account for four weeks if TikTok is not banned. Do you agree?". If they disagree, they are redirected to start again and allowed to complete their decision a second time. We asked them if they regret their choice a second time, but everyone proceeds with the next step regardless of their answer. We find that 5.6% of people regret at least one choice in one of the four scenarios they face. In accordance with our pre-registration, we exclude anyone that regrets their choice twice. Our low values of regret are likely helped by including an explanation of the deactivation procedure for Facebook. In Table A3 below, we show that our continuous WTA results are robust to dropping anyone who regrets a choice, even once.

Table A3: Regression Results Without People Who Regret First Valuation: Continuous WTA by Platform

	Instagram	**YouTube**	**Snapchat**
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	14.244***	11.770***	9.804***
	(3.585)	(3.618)	(3.042)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	7.489*	10.705***	−2.282
	(3.995)	(3.285)	(2.887)
WTA: **No TikTok Ban**	83.912***	85.566***	74.607***
	(2.146)	(1.998)	(1.408)
R-squared	0.057	0.070	0.024
Number of Observations	302	277	271

Notes: Table A3 displays the regression results for our pre-registered specification for the continuous WTA measure but dropping anyone who regrets their first valuation for any scenario. WTA: No TikTok Ban represents the average WTA to deactivate the platform when there is no TikTok ban. Δ_{WTA}: Individual TikTok Deactivation - No TikTok Ban is the change in WTA when going from no TikTok ban to individual TikTok deactivation. Δ_{WTA}: TikTok Ban - Individual TikTok Deactivation is the change in WTA when going from individual TikTok deactivation to TikTok ban. These regressions were pre-specified. Robust standard errors are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A12: Fraction with Higher or Lower Valuation By Scenario Without People Who Regret First Valuation



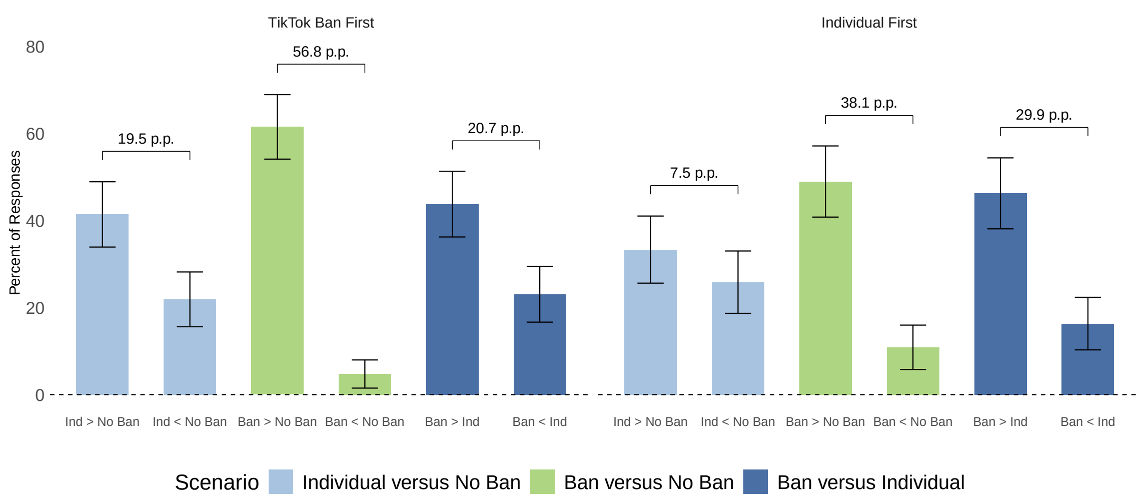
Notes: By platform and excluding those who regret their first valuation, Figure A12 illustrates differences in the valuation of alternative apps across three scenarios: no TikTok ban, individual TikTok deactivation, and a TikTok ban. Panel a) is for Instagram, b) for YouTube, and c) for Snapchat. For each platform, the light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative option platform during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

Table A4: Continuous WTA Results by Order of Scenario: Instagram

	TikTok Ban First	**Individual First**
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	14.485***	12.706***
	(4.845)	(4.887)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	9.541	5.104
	(6.397)	(3.724)
WTA: **No TikTok Ban**	82.482***	84.395***
	(3.389)	(2.103)
R-squared	0.057	0.058
Number of Observations	169	147

Notes: Table A4 displays the regression results for the continuous WTA for Instagram by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). WTA: No TikTok Ban represents the average WTA to deactivate the platform when there is no TikTok ban. Δ_{WTA}: Individual TikTok Deactivation - No TikTok Ban is the change in WTA when going from no TikTok ban to individual TikTok deactivation. Δ_{WTA}: TikTok Ban - Individual TikTok Deactivation is the change in WTA when going from individual TikTok deactivation to TikTok ban. Robust standard errors are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A13: Fraction with Higher or Lower Valuation By Order of Scenario: Instagram



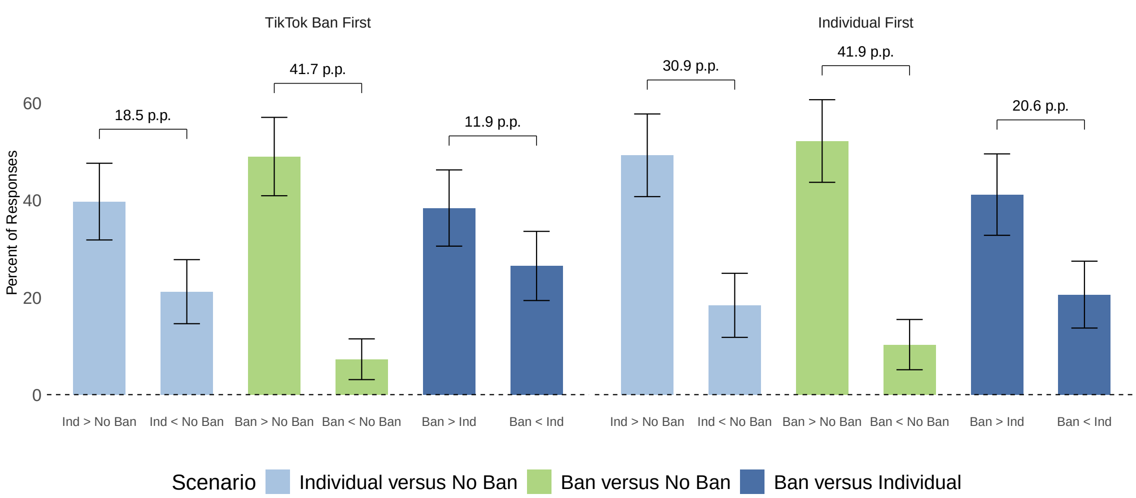
Notes: Figure A13 illustrates the fraction with higher or lower valuation by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). The light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative option platform during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

Table A5: Continuous WTA Results by Order of Scenario: YouTube

	TikTok Ban First	Individual First
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	12.050***	12.162**
	(3.965)	(5.834)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	9.185**	12.147**
	(3.620)	(5.231)
WTA: **No TikTok Ban**	79.142***	90.809***
	(2.027)	(3.377)
R-squared	0.097	0.060
Number of Observations	151	136

Notes: Table A5 displays the regression results for the continuous WTA for YouTube by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). WTA: No TikTok Ban represents the average WTA to deactivate the platform when there is no TikTok ban. Δ_{WTA}: Individual TikTok Deactivation - No TikTok Ban is the change in WTA when going from no TikTok ban to individual TikTok deactivation. Δ_{WTA}: TikTok Ban - Individual TikTok Deactivation is the change in WTA when going from individual TikTok deactivation to TikTok ban. Robust standard errors are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A14: Fraction with Higher or Lower Valuation By Order of Scenario: YouTube



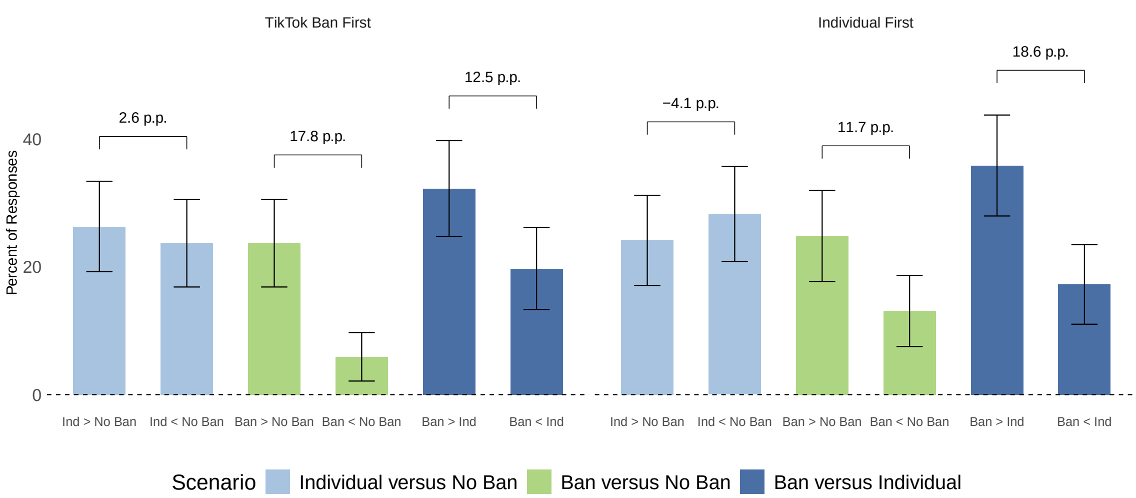
Notes: Figure A14 illustrates the fraction with higher or lower valuation by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). The light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative option platform during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

Table A6: Continuous WTA Results by Order of Scenario: Snapchat

	TikTok Ban First	Individual First
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	7.345	8.353**
	(4.987)	(3.970)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	1.859	−2.197
	(4.958)	(3.497)
WTA: **No TikTok Ban**	75.425***	72.017***
	(2.213)	(1.709)
R-squared	0.016	0.021
Number of Observations	152	145

Notes: Table A6 displays the regression results for the continuous WTA for Snapchat by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). WTA: No TikTok Ban represents the average WTA to deactivate the platform when there is no TikTok ban. Δ_{WTA}: Individual TikTok Deactivation - No TikTok Ban is the change in WTA when going from no TikTok ban to individual TikTok deactivation. Δ_{WTA}: TikTok Ban - Individual TikTok Deactivation is the change in WTA when going from individual TikTok deactivation to TikTok ban. Robust standard errors are in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

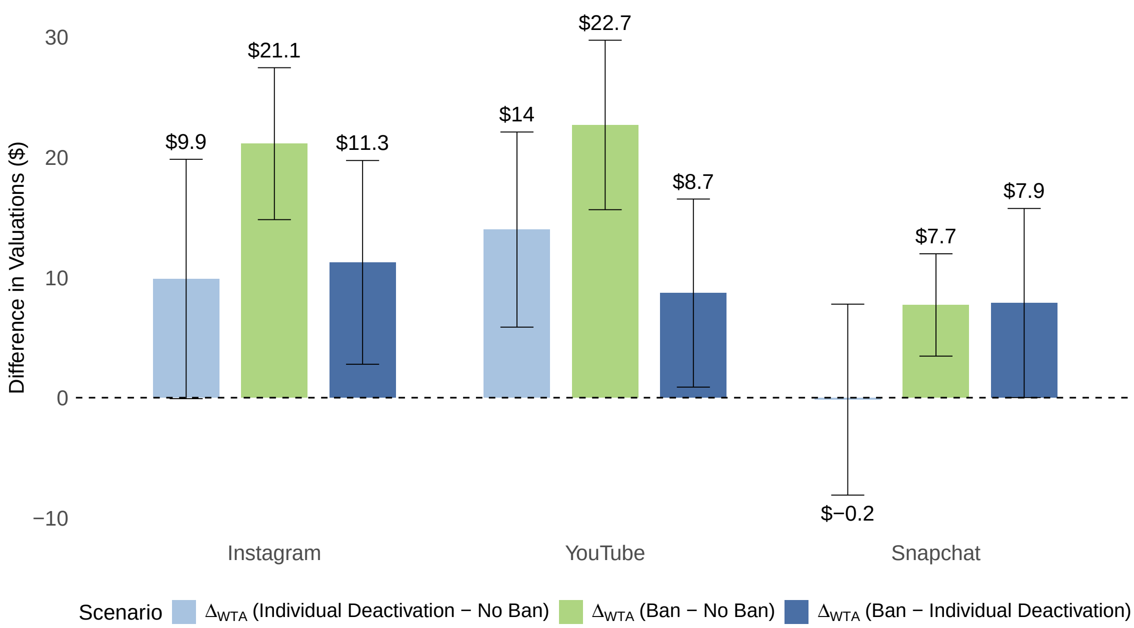
Figure A15: Fraction with Higher or Lower Valuation By Order of Scenario: Snapchat



Notes: Figure A15 illustrates the fraction with higher or lower valuation by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). The light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative option platform during individual TikTok deactivation compared to the no TikTok ban scenario. The value above the two bars displays the net fraction with a higher WTA. The green bars display the same proportions when comparing the collective TikTok ban compared to the no TikTok ban scenario. Similarly, the dark blue bars present the same proportions when comparing the TikTok ban to individual TikTok deactivation. The error bars represent 95% confidence intervals.

Implementation and Compliance As pre-specified, we selected 1 out of 10 respondents to be in the deactivation study, for a total of 90 participants. We exclude anyone with valuations at the upper bound, as these are not incentive compatible. We then conduct a random computer draw, after which we end up with 55 participants (21 for Snapchat, 15 for YouTube, and 19 for Instagram) that we invite to participate in the deactivation study. We received a response indicating interest in participation from 33 (60%) people. For YouTube and Instagram, 10 people attempted week 1 respectively (implying a 33% and 47% attrition rate). For Snapchat, 13 attempted week 1 successfully (implying a 38% attrition). The deactivation period started on January 20th and ended on February 16th. We find that 76%, or 25 out of 33, of our participants successfully completed the deactivation, for an average payout of $73. Importantly, we don't find differential compliance across platforms: our compliance rates are 70% for YouTube (7 out of 10), 80% for Instagram (8 out of 10) and 77% for Snapchat (10 out of 13).

Figure A16: Average Difference in Valuations Across Scenarios by Platform with Compliance Adjustment

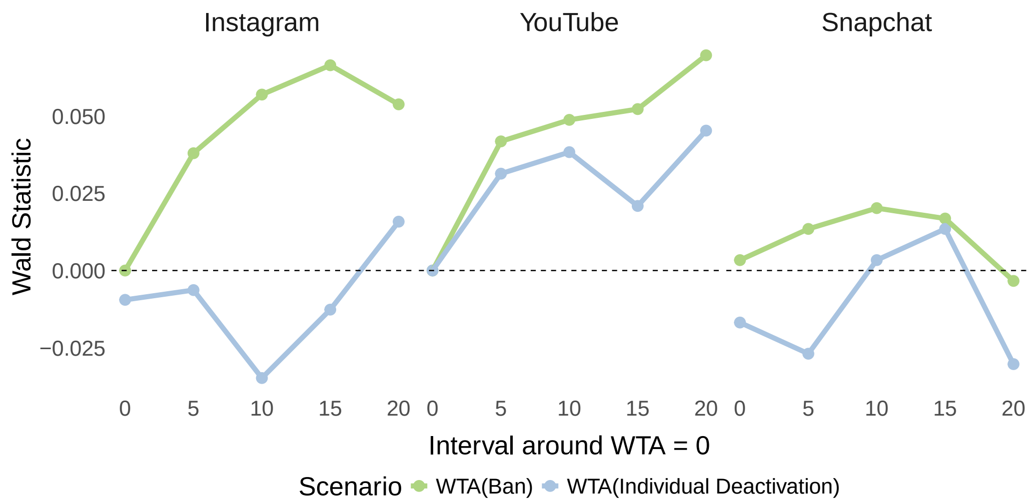


Notes: Figure A16 illustrates the differences in continuous valuations of the alternative app across our three scenarios, when correcting for the possible differential compliance between individual and collective interventions. We calculate the adjusted WTA under the individual deactivation by assuming that: $WTP^{YouTube}_{ind,measured} = p \cdot WTP^{YouTube}_{ind,true} + (1-p) \cdot WTP^{YouTube}_{noban,true}$, where p is the average compliance rate. The error bars indicate 95% confidence intervals.

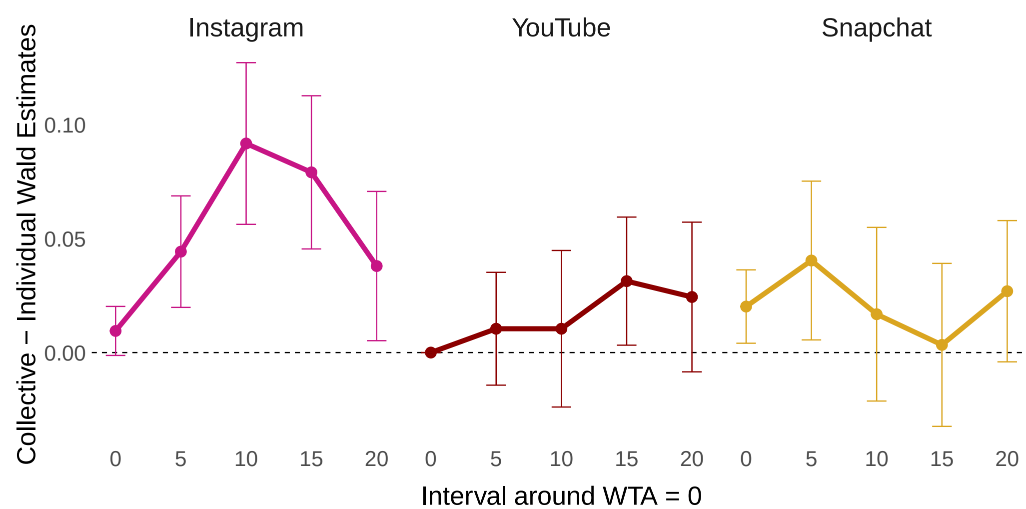
A.7 Wald Estimates

Figure A17: Wald Estimates by Platform

(a) Wald Estimates



(b) Difference in Wald Estimates



Notes: Panel a) of Figure A17 displays Wald estimates (Conlon and Mortimer, 2021) for two of our scenarios—the TikTok ban (green line) and individual TikTok deactivation (blue line)—on our three platforms (Instagram, YouTube, Snapchat). Panel b) of Figure A17 displays the difference between the Wald estimates for the collective versus individual scenario. We compute our estimates regressing the percent of people with a WTA equal to or greater than the WTA cutoff on an indicator of which scenario the value represents. The lines represent 95% confidence intervals, which we compute using a paired t-test.

B Collective Time Limit Challenge: Additional Analyses

B.1 Data Generation Process and Cleaning Procedure

Participant Recruitment and Data Collection The data was collected as a secondary outcome in a NOMO-administered pilot study primarily focused on measuring mental health outcomes. Participants were UChicago undergraduates who were recruited at the University of Chicago campus as well as in lectures at the university. The recruitment for participating in the two-week time limit challenge involved asking individuals to download the NOMO app and sign up for it using their University of Chicago email address.

Immediately after the challenge, participants were asked to submit weekly screenshots of their "Most Used Apps and Websites" activity as part of a follow-up survey emailed to them by NOMO. NOMO sent out two batches of emails with 246 out of 808 challenge sign-ups completing the survey successfully for a $15 Amazon gift card.

Measuring screen time We measure the substitution patterns of challenge participants using Apple's built-in Screen Time functionality, which tracks app-level device usage, frequency of device interaction, and total time spent. Screenshots covered three distinct intervals: one baseline week prior to the challenge and two treatment weeks during the challenge period. App-level screen-time data from participants' screenshots was extracted using OpenAI's GPT-4.1 model.

Data Cleaning Our initial dataset consists of everyone who completed NOMO's follow-up survey (N=246 survey responses). We first exclude Android respondents, who cannot provide comparable Screen Time data, removing 13.4% of initial responses. We then parse weekly app-level usage data and match survey responses to screenshots via unique identifiers. In this step we remove an additional 24.4% due to invalid uploads.[52] After these steps, our working sample contains N=161. Next, we eliminate respondents who indicated their college year as "Other" to focus on undergraduates only, yielding N=156. Finally, we restrict to participants who used at least one of the two deactivated apps (TikTok or Instagram) during the pre-treatment week, excluding 29.3% of the remaining sample and resulting in our final analysis sample N=106.

Usage data for the two challenge weeks are then aggregated into a single "challenge period" column. For each participant and each app, if the app appears in the screenshots for both periods, the app's usage value for the aggregated challenge period is calculated as the average of the two time values observed in the screenshots. If the app appears in the

[52] "Invalid uploads" are considered responses that failed to provide a full set of valid weekly screenshots; we also exclude obvious issues such as cropped or duplicate submissions and a small number of manual removals.

screenshot for only one week, the usage during the other week is assumed to be zero and the average is calculated accordingly.

A comprehensive list with our categories and classified apps can be found in Appendix Section B.3. Any participant with no observed usage of those apps in either period is assigned zero minutes in both the pre-treatment week and the aggregated "challenge" period. We then sum each user's app-level minutes to get their total category usage in each period. Before computing the first difference, we winsorize these category-specific usage values at the 95th percentile for each period to limit the impact of outliers. Descriptive summaries are calculated before winsorization.

B.2 Measurement Error

On average, each weekly screenshot captures a user's ten most-used apps. This truncation introduces two potential biases when estimating the share of time spent on TikTok and Instagram. First, if either app falls outside a user's top ten in a given week, its usage will be underreported. Second, by restricting the denominator to those ten apps we underestimate total screen time, which may inflate the computed share attributed to TikTok and Instagram.

To assess the magnitude of the second bias, we examine the share of total top-10 usage accounted for by the lowest-ranked app. For each screenshot, we compute the ratio of the least-used app's time to the total time across the top ten apps, and then we average these ratios across all screenshots. In the deactivation period, this average is roughly 1.9%, indicating that apps ranked below the top ten account for only a small fraction of screen time. Consequently, any upward bias in our TikTok/Instagram share estimates is minimal, and our figures likely represent a conservative lower bound on the true shares.

B.3 Category Classification List

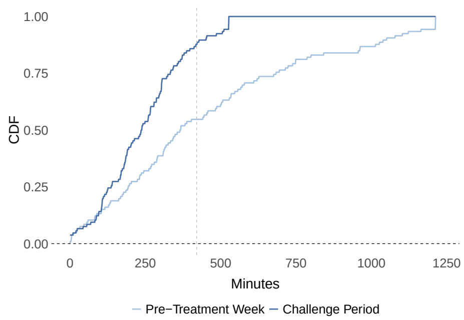
Table A7: Classification of Applications and Websites

Category	**Definition**	**Classified Apps**
TikTok & Instagram	This category includes apps affected by the 1-hour time limit.	TikTok and Instagram.
Other Social Apps	This category includes apps defined as broader social platforms built around user-generated or community-driven content.	Bluesky, Facebook, Pinterest, Reddit, Sidechat, Tumblr, Threads, Twitter, WeChat, Weibo, X, X.com, Yik Yak, YouTube, iFunny, t.me, LinkedIn, and Snapchat.
Productivity & Utility Apps	This category includes apps that support information access, organization, work, study, navigation, or everyday tasks, such as email clients, note-taking tools, browsers, and transport apps.	Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, Duolingo, Brave, Mail, ChatGPT, Microsoft Outlook, Microsoft 365 (Office) and others.
Entertainment & Media Apps	This category includes apps designed primarily for leisure, including streaming services, music and sports platforms, and mobile games.	Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, Candy Crush, Clash of Clans, Among Us, 1010!, BitLife, Block Puzzle, Boom Beach, and others.
Communication Apps	This category includes apps that are defined as apps centered around interpersonal communication or sharing without a central emphasis on content feeds.	BeReal, Discord, FaceTime, Flare, GroupMe, Jagat, KakaoTalk, LINE, Locket, Marco Polo, Meetup, Mensajes, Messages, Messenger, Monkey Run, Nextdoor, Nicegram, OpenPhone, Plato, Signal, Slack, Telegram, Telegram Messenger, WA Business, WhatsApp, WhatsApp Business, WhatsApp Messenger, Widgetable, Wizz, and World of WIT.
Other Apps	This category includes all other apps and websites not included.	Target, Amazon, Walmart, Starbucks, Taco Bell, NYTimes, Fidelity, Expedia, and others.

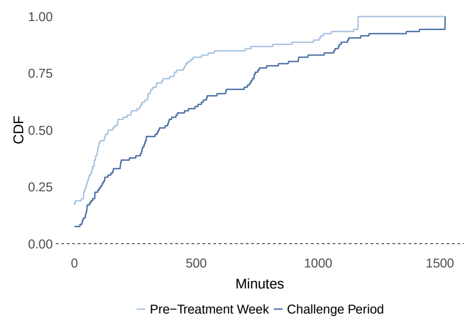
Notes: Table A7 provides a more disaggregated overview of our category definitions and app classifications. Our extracted screenshot data identifies 499 unique apps and websites in the data that were handcoded into one of the six categories "TikTok & Instagram", "Other Social Apps", "Productivity & Utility Apps", "Entertainment & Media Apps", "Communication Apps", and "Other Apps". A full list of apps and websites can be accessed as part of our replication package.

B.4 Distribution of Screen Time Minutes

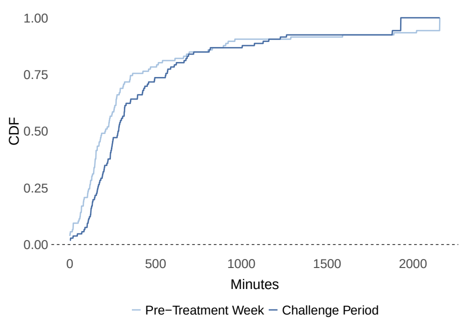
Figure A18: Cumulative Distribution Functions by App Category



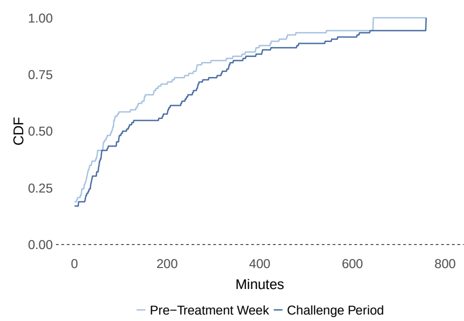
(a) TikTok & Instagram



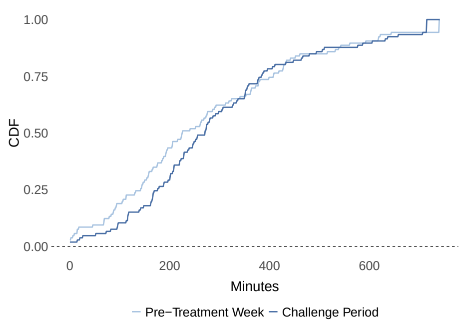
(b) Other Social Apps



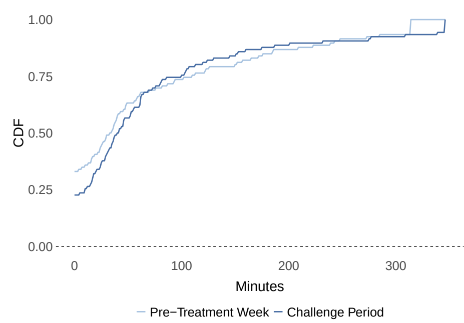
(c) Productivity & Utility Apps



(d) Entertainment & Media Apps



(e) Communication Apps

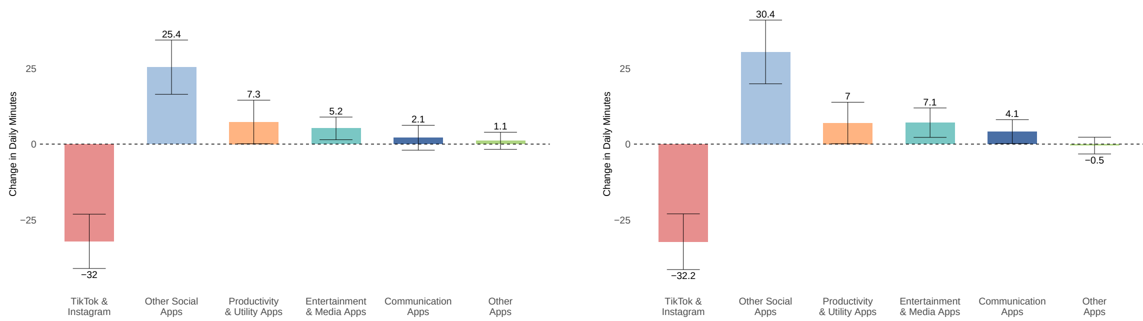


(f) Other Apps

Notes: Figure A18 plots the empirical cumulative distribution functions (CDFs) of weekly minutes spent on each app category among challenge participants. In each panel, the dark blue curve shows usage during the deactivation period and the light blue curve shows the pre-treatment week. Panel (a) adds a vertical dashed line at 1 hour/day to indicate the daily deactivation threshold. All CDFs are computed after imputing zeros for non-users and winsorizing minutes at the 95th percentile.

B.5 Dynamics of Diversion

Figure A19: Dynamics of Diversion: Week 1 versus Week 2 Substitution



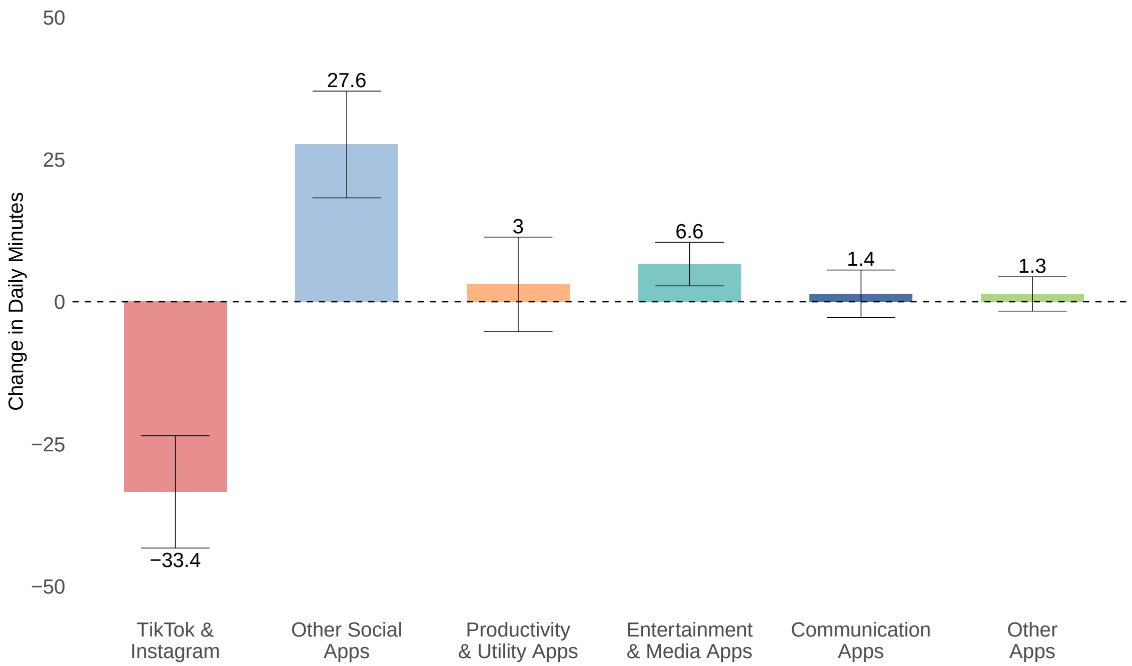
(a) Week 1 substitution patterns

(b) Week 2 substitution patterns

Notes: Panel (a) presents the average change in daily minutes spent on app categories in the first week of the time limit challenge. Panel (b) presents the average change in daily minutes in the second week of the time limit challenge. We categorize apps into six groups: (1) "TikTok & Instagram," which includes the two apps affected by the 1-hour time limit; (2) "Other Social Apps," defined as broader social platforms built around user-generated or community-driven content (e.g., YouTube, Snapchat, LinkedIn, Reddit, Pinterest, Facebook, and X); (3) "Productivity & Utility Apps," defined as applications that support information access, organization, work, study, navigation, or everyday tasks, such as email clients, note-taking tools, browsers, and transport apps (e.g., Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, and Duolingo); (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games (e.g., Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, and Candy Crush); (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds (most notably Messenger, Messages, WhatsApp, Discord, FaceTime, GroupMe, Slack, and BeReal); (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section B.3. Error bars represent 95% confidence intervals.

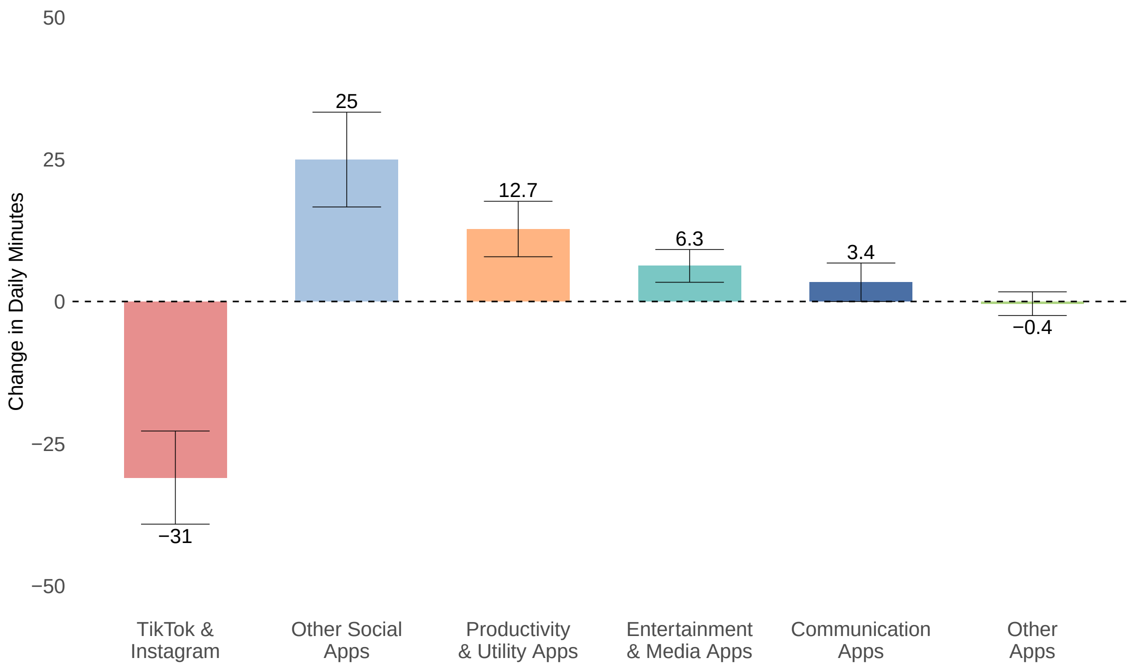
B.6 Robustness

Figure A20: Substitution pattern estimates (Compliers)



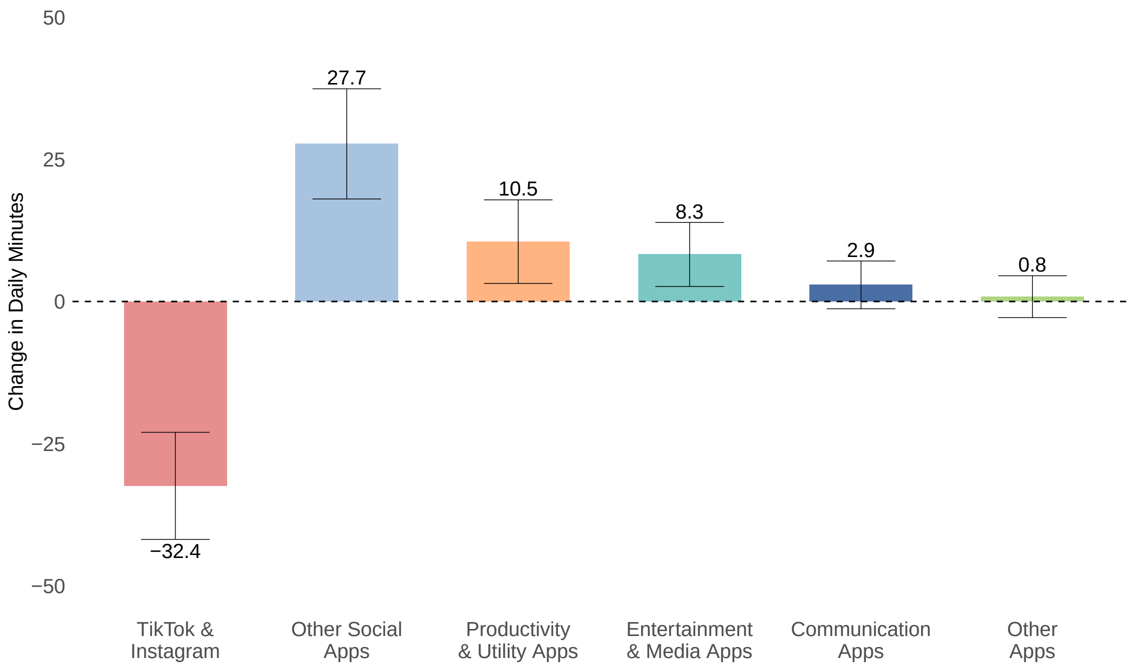
Notes: Figure A20 presents our robustness check of substitution patterns among compliers of the collective time-limit challenge. We categorize apps into six groups: (1) "TikTok & Instagram," which includes the two apps affected by the 1-hour time limit; (2) "Other Social Apps," defined as broader social platforms built around user-generated or community-driven content (e.g., YouTube, Snapchat, LinkedIn, Reddit, Pinterest, Facebook, and X); (3) "Productivity & Utility Apps," defined as applications that support information access, organization, work, study, navigation, or everyday tasks, such as email clients, note-taking tools, browsers, and transport apps (e.g., Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, and Duolingo); (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games (e.g., Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, and Candy Crush); (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds (most notably Messenger, Messages, WhatsApp, Discord, FaceTime, GroupMe, Slack, and BeReal); (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section B.3. Error bars represent 95% confidence intervals.

Figure A21: Substitution pattern estimates (Winsorizing at the 90th percentile)



Notes: Figure A21 presents our estimates for the robustness of substitution patterns among participants in the time-limit challenge. We categorize apps into six groups: (1) "TikTok & Instagram," which includes the two apps affected by the 1-hour time limit; (2) "Other Social Apps," defined as broader social platforms built around user-generated or community-driven content (e.g., YouTube, Snapchat, LinkedIn, Reddit, Pinterest, Facebook, and X); (3) "Productivity & Utility Apps," defined as applications that support information access, organization, work, study, navigation, or everyday tasks, such as email clients, note-taking tools, browsers, and transport apps (e.g., Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, and Duolingo); (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games (e.g., Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, and Candy Crush); (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds (most notably Messenger, Messages, WhatsApp, Discord, FaceTime, GroupMe, Slack, and BeReal); (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section B.3. Error bars represent 95% confidence intervals.

Figure A22: Substitution pattern estimates (Winsorizing at the 99th percentile)



Notes: Figure A22 presents our estimates for the robustness of substitution patterns among participants in the time-limit challenge. We categorize apps into six groups: (1) "TikTok & Instagram," which includes the two apps affected by the 1-hour time limit; (2) "Other Social Apps," defined as broader social platforms built around user-generated or community-driven content (e.g., YouTube, Snapchat, LinkedIn, Reddit, Pinterest, Facebook, and X); (3) "Productivity & Utility Apps," defined as applications that support information access, organization, work, study, navigation, or everyday tasks, such as email clients, note-taking tools, browsers, and transport apps (e.g., Chrome, Gmail, Google Drive, Notion, Outlook, Calendar, Maps, Uber, and Duolingo); (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games (e.g., Netflix, Spotify, Disney+, Hulu, Twitch, ESPN, Pokémon GO, and Candy Crush); (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds (most notably Messenger, Messages, WhatsApp, Discord, FaceTime, GroupMe, Slack, and BeReal); (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section B.3. Error bars represent 95% confidence intervals.