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. We find substantially higher valuations of alternative social apps under a collective TikTok ban than under an individual TikTok deactivation. Mechanism evidence shows that both anticipated content-supply shifts and social coordination partly explain the wedge, with the relative importance of each channel varying across platforms. We then show that a collective time limit challenge, where peers jointly reduce TikTok and 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 hinged critically on defining the "relevant market" in which Meta's platforms compete. The Federal Trade Commission (FTC) argued that the market comprised only "personal social networking services"—platforms like Facebook and Instagram that connect users with friends and family—excluding entertainment-based apps such as YouTube and TikTok. Meta countered that the market should include all platforms competing for user attention. In November 2025, the court ruled in favor of Meta, finding that TikTok and YouTube are reasonable substitutes based on evidence of consumer switching across platforms.[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 provide new evidence on the gap between substitution patterns that account for network effects and those that do not. 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, provide a more accurate picture of market-level substitution patterns in the presence of network effects.

[1] See Federal Trade Commission (2021). For a summary of the court's ruling, see Sullivan & Cromwell (2025). The FTC filed a notice of appeal in January 2026.

Design To study how network effects influence substitution patterns, we conduct a preregistered online experiment with 900 active U.S. TikTok users aged between 18 and 27 in early January 2025. 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 on 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.

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, which are also popular among young adults (Pew Research Center, 2024): YouTube, Instagram, and Snapchat. 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 may operate through both content production and social sharing: a larger user base increases the supply and variety of user-generated videos and raises the value of consumption through discussion with a wider set of peers.

[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, underscoring that they perceived this scenario as quite likely at the time of our experiment.

Main results: TikTok ban Holding network size fixed, we first compare valuations under *individual TikTok deactivation* and *no TikTok ban*. 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 value Instagram more 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.[5]

Comparing the *TikTok ban* with the *no TikTok ban* scenario, net fractions are 48.1, 41.8, and 14.8 percentage points for Instagram, YouTube, and Snapchat, respectively ($p < 0.01$ for all), implying that all three platforms are perceived as substitutes under collective deactivation.

Our central comparison isolates network effects by contrasting the collective *TikTok ban* with *individual TikTok deactivation*. Net fractions are positive and significant for all three platforms: 25.0, 16.0, and 15.5 percentage points for Instagram, YouTube, and Snapchat ($p < 0.01$ in each case). The Snapchat result is especially striking: this platform does not appear to substitute for TikTok under individual deactivation, yet emerges as a clear substitute under collective deactivation. Since Snapchat is primarily a messaging app, this qualitative shift suggests an important role of coordination in shaping measured substitution patterns for network goods.

Mechanisms Consistent with network effects driving the wedge, respondents in the main experiment who anticipate larger increases in friends' usage of the focal app exhibit significantly larger valuation gaps between the *TikTok ban* and the *individual TikTok deactivation* scenarios. To provide more direct evidence on mechanisms, we conduct a pre-registered mechanism survey ($N = 601$). First, usage motives differ sharply across platforms: TikTok is primarily entertainment-driven, making it more similar to YouTube and less similar to Snapchat, which is predominantly used for communication. This is consistent with individual-level substitution favoring platforms serving more similar motives. Second, un-

[5] Under quasilinear utility, our net fraction is a discrete-choice analogue of money-metric substitutability (Samuelson, 1974). Results are robust to using second-choice Wald estimates (Conlon and Mortimer, 2021); see Section 3.6.

der a collective ban, general equilibrium shifts could occur through two channels: *content supply* (creator migration) and *social coordination* (friend or general user migration). We find that the relative importance of these channels varies across platforms: creator migration is most important for YouTube, friend migration is most important for Snapchat, and Instagram lies in between.

Descriptive evidence from collective time limit A limitation of our experimental 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).[6] To address this limitation, we provide evidence from a collective time limit challenge, which restricted Instagram and TikTok use to one hour per day over a two-week period, with compliance verified via uploaded app-usage screenshots. More than 800 undergraduate students from the University of Chicago, 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 compared to the pre-treatment period 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%. 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, this evidence should be interpreted with caution given the lack of a randomized control group.

Related literature 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; Aridor et al., 2024). The closest related evidence is provided by Aridor (2025), who studies individual-level deactivations of YouTube and Instagram and documents substitution both toward other social media platforms and toward non-digital activities. Using an exogenous six-hour Meta outage, Rehse and Valet (2025) find quantitatively similar substitution patterns among U.S. users. We

[6] 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. Finally, our estimates ignore other equilibrium responses besides direct network effects, such as changes in advertising prices (Donati and Fong, 2025).

differ from this literature in our focus on explicitly estimating how network effects affect substitution patterns.

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). Building on recent literature (Bursztyn et al., 2025a; Hagiu and Wright, 2025) that demonstrates that considering the collective nature of the outside option is crucial for welfare measurement, we show that accounting for the collective nature is also essential for 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). 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.

2 Conceptual Framework

We briefly formalize the well-known observation that network effects create a wedge between fixed-network and equilibrium substitution patterns. We use a two-product example from the canonical model of network effects (Katz and Shapiro, 1985).

A continuum of individuals choose one of two products. Individual i's utility from product j is $u(q_j) + \gamma_j^i - p_j$, where u is smooth and increasing (capturing positive network effects), q_j is network size, p_j is the price (monetary or advertising load; Anderson and Coate, 2005), and γ_j^i is a heterogeneous membership benefit. The net taste $\gamma^i := \gamma_1^i - \gamma_2^i$ has smooth density f with full support. Let $Q_j(p, q)$ denote aggregate demand for product j as a function of the price vector p and the vector of network sizes q. Equilibrium network sizes solve the fixed point $q = Q(p, q)$, imposing rational expectations.[7]

We compare the full cross-price derivative $\frac{\partial q_2}{\partial p_1}$, which accounts for network adjust-

[7] We assume $u'(q_j) < (2\|f\|_\infty)^{-1}$—the marginal network benefit is small relative to taste dispersion—which guarantees a unique equilibrium. Equilibrium solves $q_2^* = F(u(q_2^*) - u(1 - q_2^*) + p_1 - p_2) := \phi(q_2^*)$. The bound implies $|\phi'| < 1$, so uniqueness follows from the Banach Fixed Point Theorem.

ment, with the *fixed-network* derivative $\frac{\partial Q_2}{\partial p_1}$, which holds q constant.[8] Their difference is $\frac{f^2[u'(q_2^*)+u'(1-q_2^*)]}{1-f[u'(q_2^*)+u'(1-q_2^*)]} > 0$: fixed-network derivatives *underestimate* substitution. Intuitively, when the price of product 1 rises, some users switch to product 2—the direct effect. But this initial shift enlarges product 2's network and shrinks product 1's, making product 2 relatively more attractive. These feedback effects amplify the adjustment beyond what fixed-network measures capture.

This example illustrates the feedback mechanism, but it assumes single-homing and that there is no outside option. Consider a more general aggregate demand with non-negative own-network effects $\left(\frac{\partial Q_j}{\partial q_j} \geq 0\right)$ and cross-product network effects $\left(\frac{\partial Q_j}{\partial q_k}\right)$ for $k \neq j$, which arise naturally when, for example, an increase in one product's user base draws users away from competitors.[9] In a locally stable equilibrium, the derivative is:[10]

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

The first numerator term captures cross-product network effects: when p_1 rises and demand for product 1 falls, demand for product 2 shifts according to $\frac{\partial Q_2}{\partial q_1}$. When this term is zero or negative, fixed-network derivatives underestimate substitution, as above. When cross-product effects are positive and sufficiently large,[11] the bias reverses: the fall in product 1's network decreases demand for product 2, and fixed-network derivatives overestimate substitution, potentially even yielding *opposite* signs.

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.

[8] We focus on small price changes for tractability; our empirical estimates use deactivations or bans. These are informative for antitrust but differ from small-price-change tests (Conlon and Mortimer, 2021).

[9] Even when utility depends only on own network size, positive own-network effects generate negative cross-product effects in equilibrium.

[10] We assume $\frac{\partial Q_j}{\partial q_j} < 1$, that $\frac{\partial Q_j}{\partial q_k}$ and $\frac{\partial Q_k}{\partial q_j}$ share the same sign, and that the denominator is positive.

[11] This requires $\frac{\partial Q_2}{\partial q_1}\left(\left|\frac{\partial Q_1}{\partial p_1}\right| - \frac{\partial Q_1}{\partial q_2}\frac{\partial Q_2}{\partial p_1}\right) > \frac{\partial Q_2}{\partial p_1}\left(1 - \frac{\partial Q_1}{\partial q_1}\right)\frac{\partial Q_2}{\partial q_2}$.

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.[12]

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 TikTok to negotiate with potential American buyers.[13]

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.

3.2 Sample

We recruited 900 respondents from Prolific between January 6 and January 9, 2025, immediately prior to the Supreme Court hearing on January 10. The sample consists of U.S. residents aged between 18 and 27 who own iPhones and are active TikTok users. We restrict attention to iPhone users because compliance with app deactivation is verified via screenshots of iOS Screen Time usage, and to young adults because TikTok use is highly concentrated in this group: as of 2022, 54% of U.S. adults aged 18–29 use TikTok, compared to 25% in older age groups (Pew Research Center, 2022). Among respondents who entered the survey, 81% were active TikTok users, of whom 82% consented to participate in a four-week mobile app deactivation study requiring screenshot-based compliance verification. Although this consent requirement induces some selection, the degree of selection is modest relative to prior deactivation studies. To limit differential attrition, we did not

[12]Since the ban applies only to U.S. users, the strength of the coordination channel depends on the share of each user's network located in the United States. In our mechanism survey, 92% of respondents' TikTok friends are U.S.-based, suggesting that a domestic ban would displace the large majority of respondents' TikTok networks. The magnitude of the network effects we document would likely be smaller for users with substantial international networks.

[13]In late 2025, TikTok reached a deal to transfer majority control of its U.S. operations to U.S. investors.

pre-specify which apps might be deactivated before consent. As expected, attrition rates at the consent stage are 18.0%, 17.5%, and 18.6% for Instagram, YouTube, and Snapchat, respectively, and are not statistically different.[14]

Summary statistics Our sample includes 67% female participants, similar to the proportion of U.S. TikTok users aged 18-29 who are female (60%; 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. Multi-homing is widespread in our sample: 94%, 96%, and 74% percent of respondents use TikTok alongside Instagram, YouTube, and Snapchat, respectively. These rates closely match those among active TikTok users in the American Trends Panel (88%, 96%, and 79%; Pew Research Center, 2024). Table A1 in the Online Appendix reports summary statistics by treatment arm, confirming successful randomization: no observed characteristic differs significantly across arms (all p-values > 0.10).

Pre-registration The study was pre-registered on AsPredicted (#206616). The registry documents the study design, hypotheses, outcomes, sample size, and exclusion criteria.

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. Figure A1 presents an overview of the experimental design. Details on the experimental instructions are available at this link.

Background information on the ban We begin by informing all respondents about the potential U.S. TikTok ban: citing national security concerns over ByteDance ownership, Congress enacted a "sell-or-ban" law in April 2024 with a January 19, 2025 deadline. With the Supreme Court appeal scheduled for January 10th, we emphasize that a nationwide ban remained possible.

[14]Following consent, participants completed two comprehension checks covering the compliance procedure and the deactivation duration, which were passed by 94% and 84% of respondents; consistent with the pre-analysis plan, we exclude participants who failed either check.

WTA elicitation instructions We elicit respondents' valuations using a BDM procedure. Participants state the minimum compensation required to deactivate their focal app for four weeks under each scenario. We impose an upper limit of \$500 and a lower limit of \$0.[15] A series of best practices are implemented in our elicitation process. First, we include an example 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.[16] 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.[17] We incentivize our experiment by informing participants that 1 in 10 respondents will be randomly selected to take 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.

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 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.

[15]Top- and bottom-coding are minimal: only 7.78% of respondents enter \$500 and only 3.22% enter \$0.

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

[17]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.

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.[18] We then ask how much additional compensation they would require to 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?*".[19]

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 Our design elicits *incentivized* rather than hypothetical valuations in an environment where both individual and collective deactivation are credible outcomes, given the substantial legal uncertainty. This uncertainty is salient to participants: on average, respondents assign a 46% probability to a TikTok ban, closely matching contem-

[18]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.

[19]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.

poraneous Polymarket beliefs (42%; Appendix Figure A2).[20] A second key feature is the within-subject structure, which allows us to compare valuations across three counterfactuals—no deactivation, individual deactivation, and collective deactivation—within the same respondent. This increases statistical power and is particularly valuable given sample-size constraints for our target demographic while eliciting valuations for multiple platforms.

Focal apps We consider three platforms—Instagram, YouTube, and Snapchat—that plausibly substitute for TikTok but differ in how network effects arise. Instagram shares functionality with TikTok, as it combines short-form video and creator-driven trends with substantial social interaction within users' personal networks through direct messaging, story responses, and interactive features. YouTube also closely overlaps with TikTok, primarily on the content-supply side—user-generated video, algorithmic recommendation, and creator monetization, extended to short-form content via YouTube Shorts—as social engagement is organized around interest-based communities rather than personal friendships. Snapchat differs most sharply: its core functionality centers on ephemeral, private communication within tight social circles, with public content discovery (via Spotlight) playing a secondary role.

3.4 Results

As pre-registered, Figure 1 displays 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. 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 TikTok and that platform.

Individual deactivation versus no ban The two light blue bars in Figure 1 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. We observe substantial positive net effects for Instagram (13.9 p.p., $p < 0.01$) and YouTube

[20]The incentive-compatible elicitation also mitigates concerns about social desirability bias (Bursztyn et al., 2025b) and experimenter demand effects (de Quidt et al., 2018).

(24.4 p.p., $p < 0.01$), indicating that TikTok tends to be a substitute for these platforms absent network considerations. Conversely, Snapchat shows no significant net effect, suggesting that, on average, it is equally perceived as a substitute and complement among participants.[21]

Ban versus no ban The green bars present comparisons between the *TikTok ban* scenario and the baseline *no TikTok ban* scenario. 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.

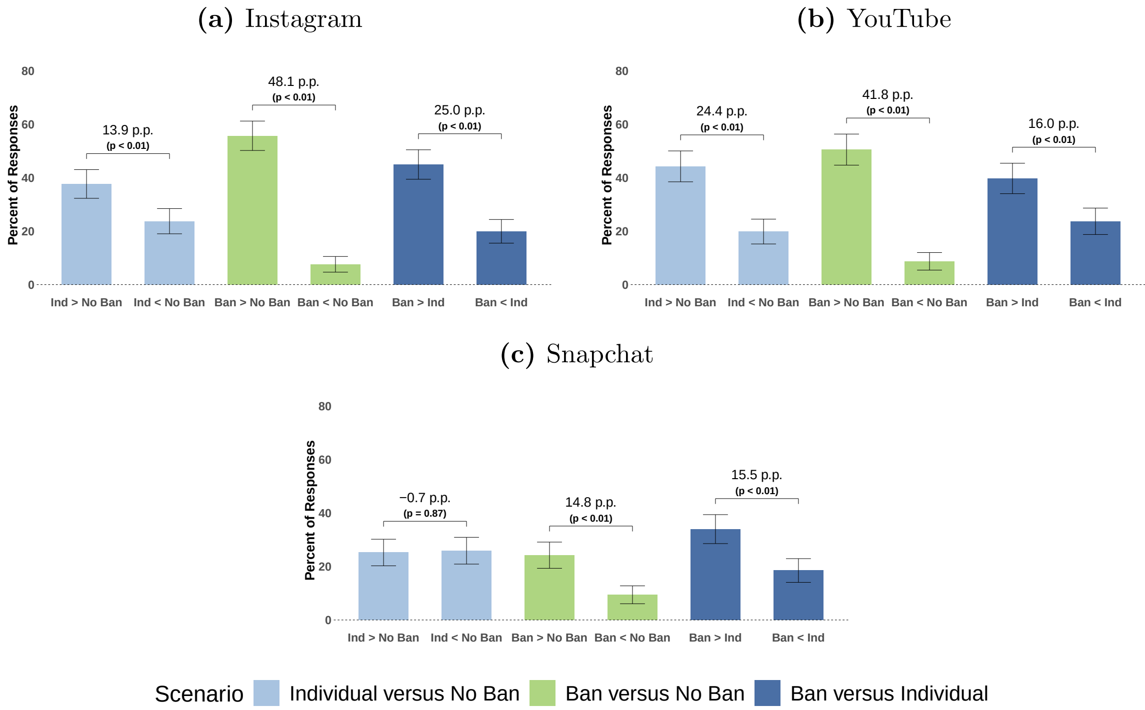
Ban versus individual deactivation Finally, we turn to our core interest—network effects. The dark blue bars show significant positive differences for Instagram (25.0 p.p.) and YouTube (16.0 p.p.), indicating that valuations rise substantially when TikTok deactivation occurs collectively rather than 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.

Interpreting effect sizes To interpret effect sizes, we compare the difference in *average* WTA between collective and individual TikTok deactivation to the *average* WTA 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. Appendix B.4 provides additional details on average valuations across platforms.

[21] 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.

Figure 1: Fraction with Higher or Lower Valuation By Scenario



Notes: By platform, Figure 1 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.

Robustness We address several potential concerns. First, estimates remain robust when excluding respondents who regretted any valuation choices, alleviating concerns about hypothetical bias. Second, results are consistent regardless of whether the ban or individual deactivation scenario was presented first, ruling out order effects. Finally, of 55 participants selected for deactivation, 60% agreed to participate with a 76% compliance rate, providing further evidence that our design was perceived as credible. Compliance rates were similar across platforms (70–80%), and results hold after adjusting for potential compliance differences between collective and individual treatments. We provide further details on these robustness checks in Appendix C.

Self-reported substitution intentions While our valuation results identify substitution in willingness to pay, they do not directly map into usage, the primary measure of quantity for ad-supported platforms. We therefore study respondents' self-reported time-use intentions. Figure 2 plots, for each activity, the share expecting higher (or lower) time use under a collective TikTok deactivation relative to an individual deactivation; the net substitution is the difference in the "more time" shares across the two scenarios.[22] Respondents anticipate reallocating time toward other social apps under a ban: the net increase is 4.44 p.p. ($p < 0.05$) for Instagram, 6.44 p.p. ($p < 0.01$) for YouTube, and 3.22 p.p. ($p < 0.05$) for Snapchat.[23] By contrast, respondents predict more diversion to non-social activities under individual deactivation, with net differences of -4.44 p.p. ($p < 0.05$) for phone games and -3.67 p.p. ($p = 0.056$) for meditation. Planned laptop time is also lower under collective deactivation, but the difference is not statistically significant ($p = 0.263$).[24]

Taken together, these patterns imply that accounting for network effects makes other social platforms closer substitutes for TikTok, whereas non-social digital activities are comparatively weaker substitutes. Individual-level interventions therefore understate substitution toward competing social apps.

3.5 Mechanisms

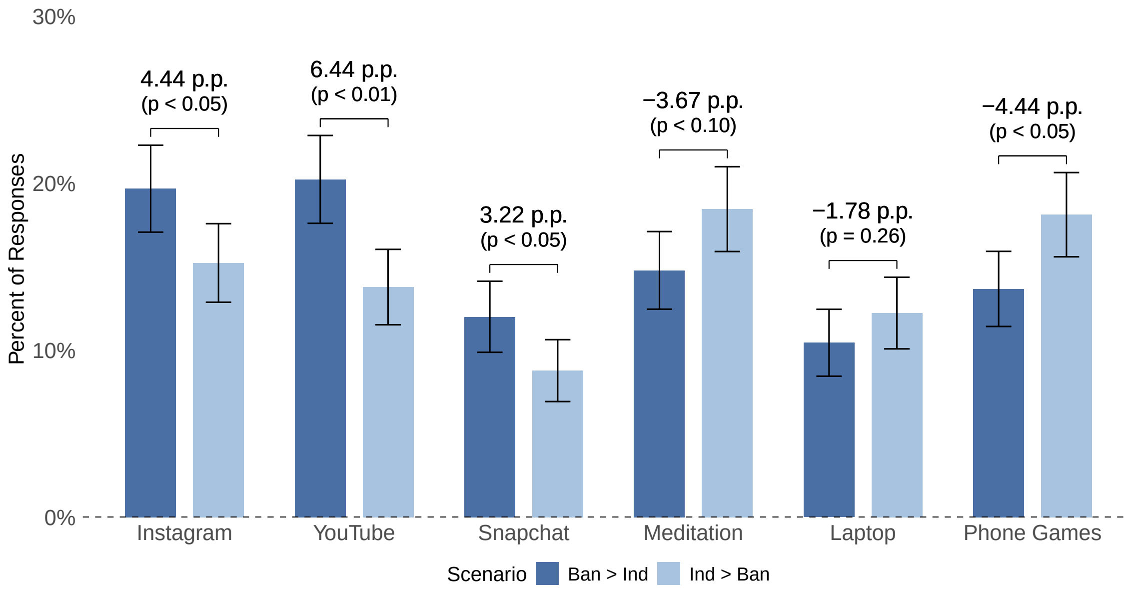
We next present two pieces of mechanism evidence. First, we show that respondents in our main experiment who anticipate larger changes in their focal app's network size exhibit larger valuation gaps between the collective and individual treatments, consistent with network effects driving substitution (Section 3.5.1). Second, we conduct a dedicated pre-registered mechanism survey ($N = 601$) that provides direct evidence on mechanisms underlying substitution patterns: how respondents use each platform at baseline predicts individual substitution patterns, while anticipated content-supply shifts (creator migration) and social coordination (friend migration) explain why collective interventions produce additional substitution.

[22] Net substitution is defined as the percentage intending to spend more time on an activity under collective TikTok deactivation minus the corresponding percentage under individual TikTok deactivation.

[23] Time-use responses need not coincide with valuation responses; see Beknazar-Yuzbashev et al. (2024).

[24] Appendix Section B.7 reports anticipated time substitution from a question asking respondents to evaluate time spent under the individual and collective scenario relative to the no-ban baseline, rather than directly comparing collective versus individual deactivation. While qualitatively similar, Figure A5 shows participants anticipate increasing time on other social platforms under both scenarios, and Figure A6 confirms that those predicting above-median increases in focal app usage exhibit significantly higher WTA ($p < 0.01$ for both conditions).

Figure 2: Fraction with Higher or Lower Predicted Time Spent Under Collective vs. Individual Deactivation



Notes: Figure 2 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.

3.5.1 Anticipated network effects

We start by showing that variation in anticipated network shifts within our main experiment maps onto variation in the valuation wedge, providing corroborating evidence from the incentivized data.

We collect data on participants' expectations 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.[25] As shown in Figure A9, 93%, 86%, and 66% of respondents expect their

[25] Due to a coding error, we only collected this data for YouTube for 57% of participants.

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 A8, we compare average valuation differences across scenarios 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.[26]

3.5.2 Tailored mechanism survey

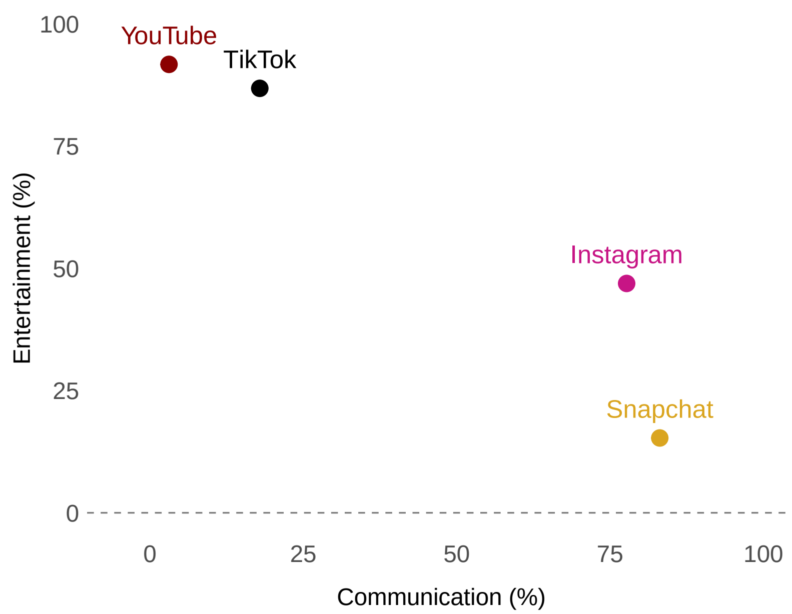
Design and sample To provide direct evidence on mechanisms, we conducted a pre-registered survey on Prolific in March 2026. Participants were randomly assigned to one of the three focal apps: Instagram, YouTube, or Snapchat.[27] In total, we recruit 601 U.S. TikTok users aged 18–27, broadly matching the demographics of our main sample. The share of female respondents is 50% (vs. 67% in the main sample), the average age is 23.1 years (vs. 23.5), and the share reporting TikTok use at least several times a week is 80% (vs. 84%).

Network effects on TikTok We first establish that TikTok functions as a social platform despite its content-centric design. Among our respondents, 99% primarily consume rather than produce content, and 92% shared with, discussed with, or received TikToks from friends in the two weeks prior to the survey. This demonstrates that TikTok's social dimension operates largely through the sharing and discussion of content rather than through communication directly on the platform, motivating why collective interventions may create a wedge compared to individual interventions.

[26]This pattern also holds when looking at the individual platforms (see Appendix Figure A8). Relatedly, we also look at heterogeneity based on time use in Appendix Figure A7 and find that network effects are important for users both above and below median baseline time usage.

[27]The survey was pre-registered on AsPredicted (#279303).

Figure 3: Platform Usage Motives: Communication vs. Entertainment



Notes: Figure 3 plots each platform in the space of communication and entertainment usage motives. Percentages represent the share of respondents who cite each motive in open-ended responses about why they use the platform. For Instagram, YouTube, and Snapchat, responses are classified using GPT 5.2 from our mechanism survey sample. The figure highlights that TikTok and YouTube occupy similar positions as entertainment-oriented platforms, while Snapchat is primarily communication-oriented and Instagram combines both motives.

Mechanisms behind individual-level substitution Having established that TikTok blends content consumption with social sharing, we now ask which focal platforms serve similar functions—and thus emerge as individual substitutes—versus those whose value depends on motives only activated under collective intervention. Open-ended responses reveal starkly different usage motives across platforms. TikTok is primarily a content consumption platform: 86.8% cite entertainment, while only 17.9% mention communication. YouTube exhibits a similar profile (91.7% entertainment, virtually no communication), whereas Snapchat is nearly the inverse (83.1% communication, 15.3% entertainment). Instagram sits in between, with many citing both communication (77.7%) and entertainment (46.9%). Figure 3 summarizes these patterns by plotting each platform in the two-dimensional space of communication and entertainment motives.[28]

These patterns help explain the individual deactivation results. YouTube, the platform

[28] We confirm these patterns with a structured question where respondents rank the importance of friends, content creators, and general users for their current level of focal app usage.

whose usage motives most closely resemble TikTok's, shows the strongest individual substitution effect. Snapchat, whose motives are most dissimilar from TikTok's, exhibits no significant net substitution under individual deactivation. This suggests that when only one user loses access to TikTok, they gravitate toward platforms that fulfill similar content-consumption needs. Baseline network overlap reinforces this pattern. In the survey, we find that 70% and 54% of TikTok creators that respondents follow are also present on Instagram and YouTube, respectively, compared to only 33% on Snapchat. Similarly, 76%, 72%, and 44% of respondents' friends are on Instagram, YouTube, and Snapchat, respectively.

Mechanisms behind the collective–individual wedge The preceding evidence helps explain why platforms differ as *individual* substitutes for TikTok. We next ask why *collective* deactivation produces additional substitution beyond what individual deactivation generates. Our main experiment already shows that anticipated user migration explains part of this wedge; the mechanism survey allows us to go further, examining the role of shifts in content supply and social coordination.

To assess the relative importance of different channels, we first ask respondents to compare their expected behavior under the two scenarios directly: whether they would spend more time on their focal app when there is a TikTok ban for everyone or when only their own TikTok account is deactivated. Consistent with our main experimental results, the majority of respondents report that they would spend more time on the focal app under a collective ban.[29] We then elicit open-ended explanations for this choice, which provide an unprompted window into respondents' reasoning. Two broad themes emerge. The first is *content supply*: respondents anticipate that TikTok creators would migrate to alternative platforms, improving the quality and diversity of available content. This theme is especially prominent among respondents assigned to YouTube and Instagram.[30] The second is *social coordination*: respondents expect friends displaced from TikTok to increase their activity on the focal platform, making it a more attractive space for communication and shared experiences. This theme is particularly salient for Snapchat, where respondents describe the platform as becoming a natural gathering point for social interactions that previously

[29] Specifically, 70.3%, 76.6%, and 84.3% of respondents assigned to Instagram, YouTube, and Snapchat, respectively, indicate they would spend more time under a collective ban.

[30] Respondents also expect creators to migrate to Instagram (91.1%) and YouTube (93.7%), with Snapchat also seeing expected creator inflows (65.2%). These anticipated shifts are absent under individual deactivation, where a single user's departure from TikTok has no bearing on the creator ecosystem of alternative platforms.

revolved around TikTok content.[31]

We then use structured ranking questions to measure the importance of these channels more directly. When asked to rank the importance of content creators switching, friends switching, and general users switching to the focal platform following a TikTok ban, the results sharpen the patterns suggested by the open-ended data. For YouTube, 60.5% rank content creator migration as the most important factor, consistent with its content-consumption orientation. For Instagram, respondents rank content creator migration (31.8%) and friend migration (39.6%), reflecting its dual role as both a content and social platform. For Snapchat, 58.8% rank friend migration as the most important factor, confirming that the collective–individual wedge for this platform is driven primarily by social coordination rather than content supply. Snapchat emerges as a collective substitute precisely because the type of network effect most important for its value—social coordination—is activated only under collective intervention. Together, the evidence indicates that the collective–individual valuation wedge operates through both content-supply and social-coordination shifts, whose relative importance varies systematically with each focal app's core function.

User utility versus non-user externalities A natural question is whether the additional substitution under collective intervention reflects genuine utility gains from a larger network or rising costs of *not* joining, such as fear of missing out on social interactions that have migrated elsewhere (Bursztyn et al., 2025a). We find suggestive evidence for the latter: substantial shares of respondents across all three focal platforms anticipate increased FOMO following a ban, implying that collective substitution partially reflects exclusion costs rather than purely intrinsic utility gains.

3.6 Diversion ratios

Our main 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.[32] To address this concern, we provide evidence of a related parameter, the second-choice Wald estimator (Conlon and Mortimer, 2021),

[31] Conversely, respondents who report spending more time under individual deactivation emphasize personal motives that do not depend on changes in the platform environment, such as needing to fill their idle time.

[32] 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).

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 A adapts this logic to a setting where network sizes adjust.

Appendix Figure A16 (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.[33] 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 A16 (b) confirms that this difference is positive 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.[34]

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. To address this limitation, we examine time-use data from a *collective* social media time-limit intervention.

[33]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.

[34]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.

4.1 The collective time-limit challenge

Context In fall 2024, NOMO (No Missing Out), a technology start-up[35], piloted a two-week collective time-limit challenge at the University of Chicago as a prototype for the app's launch. The intervention imposed a combined daily 60-minute cap on Instagram and TikTok use and targeted undergraduates enrolling via institutional email.

The challenge was collective by design. Recruitment relied on word-of-mouth among friends and targeted classroom visits, making it likely that each participant's peers were also subject to the limit. The challenge was administered through a platform built around community-driven participation, so that enrollees could see that their friends had also joined. Compliance was incentivized through rewards: theater tickets, coffee, charitable donations, and concert tickets. A total of 808 undergraduates—approximately 11% of the undergraduate population—enrolled, representing a concentrated and sizeable share of campus social networks. The campus setting is well suited to studying collective interventions, given that students' social media consumption is tightly linked to their on-campus peer networks. The results reported here, however, are only descriptive and should be interpreted cautiously because of the lack of a randomized control group.

Summary statistics NOMO collected 246 submissions from initially enrolled participants, of whom 65% used iPhones and submitted screenshot data. After excluding invalid screenshots, 161 respondents remain. Restricting attention to participants who used TikTok or Instagram during the pre-treatment week (70.7%), the final sample comprises $N = 106$ users. The mean age is 19 years; 54% are female. The sample includes 32% first-year, 36% second-year, 23% third-year, and 9% fourth-year students. During the pre-treatment week, participants averaged 258 minutes (4.30 hours) of daily app use. TikTok and Instagram—the platforms subject to the collective time-limit intervention—accounted for 68 minutes, or 26% of total usage. Average daily use amounted to 43 minutes on Instagram and 25 minutes on TikTok.

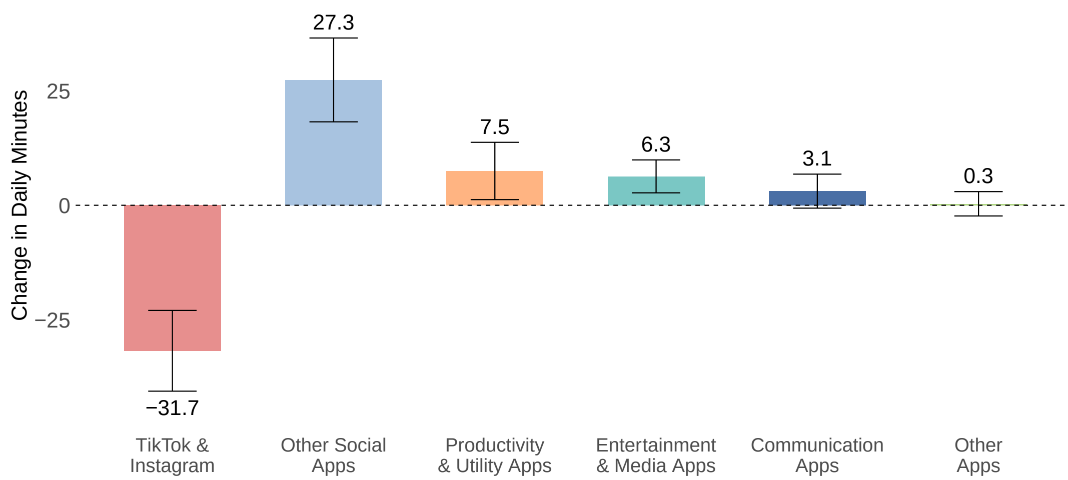
4.2 Results

Main estimates Figure 4 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 ($p < 0.01$).

[35] https://www.yesnomo.com

Notably, participants largely substituted this reduction with increased daily usage of other social apps, broadly defined (e.g., YouTube, Snapchat, LinkedIn or 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 or Google Drive) as well as for entertainment and media apps (e.g., Netflix, Spotify, or Disney+), 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, $p = 0.808$). Overall daily screen time during the study period changes by an average of 13 minutes or 5.3% compared to the baseline week.

Figure 4: 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; (3) "Productivity & Utility Apps," defined as applications supporting work, study, or everyday tasks—such as email clients, browsers, note-taking tools, and transport apps; (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games; (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds; (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section D.3. Error bars represent 95% confidence intervals.

Interpreting magnitudes 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.[36] With time use data, the Wald estimate is given by how much of the reduced Instagram and TikTok time 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 D.3.

Dynamics 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 A18, 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.

Benchmarking magnitudes We find that 86% of the reduction in TikTok and Instagram use induced by the collective time-limit challenge is reallocated to other social apps. The magnitude is larger than substitution observed in a short-run shock: Rehse and Valet (2025) document an 18.4% increase in non-Meta social media use during a six-hour Meta outage, consistent with diversion dynamics that unfold gradually due to coordination frictions. We also compare our estimate descriptively against individual-level deactivation studies, which hold network effects fixed by design. Because our setting differs from these studies along multiple dimensions—joint TikTok/Instagram reduction rather than single-platform deactivation, a self-selected campus sample, and the absence of a randomized control group—the comparison is suggestive rather than a controlled test of the network-effect channel. 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%

[36] 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 D.6), we re-estimate restricting to compliers only, with very similar results.

increase (approximately 15 minutes).[37]

Robustness and limitations In Appendix D.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. Appendix Figure A17 compares the cumulative distribution functions of time spent on each category from pre-treatment to post-treatment weeks. 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.[38] Future work should analyze the effects of randomly assigned collective versus individual interventions. Second, we examine joint TikTok/Instagram reduction, not single-platform or full deactivation effects. Network-driven substitution is likely underestimated given the study's short duration (2 weeks) and limited reach ($\approx$ 11% of undergrads). Finally, results from self-selected UChicago students may not generalize 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 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. Mechanism evidence indicates that this wedge is driven by anticipated shifts in both content supply and social coordination, with the relative importance of each channel varying by platform function: creator migration matters most for content-oriented platforms like YouTube, while friend migration is the primary driver for communication-oriented platforms like Snapchat. We additionally analyze actual time-use data from a collective social media time-limit challenge, where we find suggestively larger substitution to other social apps than has been documented in prior individual deactivation studies, though the comparison is descriptive given differences in design and sample.

[37] Allcott et al. (2025b) study young to middle-aged U.S. adults with substantially lower baseline Instagram use than our U.S. undergraduate sample.

[38] Aridor et al. (2025) show that smartphone election-content consumption was limited and stable during the 2024 U.S. Presidential election, making election-related events an unlikely source of time trends.

Our results suggest that the failure to account for network effects can 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 relative to non-social activities, yet broader within the set of social media apps. Our evidence documents the existence and magnitude of this bias but does not provide a ready-made methodology for incorporating network effects into standard market definition procedures such as SSNIP tests. Developing such operational tools remains an important direction for future work. Beyond social media, our findings suggest that similar biases may arise in other markets with network effects, though the magnitude will depend on the specific network structure.

Our results also speak directly to *FTC v. Meta*, where the court rejected a narrow market definition in favor of a broader social media market based on individual-level substitution evidence. Our findings support this conclusion but suggest such evidence understates cross-platform substitutability. More generally, network effects can flip qualitative conclusions: Snapchat appears to be neither a substitute nor complement for TikTok at fixed network size, yet emerges as a clear substitute under collective deactivation. Since antitrust analysis often requires binary classifications—whether a product is in or out of the relevant market—such qualitative shifts have direct practical consequences.

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Online Appendix: Not for publication

Our supplementary material is structured as follows. Appendix A formalizes how our experimental estimates relate to diversion ratios under individual and collective product removal. Appendix B includes additional tables and figures about the TikTok collective versus individual deactivation experiment. Appendix C includes robustness exercises and Wald estimates for the TikTok collective versus individual deactivation experiment. Appendix D includes additional tables and figures about the collective time limit challenge.

A Identifying Diversion Ratios with Network Effects

This appendix clarifies the empirical objects identified by our two interventions, adapting the second-choice logic of Conlon and Mortimer (2021) to a setting where network sizes adjust. Individual removal identifies substitution holding network sizes fixed; collective removal identifies substitution after equilibrium network adjustment. The difference between these two objects quantifies what standard individual-level diversion estimates miss when applied to collective policy interventions such as bans.

Setup. A continuum of consumers indexed by $i \in [0,1]$ choose among $J+1$ products indexed by $k \in \{0, 1, \ldots, J\}$, where 0 is an outside option. Consumer i's utility from product k is $u_{ik}(p_k, q_k)$, where p_k is the price of product k and q_k is the network size. Let p denote the vector of prices and q denote the vector of network sizes. Each consumer chooses a single product (and we discuss multihoming below):

$$d_{ij}(p, q) = \mathbb{1}_{\{j = \arg\max_k \ u_{ik}(p_k, q_k)\}}.$$

Let $\bar{q}$ denote the baseline network configuration and p the baseline price vector. We focus on removal of product j and define the individual diversion ratio, which indicates, among baseline j-users, which alternative k consumer i chooses when j is unavailable:

$$D_{jk,i}(p, q) = \mathbb{1}_{\left\{k = \arg\max_{\ell \neq j} \ u_{i\ell}(p_\ell, q_\ell)\right\}}.$$

By construction, $D_{jk,i}(p, q) \in \{0, 1\}$ and $\sum_{k \neq j} D_{jk,i}(p, q) = 1$ for all i with $d_{ij}(p, \bar{q}) = 1$.

We now consider two removal scenarios, individual and collective. In both cases, product j is eliminated from the choice set so that all baseline j-users must switch.

Assumption 1 (Individual removal)**.** *Under individual removal of product j:*

(a) Fixed networks. With a continuum of consumers, individual removal of a given user does not displace other users; network sizes remain at $\bar{q}$.

(b) Mutually exclusive and exhaustive discrete choice (single-homing). $d_{ik}(p, \bar{q}) \in \{0, 1\}$ and $\sum_k d_{ik}(p, \bar{q}) = 1$.

Assumption 1 specializes Proposition 1 of Conlon and Mortimer (2021) to the second-choice case at a fixed network configuration $\bar{q}$.[39] Under Assumption 1, we define the

[39] Proposition 1 of Conlon and Mortimer (2021) requires five conditions. Condition (a), single-homing, is our Assumption 1 (b). Condition (b), exclusion (that u_{ik} does not depend on p_j for $k \neq j$) is embedded in our utility specification $u_{ik}(p_k, q_k)$. Conditions (c) and (d), i.e., monotonicity and first stage, are satisfied by construction under product removal: all baseline j-users are compliers. Condition (e), random assignment of the price instrument, is unnecessary: with full compliance, the Wald estimator reduces to the sample average of second choices among baseline j-users, so no exogeneity condition is required. The substantive content of Assumption 1(a) is that individual removal does not change q, which holds with a continuum of consumers.

fixed-network Wald estimator among baseline j-users as the ratio of the gain in k's share to the loss in j's share under individual removal:

$$\text{Wald}_{jk}^{FN} \equiv \frac{E\big[d_{ik}^{\setminus j}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big] - E\big[d_{ik}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big]}{-\Big(E\big[d_{ij}^{\setminus j}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big] - E\big[d_{ij}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big]\Big)} \tag{1}$$

where $d_{ik}^{\setminus j}(p,\bar{q})$ denotes consumer i's post-removal choice evaluated at network configuration $\bar{q}$.

Assumption 2 (Collective removal)**.** *Under collective removal of product j:*

(a) Equilibrium consistency and rational expectations. A unique post-removal equilibrium q^* exists, with $q_j^* = 0$, and consumers' elicited choices under collective removal reflect optimization at q^*.

(b) Mutually exclusive and exhaustive discrete choice (single-homing). Same as 1(b), evaluated at the post-removal network size q^*.

Collective removal displaces a set of users, so network sizes shift from $\bar{q}$ to $q^* \neq \bar{q}$. This is precisely the channel through which network effects operate: when all j-users are displaced, they migrate across alternatives, altering network sizes and hence the attractiveness of those alternatives. In contrast with Assumption 1(a), network sizes are not fixed; they are determined in equilibrium. Assumption 2(a) embeds two requirements: that a well-defined post-removal equilibrium q^* exists, and that consumers correctly anticipate it.

Under Assumption 2, we define the post-intervention Wald estimator among baseline j-users as the ratio of the gain in k's share to the loss in j's share under collective removal:

$$\text{Wald}_{jk}^{PI} \equiv \frac{E\big[d_{ik}^{\setminus j}(p,q^*) \mid d_{ij}(p,\bar{q})=1\big] - E\big[d_{ik}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big]}{-\Big(E\big[d_{ij}^{\setminus j}(p,q^*) \mid d_{ij}(p,\bar{q})=1\big] - E\big[d_{ij}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big]\Big)} \tag{2}$$

Note that both Wald_{jk}^{FN} and Wald_{jk}^{PI} condition on the same population: baseline j-users under $(p,\bar{q})$.

Proposition 1. *Under Assumptions 1 and 2:*

(i) $\text{Wald}_{jk}^{FN} = E\big[D_{jk,i}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big]$.

(ii) $\text{Wald}_{jk}^{PI} = E\big[D_{jk,i}(p,q^*) \mid d_{ij}(p,\bar{q})=1\big]$.

(iii) The network adjustment

$$\Delta_{jk} \equiv \text{Wald}_{jk}^{PI} - \text{Wald}_{jk}^{FN} = E\big[D_{jk,i}(p,q^*) - D_{jk,i}(p,\bar{q}) \mid d_{ij}(p,\bar{q})=1\big], \tag{3}$$

and $\sum_{k \neq j} \Delta_{jk} = 0$*: the network adjustment redistributes diversion across alternatives.*

Proof. We prove part (ii); the argument for part (i) is parallel, with $\bar{q}$ replacing q^*. Consider the Wald estimator Wald_{jk}^{PI} defined in (2).

First, consider the denominator. After removal, no baseline j-user chooses j: $d_{ij}^{\setminus j}(p, q^*) = 0$ for all i with $d_{ij}(p, \bar{q}) = 1$. Since $E[d_{ij}(p, \bar{q}) \mid d_{ij}(p, \bar{q}) = 1] = 1$, the denominator equals 1. Next, consider the numerator. By single-homing (Assumption 2(b)), $d_{ik}(p, \bar{q}) \cdot d_{ij}(p, \bar{q}) = 0$ for $k \neq j$, so $E[d_{ik}(p, \bar{q}) \mid d_{ij}(p, \bar{q}) = 1] = 0$. By Assumption 2(a), each baseline j-user optimizes over $\mathcal{J} \setminus \{j\}$ at q^*, so $d_{ik}^{\setminus j}(p, q^*) = D_{jk,i}(p, q^*)$ for all i with $d_{ij}(p, \bar{q}) = 1$. The numerator therefore equals $E[D_{jk,i}(p, q^*) \mid d_{ij}(p, \bar{q}) = 1]$.

Part (iii) follows from parts (i) and (ii). For the adding-up result: since $\sum_{k \neq j} D_{jk,i}(p, q) = 1$ for any q, we have $\sum_{k \neq j} E[D_{jk,i}(p, q) \mid d_{ij}(p, \bar{q}) = 1] = 1$ at both $q = \bar{q}$ and $q = q^*$. Differencing gives $\sum_{k \neq j} \Delta_{jk} = 0$. ■

Proposition 1 establishes that both Wald estimators equal the average individual diversion ratio among baseline j-users, but evaluated at different network configurations.[40] Wald_{jk}^{FN} is the object recovered by standard approaches such as individual deactivation experiments: it measures where displaced j-users go holding network sizes fixed. Wald_{jk}^{PI} is the natural object for collective interventions such as platform bans, where removal of j displaces some users and network sizes adjust to q^*: it measures where baseline j-users go after network adjustment. The network adjustment Δ_{jk} quantifies the gap between these two objects. A nonzero Δ_{jk} implies that standard individual-level Wald estimates mismeasure diversion under collective removal. Lastly, the adding-up result shows that this gap is zero-sum: the network adjustment shifts diversion toward some alternatives and away from others. In our application, $\Delta_{jk} > 0$ for Instagram, YouTube, and Snapchat.

We now discuss two qualifications to this result.

Prices. Social media platforms charge zero monetary prices to users; the relevant analog is advertising load (Anderson and Coate, 2005). Our analysis holds advertising loads fixed: both Wald_{jk}^{FN} and Wald_{jk}^{PI} are evaluated at baseline prices p, so Δ_{jk} captures the network adjustment in partial equilibrium, holding constant the advertising side of the market. In principle, collective removal could also trigger adjustments in advertising loads or the composition of advertisers (Donati and Fong, 2025) on remaining platforms, in which case Δ_{jk} would understate the full equilibrium response. Nevertheless, in our empirical results, respondents do not report advertising loads or microtargeting as drivers of their substitution decisions.

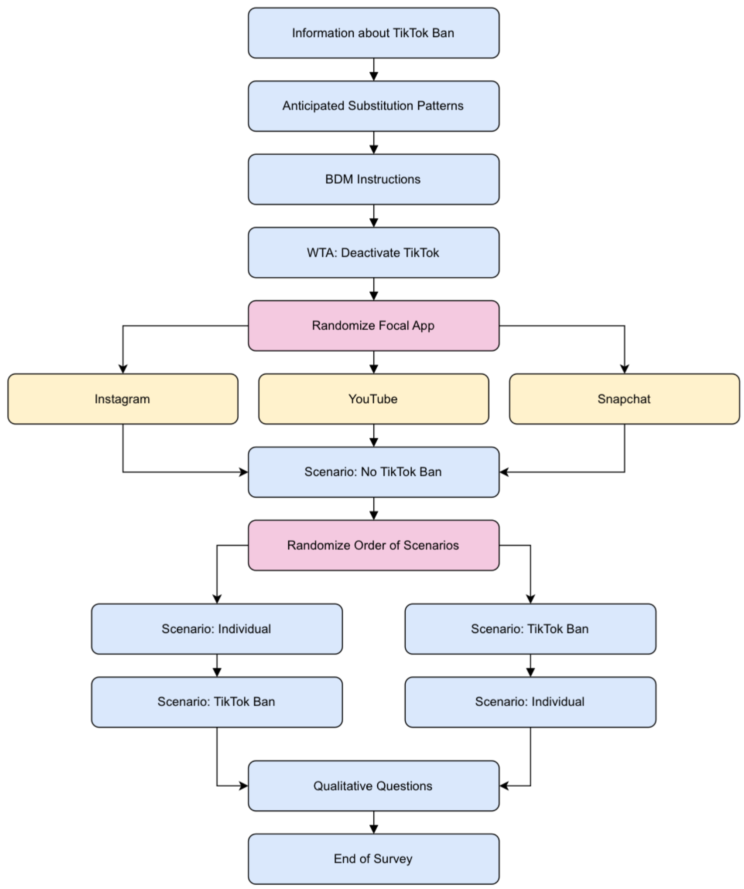
[40] Our experiment conditions on baseline j-users by design: the sample consists exclusively of active TikTok users.

Multihoming. The analysis above assumes single-homing. Under multihoming, one can redefine products as bundles of platforms following Gentzkow (2007), in which case consumers single-home over bundles and the preceding analysis applies at the bundle level. The Wald estimator remains well-defined, but the adding-up constraint of Proposition 1 relaxes: $\sum_{k \neq j} \Delta_{jk}$ need not equal zero, as collective removal may change the total number of platforms adopted.

B Deactivation Experiment: Additional Tables and Figures

B.1 Structure of The Experiment

Figure A1: Structure of the experiment: TikTok Ban Study

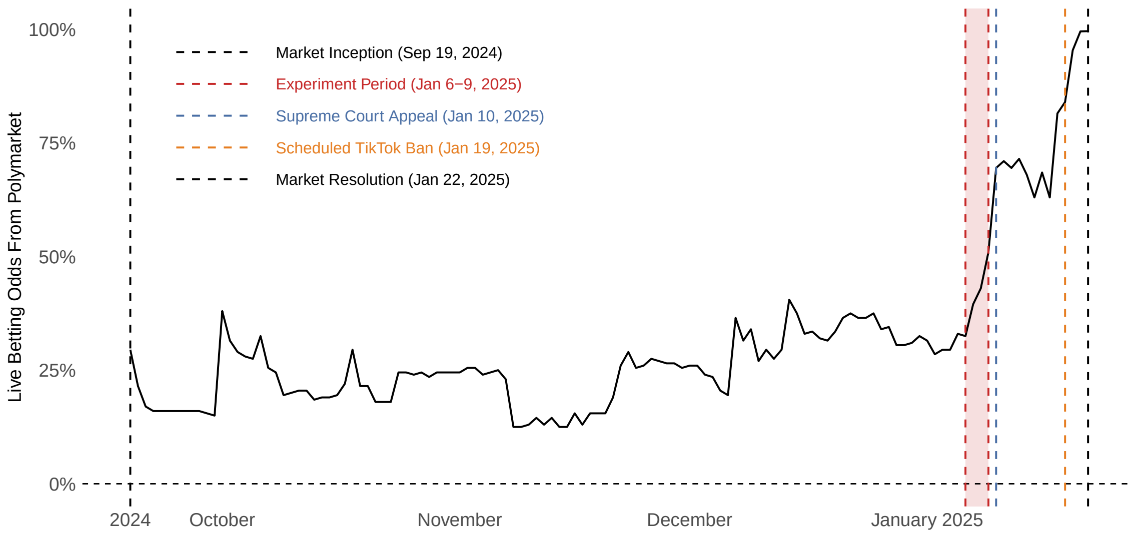


Notes: Figure A1 displays the structure of our experiment. Participants begin by receiving information about the upcoming TikTok ban and subsequently answer questions regarding their anticipated time substitution patterns to social apps. Next, the survey provides instructions for the BDM mechanism, followed 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.

B.2 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 A2, 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 A2: Implied Probability of TikTok Ban Implementation Over Time (Polymarket Betting Data)



Notes: Figure A2 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.

B.3 Sample Characteristics

Table A1: Balancing Table by Treatment Arm

	Instagram	**YouTube**	**Snapchat**	***p*-value**
Age	23.7 (2.4)	23.4 (2.4)	23.4 (2.5)	0.166
Female (%)	64.9	68.3	68.7	0.542
Student (%)	50.9	47.7	48.1	0.688
Single (%)	42.4	50.5	44.8	0.125
In a Relationship (%)	44.3	37.3	41.1	0.216
Daily TikTok User (%)	70.9	75.3	75.4	0.350
Daily Instagram User (%)	67.4	66.2	66.0	0.923
Daily YouTube User (%)	54.7	55.1	54.2	0.979
Daily Snapchat User (%)	44.3	44.6	49.2	0.410
Number of Observations	316	287	297	

Notes: This table reports summary statistics of respondent characteristics by treatment arm (platform assignment). Each participant was randomly assigned to value one of three platforms: Instagram, YouTube, or Snapchat. For binary variables, we report the percentage of respondents in each category. For continuous variables, we report means with standard deviations in parentheses. The p-value column reports results from one-way F-tests of the null hypothesis that the population means are equal across the three treatment arms. No characteristic exhibits a statistically significant difference at conventional levels (all $p > 0.10$), confirming that randomization was successful.

B.4 Average Valuations

We present pre-registered analyses of average differences in valuations of the focal apps, which measure the overall intensity of respondents' preferences. Figure A3 summarizes how average valuations differ across three key scenarios: individual TikTok deactivation, a complete TikTok ban, and the no TikTok ban baseline. Appendix Figures A4c through A4d provide inverse demand curves, both pooled and disaggregated by platform, as an alternative visualization.

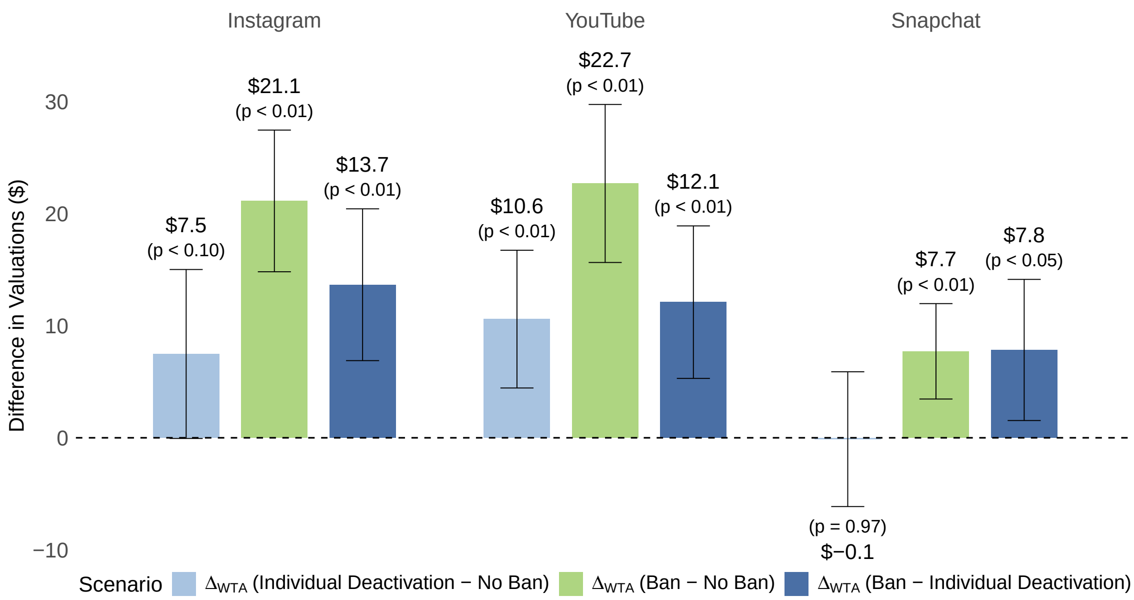
The light blue bars illustrate relatively modest valuation differences between the individual TikTok deactivation and the no TikTok ban scenarios. Specifically, these differences amount to \$7.48 ($p = 0.051$) for Instagram, \$10.59 ($p < 0.01$) for YouTube, and -\$0.12 ($p = 0.968$) for Snapchat.

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.

Figure A3: Average Difference in Valuations across Scenarios by Platform

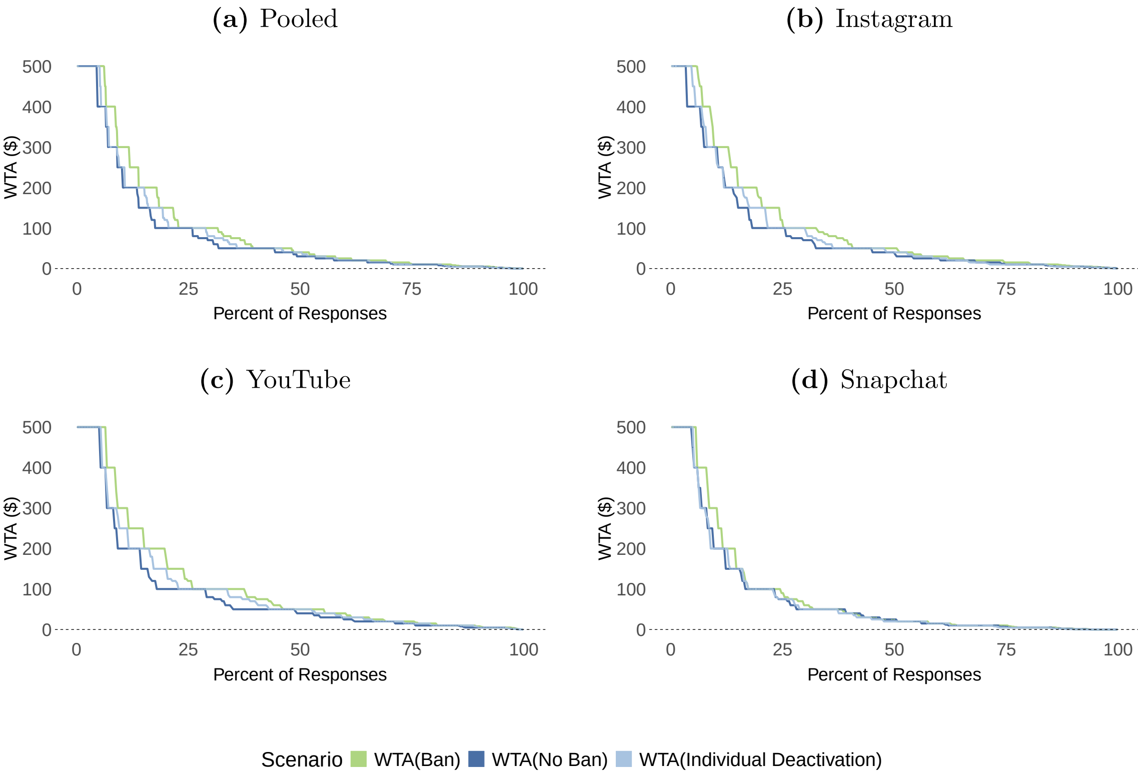


Notes: Figure A3 illustrates the differences in continuous valuations of the focal app across our three scenarios. The light blue bars depict the average difference between valuations under the individual TikTok deactivation scenario and the no TikTok ban scenario. The green bars represent the average 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. Error bars indicate 95% confidence intervals.

B.5 Inverse Demand Functions

Figure A4a displays the inverse demand curve for respondents' WTA for deactivating their assigned alternative platform 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.

Figure A4: Inverse Demand Curves



Notes: Figure A4 displays inverse demand curves for respondents' incentivized willingness-to-accept (WTA) for deactivating social apps for four weeks under three scenarios. Panel a) pools all platforms (Instagram, YouTube, and Snapchat). Panels b), c), and d) show results separately for Instagram, YouTube, and Snapchat, respectively. In each panel, the green curve shows WTA under the TikTok ban scenario, the dark blue curve shows WTA under the no TikTok ban scenario, and the light blue curve shows WTA under individual TikTok deactivation.

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 A4a, 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.

B.6 Continuous WTA Results

Table A2: 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^2	0.056	0.073	0.017
Number of Observations	316	287	297

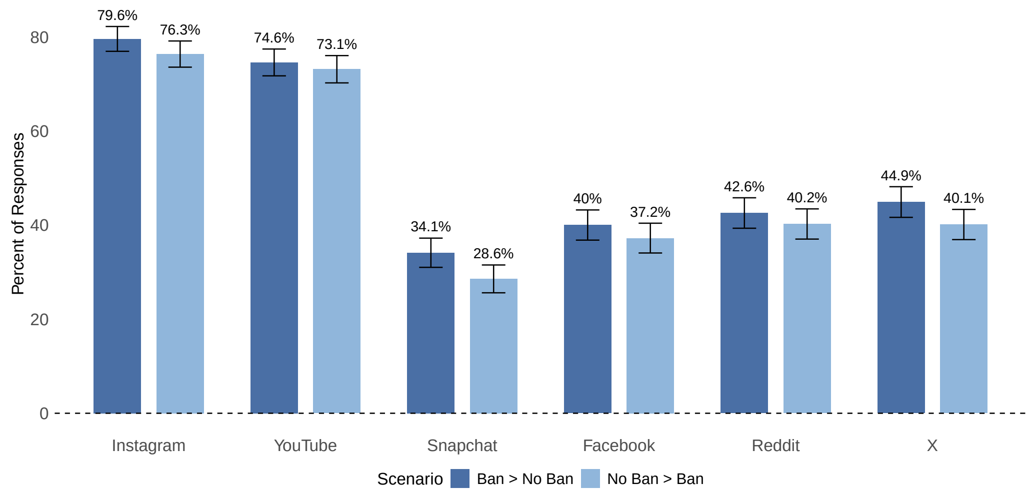
Notes: Table A2 displays the regression results for our pre-registered specification for the continuous WTA measure. WTA: No TikTok Ban represents the average WTA to deactivate the platform when there is no TikTok ban. Δ_{WTA}: Individual TikTok Deactivation - No TikTok Ban is the change in WTA when 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.7 Anticipated time substitution

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 A5, 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 A5 differ from those presented in Figure 2: 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 A5 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.

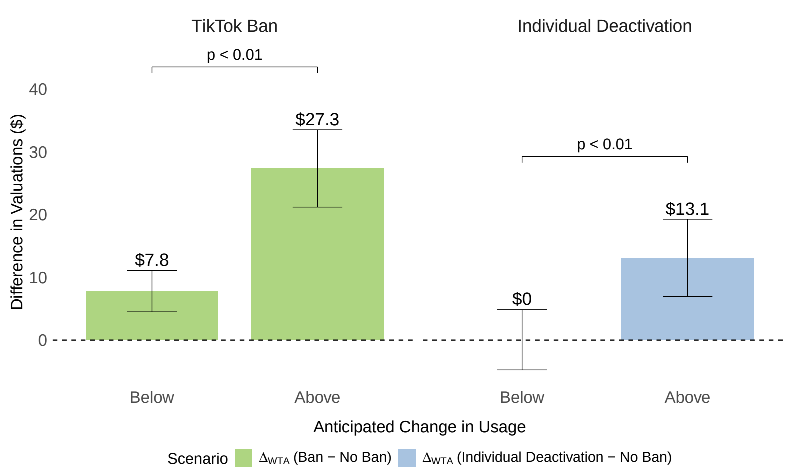
As shown in Appendix Figure A6, we find that participants who predicted above-median increases in time on their focal app exhibit a higher WTA for deactivating TikTok, compared to the no TikTok ban scenario, in both the collective ($p < 0.01$) and individual ($p < 0.01$) treatment conditions.

Figure A5: Proportion of Respondents Indicating an Increase in Time Spent on a Given Platform under Individual TikTok Deactivation and TikTok Ban Scenarios



Notes: Figure A5 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 A6: Treatment Effect and Anticipated Substitution Time Change

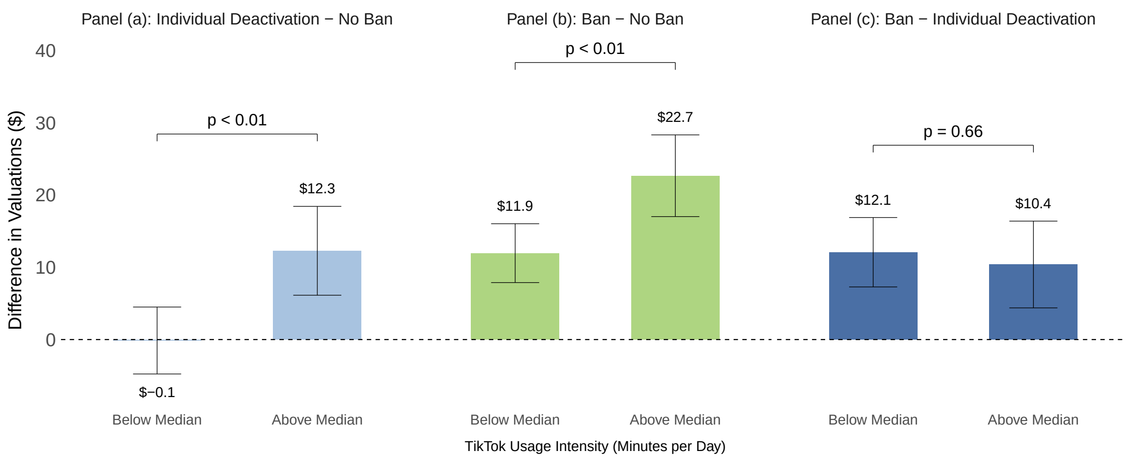


Notes: For both the TikTok ban and individual deactivation scenarios, Figure A6 displays the average change in WTA from the No Ban scenario separately for respondents with below- and above-median anticipated changes in their time use of their alternative platform under the given scenario. The error bars represent 95% confidence intervals.

B.8 Heterogeneity by Usage Intensity

We next split the sample at the median of self-reported daily TikTok screen time. Heavy users report significantly higher valuations in both scenarios, but the difference between collective and individual valuations—the network effect component—is statistically indistinguishable across groups, suggesting that network-driven substitution operates similarly for light and heavy users. Figure A7 and Table A3 present these results.

Figure A7: Treatment Effect by TikTok Usage Intensity



Notes: Figure A7 shows the treatment effect split at the median of self-reported daily TikTok usage (minutes per day). Each panel displays the mean WTA difference between two scenarios, separately for below- and above-median TikTok users: (a) Individual Deactivation − No Ban; (b) Ban − No Ban; (c) Ban − Individual Deactivation. Error bars denote 95% confidence intervals. Bracket p-values are from two-sample t-tests comparing means across usage-intensity groups.

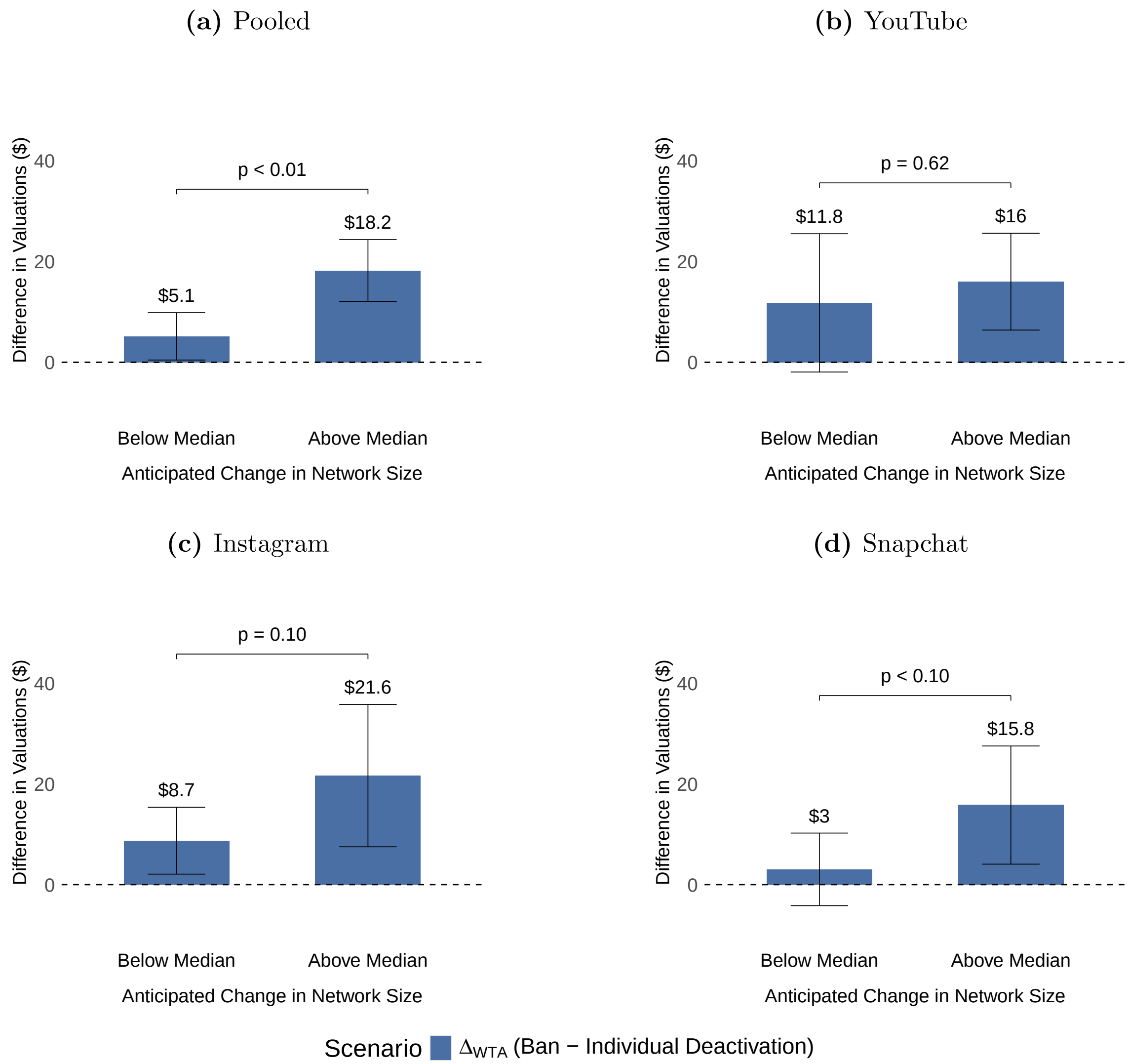
Table A3: Regression Results: Continuous WTA by Usage Intensity

	TikTok Usage Intensity	
	Below Median	**Above Median**
Δ_{WTA}: **TikTok Ban - Individual TikTok Deactivation**	12.073***	10.379***
	(2.441)	(3.053)
Δ_{WTA}: **Individual TikTok Deactivation - No TikTok Ban**	−0.127	12.271***
	(2.355)	(3.126)
WTA: **No TikTok Ban**	81.976***	79.204***
	(1.234)	(1.723)
R-squared	0.038	0.060
Number of Observations	458	442

Notes: Table A3 reports estimates from the main WTA regression estimated separately for respondents with below- and above-median self-reported daily TikTok screen time (minutes per day). The omitted category is the no-ban scenario. Standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.9 Anticipated network effects

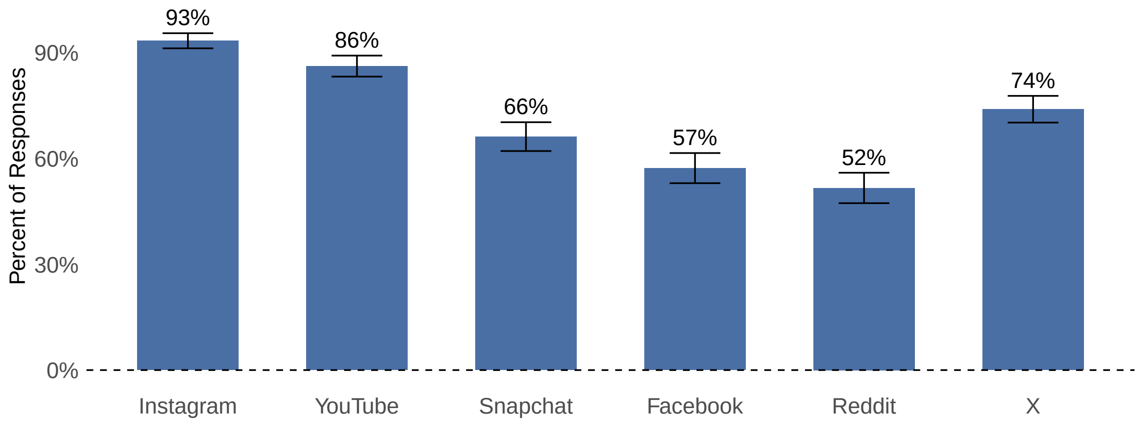
Figure A8: Treatment Effect and Anticipated Network Change (Split by Platform)



(a) Pooled (b) YouTube (c) Instagram (d) Snapchat

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 pooled across platforms. Panels (b), (c), and (d) show the difference for Instagram, YouTube, and Snapchat respectively. The error bars represent 95% confidence intervals.

Figure A9: Expected Increase in Friends' Time on Other Platforms if TikTok Is Banned



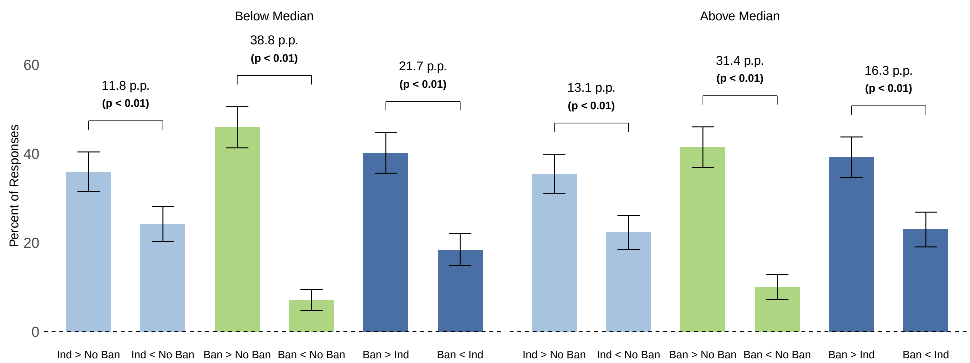
Notes: Figure A9 presents the fraction of respondents who expect their friends to increase their usage of various social media platforms following a TikTok ban, with answers being rated on a 7-point Likert scale ("Strongly decrease", "Decrease", "Slightly decrease", "Not change", "Slightly increase", "Increase", "Strongly increase"). The error bars represent 95% confidence intervals.

B.10 Robustness

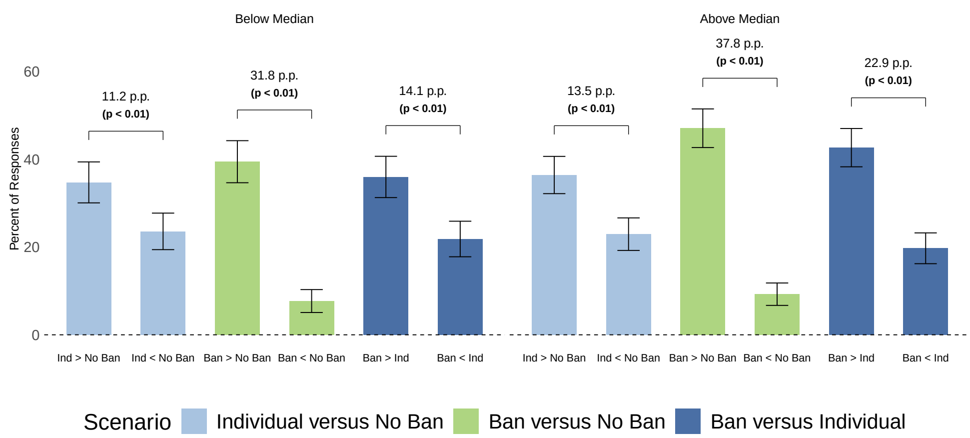
Perceived Likelihood We split the sample at the median perceived likelihood of the TikTok ban and of the individual TikTok deactivation. Figures A10a and A10b present the fraction with higher or lower valuations for each group. Table A4 reports the corresponding continuous WTA results: columns 1 and 2 split by perceived likelihood of the ban, columns 3 and 4 by perceived likelihood of individual deactivation. We pool across outside options for ease of exposition.

Figure A10: Valuation Differences by Perceived Likelihood

(a) Split by Median Perceived Likelihood of TikTok Ban



(b) Split by Median Perceived Likelihood of Individual TikTok Deactivation



Notes: Figure A10 illustrates the fraction with higher or lower valuation by scenario for those above and below the median perceived likelihood. Panel (a) splits by perceived likelihood of the TikTok ban; Panel (b) splits by perceived likelihood of the individual TikTok deactivation. The light blue bars show the proportion of individuals with a higher or lower WTA during individual deactivation compared to the no ban scenario. The green bars display the same proportions comparing the collective ban to no ban. The dark blue bars compare the ban to individual deactivation. Values above bars display the net fraction with a higher WTA. Error bars represent 95% confidence intervals.

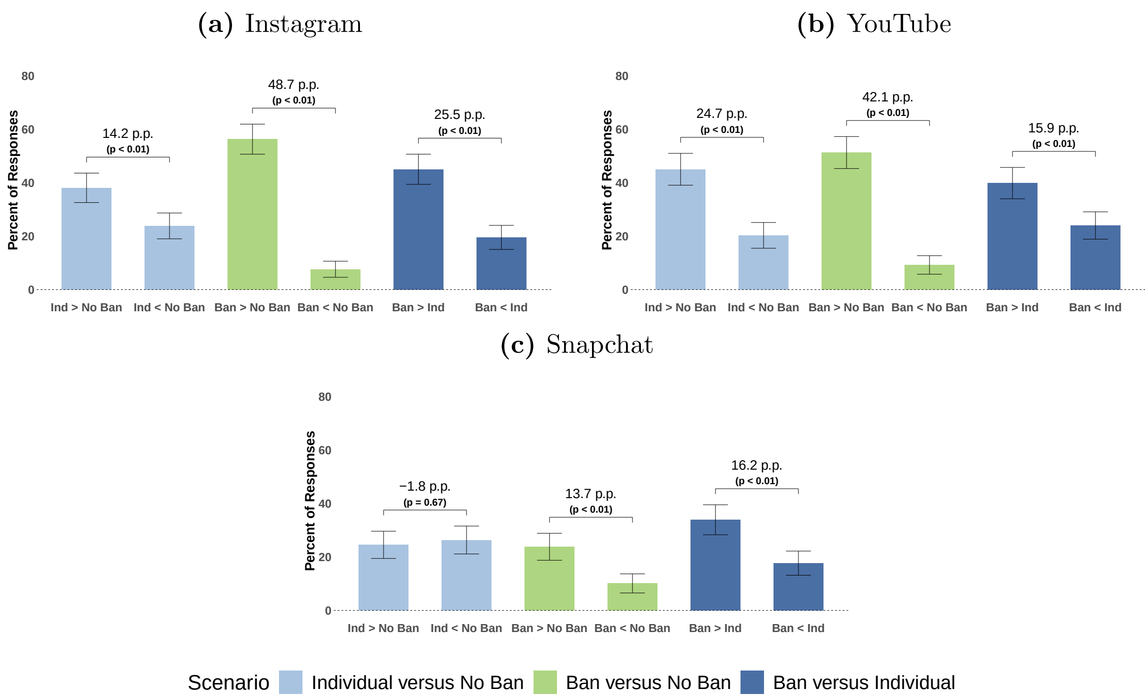
Table A4: Continuous WTA by Median Perceived Likelihood Split

	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^2	0.047	0.044	0.035	0.059
Number of Observations	451	449	403	497

Notes: Table A4 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 A5 below, we show that our continuous WTA results are robust to dropping anyone who regrets a choice, even once.

Figure A11: Valuation Differences Excluding Respondents Who Regret First Valuation



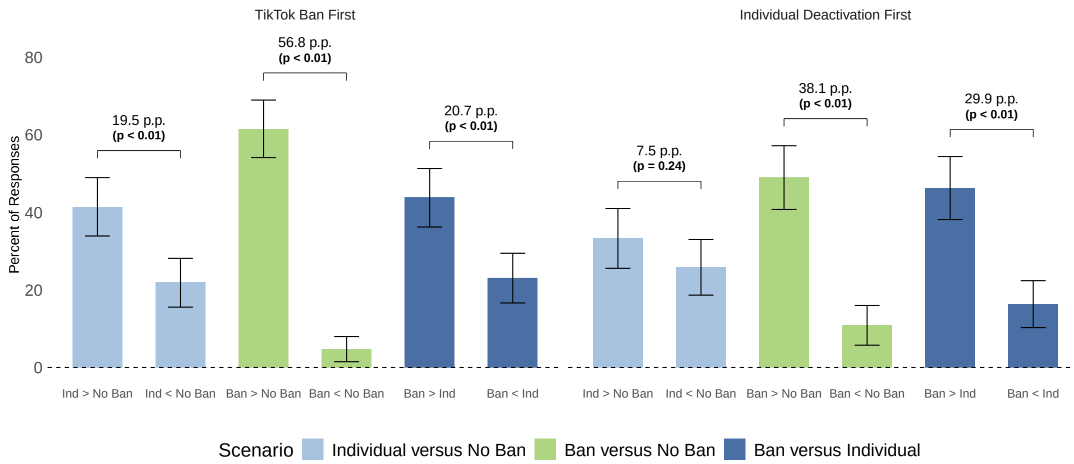
Notes: Figure A11 illustrates differences in the valuation of alternative apps across three scenarios by platform, excluding those who regret their first valuation. For each platform, the light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative 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. Error bars represent 95% confidence intervals.

Table A5: Continuous WTA Excluding Respondents Who Regret First Valuation

	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^2	0.057	0.070	0.024
Number of Observations	302	277	271

Notes: Table A5 reports regression results for the continuous WTA, dropping respondents who regret their first valuation. The omitted category is the no-ban scenario. 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: Valuation Differences by Order of Scenario: Instagram



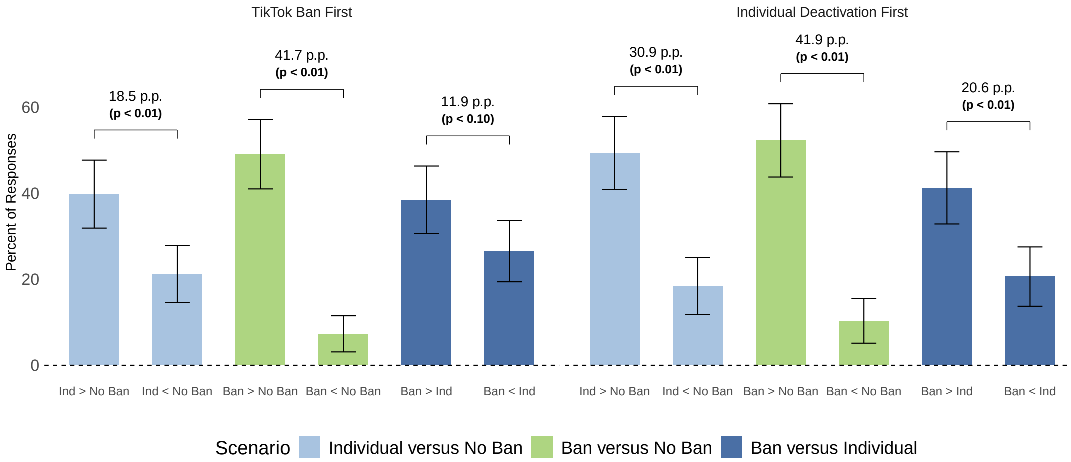
Notes: Figure A12 illustrates the fraction with higher or lower valuation by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). The light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative 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: 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^2	0.057	0.058
Number of Observations	169	147

Notes: Table A6 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). The omitted category is the no-ban scenario. 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: Valuation Differences by Order of Scenario: YouTube



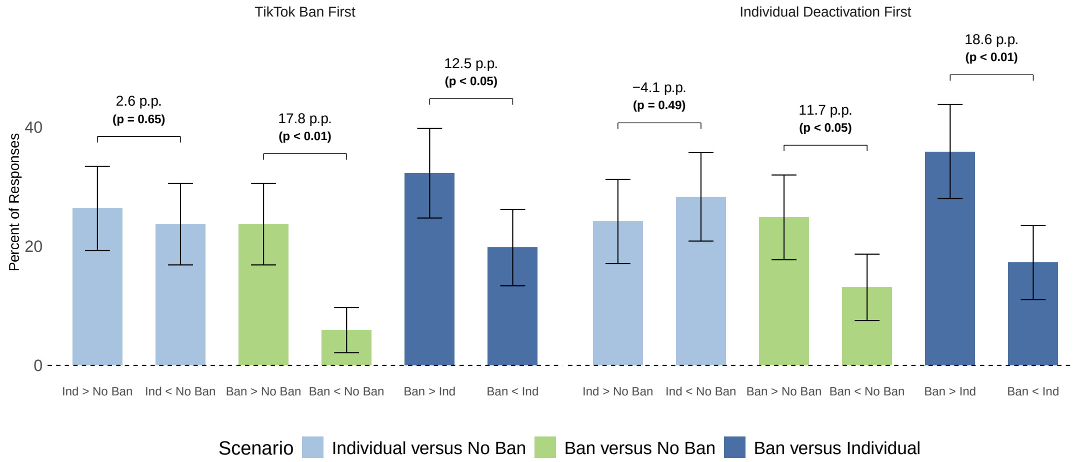
Notes: Figure A13 illustrates the fraction with higher or lower valuation by the order the second and third scenarios were presented in (i.e., either TikTok ban first or individual TikTok deactivation first). The light blue bars show the proportion of individuals who have a higher or lower WTA to deactivate their alternative 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 A7: 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^2	0.097	0.060
Number of Observations	151	136

Notes: Table A7 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: Valuation Differences by Order of Scenario: Snapchat



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 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 A8: 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^2	0.016	0.021
Number of Observations	152	145

Notes: Table A8 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Robustness

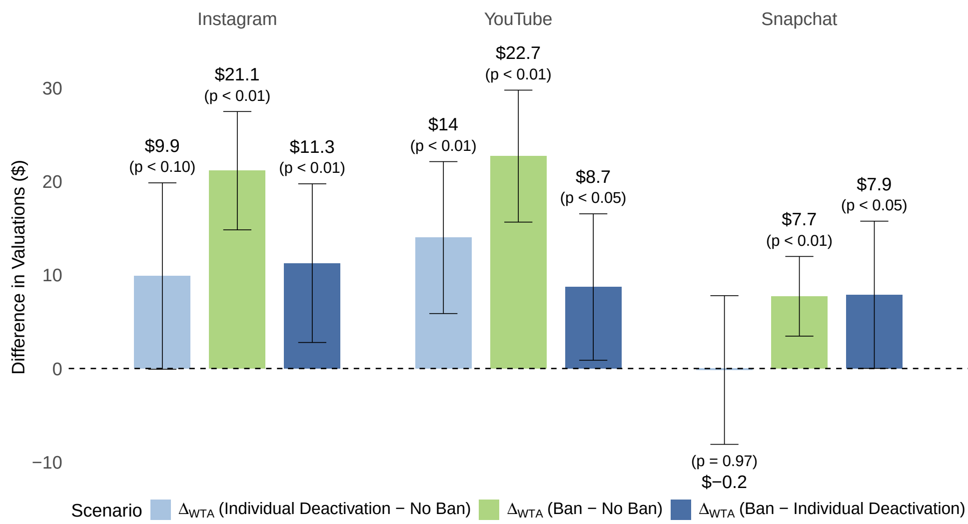
Perceived Probability In normal times, studying incentivized valuations under collective deactivation is difficult because the deactivation may be perceived as having a low probability of occurring. Given the large amount of uncertainty about the TikTok ban, we found it ex ante likely that respondents 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 A4, Figure A10a, and Figure A10b 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 A5 and Figure A11, 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 A6, Table A7, Table A8, Figure A12, Figure A13, and Figure A14.

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 (implying a 33% and 47% attrition rate). For Snapchat, 13 attempted week 1 (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 A15: Valuation Differences with Compliance Adjustment



Notes: Figure A15 illustrates the differences in continuous valuations of the alternative platform 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: $WTA^{platform[X]}_{ind,measured} = p \cdot WTA^{platform[X]}_{ind,true} + (1-p) \cdot WTA^{platform[X]}_{noban,true}$, where p is the average compliance rate. Error bars indicate 95% confidence intervals.

Threshold for valuation comparisons Our main results classify respondents as having "higher" or "lower" valuations based on any strict inequality across scenarios. To address the concern that marginal differences may not reflect meaningfully different preferences, we re-compute net fractions using increasingly conservative definitions of "equal" valuations: two valuations are classified as equal if they differ by less than 5%, 10%, or 15%. Table A9 reports these results. The net fractions are very similar across all the thresholds.

Table A9: Robustness of Net Valuation Fractions to Threshold Definitions

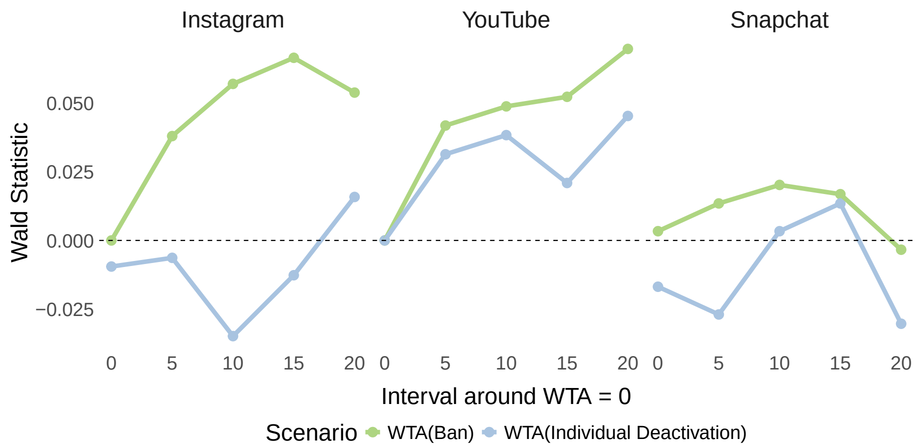
	Individual Deactivation vs No Ban			Ban vs No Ban			Ban vs Individual Deactivation		
	Instagram	YouTube	Snapchat	Instagram	YouTube	Snapchat	Instagram	YouTube	Snapchat
0% (Baseline)	13.9%	24.4%	-0.7%	48.1%	41.8%	14.8%	25.0%	16.0%	15.5%
5%	13.9%	24.4%	-0.7%	47.8%	41.8%	14.5%	25.0%	15.7%	15.5%
10%	13.6%	24.0%	-1.0%	47.2%	42.2%	13.8%	25.9%	15.7%	15.8%
15%	13.6%	23.3%	-1.0%	45.6%	41.5%	13.5%	26.3%	16.4%	15.2%

Notes: A9 reports the net fraction of respondents with a higher valuation under scenario A relative to scenario B for each platform, using alternative threshold definitions for "equal" valuations. Two valuations are classified as equal if they differ by less than the stated threshold percentage. The baseline row (0%) corresponds to the strict inequality used in our main analysis.

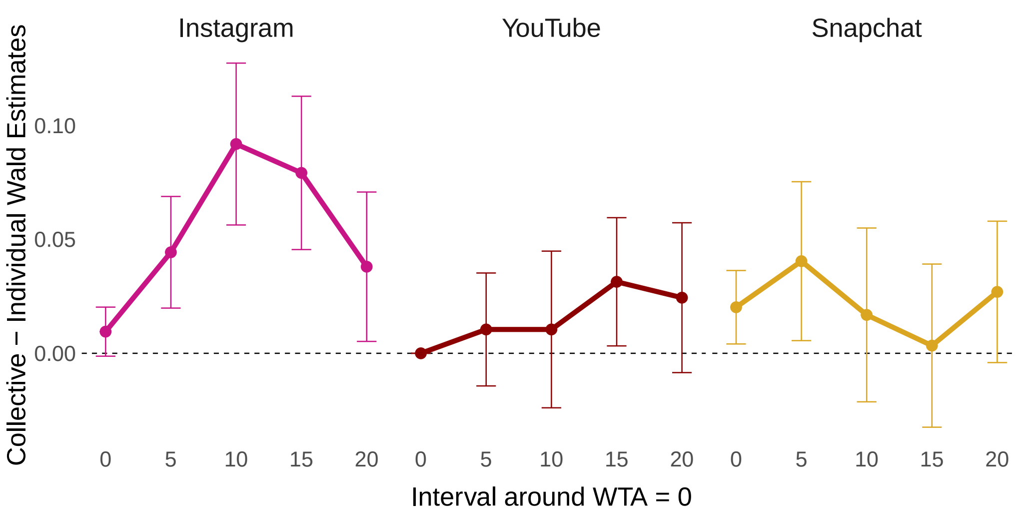
C.1 Wald Estimates

Figure A16: Wald Estimates by Platform

(a) Wald Estimates



(b) Difference in Wald Estimates



Notes: Panel a) of Figure A16 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 A16 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.

D Collective Time Limit Challenge: Additional Analyses

D.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.[41] 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

[41] "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 D.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.

D.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.

D.3 Category Classification List

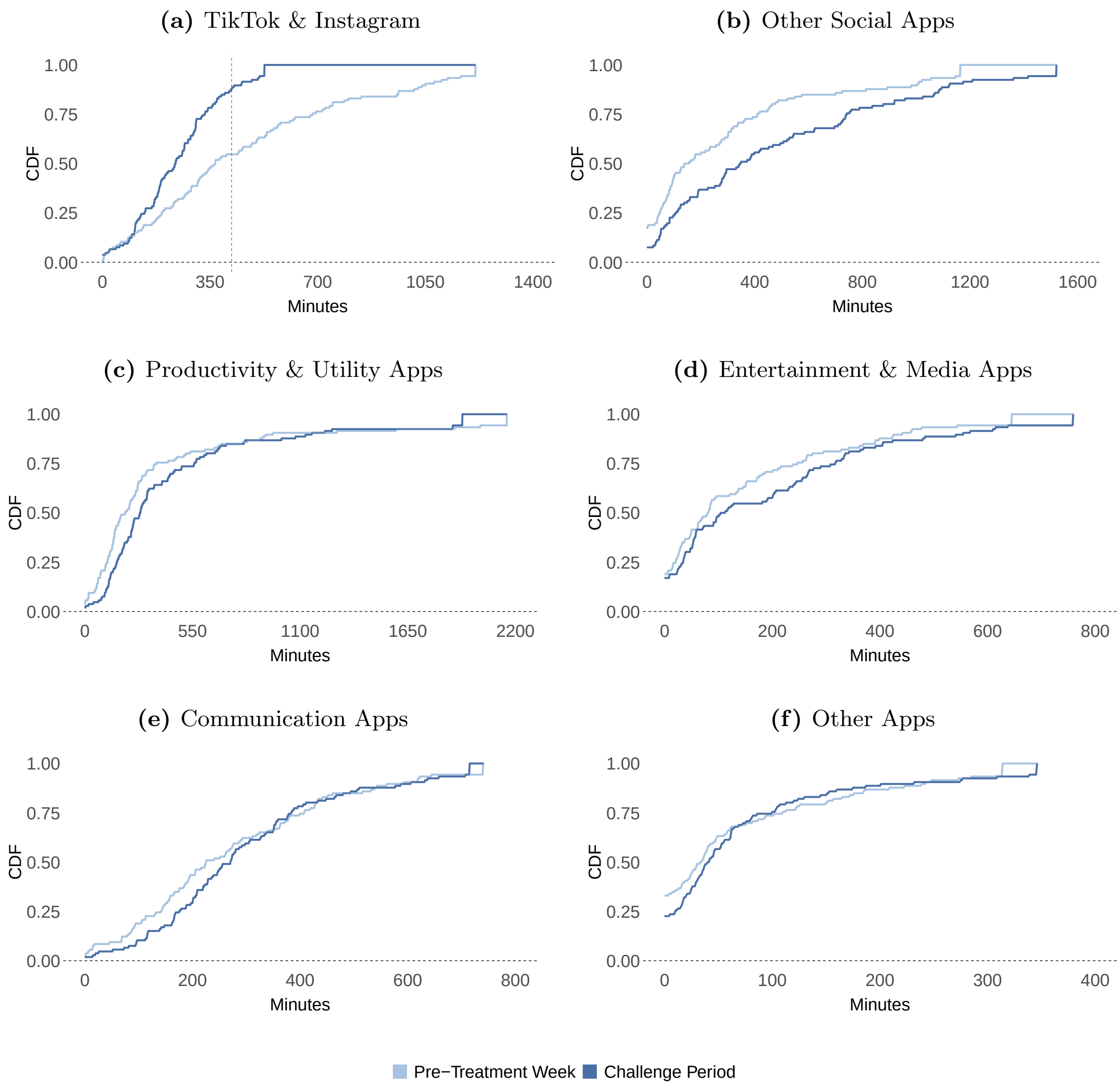
Table A10: Classification of Applications and Websites

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, WeChat, Weibo, X, X.com, Yik Yak, YouTube, iFunny, t.me, LinkedIn, and Snapchat.
Productivity & Utility Apps	This category includes apps that support work, study, or everyday tasks — such as email clients, browsers, note-taking tools, 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, 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 A10 provides a more disaggregated overview of our category definitions and app classifications. Our extracted screenshot data identifies 499 unique apps and websites that were hand-coded 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.

D.4 Distribution of Screen Time Minutes

Figure A17: Cumulative Distribution Functions by App Category

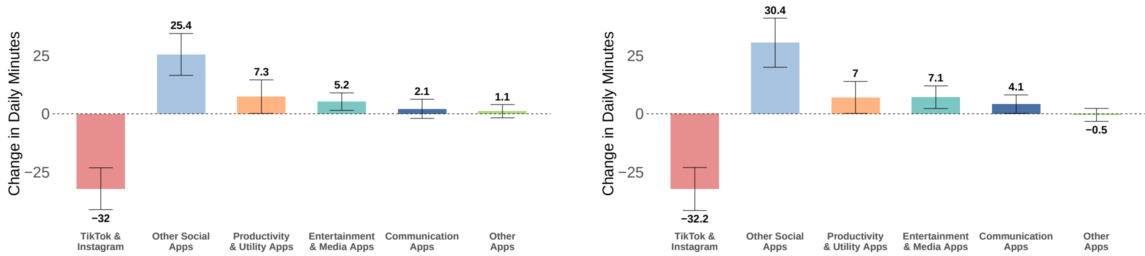


Notes: Figure A17 plots the empirical cumulative distribution functions (CDFs) of weekly minutes spent on each app category among challenge participants. In each panel, the dark blue curve shows usage during the deactivation period and the light blue curve shows the pre-treatment week. Panel (a) adds 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.

D.5 Dynamics of Diversion

Figure A18: Dynamics of Diversion: Week 1 versus Week 2 Substitution

(a) Week 1 substitution patterns **(b)** Week 2 substitution patterns

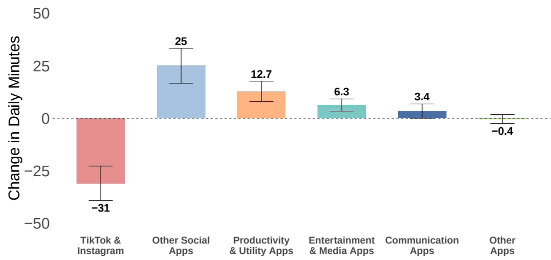


Notes: Panel (a) presents the average change in daily minutes spent on app categories in the first week of the time limit challenge. Panel (b) presents the average change in daily minutes in the second week of the time limit challenge. We categorize apps into six groups: (1) "TikTok & Instagram," which 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; (3) "Productivity & Utility Apps," defined as applications supporting work, study, or everyday tasks—such as email clients, browsers, note-taking tools, and transport apps; (4) "Entertainment & Media Apps," defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games; (5) "Communication Apps," defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds; (6) All remaining apps and websites are grouped into "Other Apps." A comprehensive list of classified apps is provided in Appendix Section D.3. Error bars represent 95% confidence intervals.

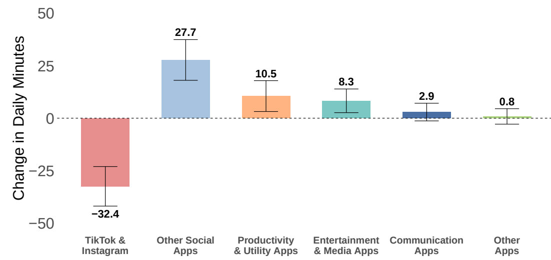
D.6 Robustness

Figure A19: Robustness of Substitution Pattern Estimates

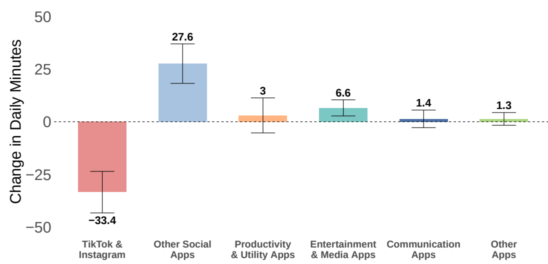
(a) Winsorizing at the 90th Percentile



(b) Winsorizing at the 99th Percentile



(c) Compliers Only



Notes: Figure A19 presents robustness checks for substitution pattern estimates. Panels a) and b) winsorize screen time at the 90th percentile and 99th percentile respectively. Panel c) restricts the sample to 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; (3) “Productivity & Utility Apps,” defined as applications supporting work, study, or everyday tasks—such as email clients, browsers, note-taking tools, and transport apps; (4) “Entertainment & Media Apps,” defined as applications designed primarily for leisure, including streaming services, music and sports platforms, and mobile games; (5) “Communication Apps,” defined as apps centered on interpersonal communication or sharing without a central emphasis on content feeds; (6) All remaining apps and websites are grouped into “Other Apps.” A comprehensive list of classified apps is provided in Appendix Section D.3. Error bars represent 95% confidence intervals.