One-Sided Regulation, Two-Sided Responses: Evidence from Food Delivery*

Michael Rolland Sullivan

University of British Columbia

October 26, 2026

Please find the Online Appendix here.

Abstract

Platform regulations often cap fees on just one side of a two-sided market. Theory suggests that such caps raise fees on the other side. Capping merchant fees, e.g., may raise consumer fees, thereby reducing merchants' platform sales. Yet merchants may benefit if higher consumer fees induce substitution toward off-platform sales, which are commission-free. Using difference-in-differences, I estimate the effects of caps on US food delivery platforms' restaurant commissions. I find that caps raise consumer fees and hence reduce platform order volumes. However, off-platform sales rise enough to offset these losses fully and amplify the caps' benefits to restaurants.

*Email address: michael.sullivan@sauder.ubc.ca. This project draws on research supported by the Social Sciences and Humanities Research Council. I thank Guillermo Marshall for helpful comments.

1 Introduction

Examples abound of regulations restricting platform conduct on one side of a two-sided market; consider merchant fee caps in the payment card industry, minimum pay standards in ride-hailing, and prohibitions on price parity clauses in online hotel booking.[1] The effects of such interventions are shaped by the responses they provoke on both sides of the market. Capping, for instance, a platform's merchant-side fees—which are typically called *commissions*—directly benefits merchants, yet theory suggests that such a cap may lead the platform to increase the fees that it charges to consumers. This would reduce consumer use of the platform and hence merchants' platform sales, counteracting the benefits of a commission cap to merchants. If, however, consumers facing higher fees choose to circumvent the platform and instead purchase directly from merchants, merchants may avoid commission payments altogether and hence be better off. Furthermore, a commission cap may lead more merchants to join the platform, possibly offsetting the reduction in platform ordering effected by consumer fee increases. In short, the effects of one-sided platform regulation are nuanced; ascertaining them requires an understanding of how all sides' responses are interdependently determined.

In this article, I empirically assess how responses on both the consumer and merchant sides of a platform market shape the overall effects of caps on platforms' merchant-side commissions. The commission caps that I study were imposed by local governments on food delivery platforms in the US during the COVID-19 pandemic. Just before the introduction of caps, delivery platforms such as DoorDash and Uber Eats charged restaurants a commission of, typically, 30% of order value. They also charged consumers per-order fees. Over the course of 2020–2021, more than 70 jurisdictions covering about 60 million people capped restaurant commission rates, most at 15%. By early 2021, 18% of US restaurants were located in a jurisdiction with a cap. Proponents of commission caps argued that these policies were necessary to ensure local restaurants' continued profitability despite the rise of powerful platforms charging high commission rates during the beginning of the COVID-19 pandemic. Given that commission caps were intended to benefit

[1] Examples include, in payment cards, the Reserve Bank of Australia's 2003 interchange reforms, the US Durbin Amendment, and the European Union's Interchange Fee Regulation of 2015 (Chang et al., 2005; Evans et al., 2015; Kay et al., 2018); in ride-hailing, New York City's 2019 minimum earnings rule for app-based drivers and California's Proposition 22 (Parrott and Reich, 2018); and in online hotel booking, France's *Loi Macron* of 2015 and the Bundeskartellamt's 2015 decision against Booking.com (Hunold et al., 2018; Mantovani et al., 2021; Ennis et al., 2023).

restaurants, I focus in particular on how responses on each side of the market affected restaurant welfare.

The economics of two-sided markets suggests subtleties in the effects of commission caps. One fundamental concept in two-sided markets is that of the *seesaw effect*, whereby a force that depresses platform fees on one side of the market tends to raise them on the other side (Rochet and Tirole 2003; Rochet and Tirole 2006; Weyl 2010). The seesaw effect suggests that commission caps raise food delivery platforms' consumer fees. To understand the effect, consider the platform's problem in setting its consumer fee. Although the platform increases its margin when it raises its consumer fee, it also reduces sales and hence its revenues from restaurant commissions. When commissions are capped, however, the platform loses less restaurant-side revenue from boosting its consumer fee, thus encouraging it to do so. Fee hikes owing to the seesaw effect directly hurt consumers.

Another crucial concept in two-sided markets is that of *network externalities*. This term refers to the benefits that users on each side of a platform enjoy from participation on the opposite side (Rochet and Tirole 2003; Armstrong 2006; Rysman 2009). In food delivery, consumers are attracted to platforms offering a greater variety of nearby restaurants whereas restaurants are attracted to platforms that provide access to many consumers. When a platform raises consumer fees, it narrows its consumer base and thus reduces restaurant sales on the platform, hurting restaurants. Network externalities running from the restaurant side to the consumer side are also relevant. A commission cap raises restaurant margins on food delivery platforms, thus encouraging restaurants to join platforms. This benefits consumers who enjoy restaurant variety. Consumer benefits from increased variety may counteract their losses from fee hikes, making the impact of a commission cap on platform order volumes and consumer welfare theoretically ambiguous. The tension reflects a general feature of platform pricing: the fee a platform charges on one side balances the margin earned on that side against the value of participation on that side to the other side (Armstrong, 2006; Weyl, 2010), so a regulation that alters the fee on one side changes participation on both. Wang and Wright (2024) analyze fee caps in such a setting.

A third important concept is that of *offline substitution* (Edelman and Wright 2015; Wang and Wright 2024; Gomes and Mantovani 2025; Hagiu and Wright 2025). Restau-

rants available on food delivery platforms typically also sell meals directly to consumers, whether for take-out or dine-in. Platform ordering and off-platform direct ordering are likely substitutes for consumers. Therefore, commission caps inducing platforms to raise their consumer fees may consequently lead consumers to switch from platform ordering to direct ordering. This could benefit restaurants given that direct sales do not carry commission charges. But the benefit is hardly assured. As noted, commission caps may lead more restaurants to join food delivery platforms, thus diverting commission-free direct sales to the platform channel and reducing restaurant profitability. Furthermore, if platform and direct ordering are only weakly substitutable, fee increases would reduce overall restaurant ordering across the platform and direct channels, thus depressing restaurant revenues.

Little empirical evidence exists on the complex set of responses to one-sided platform regulations stemming from seesaw effects, network externalities, and offline substitution. It is the goal of this article to provide such evidence. To study the effects of commission caps, I assemble a rich collection of datasets on the US food delivery industry. The primary datasets are two distinct panels of consumer restaurant orders that together include sales records and item-level pricing information by ZIP code. I supplement these consumer panels with data on all restaurants listed across major delivery platforms, as well as data harvested from platform websites that capture fees. Together, these data sources provide detailed information on pricing, platform participation, and delivery conditions for hundreds of thousands of orders.

I estimate the effects of commission caps using a difference-in-differences (DiD) approach that exploits the staggered adoption of caps across regions. The results rest on the parallel-trends assumption generally underlying DiD analysis. In my setting, this assumption requires that outcomes in places introducing caps would have followed the same trend as outcomes in places without caps had caps not been introduced. The primary threat to this assumption is the COVID-19 pandemic: severe COVID-19 episodes may both lead cities to adopt caps and also directly impact restaurant ordering and restaurants' platform adoption. This would lead ordering and adoption outcomes to diverge in those cities that adopted caps even if the caps had not been introduced. To address this threat, I control for various measures of COVID-19 severity and the local policy response

to COVID-19.

Another concern is that cities introduced commission caps in a staggered fashion across time, and that caps imposed at different times could have heterogeneous effects. This is precisely the setting in which the standard two-way fixed effects (TWFE) DiD estimator is known to exhibit bias. I therefore report results from recent estimators that are robust to heterogeneous treatment effects under staggered treatment, including the interaction-weighted (IW) estimator of Sun and Abraham (2021)—which I adopt as the preferred estimator—and the estimator of Callaway and Sant'Anna (2021).

The first outcome that I analyze is platform consumer fees. I find that, consistent with the theory of the seesaw effect, platforms raised their consumer fees upon the introduction of commission caps. In aggregate across platforms, caps of 15% or lower raised fees by 12% according to both the preferred IW estimator and the TWFE estimator, and by 10% according to the Callaway and Sant'Anna (2021) estimators. I additionally find fee increases for each major platform individually. Furthermore, event-study analysis suggests that platforms immediately raise their consumer fees in response to commission caps and then gradually further increase them as time passes since the implementation of the cap; I find that consumer fees rise by 22% seven months after a commission cap has been introduced. This fee increase recovers, though, less than a third of the revenue that a platform loses from a typical transaction given a 15 percentage point reduction in its restaurant commission rate.

Although consumer fee hikes stand to reduce platform ordering, commission caps also prompt increased restaurant adoption of platforms, which has the opposite effect. Indeed, I find that commission caps raise the number of restaurant listings on platforms by 12–14%. This increase is driven by changes on both the extensive and intensive margins: the number of unique restaurants listed on at least one platform rose by 4–6%, so that about a fifth of the new listings came from restaurants joining their first platform and the remainder from restaurants already on some platform joining another.

The net effect of higher fees and greater restaurant take-up of platforms turns out to be negative. Under the preferred IW estimator, I find that commission caps reduce the number of orders placed on platforms by 8%. A potential countervailing response could be an increase in order sizes, as consumers could economize on higher fees by placing

fewer but larger orders. However, I find no effect of commission caps on order size. Furthermore, I estimate effects on total platform spending (excluding fees) that are in the same 6–9% range that I find for order counts.

Although commission caps reduced platform ordering, this reduction was entirely offset by an increase in orders placed directly at restaurants. Indeed, I find that off-platform order counts rose by 1–5% across the estimators considered. I additionally find the overall effect of commission caps on restaurant spending to be a precisely estimated zero. Thus, the ultimate consequence of the seesaw effect of commission caps on consumer fees was to steer consumers away from platform ordering and toward direct ordering.

The intended goal of commission caps was to bolster restaurant profitability by expanding their margins on platform orders. Naively, the benefit of a cap to restaurants is the direct saving in commission: cutting the rate from 30% to 15% raises a restaurant's revenue net of commission on a typical platform order by 21%. This estimate is naive because, reflecting the seesaw effect, caps prompt consumer fee increases that reduce platform ordering. Back-of-the-envelope calculations show that accounting for lost platform sales erases most of the direct gain, reducing it to 7%. Offline substitution reverses this conclusion. The lost platform sales are fully replaced by direct, off-platform orders, on which restaurants pay no commission, so that accounting for offline substitution restores and even expands upon the direct gain, raising it to 24%. In essence, the seesaw effect on fees had only a minor positive impact on restaurant revenues due to offline substitution. This central finding of the article highlights the importance of accounting for cross-side dependencies and offline substitution in assessing the effects of platform regulations touching directly on just one side of a two-sided market.

1.1 Related literature

The article's hypothesized responses to platform regulations stem from theoretical models of two-sided markets developed by Rochet and Tirole (2003), Armstrong (2006), Rochet and Tirole (2006), and Weyl (2010).[2] The article's emphasis on the distinct effects of regulation of on- and off-platform sales additionally reflects findings of more recent theoretical studies indicating an important role for off-platform sales in determining the

[2]I use the terms two-sided and platform market interchangeably. See Rysman (2009) and Jullien et al. (2021) for surveys of the literature on two-sided markets.

efficiency of fee regulations (Wang and Wright 2024; Gomes and Mantovani 2025; Hagiu and Wright 2025). My results likewise suggest the importance of accounting for offline substitution in assessing platform regulation.

The closest empirical study to my own is that of Li and Wang (2025b), who similarly estimate effects of commission caps using DiD methods. They use a combination of restaurant-level foot traffic data and payment card data, and they focus particularly on caps that exempted large restaurant chains. Among their main findings is that such caps reduced the takeout visits and net revenue of independent restaurants while raising those of large chains. Additionally, Li and Wang (2025b) link these effects to platform responses to caps, including increases in delivery fees and a shift in their promotional efforts toward chains and toward restaurants in neighbouring uncapped localities.

My article complements Li and Wang (2025b) in several ways. First, whereas they focus on the differential effects of the subset of caps that exempted chains on independent and chain restaurants, I abstract away from restaurant heterogeneity. My focus is instead on how seesaw effects and offline substitution interact to shape caps' aggregate effects on restaurants. This difference in focus is related to differences in the datasets we use: whereas the foot traffic data of Li and Wang (2025b) permit an analysis of the performance of individual restaurants—which is challenging with the sort of consumer-level panel that I use—they cannot separate platform and direct orders, which is precisely what my data permit. This explains my focus on offline substitution as opposed to heterogeneous effects of caps across restaurant types. As for the measurement of seesaw effects, my receipt-based panels measure total consumer fees across all platforms, permitting a more complete analysis of fee effects than that of Li and Wang (2025b), who collect information on delivery fees (one component of total platform fees) for a single platform. Last, my analysis builds upon that of Li and Wang (2025b) by considering the majority of caps that did not exempt chains (see Figure 1), by studying a longer time period (January 2020 to May 2021 versus their March to September 2020), and by studying effects on order size and restaurant adoption of platforms.

Beyond food delivery, evidence on one-sided price controls in two-sided markets comes mainly from payment cards, where caps on interchange fees led banks to raise consumer fees and cut rewards (Chang et al. 2005; Wang 2012; Evans et al. 2015; Manuszak and

Wozniak 2017; Kay et al. 2018); a related literature estimates how interchange fees affect card use and models the welfare effects of regulating them (Rysman 2007; Carbó-Valverde et al. 2016; Li et al. 2020; Huynh et al. 2022; Wang 2025). Outside payment cards, Ennis et al. (2023) find that removing hotels' price-parity obligations to booking platforms lowered direct-channel prices relative to platform prices, and Robles-Garcia (2022) studies the commissions that mortgage lenders pay brokers, including the effects of capping them. On the courier side of food delivery, An et al. (2025) find that Seattle's 2024 per-task minimum pay for delivery workers raised base pay but was offset by lower tips and fewer tasks, leaving drivers' monthly earnings unchanged. These studies show that regulating prices on one side of a platform may lead to offsetting adjustments elsewhere. None examines, though, how such responses interact with both network externalities and offline substitution in shaping the overall impacts of regulation. Doing so is the principal contribution of my article.

The paper also adds to a small empirical literature on food delivery platforms (Reshef 2023; Huang and Xie 2023; Natan 2025; Lu et al. 2021; Chen et al. 2022; Feldman et al. 2022; Fisher 2024; Li and Wang 2025a; Mayya and Li 2025; Myeong et al. 2026). This literature includes my article Sullivan (2026), which uses the same data studied here but analyzes a structural model of the food delivery industry rather than conducting DiD analysis.

2 Setting and data

2.1 Background

The major US food delivery platforms in 2020–2021 were DoorDash, Uber Eats, and Grubhub, with market shares of 59%, 26%, and 13% in the second quarter of 2021. Each platform earned revenue by charging consumers and restaurants fees on a per-transaction basis. The pricing structure of the industry can be schematically represented as

$$\text{Consumer bill} = p + c,$$

$$\text{Restaurant revenue} = (1 - r)p,$$

$$\text{Platform revenue} = rp + c.$$

Here, p is the total price of the restaurant items ordered in the transaction, c is the platform's consumer fee, and r is the platform's commission rate. Several particularities of these prices and fees warrant clarification. First, the price p is the value of the consumer's order before fees. As estimated on the Edison panel that I introduce in Section 2.3, this average order value was \$26.51 in January–May 2021 across the three major platforms. Restaurants, which actively choose whether to list on a given platform or not, are free to choose their menus and the prices of menu items on platforms.[3] Next, the fee c includes all consumer-side charges levied by the platform against consumers, including fees labelled delivery fees, service fees, and regulatory response fees on receipts. Platforms often introduced new fees in explicit response to commission caps; after Chicago introduced a cap, for instance, DoorDash added a new "Chicago Fee" to orders in that city. Also, c is net of any discounts offered by platforms. On average, the consumer fee net of discounts across the leading platforms in January–May 2021 was \$4.59, based on the Edison panel referenced above. Advertised commission rates r in places without caps generally did not vary across platforms or geography; Uber Eats and Grubhub advertised 30% commissions in 2021, and DoorDash's full-service tier charged 30% in April 2021. It is possible that some restaurants negotiated lower rates, although I do not observe the restaurant-platform contracts that would be required to ascertain this possibility.

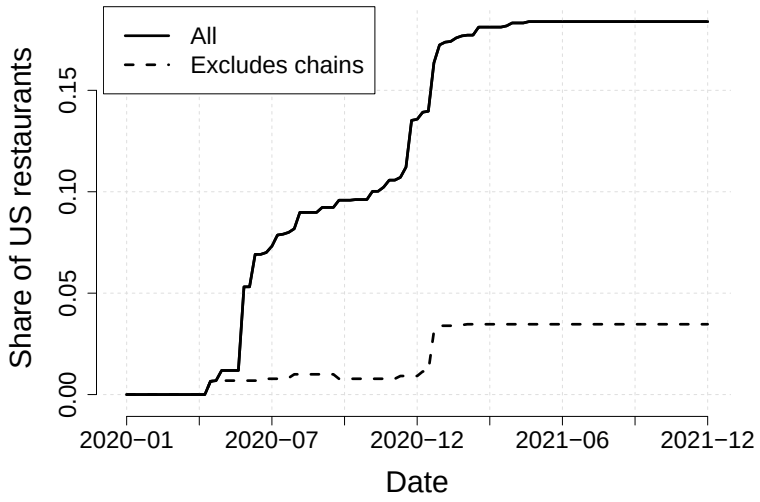
Another notable feature of the setting is multi-homing, the practice of a consumer or restaurant using multiple platforms. Multi-homing is common in food delivery among both restaurants and consumers. Additionally, consumers can bypass platforms by ordering directly from restaurants, whether on premises, for pickup, or for first-party delivery. Direct orders, for which restaurants do not pay commission, account for the large majority of restaurant orders (91% of orders and 81% of spending in January–May 2021, per the Numerator data discussed below).

2.2 Commission caps

Local governments began capping commissions in March 2020, at the outset of the US COVID-19 pandemic. Figure 1 plots the share of US restaurants located in a jurisdiction with a cap. The share rose in steps through 2020 as cities and counties adopted caps,

[3]Some platforms listed restaurants without consent early in the sample period; the practice has since declined and been outlawed in several jurisdictions (Mayya and Li 2025).

Figure 1: Share of US restaurants in jurisdictions with commission caps



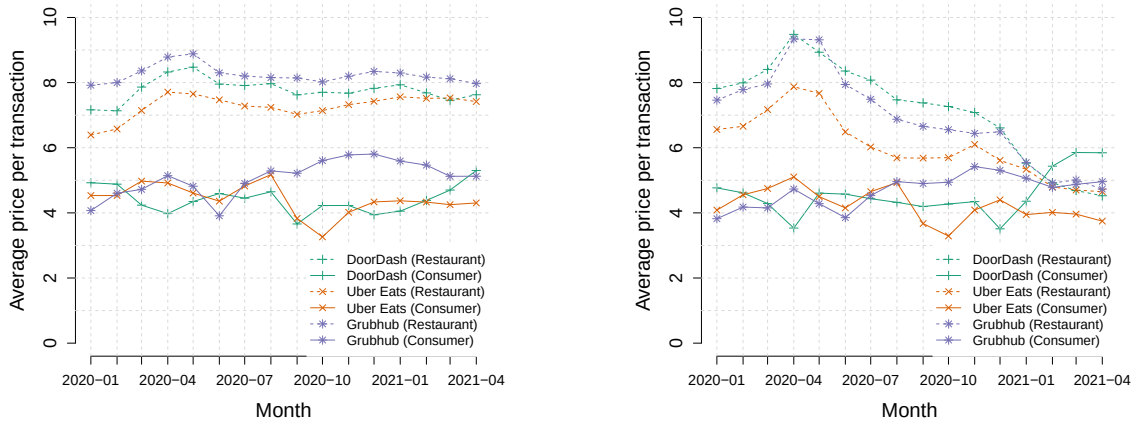
Notes: the solid line plots the share of US restaurants (from Data Axle) located in a jurisdiction with an active commission cap on each date. The dashed line plots the share located in a jurisdiction whose cap exempts chain restaurants. Cap dates are compiled from ordinances and news reports.

jumped in December 2020 when several large jurisdictions acted, and reached 18% by early 2021, where it remained through the end of the sample period. By June 2021, more than 70 local governments representing about 60 million people had enacted caps.

Of the caps introduced before 2022, 77% limited commissions to 15%, with the remainder setting limits between 10% and 20%. Most caps applied to all restaurants, but 19% exempted chains (the dashed line in Figure 1). Although I do not distinguish between caps that covered and exempted chains in my main analysis, I show in Online Appendix OA.5 that the estimated effects are robust to excluding from the sample places that adopted caps exempting chains. Although caps were often initially framed as temporary measures, several jurisdictions—including San Francisco, New York City, and Minneapolis—later made theirs permanent. The caps regulated commissions but neither platform consumer fees nor restaurant prices.

Platforms earned a significantly greater share of their fee revenue from restaurants than from consumers, except in places that enacted commission caps. For each of the three leading platforms, I compute average consumer fees and average commission revenue per order by month, separately for ZIPs with commission caps by May 2021 and those without caps by that date. Figure 2 plots the results. Where caps were never adopted, per-order commissions ran at \$7–9 and consumer fees at \$4–5 throughout the sample. Where caps

Figure 2: Platforms' average commissions and consumer fees per order, ZIPs with and without a cap



(a) ZIPs without a cap in May 2021

(b) ZIPs with a cap in May 2021

Notes: dashed lines are average per-order commissions, computed by multiplying the ZIP-level average basket subtotal in the Edison panel by 0.30 where no cap is in effect and by the cap level where one is, then averaging across ZIPs with order counts as weights. Solid lines are average per-order consumer fees from the same panel. Panel (a) averages over ZIPs without a cap in May 2021 and panel (b) over ZIPs with one.

were adopted, per-order commissions fell from \$8–9 in spring 2020 to \$4.50–5 by spring 2021 as the caps took effect. Consumer fees rose in both groups of ZIPs in early 2021, but by more where caps applied: DoorDash's average fee reached \$5.90 in capped ZIPs against \$5.30 in uncapped ZIPs.

Several factors predict whether a locality adopted a commission cap. To assess these factors, I regress an indicator for a ZIP being subject to a cap by June 2021 on ZIP characteristics, including the Democratic vote share in the 2016 US presidential election, population density as measured by the population within 5 miles of a ZIP, the share of the population in various age groups (under 20, 20–29, 30–39, 40–49, and 50–59, with over 60 being the omitted category), and the share of the population with high school diplomas, with college degrees, and with advanced degrees (for educational attainment, those without high school diplomas comprise the omitted category). Table 1 displays the results. The included features explain 20% of the variation in commission cap status, suggesting an important but limited role for the included regressors in predicting caps. Caps were more likely to have been imposed in populous, Democratic-voting, and highly educated places. In particular, a 10 percentage point increase in the Democratic vote

Table 1: Which places adopt commission caps?

Regressor	Estimate	SE
Democrat vote share (2016 pres elxn)	0.40	0.01
Population within 5 miles (millions)	0.40	0.01
Age group share: under 20	-0.09	0.02
Age group share: 20s	-0.01	0.02
Age group share: 30s	0.00	0.03
Age group share: 40s	-0.02	0.03
Age group share: 50s	-0.02	0.03
Share with HS diploma	0.03	0.02
Share with college degree	0.15	0.02
Share with advanced degree	0.33	0.03
R^2	0.20	
Mean dependent variable	0.11	

Notes: this table reports estimates from a ZIP-level linear regression of an indicator for a ZIP being subject to a commission cap by the end of June 2021 on ZIP characteristics: (i) the Democratic vote share in the 2016 presidential election in the ZIP's county (MIT Election Data and Science Lab 2018); (ii) the population within five miles of the ZIP, in millions; (iii) the shares of the population in age groups; and (iv) the shares of the population over 18 years of age in educational attainment groups.

share raises the probability of a cap by 4 percentage points, as does an additional million residents within five miles.

2.3 Data

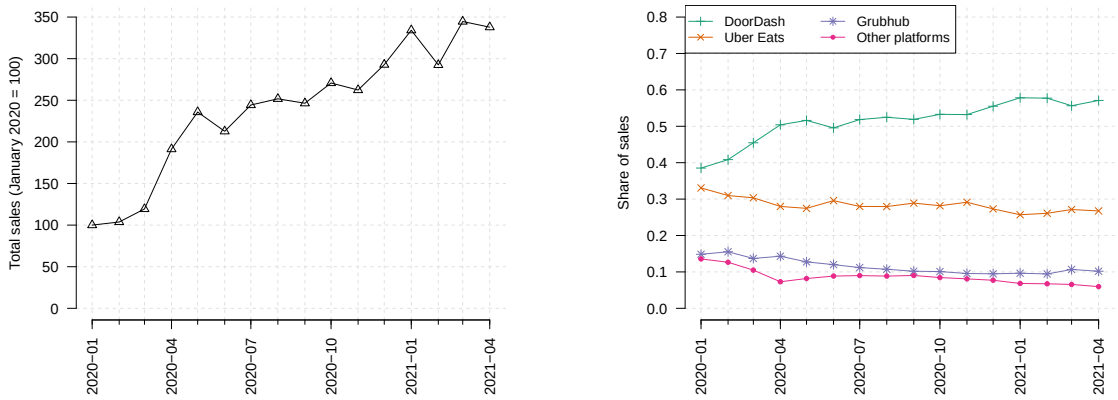
Platform fees and orders. The first dataset characterizing platform fees and orders comes from Edison, a data provider. This dataset is a platform/ZIP/month-level panel covering January 2020 to May 2021. It includes estimates of order volume, average basket size, and the components of average basket size built from a panel of email receipts capturing 2,516,994 platform orders over these 17 months, or about 148,000 per month. Edison collects these data through an email add-on that it operates, which automatically identifies and parses receipts in the inboxes of users who consent to the anonymized, aggregated use of their data. The components of basket size (i.e., the total amount paid by the consumer) reported include average fees, tips, and taxes as well as the average order value excluding them (i.e., the basket subtotal). To separate the components of the fee, I compute the service fee rate as the average service fee divided by the average subtotal, and the fixed fee as the average total fee less the average service fee. The panel covers DoorDash, Uber Eats, and Grubhub, as well as the smaller platforms Postmates and Seamless. Market shares computed from the Edison panel match those from an external panel of payment

card transactions (Sullivan 2026). Panel (a) of Figure 3 plots aggregate sales indexed to January 2020 whereas Panel (b) plots platform market shares by month over January 2020 to April 2021. The figure shows that food delivery sales increased precipitously at the outset of the COVID-19 pandemic and continued to increase even after the acute phase of the pandemic waned. Furthermore, DoorDash was the largest platform over the entire period, although its position strengthened over the period whereas those of the other platforms deteriorated.

Figure 3: Food delivery platform sales

(a) Total sales

(b) Market shares



Notes: this figure describes platform order volumes in the Edison panel. Sales are aggregate order values including fees, tips, and taxes. Panel (a) reports total sales across the four platforms, indexed to January 2020. Panel (b) reports each platform's share of the four platforms' combined sales. "Other platforms" includes Postmates and Seamless.

Consumer transactions. Numerator provides a disaggregated panel of consumer receipts collected through a mobile application that parses email receipts and accepts uploaded receipt photographs. Each record in the dataset provides information on the restaurant, the platform used (if any), the basket subtotal and total, and the name, category, and price of each item. In addition, the data include information on panelists, including various demographic variables and ZIP code of residence. The data cover orders placed through platforms and orders placed directly with restaurants, whether on premises, for pickup, or for delivery. I keep transactions by members of Numerator's core panel, whose demographic composition is designed to match the US adult population, and only panelist/months in which the panelist's email account was linked to Numerator's data-collection, to ensure broad coverage of transactions within the sample.

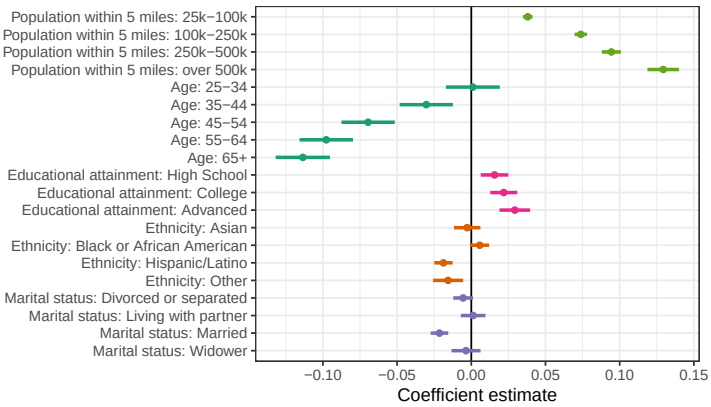
To estimate ZIP-level order volumes from the disaggregated Numerator panel, I count

each panelist's platform and direct orders in each month, average across panelists within a ZIP, and smooth the resulting ZIP/month means with a Lasso regression on ZIP, state, and month fixed effects and their interactions, choosing the penalty by ten-fold cross-validation. Multiplying fitted means by ZIP population gives estimated order volumes. The smoothing removes sampling noise from cells with few panelists and eliminates zero cells, which would otherwise preclude the log transformation. The fitted and raw means are highly correlated across ZIP/months: 0.984 for direct orders and 0.942 for platform orders. I use the same procedure for basket subtotals. In practice, I use relatively coarse three-digit rather than relatively fine-grained five-digit ZIPs here because the Numerator panel is thinner than the Edison panel.[4] Online Appendix OA.13 checks the sensitivity of the estimates to the Lasso procedure by estimating panelist-level regressions on the unsmoothed data, which yields broadly similar results.

Given that Numerator contains information on both ordering patterns and consumer demographics, it enables an assessment of which types of consumers use food delivery platforms and thus which demographic groups stand to be most affected by changes in platform fees. Figure 4 reports coefficients from a consumer/month-level linear regression whose outcome variable is an indicator for a consumer having placed at least one order on one of the four leading food delivery platforms in the month in question. The regressors include month fixed effects and indicators for the consumer belonging to demographic groups based on local population density, age, race/ethnicity, marital status, and educational attainment. Platform use rises steadily with local population density: a consumer with more than 500,000 people living within five miles is 13 percentage points more likely to order from a platform in a month than one with fewer than 25,000. Age matters only beyond 45: consumers aged 25–44 do not differ from those aged 24 or under, whereas a consumer aged 65 or over is 11 percentage points less likely to order from a platform. Platform use also rises with educational attainment, from 1.8 percentage points above consumers without a high school diploma for high school graduates to 3.3 percentage points for those with advanced degrees. Married consumers are 2.8 percentage points less likely to order from a platform than those who have never married; the other marital

[4]The median number of five-digit ZIPs belonging to a three-digit ZIP (or "ZIP3") is 36, with an interquartile range of 22–52. Furthermore, the median ZIP in the data has a population of 20,000 whereas the median three-digit ZIP has a population of 220,000. For three-digit ZIP analyses, I convert each county-level covariate to the three-digit ZIP level as a population-weighted average across the ZIPs in the three-digit ZIP, each assigned the value of its county.

Figure 4: Demographics of platform users



Notes: this figure plots coefficients and 95% confidence intervals from a linear regression of an indicator for a consumer placing at least one order on one of the four leading platforms in a month on month fixed effects and the consumer's demographics, using the Numerator static panel in 2021. The sample is restricted to panelists who linked an email account to Numeratorand to months in which the panelist placed at least one restaurant order (318,725 consumer/months; 48,487 consumers). The mean of the dependent variable is 8.6%. "Population within 5 miles" is the population within five miles of the consumer's ZIP. Omitted categories are fewer than 25,000 people within five miles, age 24 or under, no high school diploma, white, and never married. Coefficients on gender, employment status, household size, income group, presence of children, and Census region are included in the regression but not plotted. Standard errors are clustered by consumer.

statuses do not differ significantly from never married. Differences by race/ethnicity are generally small.

Restaurant listings. I obtain data on restaurant uptake of food delivery platforms from YipitData, another commercial data provider. The dataset that they provide records every US restaurant listed on each major platform in each month. A listing is a restaurant/platform pair; this means, e.g., a restaurant on DoorDash and Uber Eats contributes two listings in the dataset. I aggregate listings to the ZIP/month level and express them per million residents. Here, I use the three-digit ZIP concept as three-digit ZIPs are large enough to contain both the restaurants serving a local population and that population, which is important when studying per capita outcomes. To obtain information on restaurants that had not adopted platforms, I use the Data Axle dataset on the universe of US business locations. I construct a subset of this dataset including only restaurants. Note that the Data Axle dataset is annual; I use the 2020 and 2021 years of the data in my analysis. Over the sample period, 63% of US restaurants were listed on at least one platform, a share that rose from 57% in January 2020 to 68% in May 2021, and a listed restaurant appeared on 1.96 platforms on average, up from 1.70 to 2.05 over the same

months.[5]

Commission caps. I compile the start date, end date, level, and chain exemption of each cap from ordinances and news reports. The dataset includes 72 caps active in March 2021. I assign each ZIP the cap of its jurisdiction.

Other data sources. I obtain various other data sources for use in constructing covariates. These include the Oxford COVID-19 Government Response Tracker index of state policy stringency (Hallas et al. 2020); new COVID-19 cases per capita by county from the Johns Hopkins CSSE repository (Dong et al. 2020); and each county's Democratic vote share in the 2020 presidential election (MIT Election Data and Science Lab 2018), which allows places with different political leanings to respond differently to the same caseload. Furthermore, the ZIP-level population and demographic variables used in my analysis come from the 2014–2019 American Community Survey.

Summary statistics and panel balance. Table 2 reports means and standard deviations of variables used in the analysis. Although the listings panel is balanced and the Numerator panel nearly so, with 96% (98% when weighting by population) of three-digit-ZIP/month cells observed, the Edison panel is unbalanced. A ZIP/platform/month observation in the Edison data is present only when Edison observes at least one receipt for the platform in the ZIP that month, so that only 41% (49%) of possible cells are observed. When I aggregate orders across platforms to the ZIP/month level, this rate rises to 59% (74%). The preferred estimator discussed in Section 3 uses all observations of an unbalanced panel. One concern that arises in using an unbalanced panel in my setting is that observations are not randomly selected into being missing, but are instead missing by virtue of limited sales. In Sections 4.1 and 4.3 and Online Appendix OA.3, however, I show that the results are robust to approaches that reduce or eliminate the lack of balance, including aggregating to the three-digit ZIP level, restricting to units observed in most months, and imputing zero orders to unobserved cells. These checks suggest that selection into

[5]The share is the number of restaurants with a listing on DoorDash, Uber Eats, Grubhub, or Postmates in the YipitData listings, matched to Data Axle restaurant locations by name and address, divided by the number of Data Axle restaurant locations, summed over all three-digit ZIPs and months. The number of platforms per listed restaurant is the number of listings divided by the number of unique restaurants listed, where listings of the same restaurant on different platforms are matched by name and location.

Table 2: Summary statistics

	Mean	SD	Obs.
Edison (ZIP/platform/month)			
Total consumer fee before discounts ($)	5.59	3.33	157,248
Service fee ($)	3.37	1.84	157,248
Delivery fee ($)	2.54	1.53	157,248
Discount ($)	1.38	3.95	157,248
Total consumer fee net of discounts ($)	4.21	5.02	157,248
Basket subtotal ($)	26.82	14.76	157,248
Orders (raw)	15.8	18.7	157,248
Orders (scaled)	9,123	10,455	157,248
Numerator (three-digit-ZIP/month)			
Platform orders (raw)	27.7	33.2	11,456
Direct orders (raw)	385.7	329.1	11,456
Panelists	162.6	124.6	11,456
Listings (three-digit ZIP/month)			
Listings	2,968	3,681	14,178
Listings per million residents	2,875	1,861	14,178
Unique restaurants per million residents	1,472	813	14,178
Covariates (ZIP/month)			
Commission cap of 15% or lower	0.07	0.26	102,380
Population (thousands)	39.9	18.7	102,380
Stringency index	44.8	19.4	102,380
New COVID-19 cases per 1,000	6.2	7.3	102,380
Democratic vote share, 2020	0.56	0.14	102,380

Notes: this table reports population-weighted means and standard deviations. The sample excludes Puerto Rico, ZIPs outside a CBSA, and ZIPs ever subject to a cap above 15%. The statistics displayed in the "Edison (ZIP/platform/month)" panel pool observations from DoorDash, Uber Eats, and Grubhub. "Orders (raw)" are the numbers of orders observed in the receipts of each provider's panelists. For Edison, this includes orders on the platform by panelists residing in the ZIP in the month. For Numerator, it includes platform and direct orders by panelists residing in the three-digit ZIP in the month, with "Panelists" describing the number of such panelists. "Orders (scaled)" are Edison's estimates of the total number of orders placed on the platform in the ZIP/month based on raw order counts. "Commission cap of 15% or lower" is the treatment indicator.

the sample does not drive the results.

3 Empirical strategy

I estimate the effects of commission caps on various outcomes of interest using a DiD approach. To build intuition, I provide below estimating equations for the two-way fixed effects (TWFE) estimator. The preferred estimates, though, derive from alternative estimators that are free of problems suffered by TWFE when treatment effects are heterogeneous and treatment is staggered.

Consider an outcome y_{fzt} of platform f in ZIP z and month t (e.g., the log of DoorDash's

average consumer fee). The estimating equation of the TWFE estimator is

$$\underbrace{y_{fzt}}_{\text{Outcome}} = \underbrace{\psi_{fz} + \phi_{ft}}_{\substack{\text{ZIP and month} \\ \text{fixed effects}}} + \underbrace{\delta_f x_{zt}}_{\text{Treatment}} + \underbrace{w'_{zt}\beta}_{\text{Controls}} + \epsilon_{fzt}, \tag{1}$$

where ψ_{fz} are platform/ZIP fixed effects, ϕ_{ft} are platform/month fixed effects, x_{zt} indicates that ZIP z is subject to a commission cap of 15% or lower in month t, and w_{zt} are the COVID-19 covariates. The coefficient δ_f is the effect of a cap on platform f's outcome. For outcomes that are not platform-specific, such as Numerator order counts or restaurant listings, the platform subscript is dropped. I focus on caps of 15% (the most common level) or lower, dropping ZIPs with higher caps from the main sample. To ensure robustness to this approach, I additionally estimate specifications with a continuous treatment equal to the commission rate in force (30% absent a cap) and specifications in which any cap counts as treatment.

A dynamic variant of the estimating equation (1) replaces the single treatment indicator with indicators for months relative to a cap's introduction,

$$y_{fzt} = \psi_{fz} + \phi_{ft} + \sum_{\tau=-\bar{\tau}}^{\bar{\tau}} \delta_{f\tau}\, x_{z,t-\tau} + \delta_f^{+} \sum_{\tau>\bar{\tau}} x_{z,t-\tau} + \delta_f^{-} \sum_{\tau<-\bar{\tau}} x_{z,t-\tau} + w'_{zt}\beta + \epsilon_{fzt}, \tag{2}$$

where $x_{z,t-\tau}$ equals one if a cap was first imposed in ZIP z in month $t-\tau$, and $\bar{\tau} = 7$.

To summarize the fee response across platforms in a single estimate, I also estimate (1) and (2) on the stacked platform/ZIP/month panel of the three major platforms, retaining the platform/ZIP and platform/month fixed effects but restricting the effect of a cap to be common across platforms ($\delta_f = \delta$ and $\delta_{f\tau} = \delta_\tau$). For the alternative estimators described below, the cross-sectional unit is defined as a platform/ZIP pair; the Callaway and Sant'Anna (2021) estimator additionally includes platform indicators among its covariates, so that each treated pair is compared with control pairs on the same platform.

Estimators. Ordinary least squares applied to (1) and (2) is the TWFE estimator. TWFE is known to be biased in cases with heterogeneous treatment effects and staggered treatment (de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). I therefore report estimates from other estimators. My preferred estimator is the interaction-

weighted (IW) estimator of Sun and Abraham (2021). Unlike the Callaway and Sant'Anna (2021) estimator, which conditions only on time-invariant covariates, it controls for time-varying COVID-19 conditions that may have coincided with cap adoption, and it uses every observation in an unbalanced panel rather than only units observed in both periods of each comparison. To establish robustness to this choice of estimator, I additionally compute the Callaway and Sant'Anna (2021)/Sant'Anna and Zhao (2020) estimator, obtaining results both from the versions of the estimator using never-treated units and using not-yet-treated units as controls.

Another concern in DiD analysis is the endogeneity of treatment, which is often suggested by the presence of pre-trends, i.e., non-zero estimated effects of treatment before the date of treatment. The event-study estimates reported below allow me to assess such pre-trends.

Data. I merge the datasets described in Section 2.3 to obtain the panel on which I conduct the DiD analysis. I keep only ZIPs that belong to a Core-Based Statistical Area (CBSA). Commission caps were introduced only in urban areas, so urban areas also serve as the natural control group; Online Appendix OA.4 shows that this sample restriction does not end up being important. I also exclude the few ZIPs ever subject to a cap above 15%, which are neither cleanly treated under the definition of treatment below nor cleanly untreated. The analysis of platform consumer fees is primarily based on the average consumer fee measure in the platform/ZIP/month level Edison panel; the fee measure considered here pools together the various consumer-side fees charged by delivery platforms. In analyzing order volumes, I use both order estimates from each of Edison and the Numerator panel, finding that these sales measures yield similar estimated effects of commission caps on platform order volumes. Given that Numerator but not Edison includes data on restaurants' off-platform sales volumes, I use measures of off-platform sales derived solely from the Numerator data. In estimating effects of commission caps on restaurant uptake of platforms, I use the YipitData dataset to compute the number of restaurant listings on platforms by ZIP, which is the primary outcome variable that I consider. For robustness, I also compute the share of restaurants belonging to platforms and use this as an outcome. Computing this share requires combining the YipitData data on restaurants listed on platforms with the Data Axle dataset covering all US restaurants.

Given that the Data Axle dataset is at an annual frequency, I estimate an annual DiD specification when the share of restaurants on platforms is the outcome measure; Online Appendix OA.7 describes this specification.

Identification. The primary identification assumption underlying each estimator deployed is that, conditional on controls w_{zt}, the outcome variable y_{fzt} in places with commission caps would have followed the same trend as those in places without commission caps had the commission caps not been introduced. The main threat to this parallel-trends assumption is the COVID-19 pandemic; places in which COVID-19 had especially pernicious effects may have been especially likely to introduce commission caps with the goal of protecting their local restaurants from the effects of COVID-19. This would lead to a violation of the parallel-trends assumption if COVID-19 severity also directly affected the outcome y_{fzt}, which is likely for ordering-based outcomes. To address this threat, I include COVID-19 cases in levels and in interaction with the Democratic vote share in the covariates vector w_{zt}. I include the latter interaction to reflect that more liberal areas may have been more likely to respond to COVID-19 severity by enacting commission caps, a possibility suggested by the results of Table 1. Furthermore, I control for the stringency of local COVID-19 policy using the index of Hallas et al. (2020) that measures such stringency. Also, if the parallel-trends assumption were violated, it would be likely that local outcomes would follow a distinct trend in treated areas than in non-treated areas, i.e., that there would be pre-trends. My event study estimates allow me to test for pre-trends, which I generally find to be small and statistically insignificant.

Implementation details. First, I weight ZIPs by population throughout the analysis so that the estimates describe the effect of caps on the average consumer's market rather than on the average ZIP. Second, the IW and Callaway and Sant'Anna (2021) estimators yield effects by month since treatment. The single effect that I report for each averages these dynamic effects over post-treatment months, weighting each month by its share of treated post-treatment observations. Third, standard errors are clustered by ZIP in the Edison analyses and by three-digit ZIP in the Numerator and listings analyses, for all estimators. Fourth, in the analyses at the three-digit ZIP level, a three-digit ZIP is treated when any of its ZIPs is subject to a cap of 15% or lower. Online Appendix OA.1 provides further implementation details.

4 Results

4.1 Consumer fees

The first outcomes y_{fzt} that I consider are log average platform consumer fees excluding discounts. I exclude discounts here because, in about 8% of ZIP/platform/months, average discounts exceed average fees so that the log of the fee net of discounts is undefined. Furthermore, as discussed below, I find that platforms did not adjust their discounts in response to caps. To facilitate use of log fees as the outcome, I exclude observations with average fees below one cent. However, I additionally estimate effects on fees in levels without dropping observations with low fees, and find results similar to those from the log fees specification.

Table 3 reports the estimated effects across distinct platforms and estimators. The preferred IW estimate for the total across-platform effect is 0.111 log points (12%) whereas the other estimators yield an effect of 0.092–0.109 (10–12%); each of these estimates is statistically significant at conventional levels. Furthermore, the estimated effects for each individual platform are positive and statistically significant under each estimator.

Table 3: Effects on consumer fees

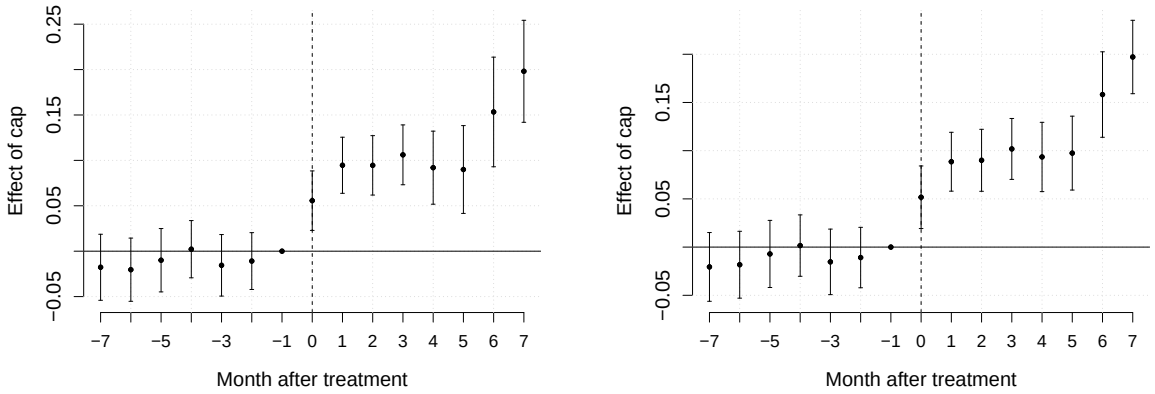
Platform	IW	CS (not yet)	CS (never)	TWFE
All platforms	0.111	0.094	0.092	0.109
	(0.017)	(0.018)	(0.018)	(0.008)
DoorDash	0.119	0.105	0.104	0.136
	(0.023)	(0.024)	(0.024)	(0.011)
Uber Eats	0.098	0.079	0.074	0.063
	(0.026)	(0.031)	(0.033)	(0.013)
Grubhub	0.108	0.096	0.098	0.123
	(0.034)	(0.035)	(0.036)	(0.019)

Notes: this table reports estimates of the effect of a commission cap of 15% or lower on the log of a platform's average consumer fee before discounts in a ZIP/month. ZIP/months with average fees below one cent are excluded. "All platforms" reports a single effect estimated on the stacked platform/ZIP/month panel with platform/ZIP and platform/month fixed effects. "TWFE" is the two-way fixed effects estimate of δ_f in (1). "IW" is the interaction-weighted estimator of Sun and Abraham (2021); "CS" is the estimator of Callaway and Sant'Anna (2021) with not-yet-treated or never-treated units as controls. For the IW and CS estimators, the table reports averages of dynamic effects over post-treatment periods (Online Appendix OA.1). All specifications control for the COVID-19 covariates described in Section 2.3, and ZIPs are weighted by population. The sample is the Edison platform/ZIP/month panel, January 2020 to May 2021, excluding ZIPs with caps above 15%. Standard errors clustered by ZIP in parentheses.

Figure 5 plots IW and TWFE estimates of the dynamic effects in (2) for all platforms

Figure 5: Dynamic effects of commission caps on consumer fees, all platforms

(a) IW (Sun and Abraham 2021) (b) TWFE



Notes: this figure plots the "All platforms" estimates of the dynamic effects in (2), by month relative to the introduction of a commission cap of 15% or lower (month −1 is the omitted category). The estimates are obtained from the stacked platform/ZIP/month panel of DoorDash, Uber Eats, and Grubhub, in which the outcome is a platform's log average consumer fee before discounts in a ZIP/month, with platform/ZIP and platform/month fixed effects and dynamic effects common to the three platforms (Section 3). Panel (a) reports interaction-weighted estimates and panel (b) reports two-way fixed effects estimates. Dots are point estimates and bars are 95% confidence intervals based on standard errors clustered by ZIP.

together. Under both estimators, the pre-period coefficients are close to zero and individually insignificant, indicating that fees in places that later adopted caps followed a trend indistinguishable from that in places that did not. Fees rise in the month a cap takes effect and continue to rise thereafter. In the IW estimates of Panel (a), the effect is 0.056 log points (6%) in the adoption month and 0.198 log points (22%) by month 7. The TWFE results of Panel (b) are broadly similar. Online Appendix Figure OA.6 reports TWFE and IW event studies for each platform. Each shows a positive effect that grows with time since adoption.

The estimates permit a calculation of the share of commission revenue losses that platforms recover through fee increases. In the months before a cap took effect, the order-weighted average basket subtotal in ZIPs later subject to a cap was $27.50 and the average consumer fee before discounts was $5.63.[6] Reducing the commission rate from 30% to

[6]Both averages are taken over the pre-cap observations of the stacked platform/ZIP/month panel underlying the "All platforms" estimates of Table 3: the DoorDash, Uber Eats, and Grubhub observations of ZIPs whose commission rate was later capped at 15% or below, dated before the ZIP's first capped month, with a fee before discounts of at least one cent. The 15,671 observations cover 1,361 ZIPs and are weighted by orders, so that each average is a per-order average. The basket subtotal excludes fees, tips, and taxes.

15% lowers the commission charge on such an order from \$8.25 to \$4.13, a loss to the platform of \$4.13 per order. Seven months after a cap took effect, the IW estimates imply that consumer fees were 22% higher, or \$6.87 per order, an increase of \$1.23. Fee increases therefore recovered about 30% of the commission lost on each order. Using instead the IW estimate averaged over all post-treatment months, which implies a 12% increase in consumer fees, fee increases recover only about 16% of the commission lost per order. Thus, this calculation suggests that consumer fee increases recovered at most about 30% of platforms' per-transaction revenue losses from commission caps.

Consumer fee responses and market power. The fact that platforms recover less than a third of lost commission revenues via consumer fee increases suggests the presence of platform market power. When platforms earn no markups over cost due to competitive pressure, they must make up every lost dollar of restaurant commission revenue in consumer fee revenue lest their profits become negative. By contrast, highly incomplete consumer fee responses suggest that platforms earned markups before caps large enough to absorb most of the lost commission revenue.

Combining the back-of-the-envelope calculation above with the assumption that average platform margins were non-negative after the imposition of commission caps, I construct a lower bound on platform markups. Let $\bar{r} = 0.30$ be the pre-cap commission rate, let $\bar{c} = 5.63$ be the pre-cap average consumer fee, let $\delta_r = 0.5$ be the proportional change in the commission rate due to a 15% commission cap, and let δ_c be the resulting proportional change in the consumer fee. Furthermore, let $\bar{p} = 27.50$ be the average order size before fees. Later, in Section 4.3, I provide evidence that $\bar{p}$ did not respond to commission caps; hence, I assume here that it is invariant to a commission cap. Then, the platform's margin after the commission cap is

$$\bar{r} \times \bar{p} \times \delta_r + \bar{c} \times (1 + \delta_c) - \bar{mc} \geq 0, \tag{3}$$

where $\bar{mc}$ is the platform's marginal cost, which I also assume to be invariant to the commission cap. The inequality reflects my assumption that platform margins remained non-negative after the cap's imposition. Substituting in values of the non-cost quantities on the left-hand side of (3), we obtain an upper bound on costs $\bar{mc} \leq 10.99$.[7] This yields

[7]This upper bound is likely not far from actual marginal costs. Sullivan (2026) estimates a mean

a lower bound on platforms' pre-cap margins:

$$\bar{r} \times \bar{p} + \bar{c} - \bar{mc} \geq 2.89 \quad \Rightarrow \quad \text{Lerner Index} = \frac{\bar{r} \times \bar{p} + \bar{c} - \bar{mc}}{\bar{r} \times \bar{p} + \bar{c}} \geq 0.21, \tag{4}$$

so that the markup is bounded below by \$2.89 and the Lerner ratio of markup over revenue is bounded below by 21%.

I conduct a variant of the DiD analysis described by Table 3 to test the hypothesis that limited fee responses reflect market power. In this variant, I include an interaction between the treatment indicator and a proxy for the platform's market power in the metropolitan area (i.e., CBSA): the log of the fixed per-order fee that it charged there before caps.[8] Consumer fees are a suitable proxy for platform markups only if heterogeneity in costs and order sizes across regions is limited. Indeed, when costs and subtotals are roughly constant across regions and with commission rates fixed at 30%, a higher fee indicates a larger markup. I expect the interaction coefficient to be negative. The idea is that platforms earning higher markups have greater scope for markup adjustment, and require smaller consumer fee responses to preserve positive margins. Online Appendix Table OA.3 reports the results. Pooling the three platforms, with the effect of a cap allowed to differ by platform and a common interaction, I estimate a negative and significant interaction of -0.069 (standard error 0.013). For scale, a doubling of the pre-cap fixed fee corresponds to a fee response smaller by 0.048 log points, about half of the average effect in Table 3. I additionally find negative interactions for each individual platform.

A platform's pre-cap market share in the metro gives a supporting result. Interacting the treatment indicator with the platform's share of platform orders in the CBSA over January–February 2020, before any cap, the pooled interaction is -0.091 (standard error 0.037): a platform whose pre-cap market share was 10 percentage points higher raised its fees by about 0.009 log points less. See Online Appendix OA.11 for details.

marginal cost of \$9.42 per order across DoorDash, Uber Eats, and Grubhub; Ahuja et al. (2021) report a marginal cost of \$8.20 per order in a 2021 analysis of US food delivery; and DoorDash's website stated in September 2022 that its base pay to couriers, the main component of marginal cost, ranges from \$2 to \$10 or more per delivery.

[8]The pre-cap fixed fee is the platform's order-weighted mean fixed fee (the consumer fee before discounts less the service fee) in the CBSA over January–June 2020, computed from ZIP/months not yet subject to a cap in the Edison panel.

Robustness. To establish the robustness of my findings, I estimate alternative DiD specifications. First, Table 4 reports TWFE estimates with fees in levels (as opposed to logs) as the outcome variable y_{fzt} and with a continuous treatment variable equal to the commission rate in force. I use TWFE for each specification of Table 4 given that it straightforwardly accommodates continuous treatment and yielded estimates similar to those from robust estimators for the baseline estimates of Table 3. In levels, a cap raised fees before discounts per order by \$0.64. Given that the mean consumer fee before discounts in capped ZIPs was \$5.63, the 0.109 log point (11.5%) TWFE estimate of Table 3 implies a similar increase of \$0.65. This is in line with the \$0.42 increase in delivery fees that Li and Wang (2025b) find on one platform over March–September 2020. With the continuous treatment, a one percentage point reduction in the commission rate raised DoorDash's fee by \$0.054 per order, or 0.92% of the fee; a cut from 30% to 15% therefore implies an increase of \$0.81, which is again similar to the effect implied by the binary-treatment estimate.

Online Appendix OA.2 reports fee estimates under additional variations of the DiD specification. These variations involve restricting the sample to July 2020 onward, after dining room closures were lifted, and to 2021 alone, and counting any cap as treatment; Online Appendix OA.5 additionally reports estimates dropping jurisdictions whose caps exempt chains. The estimates are similar to those presented in Table 3, with effects for all platforms ranging from 0.031 to 0.113 log points under the IW estimator, 0.064–0.138 under the Callaway and Sant'Anna (2021) estimators, and 0.072–0.105 log points under the TWFE estimator, all statistically significant at conventional levels. The results for individual platforms also remain positive and typically significant.

I also estimate responses of individual components of food delivery platforms' consumer fees; these components include various fixed (i.e., invariant to the order size) fees, service fees that are proportional to the order value, and discounts. To assess which element of fees responded to commission caps, I repeat the DiD analysis described above but with each individual type of fee as the outcome measure. Pooling the three platforms, the fixed fee rose by 0.094 log points and the service fee rate by 0.104 log points, whereas discounts showed no statistically significant change. These results, which appear in Online Appendix Table OA.21, suggest that platforms raised both fixed fees and service fees in

Table 4: Effects on consumer fees (robustness)

Specification	All platforms	DoorDash	Uber Eats	Grubhub
Level fee, discrete treatment	0.64	0.80	0.40	0.66
	(0.06)	(0.08)	(0.10)	(0.11)
Level fee, continuous treatment (rate)	-4.23	-5.38	-2.69	-4.29
	(0.37)	(0.53)	(0.68)	(0.74)
Log fee, continuous treatment (rate)	-0.73	-0.92	-0.42	-0.80
	(0.05)	(0.07)	(0.08)	(0.13)
Log fee, continuous treatment (log rate)	-0.16	-0.20	-0.09	-0.17
	(0.01)	(0.02)	(0.02)	(0.03)

Notes: this table reports TWFE estimates of δ_f in (1) on the Edison panel with the controls, weights, and sample of Table 3; "All platforms" is estimated on the stacked platform/ZIP/month panel (Section 3). "Level fee" specifications use the fee before discounts in dollars per order as the outcome, including ZIP/months with zero or negative fees. "Continuous treatment (rate)" specifications replace the cap indicator with the commission rate in force in the ZIP, equal to the cap level where a cap applies and to 0.30 otherwise; "(log rate)" uses its log. Standard errors clustered by ZIP in parentheses.

response to commission caps, but did not reduce their discounts.

As noted in Section 2.3, the Edison fee panel is unbalanced. To assess robustness to balance, I estimate effects on fees on panels that reduce or eliminate imbalance. These include the ZIP/platform panel restricted to units observed in at least 12 of the 17 months, and a panel aggregated to the three-digit ZIP/platform/month level, for which only 8% of population-weighted cells are missing. Across these panels the TWFE estimate is 0.098–0.103 against 0.109 here, and the IW and Callaway and Sant'Anna (2021) estimates are 0.073–0.099 against 0.092–0.111. Reducing the lack of balance therefore leaves the estimated fee effect essentially unchanged.

The results are similarly robust to dropping places that adopted commission caps exempting chain restaurants. In Online Appendix OA.5, I show that the total fee effect estimated using the IW estimator is 0.113 log points when such places are dropped (compared to 0.111 when they are included), and that the results from other estimators and for specific platforms are also robust.

4.2 Restaurant uptake of platforms

In the preceding section, I established a seesaw effect whereby commission caps led platforms to raise their consumer fees. These fee increases stand to hurt consumers and reduce ordering on platforms. However, network externalities suggest a countervailing

effect. Reducing commissions raises restaurant margins on platforms and thus makes platform membership more appealing to restaurants. One effect of commission caps may thus be to increase restaurant uptake of platforms. Due to these network externalities, expanded restaurant uptake of platforms makes platforms more enticing to consumers, possibly offsetting or even reversing the negative impacts of fee hikes.

I empirically assess the possibility that commission caps expanded restaurant uptake of platforms using DiD analysis similar to that presented in the preceding section. The first outcome measure that I consider is the number of restaurant listings per million residents in a three-digit ZIP.[9] This listings count covers the three major platforms as well as Postmates, the fourth largest platform in the sample period.

Commission caps could increase restaurant uptake of platforms via both extensive margin effects, wherein restaurants previously not listed on platforms join one, and intensive margin effects wherein restaurants previously listed on platforms join additional platforms. To disentangle these effects, I also use as an outcome the number of unique restaurants listed on at least one platform per million residents.[10] Results for this outcome characterize the extensive margin effects alone.

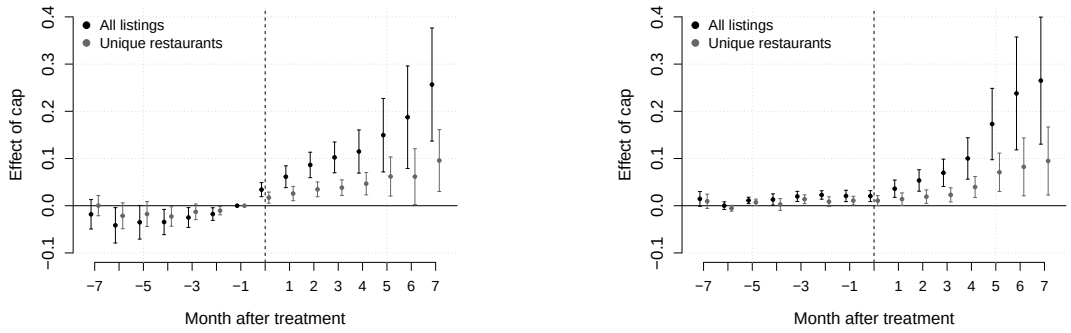
Figure 6 plots the dynamic estimates for both outcomes under the IW estimator and the Callaway and Sant'Anna (2021) estimator with not-yet-treated controls; Online Appendix Figure OA.5 reports the other estimators. So that the effects can be read as proportional changes, I scale the estimated coefficients by the population-weighted mean of the dependent variable in April 2020 (2,537 listings and 1,397 unique restaurants per million). Under the IW estimator, listings rise steadily after a cap takes effect, from 3% in the month the cap takes effect to 26% seven months later. The effects for the number of unique restaurants listed are 2% at month one and 10% at month seven. Some of the pre-period coefficients are significant, although they are small in magnitude. Relative to fees, as analyzed in Section 4.1, listings take longer to respond to commission caps. This likely reflects that it is straightforward for platforms to programmatically change

[9] I use per capita outcomes rather than log outcomes because observations with no listings are common for some platforms. In the estimation sample of 14,178 three-digit-ZIP/months, the share with zero listings is 3.8% for total listings and for unique restaurants listed, 10.9% for DoorDash, 5.7% for Grubhub, 28.0% for Uber Eats, and 21.9% for Postmates.

[10] I identify the same restaurant across platforms by matching listings on restaurant names and locations.

Figure 6: Dynamic effects of commission caps on restaurant listings

(a) IW (Sun and Abraham 2021) (b) Callaway and Sant'Anna (2021)



Notes: this figure plots interaction-weighted (Sun and Abraham 2021; panel a) and Callaway and Sant'Anna (2021) (with not-yet-treated units as controls; panel b) estimates of the dynamic effects of a commission cap of 15% or lower on the number of restaurant listings on food delivery platforms per million residents in a three-digit ZIP (black) and on the number of unique restaurants listed on at least one platform per million residents (grey), each divided by the population-weighted mean of the outcome in April 2020 (2,537 and 1,397). A listing is a restaurant/platform pair; listings of the same restaurant on different platforms are matched by name and location. Month −1 is the omitted category in panel (a); the Callaway and Sant'Anna (2021) pre-period estimates in panel (b) compare each month with the preceding month. Bars are 95% pointwise confidence intervals based on standard errors clustered by three-digit ZIP.

their fees immediately, whereas restaurant adoption of a platform may involve managerial decision-making processes, contracting with platforms, setting up menus and point-of-sale systems for platforms, and training staff to interface with platforms, each of which takes time.

Table 5 reports overall effects by platform and estimator. Across estimators, caps raised total listings per capita by 11.5–13.6% and the number of unique restaurants listed per capita by 4.4–6.0%. Under the IW estimator, these correspond to about 330 additional listings and 73 additional unique restaurants per million residents. If each newly listed restaurant joined a single platform, restaurants joining their first platform account for about a fifth of the additional listings; the remainder came from restaurants already on a platform joining others. The increase in listings was concentrated on the smaller platforms: listings rose by 17–19% on Uber Eats, 16–19% on Grubhub, and 24–28% on Postmates, but by only 1.4–3.2% on DoorDash. Given that DoorDash had the largest restaurant network before commission caps, it had relatively little room for further expansion at the time caps were introduced.

Table 5: Effects of commission caps on restaurant listings per capita

Outcome	IW	CS (not yet)	CS (never)	TWFE
Total listings	0.130	0.115	0.116	0.136
	(0.025)	(0.025)	(0.026)	(0.025)
Number of unique restaurants listed	0.052	0.044	0.044	0.060
	(0.014)	(0.014)	(0.014)	(0.014)
DoorDash listings	0.021	0.015	0.014	0.032
	(0.015)	(0.014)	(0.014)	(0.014)
Uber Eats listings	0.191	0.173	0.176	0.188
	(0.032)	(0.032)	(0.032)	(0.031)
Grubhub listings	0.181	0.160	0.163	0.186
	(0.030)	(0.030)	(0.030)	(0.029)
Postmates listings	0.271	0.239	0.242	0.276
	(0.057)	(0.059)	(0.059)	(0.061)

Notes: this table reports estimates of the effect of a commission cap of 15% or lower on the number of restaurant listings per million residents in a three-digit ZIP/month, in total and by platform, and on the number of unique restaurants listed on at least one platform per million residents, each divided by the population-weighted mean of the outcome in April 2020 so that entries are proportional effects. These means were 2,537 (total), 1,397 (unique restaurants), 1,030 (DoorDash), 639 (Uber Eats), 547 (Grubhub), and 322 (Postmates). Each column is an estimator; see the notes to Table 3. The sample period is January 2020 to May 2021. Areas with caps above 15% are excluded from the analysis. Standard errors clustered by three-digit ZIP appear in parentheses.

Robustness. As a check on these estimates, I separate the two margins with a different design and data source. Combining the YipitData listings with the Data Axle census of restaurants, which includes restaurants that have not joined any platform, I estimate a two-period DiD comparing January 2020 with January 2021 on a ZIP-level panel, as well as a cross-sectional comparison of capped and uncapped ZIPs within the same metro area in May 2021; Online Appendix OA.7 describes both designs. In the two-period design, caps raised the share of a ZIP's restaurants on at least one platform by 3.9 percentage points and the number of platforms per restaurant by 0.077. The within-metro comparison yields 4.0 percentage points and 0.124. Consistent with the estimates from a monthly panel as presented above, both margins responded: caps brought restaurants onto platforms for the first time and led restaurants already on a platform to join others. The two designs differ on the split between the margins, however. If each new adopter joined one platform, the 3.9 percentage point rise in the share of restaurants on a platform accounts for about half of the 0.077 rise in platforms per restaurant in the two-period design, against the fifth of new listings attributed to first-time adopters by the monthly estimates above.

Dropping localities that adopted commission caps exempting chain restaurants leaves the results substantially unchanged. In Online Appendix OA.5, I show that the effect on total listings per capita estimated using the IW estimator is 11.6% when such places are dropped compared to 13.0% when they are included. The estimated effects on unique restaurants listed (4.2% against 5.2%) and from other estimators (11.1–15.2% against 11.5–13.6% for total listings) are similarly close.

Discussion. The results above indicate that commission caps, which directly raise restaurant profit margins on platform sales, led more restaurants to join platforms. This response benefits consumers on account of network externalities, as explained above. It may, however, offset the benefit that restaurants enjoy from commission caps. Restaurant entry onto platforms stands to reduce restaurant profitability on account of fixed costs of platform membership and offline business stealing. It may be costly for restaurants to join platforms due to costs of hiring and training staff to interface with platforms, of adopting the technology required to efficiently take platform orders, and of maintaining physical space for couriers to pick up orders. Sullivan (2026) estimates restaurants' fixed costs of joining a platform in a structural model of the food delivery sector, finding these costs to be substantial at about $900/month. The other force by which restaurant entry onto platforms reduces restaurant profitability is offline business stealing: when restaurants join platforms, they may lead consumers who would otherwise order directly from a restaurant to instead order using the platform. I assess the extent of this response in Section 4.3.

4.3 Ordering patterns

The preceding sections established that commission caps prompted both increases in platform consumer fees and in restaurant uptake of platforms. The former response makes platforms less appealing to consumers whereas the latter response makes them more appealing. The net effect on platform order volumes is therefore ambiguous.

Likewise ambiguous is the effect on restaurant revenue from online sales: although restaurants pay less in commission due to commission caps, they may lose sales due to platforms' fee increases. However, the presence of the direct, off-platform ordering channel implies the possibility for consumer fee increases to benefit restaurants overall. If platform con-

sumer fee hikes induce consumers to substitute from platform to off-platform ordering, restaurants may benefit given that off-platform orders entail no costly commissions.

To assess impacts on ordering behaviour, I estimate effects of commission caps on various ordering outcomes. I do so both using sales in the Edison dataset and those from the Numerator data, which additionally contain information on off-platform ordering. The main outcome measures considered are the log platform order counts and log direct order counts in a ZIP/month observation. In addition, I estimate effects on total restaurant spending (excluding platform fees and aggregating across platform and direct orders) to assess the overall effect of commission caps on restaurant revenues.

Table 6 reports the results. Commission caps reduced platform orders in the Edison panel by 0.088 log points (8.4%) in the preferred IW specification and by 0.067–0.089 under the other estimators, all significant at the 5% level. Using the Numerator panel yields estimates of a similar magnitude, in the range of 0.050–0.086 log points. Online Appendix Table OA.23 reports estimates for each platform, which broadly corroborate the combined estimates for total platform orders. These results suggest that the consumer fee increases prompted by commission caps reduced platform ordering despite concurrent increases in restaurant platform adoption.

I next assess the extent to which increases in direct-from-restaurant ordering offset reductions in platform ordering. Consistent with the hypothesis of platform and direct ordering being substitutes, I find that commission caps raised direct ordering by 0.046 log points using the IW estimator and by 0.037–0.038 using the Callaway and Sant'Anna (2021) estimator. Because direct orders outnumber platform orders, total restaurant orders rose slightly, by 0.035 log points in the IW specification. This result partially reflects that platform orders tend to be larger than direct orders; whereas the average basket subtotal for a platform order was \$28.29 in 2020, it was \$10.91 for a direct order.[11] Therefore, the increase in the total number of orders does not necessarily indicate that commission caps raised restaurant spending. To assess the impact on spending, I specify log total restaurant spending as computed using the Numerator data as the outcome measure. I find a precisely estimated zero effect of commission caps on spending: across estimators, the estimated effects vary in sign but are uniformly under 0.01 log point in absolute

[11] Both figures are computed from the Numerator sample of Table 6 for 2020 as averages across orders. The platform figure is close to the \$27.50 pre-cap average in the Edison panel as cited above.

value. No estimate is significant even at the 50% level. It thus seems that reductions in platform spending were entirely compensated by increases in off-platform restaurant spending.

Figure 7 provides event study plots for the IW estimates. These plots show a gradual reduction of platform ordering and a gradual increase in direct ordering upon the imposition of commission caps. The platform-order pre-period coefficients are small and insignificant. The direct-order pre-period coefficients are positive, at 0.01–0.03, and some are individually significant, although they are small relative to the post-treatment effects, which reach 0.10 by month six. The gradual build-up of direct ordering is consistent with Li and Wang (2025b), whose sample ends in September 2020 and who find no rise in independent restaurants' self-takeout sales over the first fifteen weeks after a cap. Additionally, Online Appendix Table OA.13 reports separate effects on platform and direct spending.

Table 6: Effects on order volumes and total spending

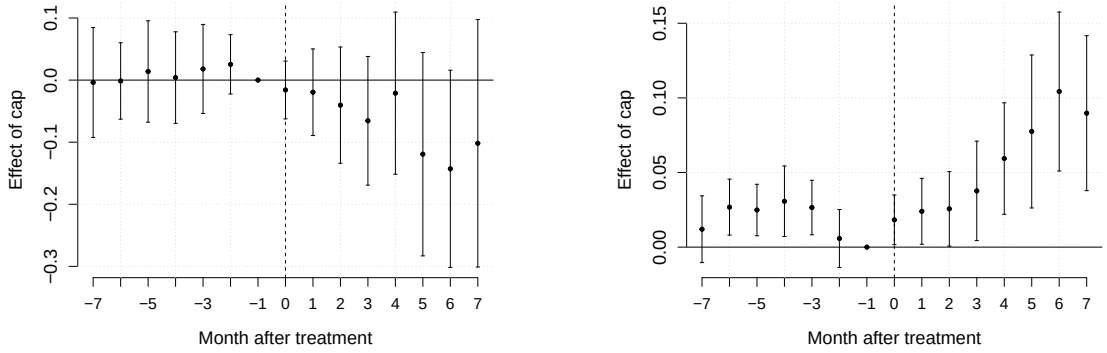
Outcome	IW	CS (not yet)	CS (never)	TWFE
Platform orders	-0.088	-0.078	-0.089	-0.067
(Edison)	(0.026)	(0.027)	(0.028)	(0.020)
Platform orders	-0.050	-0.085	-0.086	-0.064
(Numerator)	(0.039)	(0.062)	(0.043)	(0.025)
Direct orders	0.046	0.037	0.038	0.011
(Numerator)	(0.012)	(0.010)	(0.011)	(0.008)
All orders	0.035	0.023	0.023	0.013
(Numerator)	(0.012)	(0.029)	(0.012)	(0.009)
All spending	0.002	-0.007	-0.007	0.007
(Numerator)	(0.015)	(0.033)	(0.016)	(0.011)

Notes: this table reports estimates of the effect of a commission cap of 15% or lower on the log number of restaurant orders. "Platform orders (Edison)" is the log of orders placed on platforms in a ZIP/month in the Edison panel (Postmates is excluded for lack of data). The Numerator rows use the three-digit-ZIP/month panel described in Section 2.3. Direct orders are orders placed directly with restaurants and all orders is the sum of platform and direct orders. "All spending" is the log of aggregate basket subtotals (order value before fees, tips, and taxes) on platform and direct orders combined. The sample period is January 2020 to April 2021. Estimators, covariates, and weights are as in Table 3. Standard errors clustered by ZIP (Edison) or three-digit ZIP (Numerator) in parentheses.

The role of fees and network externalities. Commission caps reduced platform order volumes by prompting increases in platform consumer fees. This reduction, however, may have been attenuated by the additional restaurant adoption of platforms induced by caps.

Figure 7: Dynamic effects of commission caps on order volumes (Numerator panel)

(a) Platform orders (b) Direct orders



Notes: this figure plots interaction-weighted (Sun and Abraham 2021) estimates of the dynamic effects of a commission cap of 15% or lower on (a) the log number of orders placed on food delivery platforms and (b) the log number of orders placed directly with restaurants, from the Numerator panel. Month −1 is the omitted category. Dots are point estimates and bars are 95% confidence intervals based on standard errors clustered by three-digit ZIP.

To assess the extent of this attenuation, I estimate the effect of restaurant uptake of platforms on platform orders. To do so, I regress the log number of platform orders in a ZIP on the log number of unique restaurants listed on at least one platform within five miles of the ZIP, controlling for ZIP fixed effects, month fixed effects, and new COVID-19 cases per capita. Unique restaurants are the natural measure of variety here because most of the additional listings were restaurants already on another platform (Section 4.2), whose orders are partly diverted between platforms rather than created. Online Appendix Table OA.20 provides both details of the procedure and the results. I estimate an elasticity of orders with respect to the number of unique restaurants of 0.22 (standard error 0.04). Applied to the 4.4–6.0% increase in unique restaurants listed, the elasticity implies that the additional variety raised platform orders by 1.0–1.3%, against a net decline of 6–9%. The adoption response offset only a small fraction of the volume that the fee response removed.

Order size. Commission caps could also affect restaurant and platform revenues through responses in the size of orders as measured by the basket subtotal, the total price of an order before fees, tips, and taxes. Subtotals could change due to restaurant price changes or the practice of consumers economizing on higher per-order fees by placing fewer, larger orders. Table 7 reports the effect of caps on the log average basket subtotal per order

in the Edison panel. The estimates are close to zero and statistically insignificant across platforms and estimators. Thus, it seems that subtotals were not a relevant margin of adjustment in response to commission caps and that the decline in platform orders described by Table 6 entailed a decline in platform spending, not a consolidation of orders.

Table 7: Effects of commission caps on order size (Edison panel)

Platform	IW	CS (not yet)	CS (never)	TWFE
All platforms	-0.001	-0.006	-0.006	0.006
	(0.014)	(0.015)	(0.016)	(0.008)
DoorDash	0.017	-0.006	-0.009	0.015
	(0.022)	(0.025)	(0.026)	(0.012)
Uber Eats	-0.013	-0.010	-0.007	0.000
	(0.021)	(0.021)	(0.021)	(0.013)
Grubhub	-0.013	0.014	0.014	0.001
	(0.031)	(0.034)	(0.035)	(0.017)

Notes: this table repeats Table 3 with the log of the average basket subtotal per order (order value before fees, tips, and taxes) as the outcome. "All platforms" reports a single effect estimated on the stacked platform/ZIP/month panel of DoorDash, Uber Eats, and Grubhub with platform/ZIP and platform/month fixed effects. See the notes to Table 3. Standard errors clustered by ZIP in parentheses.

Robustness. The Edison panel records a ZIP/month only when at least one panelist order is observed, so the order and spending regressions above use an unbalanced panel of ZIP/months with recorded sales. I find, however, that filling in the panel with zeros for ZIP/months without recorded sales has a minor effect on the estimates. Upon filling in zeros, it is necessary to use a specification other than that with log orders as the dependent variable given that the log of zero is undefined. I therefore estimate effects on sales on a balanced panel with missing observations filled in with zeros by Poisson regression, which accommodates zero-valued outcomes. Both TWFE and the IW estimator are straightforwardly adapted to Poisson regression, and hence I compute estimates on the balanced panel using each of these estimators. Online Appendix Table OA.15 reports the results. The resulting order effects are -0.084 and -0.082 log points against the -0.067 and -0.088 of Table 6. Additionally, the dynamic event-study estimates match well those of the Edison event study in Online Appendix Figure OA.3 on a month-by-month basis. Also note that the Numerator panel is better balanced than the Edison panel (only 1.9% of its three-digit-ZIP/month observations are missing on a population-weighted basis) and its estimates of the effect on platform orders are similar to those from the Edison

panel. Taken together, the results from these alternative specifications suggest that the panel's lack of balance does not drive the baseline results.

The results are likewise robust to dropping localities that adopted commission caps exempting chain restaurants. In Online Appendix OA.5, I show that the effect on platform orders in the Edison panel estimated using the IW estimator is −0.110 log points when such places are dropped (compared to −0.088 when they are included), and that the Numerator estimates of the effects on platform orders (−0.059 against −0.050), direct orders (0.036 against 0.046), and total spending (0.014 against 0.002) are similarly close to those of Table 6. Across estimators, platform orders fall, direct orders rise, and total spending does not change on the restricted sample, as in the full sample.

5 Welfare calculations

Based on the estimated responses to commission caps discussed above, I provide back-of-the-envelope calculations of the overall welfare effects on each group of market participants.

Platforms. First, consider the platforms themselves. Platforms lose from commission caps both due to reductions in margins and reductions in sales. On an order with the average subtotal of $27.50 and the average consumer fee of $5.63, a reduction of the commission rate from 30% to 15% reduces platform revenue by $4.13, or 30% of the mean total revenue of $13.88 from a transaction absent a commission cap. The 22% increase in consumer fees implied by the terminal IW estimate of Figure 5 (seven months after a cap took effect) recovers $1.23 of this loss, so that revenue per order falls by 21%. Furthermore, according to the terminal IW estimate of the effect on total platform order volumes in the Edison panel, also seven months after a cap took effect, platforms lose 12% of their sales.[12] Thus, the total effect on platform revenues is a reduction of 30%. Making a similar calculation for profits requires an estimate of platform marginal costs. Using the mean marginal cost of $9.42 as estimated by Sullivan (2026) and assuming that this cost is invariant to the commission cap, the implied platform profit reduction is 69%:

[12]The Edison panel is the source of the fee, subtotal, and preferred order-volume estimates used in this section; its IW event study appears in Online Appendix Figure OA.3. The dynamic estimates are noisy, with month-by-month effects ranging from 10% to 19% between months three and eight. The Numerator panel of Figure 7 implies a smaller terminal reduction of 10%.

the platform's margin per order falls from \$4.46 to \$1.57, and it earns that margin on 12% fewer orders.[13] The \$9.42 cost figure is in line with external information on courier pay: DoorDash's website stated in September 2022 that its base pay to couriers ranges from \$2 to \$10 or more per delivery,[14] and Ahuja et al. (2021) report platform marginal costs of \$8.20 per order in a 2021 analysis of US food delivery. Note that the platform could have reduced courier pay (and hence its marginal costs) in response to commission caps. If so, the 69% reduction resulting from the calculation would be an overestimate of the platform's losses.

Consumers. It is more challenging to account for effects of commission caps on consumer surplus. Doing so requires a quantification of the value of restaurant variety (i.e., of network externalities): although caps raise consumer fees, they also attract new restaurants to platforms, which may enhance consumer surplus from platforms. Ignoring restaurant variety changes, commission caps raise platform fees by 22% and reduce platform sales by 12%, implying a fee elasticity of platform orders of -0.62. Interpolating a constant-elasticity demand curve between these responses, marginal consumers, who place 12% of pre-cap platform orders, lose on average \$0.58 of surplus per forgone order, about half of the fee increase, whereas inframarginal consumers lose \$1.23 per order, the amount of the fee increase. In total, consumer surplus falls by \$1.16 per pre-cap platform order, 94% of which is borne by inframarginal consumers.[15] These figures overstate consumer losses from commission caps given the expansion of restaurant variety. Estimating consumer preferences in the context of a structural model to fully account for the welfare effects of commission caps is the goal of the companion article Sullivan (2026).

Restaurants. Local governments introduced commission caps with the intention of benefitting restaurants. Certainly, caps directly benefit restaurants by lowering their commis-

[13]The marginal cost of \$9.42 is the unweighted mean of the cross-ZIP mean marginal costs that Sullivan (2026) estimates for DoorDash (\$9.38), Uber Eats (\$9.31), and Grubhub (\$9.56). These costs reflect courier compensation and I assume here that they are invariant to the cap and to order volumes. The margin per order is revenue per order minus marginal cost, $13.88 - 9.42 = 4.46$ before the cap and $10.99 - 9.42 = 1.57$ after, and total variable profit falls to $0.884 \times 1.57/4.46 = 31\%$ of its pre-cap level. I also assume that the subtotal is invariant to the cap, consistent with Table 7.

[14]See https://help.doordash.com/consumers/s/article/How-do-Dasher-earnings-work.

[15]The demand curve is $Q(c) = Q_0(c/c_0)^{\varepsilon}$, where $c_0 = 5.63$ and Q_0 are the pre-cap average consumer fees and order volumes. Additionally, $\varepsilon = -0.1232/0.1981 = -0.62$ is the ratio of the terminal (month-7) IW estimates for platform orders and fees. The consumer surplus loss is the area under this curve between c_0 and the post-cap fee $c_1 = 6.87$; inframarginal consumers lose $Q_1(c_1 - c_0)$ and marginal consumers lose the remainder.

sion payments on platform orders. A restaurant, for instance, pays $4.13 less commission on an order with the average $27.50 subtotal when the commission rate falls from 30% to 15%. This raises the restaurant's revenue (net of commission) on the order from $19.25 to $23.38, a 21% increase.

Given the possibility of a seesaw effect by which commission caps raise platform consumer fees, an unintended consequence of caps could be that they reduce sales and hence restaurant revenues on platforms. As noted above, I find that commission caps reduce platform orders by 11.6% seven months after imposition. If these orders were entirely lost to restaurants, the net effect of a commission cap would be to raise restaurants' platform revenue net of commission by only 7% (from $19.25 to $20.67 per pre-cap platform order), rather than by the 21% cited above, which did not account for reductions in ordering.

The conclusion that seesaw fee increases undo restaurant benefits from caps is itself overturned by offline substitution. Indeed, I find that the reductions in platform spending effected by caps are entirely offset by increases in direct-from-restaurant spending (Table 6). This neutralizes the reduction in revenue from the loss of platform orders discussed in the preceding paragraph and in fact raises restaurant revenues net of commission, as restaurants pay no commissions on off-platform orders.[16] In particular, replacing a platform order by a direct order eliminates the remaining 15% commission, which amounts to $4.13 on an average subtotal of $27.50. Given that commission caps convert 11.6% of platform orders into direct orders, these commission savings of $4.13 per converted order equal $0.48 per pre-cap platform order. Overall, the revenue increase per pre-cap platform order rises from the 21% cited above to 24% (from $23.38 to $23.86 relative to the pre-cap $19.25) due to substitution toward offline ordering. Thus, from the restaurant's perspective, consumer fee increases are not an unintended by-product of commission caps. Instead, due to their role in prompting offline substitution, fee increases constitute an important mechanism underlying restaurant benefits from commission caps.

[16]Restaurant profits similarly rise on account of substitution from platform to direct ordering as long as restaurant costs are similar between these two ordering channels.

6 Conclusion

In this article, I empirically characterized responses to one-sided commission regulation. Particularly important in understanding these responses are the concepts of seesaw effects, network externalities, and offline substitution. In a seesaw effect, commission caps prompt increases in consumer fees. This reduces consumer usage of platforms and, given the network externalities at play, consequently harms restaurants. However, network externalities running from the restaurant to the consumer side stand to limit harms: lower commissions lead more restaurants to join platforms, thus benefitting consumers and possibly attenuating reductions in platform ordering. Last, offline substitution suggests that lost platform orders need not hurt restaurants. Indeed, if consumers who stop ordering through platforms begin placing orders directly from restaurants—orders that entail no commissions for restaurants—reductions in platform ordering may ultimately benefit restaurants.

The empirical results support the relevance of seesaw effects, network externalities, and offline substitution in understanding the impacts of platform regulation. First, I find that commission caps of 15% or lower raised platform consumer fees by 12%. I additionally find that caps prompted restaurants to join more platforms; they raised the number of restaurant listings on platforms by 12–14% and the number of unique restaurants on platforms by 4–6%. In determining the overall effect on platform ordering, the fee effect dominates; commission caps reduced platform orders by 8% on average and by 12% seven months after taking effect. As suggested in the preceding paragraph, though, offline substitution prevented this ordering reduction from hurting restaurants. Indeed, I find that commission caps had no effect on total restaurant spending. In fact, a back-of-the-envelope calculation suggests that offline substitution raises the benefit of lower commissions from 21% to 24% of restaurants' revenue net of commission per pre-cap platform order. If restaurants' lost platform orders were not recovered through increased direct sales, this revenue increase would instead fall to 7%. This result highlights the importance of offline substitution in shaping the overall effect of commission caps on restaurants, the intended beneficiaries of these policies.

Bibliography

Ahuja, Kabir, Vishwa Chandra, Victoria Lord, and Curtis Peens. 2021. "Ordering in: The rapid evolution of food delivery." `https://www.mckinsey.com/industries/technology-media-and-telecommunications/our-insights/ordering-in-the-rapid-evolution-of-food-delivery`.

An, Yuan, Andrew Garin, and Brian K. Kovak. 2025. "Delivering Higher Pay? The Impacts of a Task-Level Pay Standard in the Gig Economy." Unpublished working paper, Carnegie Mellon University.

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

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with multiple time periods." *Journal of Econometrics* 225 (2): 200–230.

Carbó-Valverde, Santiago, Sujit Chakravorti, and Francisco Rodríguez-Fernández. 2016. "The role of interchange fees in two-sided markets: an empirical investigation on payment cards." *The Review of Economics and Statistics* 98 (2): 367–381.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review* 110 (9): 2964–2996.

Chang, Howard, David S. Evans, and Daniel D. Garcia Swartz. 2005. "The effect of regulatory intervention in two-sided markets: an assessment of interchange-fee capping in Australia." *Review of Network Economics* 4 (4): 328–358.

Chen, Manlu, Ming Hu, and Jianfu Wang. 2022. "Food delivery service and restaurant: friend or foe?" *Management Science* 68 (9): 6539–6551.

Dong, Ensheng, Hongru Du, and Lauren Gardner. 2020. "An interactive web-based dashboard to track COVID-19 in real time." *The Lancet infectious diseases* 20 (5): 533–534.

Edelman, Benjamin, and Julian Wright. 2015. "Price coherence and excessive intermediation." *The Quarterly Journal of Economics* 130 (3): 1283–1328.

Ennis, Sean F., Marc Ivaldi, and Vicente Lagos. 2023. "Price parity clauses for hotel room booking: empirical evidence from regulatory change." *Journal of Law and Economics* 66 (2): 309–331.

Evans, David S., Howard Chang, and Steven Joyce. 2015. "The impact of the US debit-card interchange fee regulation on consumer welfare." *Journal of Competition Law and Economics* 11 (1): 23–67.

Feldman, Pnina, Andrew E. Frazelle, and Robert Swinney. 2022. "Managing relationships between restaurants and food delivery platforms." *Management Science*, Forthcoming.

Fisher, Jack. 2024. "Worker Welfare in the Gig Economy." Unpublished working paper.

Gomes, Renato, and Andrea Mantovani. 2025. "Regulating platform fees under price parity." *Journal of the European Economic Association* 23 (1): 190–235.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225 (2): 254–277.

Hagiu, Andrei, and Julian Wright. 2025. "Platform Traps." Unpublished working paper.

Hallas, Laura, Ariq Hatibie, Saptarshi Majumdar, Monika Pyarali, Rachelle Koch, Andrew Wood, and Thomas Hale. 2020. "Variation in US states' responses to COVID-19." `https://www.bsg.ox.ac.uk/research/publications/variation-us-states-responses-covid-19`.

Huang, Yangguang, and Yu Xie. 2023. "Search algorithm, repetitive information, and sales on online platforms." *International Journal of Industrial Organization* 88 102933.

Hunold, Matthias, Reinhold Kesler, Ulrich Laitenberger, and Frank Schlütter. 2018. "Evaluation of best price clauses in online hotel bookings." *International Journal of Industrial Organization* 61 542–571.

Huynh, Kim P., Gradon Nicholls, and Oleksandr Scherbakov. 2022. "Equilibrium in two-sided markets for payments: consumer awareness and the welfare cost of the interchange fee." Bank of Canada Staff Working Paper, No. 2022-15.

Jullien, Bruno, Alessandro Pavan, and Marc Rysman. 2021. "Two-sided markets, pricing, and network effects." In *Handbook of Industrial Organization, Volume 4*, edited by Ho, Kate, Ali Hortaçsu, and Alessandro Lizzeri Volume 4. of Handbook of Industrial Organization 485–592, Elsevier.

Kay, Benjamin S., Mark D. Manuszak, and Cindy M. Vojtech. 2018. "Competition and complementarities in retail banking: Evidence from debit card interchange regulation." *Journal of Financial Intermediation* 34 91–108.

Li, Bin Grace, James McAndrews, and Zhu Wang. 2020. "Two-sided market, R&D, and payments system evolution." *Journal of Monetary Economics* 115 180–199.

Li, Zhuoxin, and Gang Wang. 2025a. "On-demand delivery platforms and restaurant sales." *Management Science* 71 (7): 5788–5804.

Li, Zhuoxin, and Gang Wang. 2025b. "Regulating powerful platforms: evidence from commission fee caps." *Information Systems Research* 36 (1): 126–140.

Lu, Haozhe, Mingyang Sun, and Yimin Zeng. 2021. "Will the food delivery industry be monopolized?" In *Proceedings of the 2021 International Conference on Financial Management and Economic Transition (FMET 2021)*, 95–100, Atlantis Press.

Mantovani, Andrea, Claudio A. Piga, and Carlo Reggiani. 2021. "Online platform price parity clauses: evidence from the EU Booking.com case." *European Economic Review* 131 103625.

Manuszak, Mark D., and Krzysztof Wozniak. 2017. "The impact of price controls in two-sided markets: evidence from US debit card interchange fee regulation." Finance and Economics Discussion Series 2017-074. Washington: Board of Governors of the Federal Reserve System.

Mayya, Raveesh, and Zhuoxin Li. 2025. "Growing Platforms by Adding Complementors Without a Contract." *Information Systems Research* 36 (3): 1670–1690. 10.1287/isre.2023.0237.

MIT Election Data and Science Lab. 2018. "County Presidential Election Returns 2000-2020."

Myeong, JaeHo, Yongjin Park, Francis Joseph Costello, Keongtae Kim, and

Jae Hyeon Ahn. 2026. "From Bytes to Bites: Examining the Online and Offline Spillover Impacts of Online Food Delivery Platform Subscriptions." Unpublished working paper.

Natan, Olivia. 2025. "Choice frictions in large assortments." *Marketing Science* 44 (3): 593–625.

Parrott, James A., and Michael Reich. 2018. "An earnings standard for New York City's app-based drivers: economic analysis and policy assessment." report for the new york city taxi and limousine commission, Center for New York City Affairs, The New School.

Reshef, Oren. 2023. "Smaller slices of a growing pie: the effects of entry in platform markets." *American Economic Journal: Microeconomics* 15 (4): 183–207.

Robles-Garcia, Claudia. 2022. "Competition and Incentives in Mortgage Markets: The Role of Brokers." Unpublished working paper.

Rochet, Jean-Charles, and Jean Tirole. 2003. "Platform Competition in Two-Sided Markets." *Journal of the European Economic Association* 1 (4): 990–1029.

Rochet, Jean-Charles, and Jean Tirole. 2006. "Two-sided markets: a progress report." *RAND Journal of Economics* 37 (3): 645–667.

Rysman, Marc. 2007. "An Empirical Analysis of Payment Card Usage." *The Journal of Industrial Economics* 55 (1): 1–36.

Rysman, Marc. 2009. "The Economics of Two-Sided Markets." *Journal of Economic Perspectives* 23 (3): 125–143.

Sant'Anna, Pedro H.C., and Jun Zhao. 2020. "Doubly robust difference-in-differences estimators." *Journal of Econometrics* 219 (1): 101–122.

Sullivan, Michael. 2026. "Fee Optimality in a Two-Sided Market." *Journal of Political Economy*, Forthcoming.

Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics* 225 (2): 175–199.

Wang, Chengsi, and Julian Wright. 2024. "Regulating platform fees." *Journal of the European Economic Association* jvae040.

Wang, Lulu. 2025. "Regulating Competing Payment Networks." Unpublished working paper.

Wang, Zhu. 2012. "Debit card interchange fee regulation: some assessments and considerations." *FRB Economic Quarterly* 98 (3): 159–183.

Weyl, E. Glen. 2010. "A Price Theory of Multi-sided Platforms." *American Economic Review* 100 (4): 1642–1672.