Digitally Inclined Small Businesses: Effects of E-Commerce on Small Business Credit*

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ABSTRACT

We study how the entry of e-commerce platforms affects credit to local small businesses. Linking transaction data for millions of U.S. consumers to Uniform Commercial Code filings, and exploiting Uber Eats' staggered launch across 61 U.S. markets in 2016–2017, we find that delivery platform entry raises the probability that local food merchants obtain new secured credit. The increase comes from non-bank lenders, including FinTechs and equipment-finance companies, and is concentrated among small and female-owned merchants. Merchants that never join a delivery app gain at least as much as adopters, so the effect works at the market level rather than through each firm's own online sales. Entry also raises exit among food merchants. Platforms reshape local small business credit well beyond the firms that sell on them.

Keywords: e-commerce, credit, small business, food industry, gig economy, ghost kitchens

JEL classification: L81, G20, E51, G32, L26, G21, G23

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I. Introduction

The digital economy has been growing faster than the U.S. economy as a whole. According to the U.S. Bureau of Economic Analysis (BEA), its real value added grew by 6.3% in 2022, compared with 1.9% for real GDP.[1] E-commerce, defined as the remote sale of goods and services over computer networks, produced $942 billion in gross output in 2021, or 25.4% of the digital economy's gross output, and $559 billion in value added. Business-to-consumer (retail) e-commerce, which we study, accounted for about a third of this gross output.[2] E-commerce typically relies on digital payments, which are more efficient than cash for online sales and are traceable by lenders.

Despite the rapid growth of e-commerce, little is known about its effects on firm finances. This paper examines how e-commerce affects small business credit in retail trade. Our main contribution is to show that e-commerce leads to more equilibrium credit to small businesses, consistent with both direct effects of e-commerce on sales and indirect information and collateral effects.

The effects of e-commerce on credit markets are theoretically ambiguous. On the one hand, e-commerce can *increase* equilibrium credit. In Ahnert, Hoffmann, and Monnet (2025), online transactions are settled with digital payments rather than cash, leaving digital footprints that signal credit quality, as in Berg, Burg, Gombović, and Puri (2020). These hard data shift information rents from merchants to lenders by lowering lending costs, such as the costs of screening to overcome adverse selection (e.g., Stiglitz and Weiss, 1981; Bester, 1985) and monitoring to address moral hazard (e.g., Holmstrom and Tirole, 1997).[3] Lenders provide more credit because of these lower costs and because higher revenue raises their expected payoffs. Merchants trade off the higher efficiency of online sales against lower information rents when choosing whether to sell online or offline. In equilibrium, some merchants adopt e-commerce, online and offline sales co-exist, and credit rationing is lower.[4] Interestingly, lenders provide more credit to *all* merchants, because

[1] See *U.S. Digital Economy: New and Revised Estimates, 2017–2022*, BEA, December 2023, the latest release.
[2] See *New and Revised Statistics of the U.S. Digital Economy, 2005–2021*, BEA, November 2022.
[3] Also see Zhu (2019) on the disciplining role of online data against moral hazard in a governance framework.
[4] We relate to the transparent digital payments equilibrium, or equilibrium with D-money, in Ahnert et al. (2025). This equilibrium is more relevant for our setting than the one with private digital payments, or equilibrium with P-money, because e-commerce transactions are rarely settled with digital money that has private features, such as on blockchains or with central bank digital currency (CBDC).

mimicking by low-quality types renders the choice of trading venue uninformative.

We argue that the relation between e-commerce and credit is likely more nuanced. First, e-commerce adoption itself can serve as a costly (albeit imperfect) signal of a merchant's type when adoption entails large fixed costs, in line with canonical studies on signaling and certification (e.g., Akerlof, 1970; Spence, 1973). For instance, white-label e-commerce requires merchants to launch an online ordering website and often to handle delivery in-house. In this case, e-commerce can reduce pooling, increasing credit to adopters while reducing credit to non-adopters. Second, e-commerce adoption—especially by many merchants at the same time—raises demand for inputs such as equipment, as adopters scale up and non-adopters upgrade to compete. Higher demand can push up collateral values in local markets and expand credit even to non-adopters. Thus, e-commerce can affect credit through channels other than higher revenue, such as information and collateral values.

On the other hand, e-commerce can *decrease* equilibrium credit by intensifying competition in local product markets that are monopolistically competitive because of transportation costs and brand differentiation. E-commerce allows distant merchants to compete with incumbents, which squeezes the profitability of all sellers and forces the weakest firms to exit. Higher default risk and moral hazard can then reduce credit supply. We estimate the equilibrium effects of e-commerce on small business credit and provide evidence that helps differentiate between supply-side and demand-side explanations.

We employ transaction data for millions of U.S. consumers to build a merchant–month panel from 2014 to 2023 with several proxies for merchant revenue, as well as merchants' industry and location (city and state). Importantly, the data show whether each transaction is physical, that is, initiated in a store, or digital, mostly reflecting online orders through merchants' own websites or apps (white-label e-commerce). Most merchants in our sample are small private firms, which are typically under-researched because of limited data availability. We match this panel by merchant name to Uniform Commercial Code (UCC) filings in nine states—Texas, Florida, California, Arizona, Mississippi, Tennessee, Virginia, Kentucky, and Oregon—to measure small business credit.

For food industry merchants, we also gather business characteristics from Dun & Bradstreet (D&B) Hoovers and hand-collect data on delivery app adoption.

We use the food industry (restaurants & groceries) as a setting to identify the causal effects of e-commerce on small business credit. Food merchants account for a substantial share of consumer-facing small businesses in the U.S., are typically small private firms that face significant credit constraints, and—as we show below—have rapidly increased their e-commerce adoption over the last decade.[5] These merchants can adopt e-commerce in two ways: online ordering for takeout or delivery through their own websites (first-party or white-label e-commerce) and online ordering through marketplaces run by delivery apps, such as Uber Eats, DoorDash, and Grubhub (third-party or delivery app e-commerce). The staggered entry of delivery apps across U.S. cities provides plausibly exogenous variation in e-commerce adoption in this industry.

We start with event studies around white-label e-commerce launch. Merchants are more likely to obtain UCC credit after they launch white-label e-commerce. Relative to the mean, the probability of a new UCC loan increases by 22.0% in the food industry and by 22.4% in other industries, and the probability of having an outstanding UCC loan increases by 13.1% and 6.1%, respectively. The increase in credit does not precede e-commerce launch but occurs several months after it. Direct revenue, which excludes revenue from delivery apps, also increases, mostly because of more orders rather than larger orders. Physical revenue increases as well, providing little evidence that online sales cannibalize offline transactions. Although we do not observe the costs of online versus physical sales, these results suggest that e-commerce is more efficient than offline sales, consistent with the assumptions in Ahnert et al. (2025).

These event studies do not allow causal interpretations. Omitted variables, such as local economic trends, could drive both e-commerce adoption and credit, and access to credit could itself affect the decision to launch e-commerce. To address these concerns, we use the staggered entry of Uber Eats across U.S. cities in difference-in-differences (DiD) and difference-in-differences-in-

[5] According to the National Restaurant Association, the restaurant industry employed 14.2 million people in 2022, nine in ten restaurants have fewer than 50 employees, and seven in ten restaurants are single-unit operations (see National Statistics).

differences (DDD) tests. By making delivery of online orders more efficient through part-time gig labor and economies of scale, Uber Eats entry raises the relative efficiency of online sales and makes e-commerce profitable for some restaurants and grocery stores that did not offer online ordering before. Consistently, Uber Eats entry increases white-label e-commerce adoption by 29.4% and overall (white-label and delivery app) e-commerce adoption by 34.0% relative to the mean in the food industry, but not e-commerce adoption in other industries in the same cities.[6]

Using this shock, we find higher UCC credit to restaurants and grocery stores over the 24 months after Uber Eats entry. Relative to the mean, the probability of obtaining a new UCC loan increases by 40.4% and the probability of having an outstanding UCC loan by 21.6%. These effects are concentrated in the food industry, while merchants in other industries in the same cities obtain fewer new UCC loans, consistent with lenders reallocating credit across industries. At the same time, Uber Eats entry increases market exit in the food industry, consistent with intensified competition. Our results are robust to an alternative control group of industries, the combined entry of the three main delivery apps (Uber Eats, DoorDash, and Grubhub), and propensity score matching. To further address the concern that Uber Eats entered growing markets, we instrument Uber Eats entry with earlier entry of UberX, Uber's ride-sharing service, which itself does not predict higher UCC credit in placebo tests. The instrumented DiD (IV–DiD) estimates are consistent with our main results.

Heterogeneity tests shed some light on the mechanisms. The effects of Uber Eats entry on UCC credit do not differ significantly between merchants that adopt e-commerce and those that do not. Because delivery app adoption entails negligible fixed costs, low-quality merchants can mimic high-quality ones at little cost, resulting in credit pooling. At the same time, the effects are stronger for small firms, female-owned firms, and unregulated food businesses, which are likely more financially constrained, with no differential effects on e-commerce adoption. These results point to lenders relaxing credit constraints. The increase in UCC credit comes from non-bank lenders, with both FinTechs and equipment finance lenders contributing, consistent with information and collateral

[6]Many ghost kitchens, which are set up exclusively for delivery and online orders, without traditional dine-in space or storefront, subsequently opened in cities of Uber Eats entry.

effects of e-commerce.

This paper contributes to three strands of literature. First, we speak to the literature on credit constraints in the presence of information asymmetry. To the best of our knowledge, our paper is the first to empirically link a firm's engagement in e-commerce to its access to credit. We build on seminal work on frictions that lead to credit rationing and market breakdowns, such as adverse selection (e.g., Stiglitz and Weiss, 1981) and moral hazard (e.g., Holmstrom and Tirole, 1997), and on recent studies of how technological advances relieve these frictions (e.g., Hau, Huang, Lin, Shan, and Sheng, 2024). Our findings point to costly e-commerce adoption as a novel, albeit imperfect, signal of credit quality, while costless adoption has no information content.

The most closely related paper is theory work by Ahnert et al. (2025), who link the credit market equilibrium to merchants' choice of trading venue (offline vs. online) through the information content of transparent digital payments, as opposed to opaque cash. Consistent with their underlying assumption, we find that e-commerce is associated with higher revenue, suggesting that it is more efficient than offline sales. Our main results are also in line with their transparent digital payments equilibrium, in which positive shocks to the relative efficiency of online sales—more efficient food delivery in our setting—increase e-commerce adoption and reduce credit rationing, as more precise signals from e-commerce reduce the information rents that lenders cede to merchants. In contrast to the mimicking equilibrium in Ahnert et al. (2025), in which e-commerce adoption itself is uninformative, we find some evidence consistent with lenders valuing e-commerce adoption as a signal of credit quality when it is costly, as in the case of white-label e-commerce. Yet, the effects of Uber Eats entry do not differ between adopters and non-adopters of delivery apps, consistent with mimicking and credit pooling when adoption costs are low.

In contemporaneous work, Zhang, Chen, et al. (2023) show that higher online sales reduce interest expense for listed firms in China and attribute this result to alternative data lowering financing costs. By contrast, we study how e-commerce adoption, rather than the level of online sales, affects the extensive, rather than the intensive, margin of credit to small U.S. firms. These firms are typically severely credit constrained because of information asymmetry and higher business

risk, so access to credit is of first-order importance to them. To the extent that common factors drive both credit rationing and loan pricing, our findings are consistent with theirs on the cost side.

Second, our paper relates to the growing literature on small business credit, which has rapidly expanded with more availability of data on small firms, especially as a result of the growth in FinTech lending (see, e.g., Gopal and Schnabl, 2022; Balyuk, Berger, and Hackney, 2026; Beaumont, Tang, and Vansteenberghe, 2026). We contribute to this literature by showing that small businesses obtain more credit when they start offering e-commerce and that this credit comes from non-bank lenders. We also find evidence of lender specialization, as in Gopal (2021): The increase in credit to food merchants after Uber Eats entry is accompanied by lower credit to merchants in other industries in the same cities and, to a much smaller extent, to food merchants in other cities. These results are consistent with the predominantly local nature of small business lending and the capital constraints faced by smaller banks and non-bank lenders (e.g., Black and Strahan, 2002; Berger and Udell, 1995; Berger and Black, 2011).

Third, we add to the literature on e-commerce, which has mostly focused on consumer benefits, consumers' engagement with online technologies, and competition versus integration between online and brick-and-mortar channels.[7] A recent strand of this literature studies the food delivery platforms that we use for identification. Li and Wang (2025) find that adopting an on-demand delivery platform raises restaurant sales, but the gains accrue disproportionately to chains once platform commissions are netted out. Raj and Eggers (2026) show that greater platform penetration increases establishment exit and reduces business dynamism. Cong, Yang, and Zhang (2024) show that firms with e-commerce experienced a smaller drop and a quicker rebound in sales at the onset of the Covid-19 pandemic. These studies focus on product-market outcomes; we instead examine how e-commerce affects small businesses' access to credit.

[7] On consumer benefits, see, e.g., Couture, Faber, Gu, and Liu (2021), Jo, Matsumura, and Weinstein (2019), and Brynjolfsson, Hu, and Smith (2003); these stem from reductions in search costs, transaction frictions, and market imperfections such as logistical barriers (Couture et al., 2021), price dispersion (Jo et al., 2019), or limited menus (Brynjolfsson et al., 2003). Using transaction data, Dolfen, Einav, Klenow, Klopack, Levin, Levin, and Best (2023) find that gains from e-commerce mostly accrue to high-income individuals. On the impact of online technologies on consumers' engagement with e-commerce, see, e.g., Todri (2022). On competition and integration between e-commerce and brick-and-mortar stores, see, e.g., Brynjolfsson, Hu, and Rahman (2009), Forman, Ghose, and Goldfarb (2009), and Gallino and Moreno (2014).

The remainder of the paper is organized as follows. Section II describes our data and summary statistics. Section III reports the results of event studies around white-label e-commerce launch and discusses endogeneity concerns. Section IV presents our identification strategy and DiD results. Section V presents the results of heterogeneity tests. Section VI concludes.

II. Data

In this section, we describe our data sources, construction of measures, and summary statistics, including white-label e-commerce trends. We validate the sample by comparing the data to merchant surveys.

A. Merchant Panel from Consumer Transaction Data

We derive our main sample from proprietary consumer transaction data in the U.S. The data come from a large aggregation and analytics platform that provides personal financial management services to clients of multiple banks and FinTech firms, including several top ones. The platform obtains data from its institutional partners. This partnership makes the coverage wider and the sample less prone to active selection issues compared to similar data from aggregators working directly with consumers. The data resemble data from the JPMorgan Chase Institute (see, e.g., Ganong and Noel, 2019), but for multiple institutions rather than exclusively for JPMorgan Chase.

The data comprise de-identified bank (e.g., checking, savings, debit card) and credit card transactions for 63 million U.S. consumers from January 2010 to June 2023 (9.5 million per month). The data panels include transaction date, amount, merchant, type (debit vs. credit), spending category, payment origin, transaction location, memo, and other attributes. Some of these attributes come directly from financial institutions (e.g., amount, date, and memo), while others result from the provider enriching the data. For example, the provider uses natural language processing algorithms to extract merchant names from transaction memos. We observe not only merchant names but also the spending category and location (city and state) of each transaction, which allow us to infer

merchants' industry and location. Due to the provider expanding partnerships in the first several years, we cut the sample to start in 2014 (after creating any rolling-window and lagged measures).

We form our merchant-level panel as follows. We proxy for merchant revenue by summing up individual transactions (number and amount) for each merchant and month. We do so separately for bank account and credit card transactions, separately for physical versus digital transactions, their combinations, and totals. We use the payment origin attribute to distinguish between physical and digital transactions. We refer to digital transactions with a merchant as white-label e-commerce, because consumers pay the merchant directly, for example, when ordering online through the merchant's own website or app. By contrast, consumers pay delivery apps rather than merchants for orders placed through these apps, so these orders appear in the data as transactions with the platforms. Therefore, our revenue measures capture direct revenue from physical and white-label e-commerce transactions but not revenue from delivery apps.

Order size is the average transaction amount, per payment mode (bank vs. credit card) and origin (physical vs. digital). We proxy for volatility in merchant revenue by calculating the 6-month-rolling-window coefficient of variation of direct revenue for each merchant. While our transaction-based measure underestimates revenue levels because the data omit some consumers transacting with merchants in our sample, we verify that the trends in merchant revenue in our panel are generally representative of survey data (also see Aiello, R Baker, Balyuk, Di Maggio, Johnson, and Kotter, 2025). This measurement approach offers an advantage over establishment databases commonly used to study small firms. In their study of delivery platforms and restaurants, Raj and Eggers (2026) note that sales and employment figures in the National Establishment Time Series (NETS) are frequently imputed—particularly for smaller establishments—and restrict their establishment-level analysis to survival for this reason. Because our measures are constructed from realized consumer transactions rather than imputations, we are able to observe performance directly for precisely the small merchants for whom establishment-level data are least reliable.

Because orders placed through delivery apps do not appear as transactions with merchants in our data, we conduct extensive hand-collection work to identify which food industry merchants

in our data joined food delivery apps: Uber Eats (including Postmates), DoorDash (including its Order Online platform), and Grubhub (including Seamless). We use these data to construct measures of e-commerce adoption that include delivery app e-commerce in addition to white-label e-commerce (see Internet Appendix C for details).

We then create time-invariant characteristics for each merchant by summarizing spending categories and transaction locations for each merchant across the entire sample. Specifically, for each merchant, we proxy for industry by selecting the most frequent spending category in which consumers in our data transact with this merchant, excluding any salary/reimbursement-related categories (only debit transactions). This approach is reasonable because our sample comprises mostly small businesses and payment processors typically report the transaction category associated with a merchant's main line of business. More than 68.0% of merchants have transactions in only one category. The top category comprises over 90% of transactions for 86.3% of merchants and over 75% of transactions for 91.9% of merchants in our sample. Similarly, we proxy for the merchant's location using the most frequent geolocation (i.e., city and state) of transactions with this merchant in the data. The overwhelming majority of merchants are exclusively or predominantly located in one city, consistent with their categorization as small businesses. We verify this approach to industry and location classification via manual Google searches for a small random sample of firms.[8]

We impose several restrictions on the merchants that we include in the final sample. First, we only keep merchants that appear in the data for at least 12 months. Second, we exclude merchants for which we likely have insufficient data by keeping only merchants with at least 60 transactions total, at least $1,200 in transaction amount total, at least an average of five transactions per month, and at least an average of $100 in transaction volume per month. Third, as customary, we exclude any merchants in the financial and utilities categories. The resulting sample is 44,137 unique merchants across all states, which we further restrict to 15,631 merchants in nine states based on UCC loan data availability (see below).

[8]For a sample of delivery app adopters in the food industry, we find that the merchants' city–state in our data aligns extremely well with the city–state in the address listed on their delivery app pages.

B. *UCC Loan Data*

We match our merchant-level panel to data on small business credit using official Uniform Commercial Code (UCC) filings for nine states: Texas, Florida, California, Arizona, Mississippi, Tennessee, Virginia, Kentucky, and Oregon (due to data availability). See Edgerton (2012); Gopal and Schnabl (2022); Balyuk, Prabhala, and Puri (2021) for detailed descriptions of the data.[9] Wherever UCC data are available for download, we use a combination of exact matching, fuzzy matching, and manual checks to match these data to our merchant panel by merchant name. Wherever UCC data are only available via a searchable website, we manually search for a merchant's name and its variations and hand-collect the output after determining that the debtor is likely a match.[10] We assume that a merchant located in these states does not have a UCC loan if unmatched.[11]

We are able to match 2,072 out of 15,631 (13.26%) merchants in these nine states to UCC filings, with the overwhelming majority of the matches being exact name matches. These 15,631 merchants constitute our main sample. The matched merchants obtained 3,887 UCC loans during our sample period, with some merchants obtaining multiple loans. Specifically, 65.15% obtained only one loan, 17.71% obtained two loans, 8.30% obtained three loans, 3.28% obtained four loans, and the remaining 5.65% obtained five or more UCC loans. We use this information to create a dummy variable that takes the value of 0, switches from 0 to 1 in the month a merchant obtains a UCC loan, and stays at 1 thereafter (*New Loan* (1/0)).

UCC filings do not report the loan amount or repayment status. However, they report filing and expiration (lapse) dates as well as the secured party associated with each filing. We measure the duration of each UCC filing as the number of months between the filing and lapse dates. The overwhelming majority of UCC filings, or 76.63%, are for 60 months (5 years), which is the

[9] The UCC data come from the filing systems maintained by the relevant state authorities. E.g., California Secretary of State's UCC Search website is available at https://bizfileonline.sos.ca.gov/search/ucc.

[10] Most merchant cities in our merchant panel closely align with the names of cities for matched debtors in the UCC data. Whenever they do not, we only keep matches where cities appear to be closely related (e.g., neighborhoods or districts such as Hollywood).

[11] Missed matches introduce measurement error in our credit outcomes, which is unlikely to be correlated with the timing of Uber Eats entry and should, if anything, attenuate our estimates.

default period of effectiveness of UCC filings, with another 19.71% for 120 months (10 years), and the remaining 3.66% spread across other durations. We further assume that each UCC loan is outstanding for the entire duration of the UCC filing and create a dummy for whether a merchant has at least one UCC loan outstanding in a given month (*Outstanding Loan* (1/0)). We use these two variables as outcomes in our empirical tests. We also classify the secured party on each UCC filing as a bank or a non-bank lender based on its name and business description and further identify two main types of specialized lenders—FinTechs and equipment finance lenders (see Internet Appendix C for details).

C. Uber Eats and Other Delivery App Data

Our identification strategy relies on quasi-exogenous variation in e-commerce. As one source of such variation, we use staggered timing of Uber Eats entry across U.S. cities as a plausibly exogenous shock to e-commerce adoption by merchants in the food industry (restaurants and groceries). We first hand-collect data on dates of early Uber Eats entry into 61 U.S. cities (before nation-wide coverage) from Uber's official website, carefully determining cities in which merchants could use the service after entry (Table A.I).[12] Restaurants in these cities directly benefited from Uber's broad delivery network and large customer base, which created incentives for them to adopt online ordering and delivery. Uber Eats entry has also affected surrounding cities by intensifying competition among merchants for customers who went out to eat at local restaurants in the past but now have an option of ordering from restaurants further away and getting their food conveniently delivered (and possibly peer effects). This competition likely creates incentives for local merchants outside cities of Uber Eats entry to also adopt e-commerce.[13]

To account for these indirect effects, we first match cities where merchants in our sample are

[12] In an unpublished honors thesis, Collison (2020) uses Visa transaction data from 2014 to 2017 to study delivery platform entry, defining entry as three consecutive months in which more than 0.3% of a county's restaurant sales run through delivery platforms. We instead use platform announcement dates, which are determined by the platform's expansion strategy rather than by local merchant sales, and therefore are not mechanically related to our outcomes.

[13] Such spatial spillovers are documented in the platform literature: Li and Wang (2025) instrument restaurant-level platform adoption with peer adoption in surrounding 10–25-mile markets, consistent with adoption spillovers operating at roughly the radius we use below.

located and Uber Eats entry cities to Metropolitan Statistical Areas (MSAs) using crosswalks from UnitedStatesZIPCodes.org and the U.S. Census Bureau. We then assign the Uber Eats entry date to all cities within the same MSA, restricting to a 25-mile driving distance between each merchant city and an Uber Eats city. We use the Google Directions API to compute the driving distance between these cities. In case of multiple entry dates per merchant city, we only keep the earliest one. We refer interested readers to Appendix A for more details. Following this procedure, we assign an Uber Eats entry date to merchants in 1,766 out of 8,775 cities in all states in our data. Fig. A.1 shows considerable variation in the timing of Uber Eats entry in 2016–2017. We use this variation in our difference-in-differences (DiD) and heterogeneity analyses in Section IV and Section V.

We hand-collect roll-out dates for other delivery apps, which are DoorDash and Grubhub, and construct entry shocks using the same city–MSA mapping and 25-mile distance procedure as for Uber Eats. Figs. A.2 and A.3 show the monthly timing of the DoorDash and Grubhub entry, respectively. Both figures show staggered entry across cities and states from 2015 to 2018, although Uber Eats entry seems to have the most variation across time.We also obtain from Burtch, Carnahan, and Greenwood (2018) the cities and entry dates of Uber's ride-sharing service, UberX, whose entry preceded Uber Eats entry. We use UberX entry as an instrument for Uber Eats entry in IV–DiD tests and in placebo analyses. We also do extensive hand-collection of data on the partnerships between merchants in our data with each of these apps: Uber Eats (and its Postmates), DoorDash (and its Order Online), and Grubhub (and its Seamless).

In addition to data on delivery apps, we hand-collect data on the locations and entry dates of large ghost kitchen operators across U.S. cities (Appendix B). Our final sample includes 226 ghost kitchens. Fig. B.1 shows staggered ghost kitchen entry across cities and states from 2018 to 2023.

D. Other Data

We also gather data from additional sources. We hand-match our food industry merchants to D&B Hoovers for business descriptions, detailed industry codes, which we further use to identify merchants those selling primarily alcohol, tobacco, and other regulated products, and flag female-

owned businesses. In addition, we obtain city-level population statistics from the U.S. Census Bureau's Population Estimates and median gross rent for occupied housing units from the Census Bureau's American Community Survey (ACS). Both variables are measured as of 2015, prior to the earliest Uber Eats entry year, and are used for propensity score matching (PSM).

E. Summary Statistics

Fig. 1 shows trends in white-label e-commerce adoption from 2014 to 2023: The share of transaction amounts coming from e-commerce, the share of merchants offering e-commerce, and the number of merchants with their first e-commerce transaction in each year.[14] In the food industry (Panel A), the e-commerce share of revenue increases from 1.11% in 2014 to 7.82% in 2023, while the share of merchants offering e-commerce increases from 1.40% to 10.11%. The number of new white-label e-commerce adopters in the food industry increases until 2020 and declines thereafter. Other industries begin at higher adoption levels and experience a smaller increase until the end of the sample (Panel B): The e-commerce share of revenue increases from 7.37% to 11.20%, and the share of merchants offering e-commerce increases from 10.83% to 13.82%. Internet Appendix Fig. IA.1 shows similar trends for merchants in all states.

Table I summarizes the 15,631 merchants in our main sample by spending category (industry). Restaurants comprise 34.65% of the sample, and groceries comprise another 7.68%. White-label e-commerce accounts for 5.91% of direct orders at restaurants and 2.27% of orders at grocery stores, compared with 6.01% across all merchants. Internet Appendix Table IA.I reports the corresponding statistics for all 44,137 merchants in all states.

Table II provides merchant–month summary statistics separately for the food industry and other industries in our main sample. The mean (median) monthly direct revenue is \$722.0 (\$291.7) in the food industry and \$1,415.9 (\$551.8) in other industries. Merchants average 20 monthly orders in the food industry and 12 in other industries. Credit cards account for 32.4% and 36.9%

[14] Our estimates are lower than the estimate in Dolfen et al. (2023) that e-commerce reached 8% of consumption by 2017. This difference is likely due to differences in data and measurement. Dolfen et al. (2023) use Visa credit and debit card transactions across all merchants, including large online retailers, while our sample consists of small merchants and our e-commerce measures include only white-label e-commerce transactions.

of direct orders, while white-label e-commerce accounts for 4.6% and 7.3%, respectively. The mean white-label e-commerce order is larger than the mean physical order in both groups: $93.6 versus $40.1 in the food industry and $237.5 versus $137.6 in other industries. The probability of a new UCC loan is 7.8% in the food industry and 10.1% in other industries, while the probability of an outstanding UCC loan is 11.3% and 16.0%, respectively. Internet Appendix Table IA.II reports revenue statistics for merchants in all states.

III. Results: Event Studies

This section reports the results of event studies around white-label e-commerce adoption by merchants. We first test whether merchants are more likely to obtain UCC credit after they launch white-label e-commerce. We then test whether white-label e-commerce is associated with higher revenue and whether it cannibalizes physical transactions. Lastly, we discuss endogeneity concerns that motivate our identification strategy.

A. Event Study around White-Label E-Commerce Adoption

Our first set of tests are dynamic event-study regressions around the first month when each merchant started to offer white-label e-commerce. Specifically, we run regressions of the following form:

$$Y_{it} = \sum_{k=-24}^{24} \beta_k \, \textit{Treated} \ (1/0)_i \times \mathbb{1}\{k = t - t_i\} + \sum_{k=-24}^{24} \gamma_k \mathbb{1}\{k = t - t_i\} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \qquad (1)$$

where Y_{it} is the outcome and β_k are the coefficients of interest. *Treated* $(1/0)_i$ (*WL E-Comm* (1/0) in Table III) is an indicator that takes the value of 1 if a merchant engages in white-label e-commerce and 0 otherwise. Matrix $\mathbb{1}\{k = t - t_i\}$ is a set of *Event Month* dummies, where t_i $(k = 0)$ is the month of the first white-label e-commerce transaction. The event window is $k = -24$ to $k = 24$, with the reference month $k = -1$. We randomly assign $k = 0$ with the respective event window to the control group. Parameter α_i is merchant fixed effects (FEs) and ζ_{st} represents

month–year × industry fixed effects. Standard errors (SEs) are clustered at the merchant level.

We next estimate static event-study regressions with the interaction term between *Treated* and *Post*. The regression specification is as follows:

$$Y_{it} = \beta \, Treated \ (1/0)_i \times Post \ (1/0)_{ik} + \gamma Post \ (1/0)_{ik} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \tag{2}$$

where $Post \ (1/0)_{ik}$ is an indicator that takes the value of 1 in the month of the first white-label e-commerce transaction and 24 months thereafter and 0 otherwise. Other variables are defined similarly to Equation (1). As before, standard errors are clustered at the merchant level.

A.1. UCC Loans

We start by testing how small business credit changes around white-label e-commerce adoption. Adopting white-label e-commerce likely entails substantial fixed costs. Merchants need to launch and maintain an online ordering website or app, integrate online payments with their point-of-sale systems, and often handle delivery in-house, which requires hiring drivers and buying or leasing delivery vehicles. Unlike delivery app adoption, which is essentially costless for merchants (Section V.A), white-label e-commerce adoption can therefore serve as a costly (albeit imperfect) signal of a merchant's type, because lower-quality merchants are less likely to pay the fixed cost to adopt.

We estimate Equations (1) and (2) with measures of UCC loans to merchants as dependent variables. We use two measures of UCC credit. *New Loan* (1/0) takes the value of 0, switches from 0 to 1 in the month a merchant obtains a UCC loan, and stays at 1 through the event window. *Outstanding Loan* (1/0) takes the value of 1 from the month a merchant obtains a UCC loan through the UCC filing lapse month and 0 otherwise.

Fig. 2 provides the first evidence on small business credit around white-label e-commerce adoption. It reports estimated coefficients β_k from Equation (1). Panel A presents the food industry, while Panel B presents other industries. We find that firms receive more UCC credit after they start offering white-label e-commerce. The pre-event estimates are generally close to zero in both

panels, supporting the parallel trends assumption, and their confidence intervals include zero. After e-commerce adoption, the estimated probabilities of both a new UCC loan and an outstanding UCC loan become positive. In the food industry, the increase in new UCC loans becomes statistically significant around Event Month $k = 8$ and remains significant through the end of the event window. The increase in outstanding UCC loans appears earlier and remains positive over the post-event window. Thus, the increase in credit does not precede e-commerce adoption and is generally inconsistent with firms borrowing to launch e-commerce. Interestingly, the effects are not immediate. This delayed response likely reflects the time that some lenders wait to collect more accurate information about firms' e-commerce and physical operations after e-commerce launch.

Table III formalizes these tests using Equation (2). The results are consistent with more UCC credit after white-label e-commerce adoption, and the economic magnitudes are material. In the food industry, the probability of obtaining a new UCC loan increases by 21.96% relative to the event window mean, and the probability of having an outstanding UCC loan increases by 13.05% (Panel A, Columns (1)–(2)). In other industries (Panel B), the corresponding increases are 22.35% and 6.14%, respectively. All four coefficients are statistically significant.

A.2. Merchant Revenue

We next examine changes in merchant revenue and its components after white-label e-commerce adoption. Columns (3)–(6) of Table III report the results of regressions from Equation (2) with measures of direct (non-delivery app) revenue as dependent variables. We find that direct revenue increases after merchants start to provide white-label e-commerce, and so does the number of orders. Economic magnitudes are sizable but reasonable. In the food industry (Panel A), white-label e-commerce adoption is associated with increases of 14.68% in direct revenue, 7.66% in the number of orders, and 6.52% in order size (Columns (3)–(5)). In other industries (Panel B), direct revenue and the number of orders increase by 14.34% and 12.86%, respectively, while the coefficient for order size is not statistically significant.[15] Therefore, most of the effects on revenue come from

[15] We calculate the economic magnitudes for logged outcomes as $[\exp(\beta) - 1] \times 100\%$.

more orders rather than larger order size.

E-commerce does not seem to cannibalize physical transactions. Rather, physical revenue increases as well after white-label e-commerce adoption by merchants. Physical revenue increases by 8.20% in the food industry and 4.81% in other industries (Column (6)). In our event-study tests, we cannot differentiate between white-label e-commerce adoption having positive spillover effects on offline sales (e.g., because of better merchant visibility) or non-adopters losing clients to adopters, but both channels are likely at play.

The Internet Appendix confirms that these measures increase at the time of white-label e-commerce adoption and that the effects persist after launch. Internet Appendix Fig. IA.2 plots coefficients β_k from Equation (1) for log direct revenue and log physical revenue. Direct revenue increases sharply in the month of white-label e-commerce adoption and remains elevated thereafter in both panels. Physical revenue also increases, providing little evidence that online sales cannibalize physical transactions.[16] Fig. IA.3 shows similar patterns when we include merchants in all states in our data. Fig. IA.4 presents the corresponding results for the number of orders and order size, while Fig. IA.5 separately presents revenue from bank account and credit card transactions. These measures also increase in the month of adoption and remain elevated afterward. The exceptions are order size in other industries, consistent with its insignificant coefficient in Table III, and credit card revenue in other industries, which increases less.

B. Endogeneity Concerns

The results so far are consistent with white-label e-commerce adoption being associated with more credit. These results are not causal. There may be omitted variables that are correlated with both e-commerce and credit provided to merchants. For example, local economic opportunities

[16] We also find a decline in revenue volatility, measured as the 6-month-rolling-window coefficient of variation of direct revenue, after white-label e-commerce adoption in both the food industry and other industries, with most post-adoption estimates becoming negative from around Event Month $k = 3$ onward (bottom graphs of Fig. IA.2). This result points to more business stability after e-commerce adoption. Lower revenue volatility should decrease firms' demand for credit, because small businesses often borrow to bridge cash flow shortfalls (e.g., Benetton and Buchak, 2024). Therefore, the increase in UCC credit after white-label e-commerce adoption is more likely to reflect higher credit supply rather than liquidity-driven credit demand.

could create incentives for e-commerce adoption and at the same time increase supply of credit to local merchants. Similarly, merchants with better growth prospects may be more likely both to adopt white-label e-commerce and to obtain credit. Reverse causality is also possible. For instance, merchants may seek loans to finance the fixed costs of launching or scaling up their e-commerce offerings. Reverse causality is less plausible given that we observe no increase in credit before the first month of e-commerce in Fig. 2. If anything, there is a delay of several months between the launch of e-commerce and the increase in credit. Yet, it is still possible that firms start e-commerce in expectation of receiving more loans in the future. Therefore, we need a strategy to identify the causal effects of e-commerce on small business credit. In the next section, we use the staggered entry of Uber Eats across U.S. cities as a plausibly exogenous shock to e-commerce adoption in the food industry.

IV. Results: Difference-in-Differences (DiD)

This section addresses endogeneity concerns with the event study tests using food delivery apps for identification. It presents our strategy, specifically difference-in-differences (DiD) analyses based on the staggered entry of Uber Eats. It then discusses multiple robustness tests, including difference-in-differences-in-differences (DDD), placebo, and instrumented DiD (IV–DiD) tests. Lastly, it examines how Uber Eats entry affects direct revenue as well as market entry and exit.

A. Identification Strategy

Our identification strategy relies on the staggered roll-out of Uber's food delivery service, Uber Eats, across 61 U.S. cities from March 2016 to October 2017 (Appendix A, Table A.I). Uber Eats represents a plausibly exogenous shock to e-commerce adoption because it significantly lowered transaction costs associated with online sales in the food industry. Importantly, Uber Eats' roll-out schedule was primarily determined by Uber's strategic expansion plans rather than by the restaurants' characteristics. While it is likely that Uber did not randomly choose cities it decided

to enter (e.g., more urban and more populous cities), we argue that the *timing* of entry was less likely to be related to local economic trends. We provide evidence to this effect below.

We employ DiD tests to first verify that Uber Eats entry resulted in more e-commerce and then test whether it also resulted in more credit to merchants. Since the shock is directly relevant to restaurants and grocery stores (e.g., food marts, convenience stores), we focus on the food industry in these tests and present results separately for all other industries. The latter provide a useful comparison because they should not experience the same direct effect of Uber Eats entry. Our main tests use the full sample of treated and control cities. We report tests using propensity score matching (PSM) in Section IV.C.

We start by estimating and graphing coefficients from dynamic regressions, similar to those in Equation (1) for event study tests. We use the following regression:

$$Y_{it} = \sum_{m=-24}^{24} \beta_m \, Uber\ Eats\ (1/0)_c \times \mathbb{1}\{m = t - t_c\} + \sum_{m=-24}^{24} \gamma_m \mathbb{1}\{m = t - t_c\} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \quad (3)$$

where Y_{it} is the outcome and β_m are the coefficients of interest. *Uber Eats* $(1/0)_c$ is an indicator that takes the value of 1 if a merchant's city is shocked by Uber Eats entry and 0 otherwise. Matrix $\mathbb{1}\{m = t - t_c\}$ is a set of *Uber Eats Month* dummies, where t_c $(m = 0)$ is the month of Uber Eats entry into a city. The event window is $m = -24$ to $m = 24$, with the reference month $m = -1$. We randomly assign $m = 0$ with the respective event window to the control group. Parameter α_i is merchant fixed effects (FEs) and ζ_{st} represents month–year $\times$ industry FEs. Standard errors are clustered at the merchant level.[17]

We further estimate formal regressions where we use a pre/post dummy for months before and after Uber Eats entry, similar to Equation (2). The regression specification is as follows:

$$Y_{it} = \beta \, Uber\ Eats\ (1/0)_c \times Post\ (1/0)_{cm} + \gamma Post\ (1/0)_{cm} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \quad (4)$$

[17]Since treatment is at the city level, to test for robustness to two alternative ways of clustering: city–state level clustering and double-clustering at the city–state and month levels (Abadie, Athey, Imbens, and Wooldridge, 2023). The results remain robust with very little change in t-statistics and significance levels.

where $Post\ (1/0)_{cm}$ is an indicator that takes the value of 1 in the month of the Uber Eats entry into a city and 24 months thereafter and 0 otherwise. Other variables are defined similarly to Equation (3). As before, standard errors are clustered at the merchant level.

Lastly, to formally compare the effects between the food industry and other industries, we estimate difference-in-differences-in-differences (DDD) regressions in which the food industry is the treated industry and other industries serve as the control group:

$$\begin{aligned} Y_{it} = {} & \rho\, Uber\ Eats\ (1/0)_c \times Post\ (1/0)_{cm} \times Food\ (1/0)_i + \beta\, Uber\ Eats\ (1/0)_c \times Post\ (1/0)_{cm} \\ & + \theta\, Food\ (1/0)_i \times Post\ (1/0)_{cm} + \gamma\, Post\ (1/0)_{cm} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \end{aligned} \tag{5}$$

where ρ is the main coefficient of interest and $Food\ (1/0)_i$ is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The main effects of *Uber Eats* (1/0) and *Food* (1/0) and their interaction are absorbed by merchant FEs. Other variables are defined similarly to Equation (4). As before, standard errors are clustered at the merchant level.

B. UCC Loan DiD and DDD

We first validate the shock by relating Uber Eats roll-out to e-commerce adoption by merchants in the food industry. *WL E-Comm* (1/0) takes the value of 1 in the month of the first white-label e-commerce transaction and stays at 1 thereafter. *WL + App E-Comm* (1/0) is defined similarly but also includes delivery app adoption, for which we hand-collect data in the food industry only. Therefore, the two measures coincide in other industries. We then test whether Uber Eats entry resulted in more UCC credit, measured by *New Loan* (1/0) and *Outstanding Loan* (1/0) as in Section III.A.

Fig. 3 reports the results from Equation (3). Panel A presents the results for the food industry, while Panel B presents the results for all other industries. We find that white-label e-commerce adoption increases in the food industry after Uber Eats entry into a city (top graphs). The pre-entry estimates are small, and their confidence intervals include zero, supporting the parallel trends

assumption. If anything, the likelihood of merchants adopting e-commerce increases with a few-month lag after entry. This pattern suggests limited, if not absent, anticipation of Uber Eats entry. By contrast, we do not observe a comparable post-entry increase in e-commerce adoption by merchants in other industries. This comparison mitigates the concern that the increase in the food industry reflects local economic conditions, such as time-varying consumer preferences for online transactions, that should also affect other industries.

The middle and bottom graphs of Fig. 3 show an increase in the probability of restaurants and grocery stores obtaining a new UCC loan and having an outstanding UCC loan after Uber Eats entry. The pre-entry estimates are close to zero, and their confidence intervals include zero. The probability of having an outstanding UCC loan goes up right after the month of roll-out, while the probability of a new UCC loan increases more gradually, and both keep increasing over the next 24 months. By contrast, we do not observe a comparable increase in UCC credit in other industries.

Table IV formalizes these tests using Equation (4). In the food industry, Uber Eats entry increases the probability of white-label e-commerce adoption by 29.43% relative to the analysis window mean (Panel A, Column (1)). The effect is larger when we also account for delivery app adoption: *WL + App E-Comm* (1/0) increases by 34.03% (Column (2)). These results show that Uber Eats entry is indeed followed by more online sales by restaurants and grocery stores.

The relevance of Uber Eats entry to e-commerce adoption is further confirmed by the subsequent entry of ghost kitchens, which are set up exclusively for delivery and online orders, without traditional dine-in space or storefront (Appendix B). Many ghost kitchens opened in cities of Uber Eats entry. Specifically, ghost kitchens opened in 30 of the 61 Uber Eats entry cities in Table A.I (49.2%), including Los Angeles, Chicago, New York, and Houston, on average about four years after Uber Eats entry (between 27 and 75 months later). Ghost kitchens also opened in regions of Uber Eats entry, such as Orange County and Tampa Bay, and in nearby cities, such as Pasadena and Santa Monica near Los Angeles, Oakland and San Mateo near San Francisco, and Plano and Frisco near Dallas.

Turning to credit, Uber Eats entry increases the probability of a new UCC loan by 40.41% and

the probability of an outstanding UCC loan by 21.65% in the food industry (Panel A, Columns (3)–(4)). These results are consistent with our dynamic DiD tests in Fig. 3. Of note, the coefficients of the post dummy itself are negative and statistically significant at the 10% level, but their magnitudes are less than half those of the interaction terms. Therefore, if there is some redistribution of credit away from merchants in the same industry located in cities not affected by Uber Eats entry, this redistribution across local credit markets is relatively small.

On the other hand, Panel B shows that merchants in other industries located in cities of Uber Eats entry are less likely to obtain a new UCC loan after the entry. The coefficient for new UCC loans is negative and statistically significant at the 5% level. The coefficient for e-commerce adoption is also negative, although statistically significant only at the 10% level, while the coefficient for outstanding UCC loans is not statistically significant.

What could explain these findings given that one should expect industries other than the food industry to not be directly affected by Uber Eats entry and given our finding of no comparable increase in their e-commerce adoption? One possible explanation is that lenders have funding constraints, which typically bind for smaller lenders that specialize in small business lending. If these lenders see more opportunities in businesses positively affected by Uber Eats entry and e-commerce, one should expect these lenders to reallocate their funds toward merchants in the affected industries and away from merchants in other industries in cities of Uber Eats entry. We find evidence consistent with such reallocation by non-bank lenders in Section V.C. Therefore, the aggregate effects of e-commerce on credit markets are less straightforward. While it is possible that e-commerce adoption could expand the overall credit supply by creating new business opportunities that were negative-NPV projects before but are now profitable, it could lead to redistribution of credit across industries or local credit markets with little to no change in overall credit supply.

Table V formally compares the effects between the food industry and other industries using Equation (5). Relative to other industries, Uber Eats entry increases white-label e-commerce adoption in the food industry by 21.18% relative to the analysis window mean and *WL + App E-Comm* (1/0) by 39.49% (Columns (1)–(2)). The corresponding increases are 60.23% for new

UCC loans and 16.60% for outstanding UCC loans (Columns (3)–(4)). All four coefficients are statistically significant. The DDD coefficient for new UCC loans is larger than the DiD coefficient for the food industry in Table IV, likely because Uber Eats entry also reduces new UCC loans to merchants in other industries. These results show that the increase in e-commerce and UCC credit following Uber Eats entry is concentrated in the food industry directly affected by the shock.

Internet Appendix Table IA.VI further splits cities of Uber Eats entry into core markets, which are the cities announced by Uber, and other markets, which are adjacent cities within the same MSA and a 25-mile driving distance (sparse markets in Uber's terminology). The DDD estimates are similar in both types of markets. The triple-interaction coefficients for white-label e-commerce and new UCC loans are positive, statistically significant, and of similar magnitude in core and other markets. The coefficients for outstanding UCC loans are also positive in both types of markets, although statistically significant only in other markets. These results support our assignment of Uber Eats entry dates to cities surrounding Uber Eats entry cities (Section II).

C. Robustness to Controls, Other Delivery Apps, and PSM

We conduct several robustness tests. First, we verify that our results are robust to the choice of other industries as a comparison group. Internet Appendix Table IA.III refines this group to focus on local services and non-food retail by excluding merchants in the following industries: Automotive/Fuel, Education, Healthcare/Medical, Office Expenses, Services/Supplies, and Travel. Because we estimate DiD regressions separately by industry, the results for the food industry are the same as in Table IV (Panel A). In the remaining other industries, Uber Eats entry has no statistically significant effect on e-commerce adoption or outstanding UCC loans, while the coefficient for new UCC loans is negative and statistically significant only at the 10% level (Panel B). Thus, Uber Eats entry does not increase e-commerce adoption or UCC credit in industries that are more comparable to the food industry either.

Second, we repeat our analyses using the combined staggered entry of Uber Eats, DoorDash, and Grubhub, and find patterns similar to the ones using Uber Eats alone. *Delivery App* (1/0) takes

the value of 1 if a merchant's city was first shocked by either Uber Eats, DoorDash, or Grubhub entry and 0 otherwise, and *Post* (1/0) is defined relative to the first delivery app entry. Internet Appendix Fig. IA.6 shows that white-label e-commerce adoption and UCC credit increase in the food industry after delivery app entry, with no comparable changes in other industries. Internet Appendix Table IA.IV presents the DDD regressions. Relative to other industries, delivery app entry increases white-label e-commerce adoption in the food industry by 16.41% relative to the analysis window mean, *WL + App E-Comm* (1/0) by 80.40%, the probability of a new UCC loan by 53.05%, and the probability of an outstanding UCC loan by 14.47%, with the latter coefficient statistically significant only at the 10% level. Accounting for the small and statistically insignificant coefficients for other industries, the total effects in the food industry are 16.61%, 80.57%, 43.03%, and 12.81%, respectively.

Third, to mitigate concerns about differential pre-treatment trends, we repeat our analyses after PSM to improve the comparability between treated and control cities along key pre-period economic conditions. Specifically, we keep all treated cities and match each of them to three control cities with the closest propensity scores, estimated using log population and log median housing rents as of 2015. We then drop unmatched cities from the control group. Internet Appendix Fig. IA.7 presents the matched-sample dynamic estimates, which show patterns similar to those in Fig. 3, although they are less precisely estimated. Internet Appendix Table IA.V reports the DDD regressions. The triple-interaction coefficients for white-label e-commerce, *WL + App E-Comm* (1/0), and new UCC loans are positive and statistically significant, while the coefficient for outstanding UCC loans is positive but not statistically significant. The matched-sample results therefore continue to show more e-commerce adoption and more UCC credit in the food industry after Uber Eats entry, although the results are statistically weaker because of a smaller sample remaining after the matching.

D. Placebo and IV–DiD

One remaining concern is that Uber Eats entry is correlated with observable and unobservable differences between cities, including because Uber Eats entered larger cities and we are unable to achieve perfect balance between treated and control cities with PSM. Hence, we employ a supplementary strategy where we instrument for Uber Eats entry with earlier entry of UberX, Uber's ride-sharing service. Uber Eats entry is more likely in cities that UberX entered earlier, because Uber Eats relied on Uber's existing network of drivers and customers. Before we use UberX entry as an instrument, we verify that it itself does not predict an increase in UCC credit in the food industry relative to other industries.

Internet Appendix Fig. IA.8 reports the results from Equation (3) in which we replace Uber Eats entry with UberX entry. *UberX* (1/0) takes the value of 1 if a merchant's city was shocked by UberX entry and 0 otherwise, and *Post UberX* (1/0) takes the value of 1 in the month of the UberX entry and 24 months thereafter and 0 otherwise. We do not observe an increase in e-commerce adoption or UCC credit in the food industry after UberX entry. Internet Appendix Table IA.VII confirms these results. In the food industry, the coefficients for new and outstanding UCC loans are small and not statistically significant, while the coefficients for both e-commerce measures are negative (Panel A). In other industries, the coefficient for new UCC loans is negative and statistically significant only at the 10% level, while the other coefficients are not statistically significant (Panel B). Therefore, UberX entry itself does not predict an increase in e-commerce adoption or UCC credit in the food industry.

We further focus on UberX entry in cities that Uber Eats subsequently entered. Internet Appendix Fig. IA.9 and Table IA.VIII report the results of DiD regressions in which the coefficients of interest are the interactions between *Uber Eats* (1/0) and *UberX Month* dummies and between *Uber Eats* (1/0) and *Post UberX* (1/0), respectively. In these cities, UberX entry does not increase UCC credit in the food industry either: The coefficients for new and outstanding UCC loans are negative and not statistically significant (Panel A). The coefficient for white-label e-commerce is also not statistically significant, while the coefficient for *WL + App E-Comm* (1/0) is positive and

statistically significant, possibly reflecting delivery app entry during the post-UberX period. In other industries, the coefficient for outstanding UCC loans is positive and statistically significant (Panel B), which works against our finding of stronger credit effects in the food industry.

Table VI reports IV–DiD regressions in which the interaction between Uber Eats entry and the post period is instrumented with the interaction between UberX entry and the post period. The first-stage coefficients of the UberX interaction term are positive and statistically significant at the 1% level in both the food industry and other industries (Column (1)), with t-statistics of 18.06 and 22.17, respectively, supporting the relevance of the instrument. In the food industry, the instrumented Uber Eats interaction coefficients are positive and statistically significant for both new and outstanding UCC loans (Panel A, Columns (4)–(5)). The coefficient for *WL + App E-Comm* (1/0) is positive and statistically significant at the 1% level, while the coefficient for white-label e-commerce is not statistically significant (Columns (2)–(3)). The IV–DiD estimates are larger than the DiD estimates in Table IV, although they are less precisely estimated. The second-stage coefficients are not statistically significant in other industries (Panel B).

E. Direct Revenue, Market Entry and Exit

Lastly, we examine how Uber Eats entry affects merchants' direct revenue as well as market entry and exit. Table VII reports the results of DDD regressions from Equation (5) with various proxies of direct revenue (Columns (1)–(4)) and measures of market entry and exit (Columns (5)–(6)) as dependent variables. Direct revenue declines after Uber Eats entry, but this decline is not specific to the food industry. The coefficients of the interaction term between *Uber Eats* and *Post*, which capture the effects in other industries, are negative and statistically significant for log direct revenue, the number of orders, and physical revenue (Columns (1)–(2) and (4)). At the same time, the triple-interaction coefficients are small and not statistically significant (Columns (1)–(4)).

Because direct revenue does not include revenue from delivery apps, these estimates likely overstate any decline in total revenue in the food industry, where merchants can shift some of their orders to delivery apps. Given the comparable declines in other industries, platform-specific com-

petition is unlikely to be the sole explanation for the decline in direct revenue, although it plausibly contributes in the food industry. Li and Wang (2025) show that delivery platforms redistribute sales toward chains once commissions are netted out, and Raj and Eggers (2026) find that greater delivery platform penetration across U.S. restaurant markets from 2012 to 2018 raises establishment exit and market concentration and lowers employment, consistent with intensified competition among incumbents following platform emergence. Consistent with these studies, Columns (5)–(6) show that Uber Eats entry increases market exit in the food industry relative to other industries, while it does not affect market entry. These regressions include city–state FEs instead of merchant FEs to capture the rate of entry and exit.

Internet Appendix Fig. IA.10 provides supporting evidence. Physical revenue declines in both the food industry and other industries, with the estimates becoming persistently negative and statistically significant from around Uber Eats Month $m = 9$ in the food industry and $m = 8$ in other industries. Of note, revenue volatility does not change either after Uber Eats entry.

V. Results: Heterogeneity by Firm, Product, and Lender Type

In this section, we describe the results of heterogeneity tests to shed some light on the possible mechanisms behind our main finding. We start by examining whether the effects of Uber Eats entry on credit differ between merchants that adopt e-commerce and those that do not. We then test whether these effects differ by firm and product type and by lender type.

A. Heterogeneity by E-Commerce Adoption

We use the following regression specification to compare e-commerce adopters to non-adopters:

$$\begin{aligned} Y_{it} = {} & \rho\, \textit{Uber Eats}\ (1/0)_c \times \textit{Post}\ (1/0)_{cm} \times \textit{Adopter}_i + \beta\, \textit{Uber Eats}\ (1/0)_c \times \textit{Post}\ (1/0)_{cm} \\ & + \theta\, \textit{Adopter}_i \times \textit{Post}\ (1/0)_{cm} + \gamma \textit{Post}\ (1/0)_{cm} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \end{aligned} \tag{6}$$

where ρ is the main coefficient of interest, alongside β. Variable $\textit{Post}\ (1/0)_{cm}$ is an indicator that

takes the value of 1 in the month of Uber Eats entry into a city and 24 months thereafter and 0 otherwise. $Adopter_i$ is either *E-Comm Adopter* (1/0), an indicator for merchants that engaged in (white-label or app) e-commerce within 24 months of Uber Eats entry, or *App Adopter* (1/0), an indicator for merchants that adopted delivery app e-commerce within 24 months of Uber Eats entry. Other variables are defined similarly to Equation (3). Standard errors are clustered at the merchant level.

Table VIII presents the results by e-commerce adoption status. In the food industry, the triple-interaction coefficients are not statistically significant for either new or outstanding UCC loans (Columns (1)–(2)). At the same time, the coefficients of the interaction term between *Uber Eats* and *Post* remain positive and statistically significant. Uber Eats entry increases the probability of a new UCC loan by 1.70 percentage points (pp) and the probability of an outstanding UCC loan by 1.41 pp for merchants that do not adopt e-commerce. In other industries, the triple-interaction coefficients are not statistically significant either, while the probability of a new UCC loan declines by 1.34 pp after Uber Eats entry (Column (3)), consistent with our DiD results in Table IV.

Internet Appendix Table IA.IX further splits adopters into early adopters, which engaged in e-commerce within 24 months prior to Uber Eats entry, and late adopters, which first engaged in e-commerce during the month of Uber Eats entry or within 24 months after entry. We find no detectable differences between these adoption cohorts: Neither early nor late adopters obtain more UCC credit after Uber Eats entry than non-adopters, while the coefficients of the interaction term between *Uber Eats* and *Post* remain positive and statistically significant in the food industry. If anything, late adopters in the food industry are less likely to obtain a new UCC loan, although this coefficient is statistically significant only at the 10% level.

Table IX focuses on delivery app adoption. We conduct these tests for the food industry only, because we hand-collect data on delivery app partnerships only for this industry. Columns (1)–(2) define adopters in terms of all major delivery apps, while Columns (3)–(4) define adopters as Uber Eats adopters specifically. The results are similar to those in Table VIII. The triple-interaction coefficients are not statistically significant, with the exception of the coefficient for new

UCC loans among delivery app adopters, which is negative and statistically significant at the 10% level (Column (1)). The coefficients of the interaction term between *Uber Eats* and *Post* remain positive and statistically significant in all four columns, ranging from 1.76 pp to 2.06 pp. Therefore, even merchants that do not adopt Uber Eats itself obtain more UCC credit after Uber Eats entry.

We note that e-commerce adoption is endogenous. Therefore, these tests do not identify the causal effects of take-up itself. Rather, they show whether the effects of Uber Eats entry on credit differ ex post between merchants that choose to adopt e-commerce and those that do not. The absence of detectable differences is consistent with the mimicking equilibrium in Ahnert et al. (2025), in which lenders provide more credit to all merchants because mimicking by low-quality types renders the choice of trading venue uninformative. Mimicking is plausible in our delivery app entry tests, because delivery app adoption is essentially costless. Delivery platforms offer merchants help with integration and charge commissions as a percentage of revenue, so fixed adoption costs are negligible relative to those of white-label e-commerce.

Consistently, most of the increase in e-commerce adoption after Uber Eats entry comes from delivery apps. In the food industry, Uber Eats entry increases *WL + App E-Comm* (1/0) by 8.48 pp but *WL E-Comm* (1/0) by only 3.00 pp (Table IV). This pattern is also consistent with platform emergence acting on the local market as a whole. Raj and Eggers (2026) argue that establishments choosing not to engage with a distribution platform are nonetheless affected by its emergence, as consumption shifts toward the platform channel and merchants come to compete with a different set of peers. To the extent that lenders update their assessment of an entire local market once delivery platforms arrive, or that intensified competition from e-commerce drives up collateral values, non-adopters can also obtain more credit.

B. Heterogeneity by Firm and Product Type

If e-commerce adoption helps relieve credit constraints faced by small businesses through digital footprints or higher collateral values, one should expect smaller firms to benefit more from these channels. The same logic applies to other firms that are more likely to face financial constraints.

We test this conjecture using the following regression specification:

$$Y_{it} = \rho\, \textit{Uber Eats}\ (1/0)_c \times \textit{Post}\ (1/0)_{cm} \times \textit{Type}_i + \beta\, \textit{Uber Eats}\ (1/0)_c \times \textit{Post}\ (1/0)_{cm} + \theta\, \textit{Type}_i \times \textit{Post}\ (1/0)_{cm} + \gamma \textit{Post}\ (1/0)_{cm} + \alpha_i + \zeta_{st} + \varepsilon_{it}, \tag{7}$$

where ρ is the main coefficient of interest, alongside β. $\textit{Type}_i$ is an indicator for a small firm (*Small* (1/0)), a female owner (*Female* (1/0)), or an unregulated food business (*Non-Alcohol* (1/0)). Other variables are defined similarly to Equation (6). As before, standard errors are clustered at the merchant level.

Table X presents the results by firm and product type in the food industry. Panels A and B show that the effects of Uber Eats entry on UCC credit are stronger for small firms and female-owned firms, which are likely more credit constrained (e.g., Morazzoni and Sy, 2022; Brock and De Haas, 2023). Specifically, the effects are 4.80 pp higher for new UCC loans and 5.36 pp higher for outstanding UCC loans among small firms (Panel A, Columns (3)–(4)). The corresponding differences are 4.04 pp and 3.54 pp among female-owned firms (Panel B). By contrast, the coefficients of the interaction term between *Uber Eats* and *Post* are small and not statistically significant in both panels, suggesting that the increase in UCC credit after Uber Eats entry is concentrated among these firms. At the same time, we find no differential effects on e-commerce adoption. Uber Eats entry increases e-commerce adoption by small and female-owned firms as much as by other firms, apart from a marginally significant difference of 6.81 pp in *WL + App E-Comm* (1/0) among small firms (Columns (1)–(2)). If the higher credit reflected higher demand for credit by merchants expanding online, one should expect the credit effects to be stronger where e-commerce adoption increases more. This is not what we find. Therefore, these results are supportive of the supply-side effects: Lenders appear to relax credit constraints for firms that are more likely to face them.

Panel C presents the results by product type. We focus on merchants primarily selling regulated food such as alcohol, tobacco, or other restricted substances versus unregulated food. On the one hand, unregulated food is easier to deliver due to regulatory restrictions on the delivery of regulated products, which could make e-commerce more attractive for these businesses. On the other hand,

unregulated food typically means thinner profit margins, whereas restaurants with high alcohol sales are generally less financially constrained with respect to daily cash flow and internal funding. We find that the effects of Uber Eats entry are 3.79 pp higher for new UCC loans and 3.71 pp higher for outstanding UCC loans among unregulated food businesses. The coefficients of the interaction term between *Uber Eats* and *Post* are not statistically significant. Given that we find no differential effects on e-commerce adoption in Panel C (Columns (1)–(2)), these results also likely capture financial constraints rather than easier delivery.

C. Heterogeneity by Lender Type

Lastly, we examine heterogeneity by lender type, which allows us to gain better insight into the possible mechanisms behind the effects of e-commerce on credit. We classify the secured party on each UCC filing as a bank or a non-bank lender and further identify two main types of specialized lenders—FinTechs and equipment finance lenders (see Internet Appendix C for details). We then estimate DDD regressions in which the food industry is the treated industry and other industries serve as the control group, as in Table V, separately for UCC loans from each lender type.

Table XI splits UCC lending by bank vs. non-bank lenders. The effects come from non-bank lenders. Uber Eats entry increases the probability that merchants in the food industry obtain a new UCC loan from a non-bank lender by 2.07 pp and the probability that they have an outstanding UCC loan from a non-bank lender by 1.69 pp, relative to merchants in other industries (Columns (3)–(4)). By contrast, the corresponding coefficients for banks are smaller, 0.895 pp and 0.726 pp, and not statistically significant (Columns (1)–(2)). This finding is in line with the growing role of finance companies and FinTech lenders in small business lending (Gopal and Schnabl, 2022). Of note, the probability that merchants in other industries obtain a new UCC loan from a non-bank lender declines by 0.875 pp after Uber Eats entry, while the corresponding coefficient for banks is not statistically significant. This pattern is consistent with non-bank lenders reallocating credit from other industries toward the food industry, that is, with credit redistribution rather than aggregate credit expansion.

We then dive into two main types of specialized lenders, FinTechs and equipment finance lenders (Table XII). Both types of lenders contribute to the increase in UCC credit in the food industry. For FinTechs, the DDD coefficients are 1.21 pp for new UCC loans and 0.956 pp for outstanding UCC loans (Columns (1)–(2)). The corresponding coefficients for equipment finance lenders are 1.04 pp and 0.932 pp (Columns (5)–(6)). All four coefficients are statistically significant, albeit the coefficient for outstanding UCC loans from FinTechs only at the 10% level. The effects are weaker for non-bank FinTechs: 0.654 pp for new UCC loans, which is statistically significant at the 10% level, and 0.482 pp for outstanding UCC loans, which is not statistically significant (Columns (3)–(4)). Therefore, part of the FinTech effects comes from banks that we classify as FinTechs, such as FinTech partner banks. Similar to non-bank lenders overall, FinTechs and equipment finance lenders provide fewer new UCC loans to merchants in other industries after Uber Eats entry, by 0.589 pp (significant at the 10% level) and 0.669 pp, respectively, pointing to credit reallocation.

Taken together, these results are consistent with indirect channels through which e-commerce affects credit access, including information effects and collateral effects. FinTech lenders rely on hard data, such as digital footprints, in their credit decisions (e.g., Berg et al., 2020). Digital payments that accompany e-commerce generate such data and reduce the costs of screening and monitoring merchants (Ahnert et al., 2025). This can expand credit in growing e-commerce markets to both adopters and non-adopters, as low-quality merchants mimic high-quality ones due to the low cost of delivery app adoption. Equipment finance lenders, in turn, lend against the equipment they finance and specialize by collateral type (Gopal, 2021). Collateral allows these lenders to extend credit to growing merchants without relying on soft information. Increased competition from e-commerce in these markets may boost collateral values, resulting in more credit regardless of the actual e-commerce adoption status.

VI. Conclusion

This paper examines the relation between e-commerce and credit to small businesses in the U.S. Our main contribution is to show that e-commerce adoption is followed by more small business credit. We find an economically meaningful increase in the probability of merchants obtaining a new UCC loan and having an outstanding UCC loan after they launch white-label e-commerce. The increase in credit does not precede e-commerce adoption but rather takes place months after the launch and remains elevated thereafter, consistent with lenders needing time to learn about firms' e-commerce operations.

We then use the staggered entry of Uber Eats as a plausibly exogenous shock to e-commerce adoption in the food industry. Uber Eats entry increases e-commerce adoption and UCC credit in this industry but not in other industries. We find consistent results with propensity score matching, alternative delivery app shocks, and an IV–DiD specification that instruments Uber Eats entry with earlier ride-share UberX entry.

Our tests of the possible mechanisms suggest that, while both the demand and supply channels are likely at play, lenders increase credit to small businesses. The effects do not differ significantly between e-commerce adopters and non-adopters, consistent with mimicking under low adoption costs. At the same time, the effects are stronger for small firms, female-owned firms, and unregulated food businesses, which are likely more financially constrained, with no differential effects on e-commerce adoption. The effects come from non-bank lenders, with both FinTechs and equipment finance lenders contributing, consistent with information and collateral effects of e-commerce.

These findings are important for managerial decisions and policy. For small business owners, our results suggest that the arrival of delivery platforms can ease access to credit, especially for small and female-owned firms, although lenders do not appear to reward delivery app adoption itself. For policymakers, our results highlight that digital payment data and non-bank lenders can relax credit constraints of underserved small businesses. However, credit reallocation across industries suggests that the aggregate credit effects may be limited.

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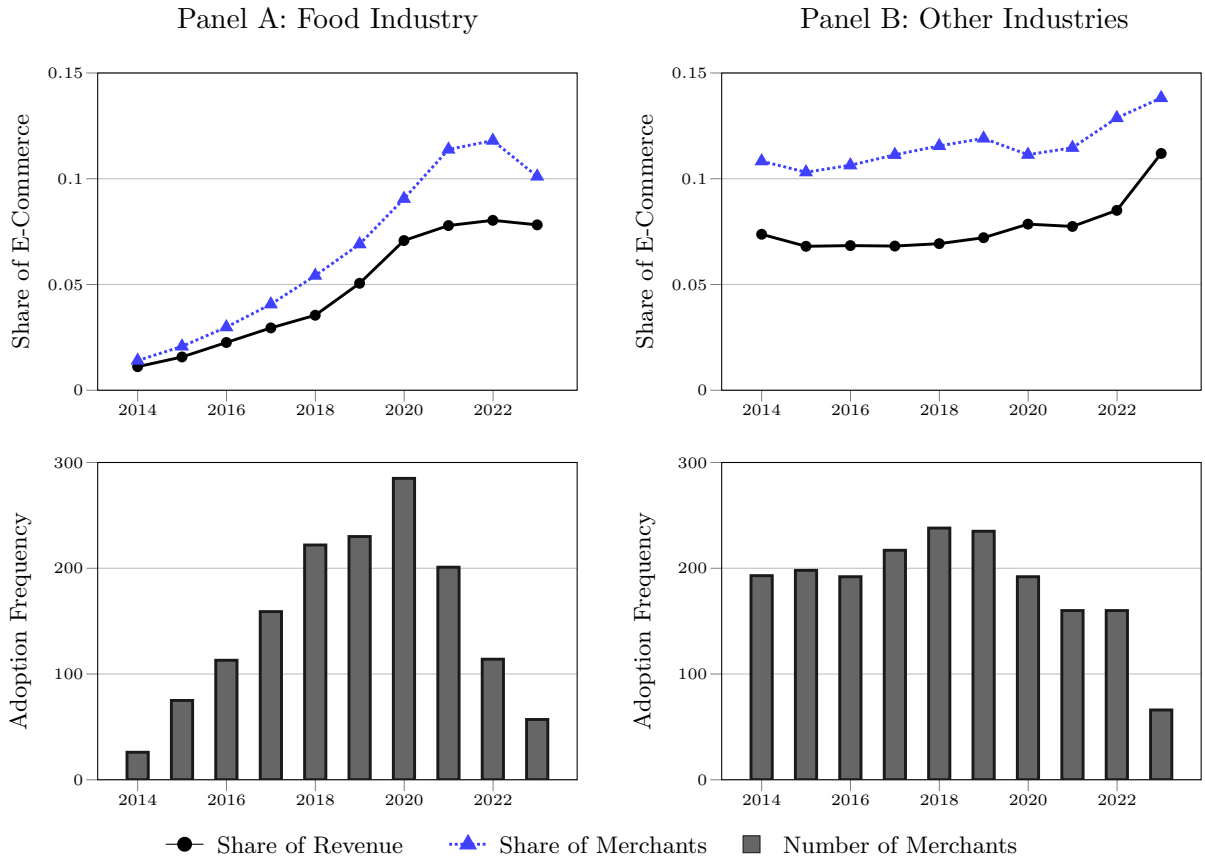
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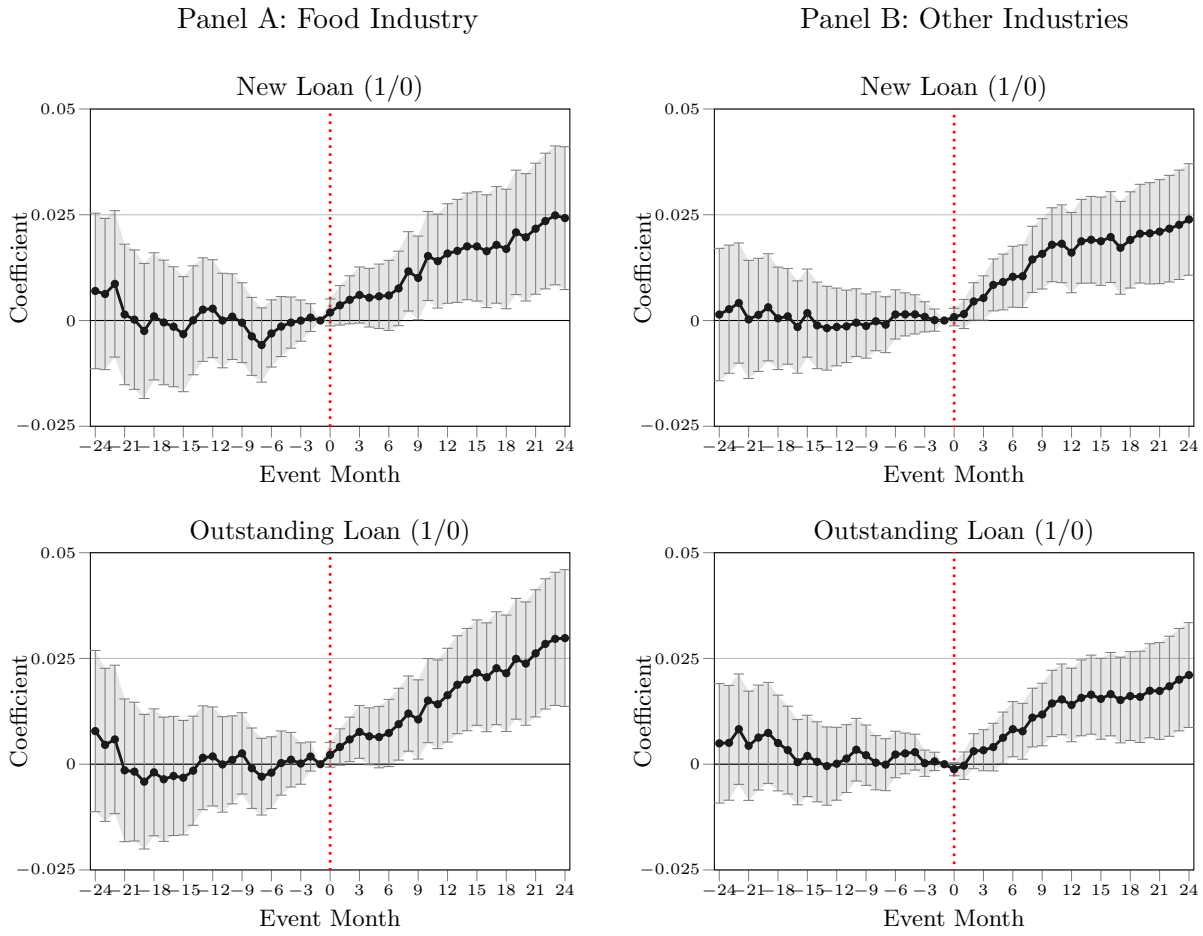
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Figure 1. Trends in E-Commerce Adoption



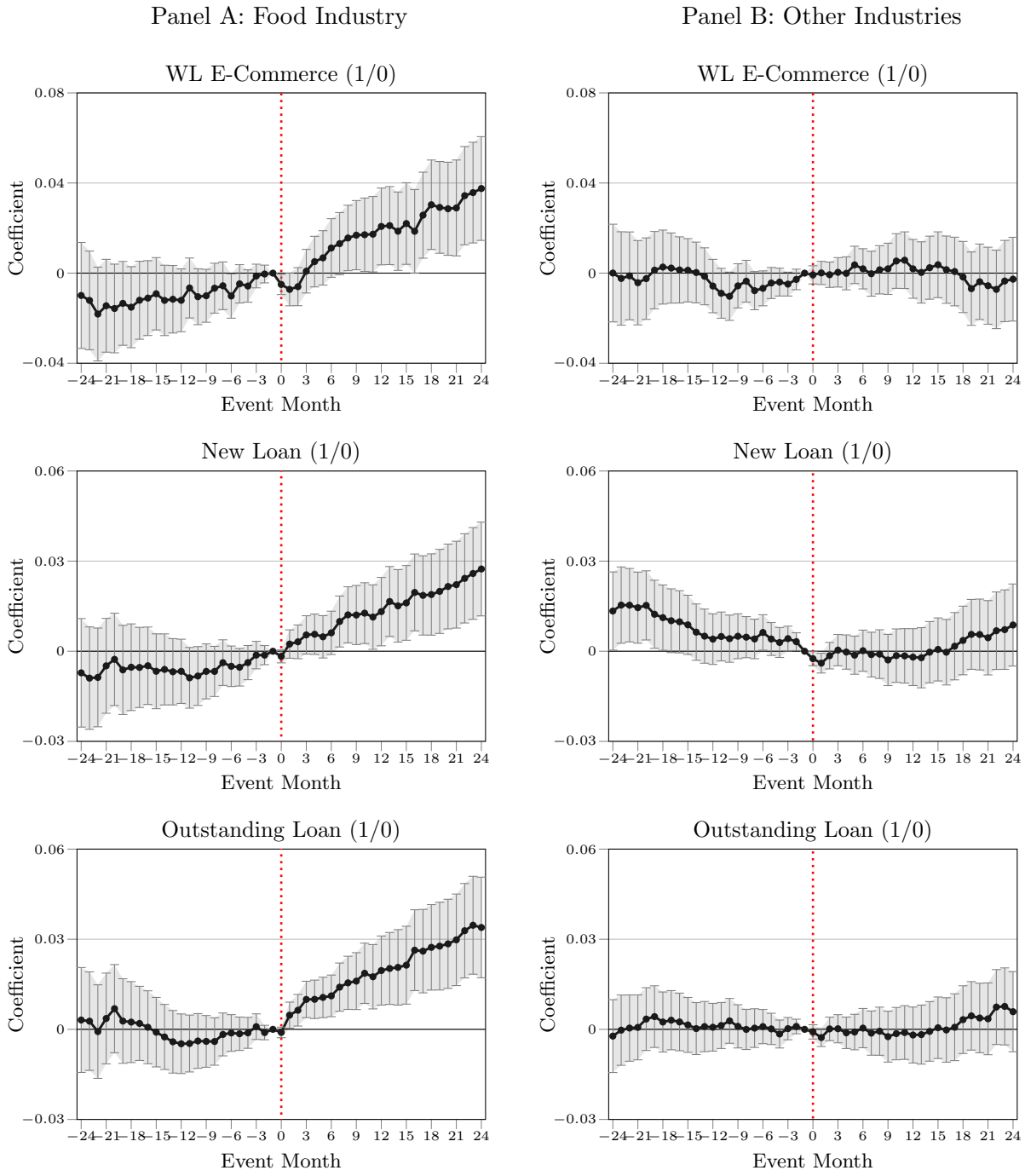
These figures illustrate trends in e-commerce adoption over time. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. We measure e-commerce adoption in three ways: the share of transaction amounts coming from e-commerce (black solid line), share of merchants offering e-commerce in each year (blue dotted line), and the number of merchants with their first e-commerce transaction in each year. The 2023 adoption frequency is scaled up by a factor of 365/178 to account for our sample ending on June 28, 2023 rather than the end of year.

Figure 2. E-Commerce and UCC Loan Event Study



These figures report coefficient estimates for interaction terms between *Treated* and *Event Month* dummies, where dependent variables are measures of UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Treated* (1/0) is an indicator that takes the value of 1 if a merchant engages in white-label e-commerce and 0 otherwise. Event Month $k = 0$ is the month of the first e-commerce transaction for the treatment group. The event window is $k = -24$ to $k = 24$, with the reference month $k = -1$. We randomly assign $k = 0$ with the respective event window to the control group. The error bars and shaded areas represent 90% confidence intervals. Standard errors are clustered at the merchant level.

Figure 3. Uber Eats Entry, WL E-Commerce, and UCC Loan DiD



These figures report coefficient estimates for interaction terms between *Uber Eats* and *Uber Eats Month* dummies, where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. Uber Eats Month $m = 0$ is the month of the Uber Eats entry for cities in the treatment group. The analysis window is $m = -24$ to $m = 24$, with the reference month $m = -1$. We randomly assign $m = 0$ with the respective event window to cities in the control group. The error bars and shaded areas represent 90% confidence intervals. Standard errors are clustered at the merchant level.

Table I. Summary Statistics: Firms by Industry

This table reports descriptive statistics for our analyses sample, split by industry, after restrictions described in the main text. The variables are first aggregated at the firm level and then averaged by industry. Variable definitions are provided in Internet Appendix C. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles.

	Num of Firms (#)	Avg Direct Revenue ($/mo)	Avg Num of Orders (#/mo)	Avg Order Size ($)	Avg Pct of CC Orders (%)	Avg Pct of WL E-Comm Orders (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Automotive/Fuel	550	2,071	14	213	33.2	2.18
Education	138	2,071	13	214	32.4	15.01
Electronics/Merchandise	499	1,164	13	112	28.9	4.98
Entertainment/Recreation	1,306	1,284	15	118	37.7	8.29
Gifts	165	824	10	89	39.9	3.71
Groceries	1,201	789	18	61	30.1	2.27
Healthcare/Medical	1,416	1,284	10	137	36.7	6.04
Home Improvement	629	2,084	13	203	39.4	14.96
Office Expenses	73	1,495	12	149	35.6	11.65
Personal/Family	2,925	886	11	89	33.5	3.52
Pets/Pet Care	555	1,551	13	128	42.7	3.03
Restaurants	5,416	757	22	39	32.2	5.91
Services/Supplies	459	2,530	17	253	29.0	22.02
Subscriptions/Renewals	2	2,083	36	38	22.1	100.00
Travel	297	2,037	15	200	43.2	3.31
Total	15,631	1,108	16	96	34.0	6.01

Table II. Summary Statistics: Revenue and Credit

This table reports descriptive statistics for our analyses sample. Columns (1)–(4) describe merchants in the food industry (restaurants & groceries). Columns (5)–(8) describe merchants in all other industries in the sample. Variable definitions are provided in Internet Appendix C. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles.

		Food Industry				Other Industries			
		mean	median	SD	N	mean	median	SD	N
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Direct Revenue									
Revenue	($)	722.0	291.7	1,266.9	351,189	1,415.9	551.8	2,570.7	621,649
Num of Orders	(#)	20	10	29	351,189	12	8	15	621,649
Order Size	($)	41.6	27.9	52.3	291,054	143.9	73.2	221.5	546,695
Pct of CC Orders	(%)	32.4	25.0	31.6	291,054	36.9	33.3	32.3	546,695
Pct of WL E-Comm Orders	(%)	4.6	0.0	19.7	291,054	7.3	0.0	23.3	546,695
WL E-Comm Revenue									
Revenue	($)	27.4	0.0	158.7	351,189	89.0	0.0	424.3	621,649
Num of Orders	(#)	1	0	3	351,189	1	0	3	621,649
Order Size	($)	93.6	36.3	214.8	23,235	237.5	94.7	453.6	70,808
Pct of CC Orders	(%)	41.4	30.0	41.5	23,235	11.0	0.0	26.8	70,808
Physical Revenue									
Revenue	($)	671.9	264.0	1,189.1	351,189	1,261.3	484.7	2,284.1	621,649
Num of Orders	(#)	19	10	28	351,189	11	7	13	621,649
Order Size	($)	40.1	27.5	47.6	281,514	137.6	71.9	203.3	522,827
Pct of CC Orders	(%)	32.2	23.9	31.7	281,514	39.0	33.3	32.9	522,827
Small Business Credit									
UCC: New Loan	(%)	7.8	0.0	26.9	351,189	10.1	0.0	30.2	621,649
UCC: Outstanding Loan	(%)	11.3	0.0	31.6	351,189	16.0	0.0	36.6	621,649

Table III. White-Label E-Commerce, Credit, and Direct Revenue Event Study

This table reports the results of event study regressions where the coefficient of interest is the interaction term between *WL E-Comm* and *Post* and where dependent variables are UCC loan variables (Columns (1)–(2)) and various proxies of direct revenue (Columns (3)–(6)). Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *WL E-Comm* (1/0) is an indicator that takes the value of 1 if a merchant engages in white-label e-commerce and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the first white-label e-commerce transaction and 24 months thereafter and 0 otherwise. The event window is $k = -24$ to $k = 24$. We randomly assign the first white-label e-commerce month $k = 0$ with the respective event window to the control group. Variable definitions are provided in Internet Appendix C. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	UCC Loans		Direct Revenue			
	New Loan (1/0)	Outstanding Loan (1/0)	Log Direct Revenue	Log Num of Orders	Log Order Size	Log Phys Revenue
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Food Industry						
WL E-Comm (1/0) x Post (1/0)	0.0120*	0.0141*	0.137***	0.0738**	0.0632***	0.0788**
	(1.67)	(1.94)	(4.20)	(2.38)	(6.84)	(2.47)
Post (1/0)	-0.00257	-0.00260	0.00977	0.00614	0.00370	0.00818
	(-1.04)	(-1.10)	(0.71)	(0.48)	(0.82)	(0.59)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	198,776	198,776	169,236	169,236	169,236	162,818
R^2	0.716	0.855	0.619	0.588	0.796	0.627
Panel B: Other Industries						
WL E-Comm (1/0) x Post (1/0)	0.0131**	0.00865*	0.134***	0.121***	0.0126	0.0470**
	(2.25)	(1.65)	(5.53)	(5.60)	(1.25)	(1.98)
Post (1/0)	-0.00344	-0.00182	0.00627	-0.00439	0.0102*	-0.00471
	(-1.60)	(-0.91)	(0.57)	(-0.46)	(1.94)	(-0.43)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	275,997	275,997	246,388	246,388	246,388	236,723
R^2	0.732	0.895	0.608	0.526	0.772	0.607

Table IV. Uber Eats Entry, E-Commerce, and Credit DiD

This table reports the results of difference-in-differences (DiD) regressions where the coefficient of interest is the interaction term between *Uber Eats* and *Post* and where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Panel A: Food Industry				
Uber Eats (1/0) x Post (1/0)	0.0300***	0.0848***	0.0145**	0.0178***
	(3.26)	(5.66)	(2.19)	(2.70)
Post (1/0)	-0.0115**	0.0764***	-0.00635*	-0.00637*
	(-2.21)	(8.50)	(-1.77)	(-1.72)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	141,997	141,997
R^2	0.758	0.764	0.697	0.863
Panel B: Other Industries				
Uber Eats (1/0) x Post (1/0)	-0.0107*	-0.0107*	-0.0126**	-0.00167
	(-1.68)	(-1.68)	(-2.46)	(-0.35)
Post (1/0)	-0.00435	-0.00435	0.00487	-0.000118
	(-1.14)	(-1.14)	(1.58)	(-0.04)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	279,500	279,500	279,500	279,500
R^2	0.852	0.852	0.698	0.897

Table V. Uber Eats Entry, E-Commerce, and Credit DDD

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *Food* and where dependent variables are measures of e-commerce and UCC credit to merchants. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats (1/0) x Post (1/0) x Food (1/0)	0.0407***	0.0955***	0.0271***	0.0195**
	(3.63)	(5.86)	(3.23)	(2.39)
Uber Eats (1/0) x Post (1/0)	-0.0107*	-0.0107*	-0.0126**	-0.00167
	(-1.68)	(-1.68)	(-2.46)	(-0.35)
Post (1/0)	-0.00435	-0.00435	0.00487	-0.000118
	(-1.14)	(-1.14)	(1.58)	(-0.04)
Post (1/0) x Food (1/0)	-0.00717	0.0808***	-0.0112**	-0.00625
	(-1.11)	(8.27)	(-2.37)	(-1.34)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	421,497	421,497	421,497	421,497
R^2	0.837	0.822	0.698	0.889

Table VI. Uber Eats Entry, E-Commerce, and Credit IV–DiD

This table reports the results of instrumented difference-in-differences (IV–DiD) regressions where the coefficient of interest is the interaction term between *Uber Eats* and *Post* instrumented with an interaction term between *UberX* and *Post* and where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. Column (1) presents the first-stage results. *UberX* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by UberX entry and 0 otherwise. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	First Stage	E-Commerce		UCC Loans	
	Uber Eats (1/0) x Post (1/0)	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)	(5)
Panel A: Food Industry					
UberX (1/0) x Post (1/0)	0.354***				
	(18.06)				
Uber Eats $\widehat{(1/0)}$ x Post (1/0)		0.0335	0.157***	0.0373*	0.0393*
		(1.15)	(3.17)	(1.67)	(1.82)
Post (1/0)	0.295***	-0.0128	0.0489**	-0.0150*	-0.0145*
	(32.94)	(-1.10)	(2.45)	(-1.76)	(-1.74)
Firm FEs	Yes	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	141,997	141,997	141,997
R^2	0.793	0.758	0.763	0.696	0.862
Panel B: Other Industries					
UberX (1/0) x Post (1/0)	0.298***				
	(22.17)				
Uber Eats $\widehat{(1/0)}$ x Post (1/0)		0.0315	0.0315	0.0194	0.0195
		(1.29)	(1.29)	(0.97)	(1.02)
Post (1/0)	0.353***	-0.0224**	-0.0224**	-0.00883	-0.00916
	(54.20)	(-2.06)	(-2.06)	(-1.00)	(-1.09)
Firm FEs	Yes	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes	Yes
# Observations	279,500	279,500	279,500	279,500	279,500
R^2	0.766	0.852	0.852	0.697	0.897

Table VII. Uber Eats Entry, Direct Revenue, and Market Entry and Exit DDD

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *Food* and where dependent variables are various proxies of direct revenue (Columns (1)–(4)) and measures of market entry and exit (Columns (5)–(6)). *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	Log Direct Revenue	Log Num of Orders	Log Order Size	Log Phys Revenue	Market Entry (1/0)	Market Exit (1/0)
	(1)	(2)	(3)	(4)	(5)	(6)
Uber Eats (1/0) x Post (1/0) x Food (1/0)	-0.0206	-0.0275	0.0143	0.00335	-0.00146	0.00380**
	(-0.50)	(-0.71)	(0.97)	(0.08)	(-1.08)	(2.29)
Uber Eats (1/0) x Post (1/0)	-0.0975***	-0.0848***	-0.0121	-0.105***	0.000585	-0.00215*
	(-4.78)	(-4.68)	(-1.39)	(-5.29)	(0.53)	(-1.66)
Post (1/0)	0.0362***	0.0303**	0.00609	0.0446***	-0.000308	-0.000217
	(2.63)	(2.56)	(0.92)	(3.25)	(-0.39)	(-0.27)
Post (1/0) x Food (1/0)	-0.0331	-0.0201	-0.0153	-0.0368	0.00188	0.000805
	(-1.31)	(-0.87)	(-1.51)	(-1.46)	(1.44)	(0.54)
Firm FEs	Yes	Yes	Yes	Yes	No	No
City–State FEs	No	No	No	No	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	361,034	361,034	361,034	349,241	421,667	421,667
R^2	0.648	0.577	0.824	0.645	0.011	0.012

Table VIII. Uber Eats Entry and Credit by E-Commerce Adoption Status

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *E-Comm Adopter* and where dependent variables are measures of UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *E-Comm Adopter* (1/0) is an indicator that takes the value of 1 if a merchant engaged in (white-label or app) e-commerce within 24 months of Uber Eats entry and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	Panel A: Food Industry		Panel B: Other Industries	
	New Loan (1/0)	Outstanding Loan (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats (1/0) x Post (1/0) x E-Comm Adopter (1/0)	-0.0146	0.00474	0.00189	0.0124
	(-1.04)	(0.34)	(0.16)	(1.18)
Uber Eats (1/0) x Post (1/0)	0.0170**	0.0141*	-0.0134**	-0.00594
	(2.20)	(1.76)	(-2.35)	(-1.08)
Post (1/0)	-0.0167***	-0.0101**	-0.00313	-0.00239
	(-4.01)	(-2.34)	(-0.80)	(-0.65)
Post (1/0) x E-Comm Adopter (1/0)	0.0313***	0.0114	0.0246***	0.00731
	(3.36)	(1.29)	(2.94)	(1.01)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	279,500	279,500
R^2	0.697	0.863	0.699	0.897

Table IX. Uber Eats Entry and Credit by Delivery App Adoption – Food Industry

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *App Adopter* and where dependent variables are measures of UCC credit to merchants. The sample only includes merchants in the food industry (restaurants & groceries). Columns (1)–(2) define adopters in terms of all major delivery apps (Uber Eats, Postmates, DoorDash, Order Online, Grubhub, and Seamless). Columns (3)–(4) define adopters as Uber Eats adopters specifically. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *App Adopter* (1/0) is an indicator that takes the value of 1 if a merchant adopted delivery app (Uber Eats) e-commerce within 24 months of Uber Eats entry and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	Delivery App Adoption		Uber Eats Adoption	
	New Loan (1/0)	Outstanding Loan (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats (1/0) x Post (1/0) x App Adopter (1/0)	-0.0289*	-0.00187	-0.0252	-0.00697
	(-1.85)	(-0.12)	(-1.33)	(-0.35)
Uber Eats (1/0) x Post (1/0)	0.0206***	0.0178**	0.0176**	0.0188***
	(2.68)	(2.36)	(2.42)	(2.64)
Post (1/0)	-0.0125***	-0.00742*	-0.0101***	-0.00728*
	(-3.12)	(-1.90)	(-2.66)	(-1.90)
Post (1/0) x App Adopter (1/0)	0.0288**	0.00492	0.0282*	0.00676
	(2.47)	(0.42)	(1.94)	(0.43)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	141,997	141,997
R^2	0.697	0.863	0.697	0.863

Table X. Uber Eats Entry, E-Commerce, and Credit by Firm and Product Type – Food Industry

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and a firm or product characteristic and where dependent variables are measures of e-commerce and UCC credit to merchants. The sample only includes merchants in the food industry (restaurants & groceries). Panels A, B, and C present the results by firm size, owner gender, and product type (unregulated vs. regulated), respectively. *Uber Eats* (1/0), *Post* (1/0), the analysis window, and the control group are the same as in Table IX. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. *t*-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Panel A: Small Firm				
Uber Eats (1/0) x Post (1/0) x Small (1/0)	0.00668	0.0681*	0.0480***	0.0536***
	(0.34)	(1.78)	(3.13)	(3.29)
Uber Eats (1/0) x Post (1/0)	0.0275**	0.0652***	0.00473	0.00638
	(2.57)	(3.93)	(0.65)	(0.89)
Post (1/0)	0.00257	0.0772***	-0.00170	-0.000945
	(0.44)	(8.00)	(-0.41)	(-0.22)
Post (1/0) x Small (1/0)	-0.0333***	0.0244	-0.0270***	-0.0262***
	(-2.89)	(0.95)	(-4.66)	(-3.18)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	123,233	123,233	123,233	123,233
R^2	0.744	0.730	0.684	0.854
Panel B: Female-Owned Firm				
Uber Eats (1/0) x Post (1/0) x Female (1/0)	0.0218	0.0373	0.0404**	0.0354**
	(1.08)	(1.17)	(2.53)	(2.19)
Uber Eats (1/0) x Post (1/0)	0.0235**	0.0734***	0.00222	0.00683
	(2.20)	(4.07)	(0.33)	(1.03)
Post (1/0)	-0.0143**	0.0733***	-0.00988**	-0.00746*
	(-2.22)	(6.67)	(-2.33)	(-1.70)
Post (1/0) x Female (1/0)	0.00840	0.00962	0.0110	0.00375
	(0.74)	(0.50)	(1.30)	(0.45)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	141,997	141,997
R^2	0.758	0.765	0.698	0.863
Panel C: Unregulated Food				
Uber Eats (1/0) x Post (1/0) x Non-Alcohol (1/0)	0.0156	0.0293	0.0379***	0.0371**
	(0.64)	(0.83)	(2.73)	(2.54)
Uber Eats (1/0) x Post (1/0)	0.0168	0.0622**	-0.0169	-0.0131
	(0.74)	(1.99)	(-1.45)	(-1.03)
Post (1/0)	-0.00161	0.0153	-0.00687	-0.00250
	(-0.11)	(0.87)	(-0.74)	(-0.29)
Post (1/0) x Non-Alcohol (1/0)	-0.0117	0.0715***	0.000455	-0.00470
	(-0.76)	(3.57)	(0.05)	(-0.49)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	141,997	141,997
R^2	0.758	0.765	0.697	0.863

Table XI. Uber Eats Entry and Credit DDD by Lender Type
Banks vs. Non-Banks

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *Food* and where dependent variables are measures of UCC credit to merchants by lender type. Columns (1)–(2) and (3)–(4) present the results for loans from banks and non-banks, respectively. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	Banks		Non-Banks	
	New Loan (1/0)	Outstanding Loan (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats (1/0) x Post (1/0) x Food (1/0)	0.00895	0.00726	0.0207***	0.0169**
	(1.53)	(1.27)	(3.02)	(2.35)
Uber Eats (1/0) x Post (1/0)	-0.00556	-0.000927	-0.00875**	-0.00398
	(-1.40)	(-0.24)	(-2.26)	(-1.00)
Post (1/0)	0.00231	0.000613	0.00395*	0.00147
	(0.97)	(0.28)	(1.69)	(0.62)
Post (1/0) x Food (1/0)	-0.000944	-0.00218	-0.00998**	-0.00625
	(-0.29)	(-0.71)	(-2.57)	(-1.53)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	421,497	421,497	421,497	421,497
R^2	0.688	0.905	0.685	0.858

Table XII. Uber Eats Entry and Credit DDD by Lender Type
Specialized Lenders

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *Food* and where dependent variables are measures of UCC credit to merchants by lender type. Columns (1)–(2), (3)–(4), and (5)–(6) present the results for loans from FinTechs, non-bank FinTechs, and equipment finance lenders, respectively. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	FinTechs		Non-Bank FinTechs		Equipment Finance	
	New Loan (1/0)	Outstanding Loan (1/0)	New Loan (1/0)	Outstanding Loan (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)	(5)	(6)
Uber Eats (1/0) x Post (1/0) x Food (1/0)	0.0121**	0.00956*	0.00654*	0.00482	0.0104**	0.00932**
	(2.36)	(1.84)	(1.80)	(1.26)	(2.33)	(2.03)
Uber Eats (1/0) x Post (1/0)	-0.00589*	-0.00108	-0.00316	-0.000345	-0.00669**	-0.00328
	(-1.69)	(-0.32)	(-1.35)	(-0.14)	(-2.13)	(-1.06)
Post (1/0)	0.000850	-0.00192	-0.000382	-0.00210	0.00411**	0.00286
	(0.42)	(-0.98)	(-0.29)	(-1.45)	(2.12)	(1.52)
Post (1/0) x Food (1/0)	-0.00363	-0.000973	-0.00141	0.000832	-0.00500*	-0.00420
	(-1.36)	(-0.35)	(-0.78)	(0.43)	(-1.93)	(-1.59)
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes	Yes	Yes
# Observations	421,497	421,497	421,497	421,497	421,497	421,497
R^2	0.694	0.871	0.709	0.854	0.676	0.851

Appendix A. Uber Eats and Other Delivery App Entry

In this Appendix, we provide additional details on Uber Eats entry and describe how we construct our Uber Eats shocks, which we use to identify the causal effects of e-commerce on credit.

In March 2016, Uber launched Uber Eats as a standalone food delivery app in Los Angeles, CA, and expanded it to other major U.S. cities, such as Chicago, Houston, and San Francisco, within weeks.[18] Uber Eats was not the only food delivery platform at the time. Grubhub and DoorDash also expanded across U.S. cities during our sample period, and we use their entry in robustness tests (Section IV.C). What distinguished Uber Eats was the speed and breadth of its roll-out. Because it relied on Uber's existing network of drivers and riders, Uber Eats could offer restaurants and grocery stores (e.g., food marts, wineries) immediate access to a large delivery infrastructure and consumer base in each city it entered, sharply lowering the costs of selling online. Importantly, the timing of Uber Eats entry was determined by Uber's expansion strategy and its existing ride-sharing footprint rather than by the characteristics of local restaurants.

We hand-collect Uber Eats entry dates from announcements posted in the Uber Newsroom. Each announcement introduces the platform's launch in a new city, typically with titles such as "Orlando, Uber Eats is here" and content like "Starting today, you can use the Uber Eats app—available on iOS and Android—to order delivery from the full menus of your favorite restaurants." The complete list of cities and their corresponding entry dates is provided in Table A.I. Note that in some announcements, Uber Eats launched in a broad region rather than a single city—such as the South Bay Area in California, Tampa Bay in Florida, and the Main Line in Pennsylvania—indicating that the service became available across most cities within these regions. Therefore, we manually check which cities are included in these regional entries.

It is noteworthy that these roll-out dates mark the first collaborations between Uber Eats and local restaurants. However, entry in a particular city does not imply that only residents within the city boundaries have access to the service; rather, consumers in surrounding areas can also place orders through the app. Therefore, Uber Eats entry affected the food industry not only within the announced cities but also in broader regions included in Uber Eats' delivery coverage. To account for these broader impacts, we assign Uber Eats roll-out dates to cities in which restaurants in our sample are located by mapping them to Uber Eats entry cities within the same Metropolitan Statistical Area (MSA) and within a driving distance of 25 miles.[19] Restricting the mapping to the same MSA ensures that we capture economically integrated areas where consumers are more

[18] See, e.g., https://www.uber.com/newsroom/la-the-new-ubereats-app-is-arriving-now/ and https://www.uber.com/newsroom/ubereats-app/.

[19] We rely on Uber Eats' own delivery cut-off. See https://help.uber.com/en/merchants-and-restaurants/article/how-can-i-customize-delivery-fee-and-minimum-order-for-multiple-zones?nodeId=1b7d1043-24de-4d35-901f-610bc9d7fe7d.

Table A.I. Uber Eats Cities and Roll-out Dates

This table reports the Uber Eats roll-out schedule. The data are from https://www.uber.com/newsroom.

City	Date	City	Date	City	Date
Los Angeles, CA	3/1/2016	Marin County, CA	8/25/2016	Main Line, PA	2/13/2017
San Francisco, CA	3/15/2016	Fort Worth, TX	8/26/2016	Athens, GA	2/15/2017
Houston, TX	3/15/2016	Stamford, CT	9/7/2016	Hudson, NJ	2/15/2017
Chicago, IL	3/15/2016	Baltimore, MD	9/7/2016	Milwaukee, WI	2/16/2017
Atlanta, GA	3/22/2016	Nashville, TN	9/7/2016	San Antonio, TX	3/1/2017
New York, NY	3/22/2016	Tampa Bay, FL	9/7/2016	Richmond, VA	3/16/2017
Dallas, TX	3/22/2016	Las Vegas, NV	9/7/2016	Memphis, TN	4/13/2017
Washington, DC	3/22/2016	Cleveland, OH	9/21/2016	Hackensack, NJ	4/18/2017
Arlington, VA	4/22/2016	Cincinnati, OH	9/21/2016	Englewood, NJ	4/18/2017
Alpharetta, GA	4/27/2016	Twin Cities, MN	9/29/2016	Teaneck, NJ	4/18/2017
Roswell, GA	4/27/2016	Huntington Beach, CA	9/30/2016	Fort Lee, NJ	4/18/2017
San Diego, CA	6/14/2016	Long Beach, CA	9/30/2016	Boston, MA	4/26/2017
Williamsburg, NY	7/6/2016	Denver, CO	10/7/2016	Pittsburgh, PA	5/4/2017
Greenpoint, NY	7/6/2016	Ft. Lauderdale, FL	10/14/2016	Colorado Springs, CO	6/14/2017
Miami, FL	7/12/2016	New Orleans, LA	11/1/2016	Tucson, AZ	8/7/2017
Phoenix, AZ	7/12/2016	Orlando, FL	11/2/2016	San Marcos, TX	9/6/2017
Orange County, CA	7/29/2016	Minneapolis, MN	11/3/2016	Kansas City, MO	9/25/2017
South Bay Area, CA	8/9/2016	Seattle, WA	11/8/2016	Santa Barbara, CA	10/10/2017
Philadelphia, PA	8/15/2016	Portland, OR	11/14/2016	St. Louis, MO	10/18/2017
New Haven, CT	8/16/2016	Long Island, NY	1/26/2017		
Columbus, OH	8/16/2016	Westchester, NY	1/26/2017		

likely to cross city boundaries for dining and delivery. By contrast, cities in different MSAs may be separated by commuting patterns or geographic constraints that limit cross-city interactions, making delivery coverage implausible even within a 25-mile range.

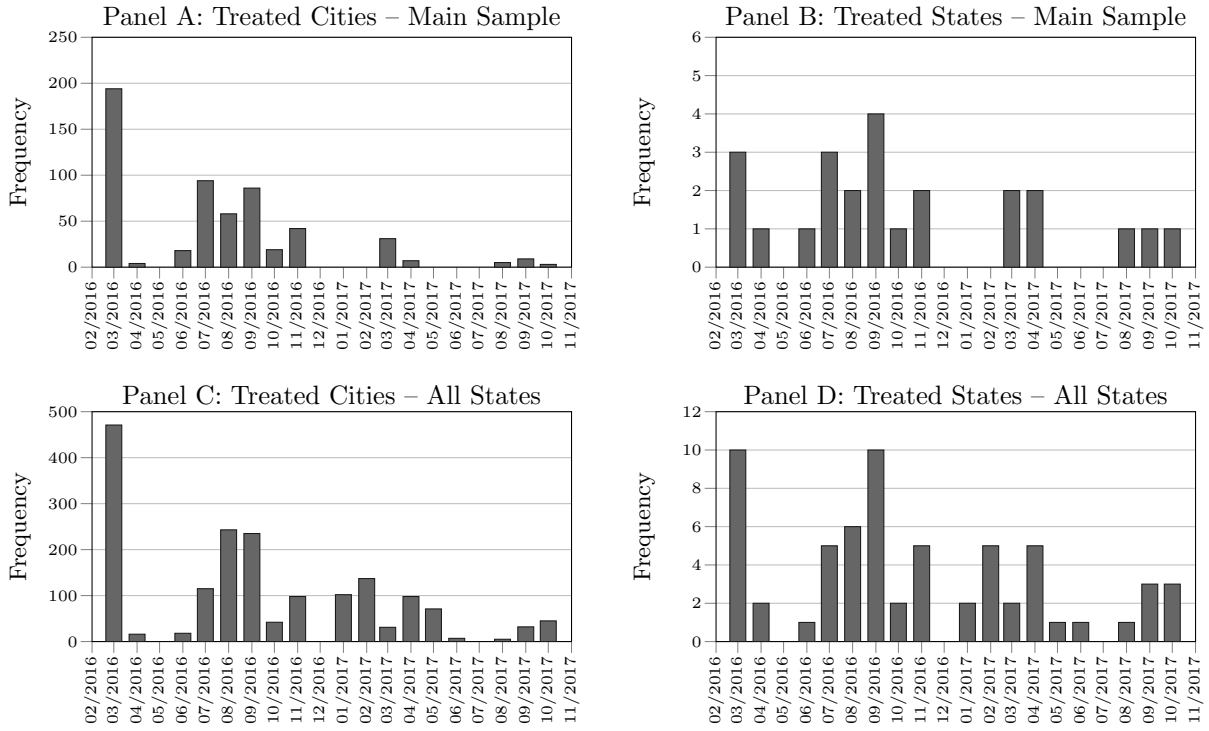
We use the following procedure to map merchant cities to Uber Eats cities. We first assign cities to their corresponding counties using the link file provided by UnitedStatesZIPCodes.org. We then map counties to MSAs based on the 2015 Delineation File of *Core Based Statistical Areas (CBSAs), Metropolitan Divisions, and Combined Statistical Areas (CSAs)* published by the U.S. Census Bureau. We rely on the 2015 historical file rather than the most recent 2023 version because the 2015 delineation better reflects the geographic definitions relevant at that time of Uber Eats entry (starting March 2016). We apply this mapping procedure to both the cities in our main sample and Uber Eats entry cities, allowing us to place both sets of cities within MSAs. In total, we are able to link 3,156 cities across 43 MSAs in 31 states to Uber Eats entry cities.

After placing both merchant cities and Uber Eats entry cities within the same MSAs, we compute the driving distance between these cities using the Google Directions API. This service operates much like Google Maps: It can directly recognize city names entered as input and automatically generate the driving route between them. A distinctive feature of the API is that it relies on Google's real-time routing algorithm, which incorporates the actual road network. When the origin and destination are supplied as city names, the API first geocodes each city to a single

representative latitude/longitude point from Google database. These points serve as the start and end coordinates of the route. The API then calculates the driving distance between the cities by returning the length of the road network route between coordinates of central points of cities in our sample and corresponding Uber Eats entry cities within the same MSA.[20]

Cities located within the 25-mile driving distance of an Uber Eats entry city in the same MSA are assigned the corresponding Uber Eats entry date. If a city is linked to multiple Uber Eats entry cities, we assign the earliest entry date among them. Following this procedure, a total of 1,766 cities receive a roll-out date, which we treat as an Uber Eats entry shock to local restaurants and groceries. Fig. A.1 provides a timeline of Uber Eats shocks, for individual cities and states. We note instances of multiple treated cities within the same state. We randomly assign a placebo Uber Eats entry month to the control group of merchants located in cities without Uber Eats entry.

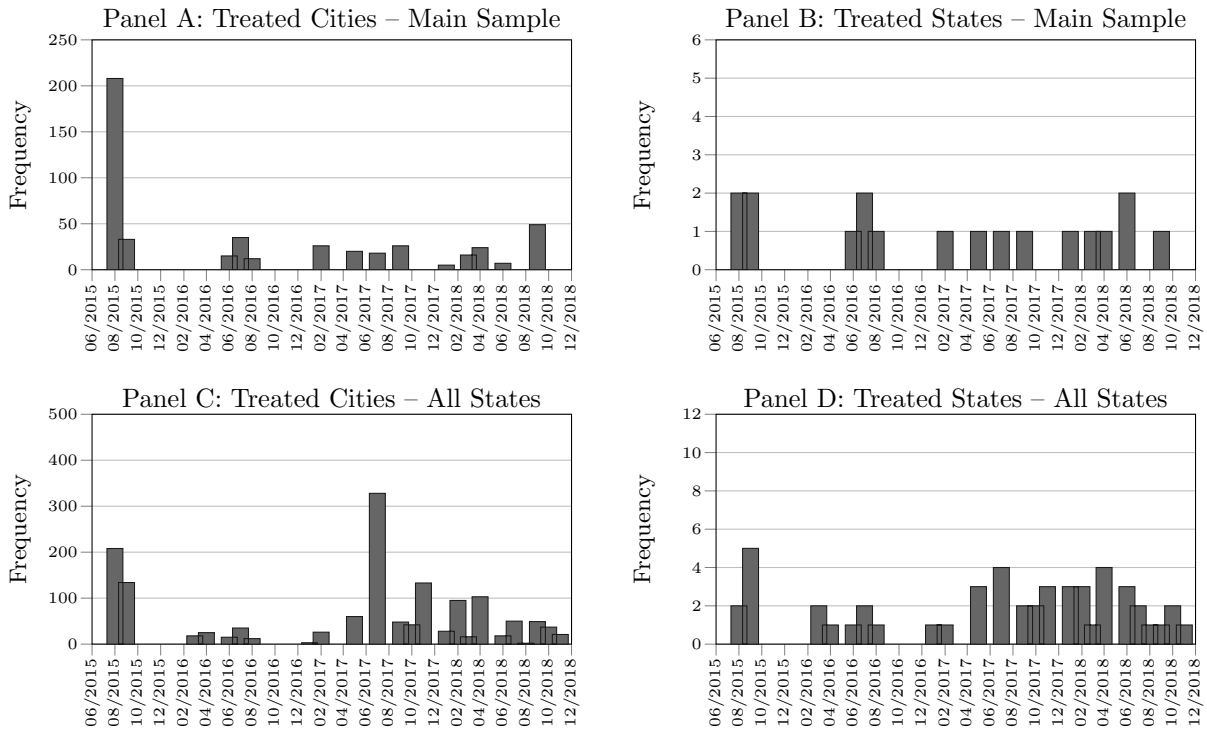
Figure A.1. Timeline of Uber Eats Entry



These figures illustrate the timeline of Uber Eats entry in the U.S. Panels A and C report the number of cities that Uber Eats entered in each month. If cities are treated several times, we only keep the earliest shock. Panels B and D report the respective number of states. Since we define shocks at the city level, the same state can be treated several times.

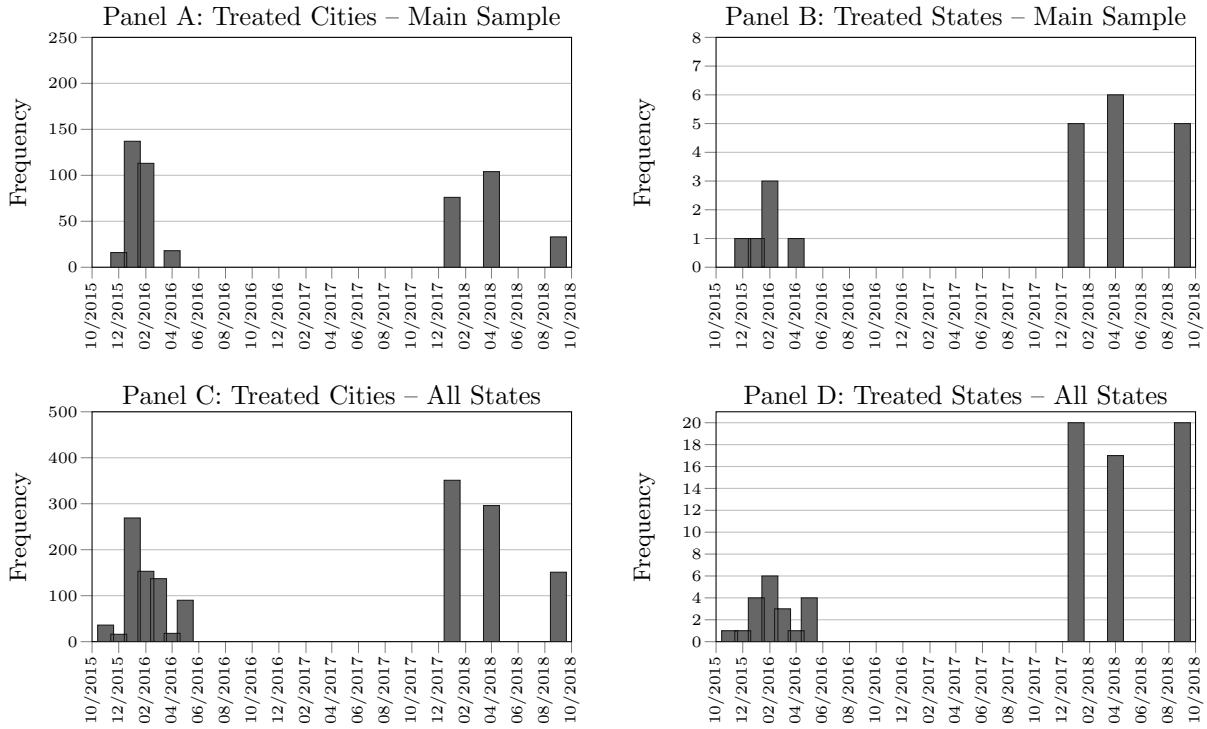
[20] See https://developers.google.com/maps/documentation/geocoding/overview and https://developers.google.com/maps/documentation/directions/overview.

Figure A.2. Timeline of DoorDash Entry



These figures illustrate the timeline of DoorDash entry in the U.S. Panels A and C report the number of cities that DoorDash entered in each month. If cities are treated several times, we only keep the earliest shock. Panels B and D report the respective number of states. Since we define shocks at the city level, the same state can be treated several times.

Figure A.3. Timeline of Grubhub Entry



These figures illustrate the timeline of Grubhub entry in the U.S. Panels A and C report the number of cities that Grubhub entered in each month. If cities are treated several times, we only keep the earliest shock. Panels B and D report the respective number of states. Since we define shocks at the city level, the same state can be treated several times.

Appendix B. Ghost Kitchen Entry

We use the opening of ghost kitchens across U.S. cities to test whether Uber Eats entry has led to the future expansion of online food ordering through facilities that are set up exclusively for delivery and online orders, without traditional dine-in space or storefront.

We start by forming a list of major ghost kitchen operators in the U.S. We focus on operators that offer fully equipped commercial kitchen spaces for rent to delivery-only restaurant brands. Examples include Cloud Kitchens, REEF Kitchens, Kitchen United, Ghost Kitchen Brands, Nextbite, Nimbus Kitchens, ChefSuite, Unfurl, C3 (Creating Culinary Communities), and others. We do not include operators that help restaurants monetize their unused kitchen capacity by creating their own virtual brands or partner with existing restaurants to offer restaurants' own menus through their kitchen space (e.g., WowBao, Zuul, Franklin Junction). We then use the official websites of these operators, delivery and travel apps (Uber Eats, DoorDash, Grubhub, Postmates, Trip Advi-

Table B.I. Ghost Kitchen Cities and Opening Dates

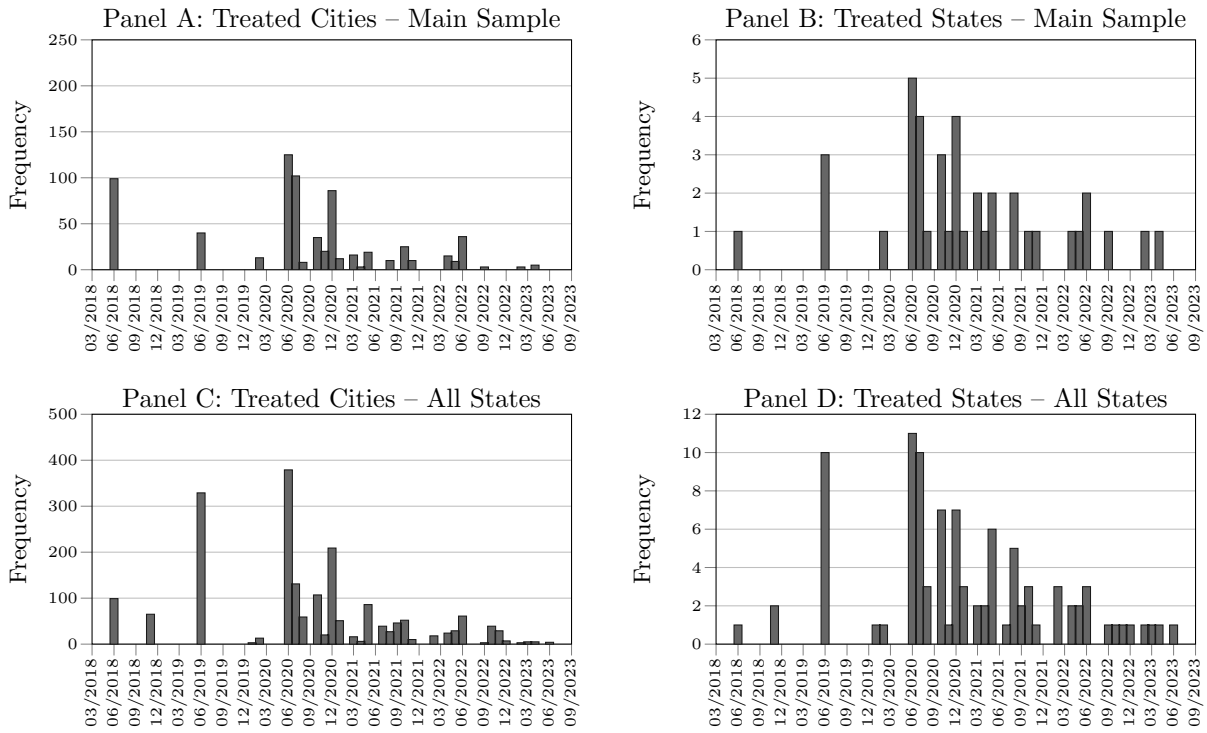
This table reports the ghost kitchen opening schedule. The data are hand-collected from multiple sources.

City	Month	City	Month	City	Month	City	Month
Los Angeles, CA	Jun-18	Saint Augustine, FL	Jul-20	Dallas, TX	Dec-20	Chester, PA	Feb-22
Pasadena, CA	Jun-18	Venice, CA	Jul-20	Detroit, MI	Dec-20	South Salt Lake City, UT	Apr-22
Chicago, IL	Nov-18	Virginia Beach, VA	Jul-20	Nashville, TN	Dec-20	Sherman Oaks, CA	Apr-22
Loves Park, IL	Jun-19	Sacramento, CA	Jul-20	Seattle, WA	Dec-20	Indianapolis, IN	May-22
Duluth, GA	Jun-19	Denver, CO	Jul-20	Long Beach, CA	Jan-21	Frisco, TX	May-22
Washington, DC	Jun-19	San Diego, CA	Jul-20	Providence, RI	Jan-21	Plano, TX	May-22
Corvallis, OR	Jun-19	Springfield, IL	Jul-20	Santa Clara, CA	Feb-21	Milwaukee, WI	Jun-22
Yucaipa, CA	Jun-19	Desert Hot Springs, CA	Jul-20	Anaheim, CA	Mar-21	Richmond, VA	Jun-22
New Brunswick, NJ	Jun-19	Toledo, OH	Jul-20	Chantilly, VA	Mar-21	Fort Lauderdale, FL	Jun-22
Philadelphia, PA	Jun-19	Wellington, FL	Jul-20	Las Vegas, NV	Apr-21	Union City, NJ	Aug-22
Vineland, NJ	Jan-20	Norfolk, VA	Jul-20	San Mateo, CA	Apr-21	Redwood City, CA	Sep-22
Scottsdale, AZ	Feb-20	Houston, TX	Jul-20	Lorton, VA	May-21	Salt Lake City, UT	Sep-22
New York City, NY	Jun-20	Gurnee, IL	Aug-20	Riverhead, NY	May-21	Santa Ana, CA	Sep-22
Gainesville, FL	Jun-20	Vernon Hills, IL	Aug-20	Raleigh, NC	May-21	Cleveland, OH	Oct-22
Austin, TX	Jun-20	Estero, FL	Aug-20	Brooklyn, NY	May-21	Culver City, CA	Oct-22
Riverdale, GA	Jun-20	San Francisco, CA	Oct-20	Mount Pleasant, SC	May-21	Clintonville, OH	Nov-22
Aurora, CO	Jun-20	Phoenix, AZ	Oct-20	New Orleans, LA	May-21	Columbus, OH	Nov-22
San Jose, CA	Jun-20	Streamwood, IL	Oct-20	Tampa, FL	May-21	Gahanna, OH	Nov-22
Oklahoma City, OK	Jun-20	Fort Wayne, IN	Oct-20	Minneapolis, MN	Jul-21	Dublin, OH	Nov-22
Fairburn, GA	Jun-20	Mankato, MN	Oct-20	San Clemente, CA	Aug-21	Boise, ID	Dec-22
Silver Spring, MD	Jun-20	Atlanta, GA	Oct-20	Blaine, MN	Aug-21	Decatur, GA	Dec-22
Daytona Beach, FL	Jun-20	San Antonio, TX	Oct-20	Mesa, AZ	Aug-21	Doral, FL	Dec-22
Oakland, CA	Jun-20	Des Plaines, IL	Oct-20	Rye, NY	Aug-21	St. Louis, MO	Feb-23
Edison, NJ	Jun-20	Staten Island, NY	Oct-20	Belleville, NJ	Sep-21	Hayward, CA	Feb-23
Miami, FL	Jun-20	Manhattan Beach, CA	Oct-20	Rochester, NY	Sep-21	Santa Monica, CA	Mar-23
Sterling, VA	Jun-20	Buffalo Grove, IL	Oct-20	Charlotte, NC	Oct-21	Fishers, IN	Mar-23
East Elmhurst, NY	Jun-20	Homewood, IL	Oct-20	Long Island City, NY	Oct-21	Carmel, IN	Mar-23
Chandler, AZ	Jun-20	Tempe, AZ	Nov-20	High Point, NC	Oct-21	Costa Mesa, CA	Mar-23
Bronx, NY	Jun-20	St. Petersburg, FL	Nov-20	Fort Worth, TX	Oct-21	Fleming Island, FL	Apr-23
Missouri City, TX	Jul-20	Baltimore, MD	Dec-20	Farmers Branch, TX	Nov-21	Lawndale, CA	May-23
Woodside, NY	Jul-20	Portland, OR	Dec-20	Astoria, NY	Nov-21	Batavia, NY	Jun-23
Anchorage, AK	Jul-20	Orlando, FL	Dec-20	New York, NY	Dec-21		

sor etc.), Google Maps, news releases, and Wayback Machine Internet archives for a comprehensive search of ghost kitchen locations owned by these major operators.

We manually gather the date when each ghost kitchen was opened by searching for announcements on operators' websites, in local media, and on platforms like Facebook or Instagram. Whenever several ghost kitchens were opened in the same city at different times, we keep the earliest of these dates. Filtering for facilities that were opened before the end of our sample yields a final sample of 226 ghost kitchens. The list of cities where these ghost kitchens are located and their corresponding opening dates is provided in Table B.I. Fig. B.1 provides a timeline of ghost kitchen entry, for individual cities (Panel A) and states (Panel B).

Figure B.1. Timeline of Ghost Kitchen Entry



These figures illustrate the timeline of ghost kitchen entry in the U.S. Panels A and C report the number of cities that ghost kitchen operators entered in each month. If cities are treated several times, we only keep the earliest shock. Panels B and D report the respective number of states. Since we define shocks at the city level, the same state can be treated several times.

Internet Appendix to

"Digitally Inclined Small Businesses: Effects of E-Commerce on Small Business Credit"

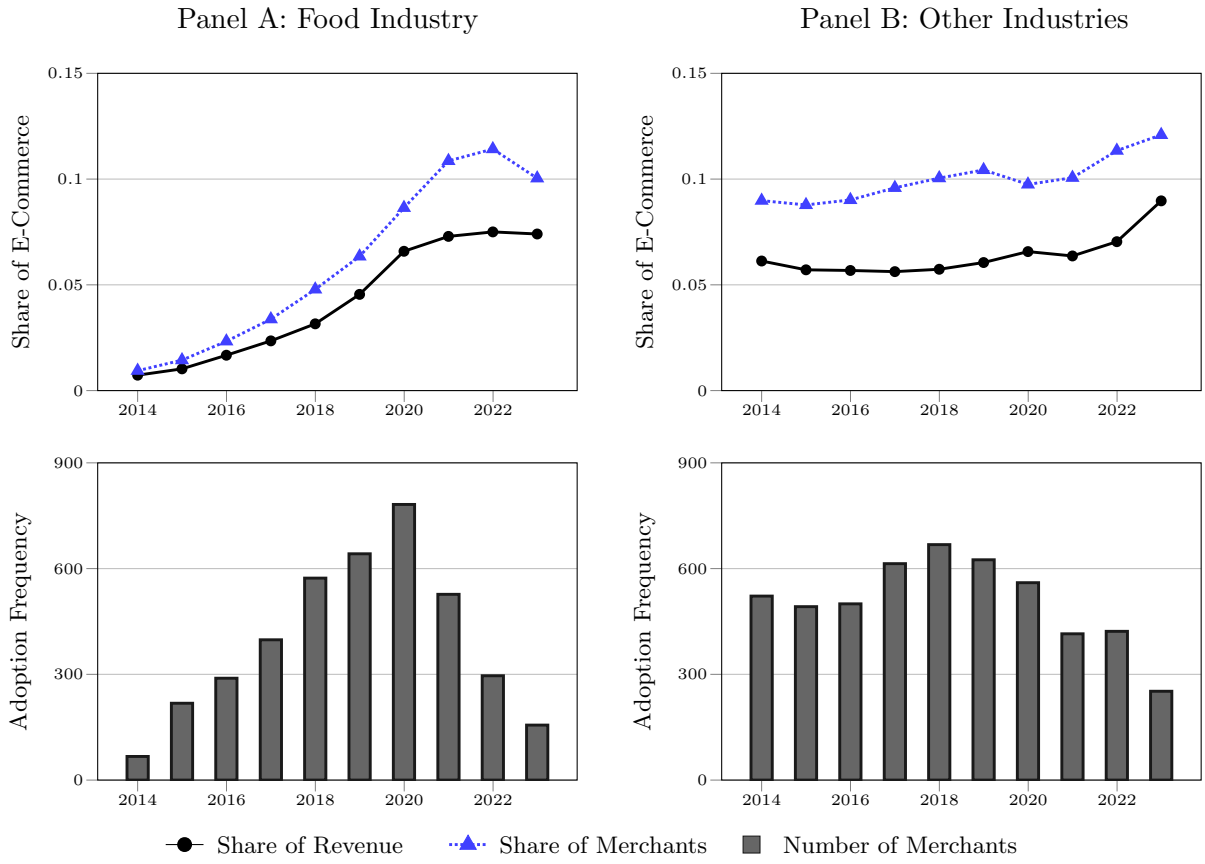
Tetyana Balyuk Zhishuo Han

October 8, 2026

FOR ONLINE PUBLICATION

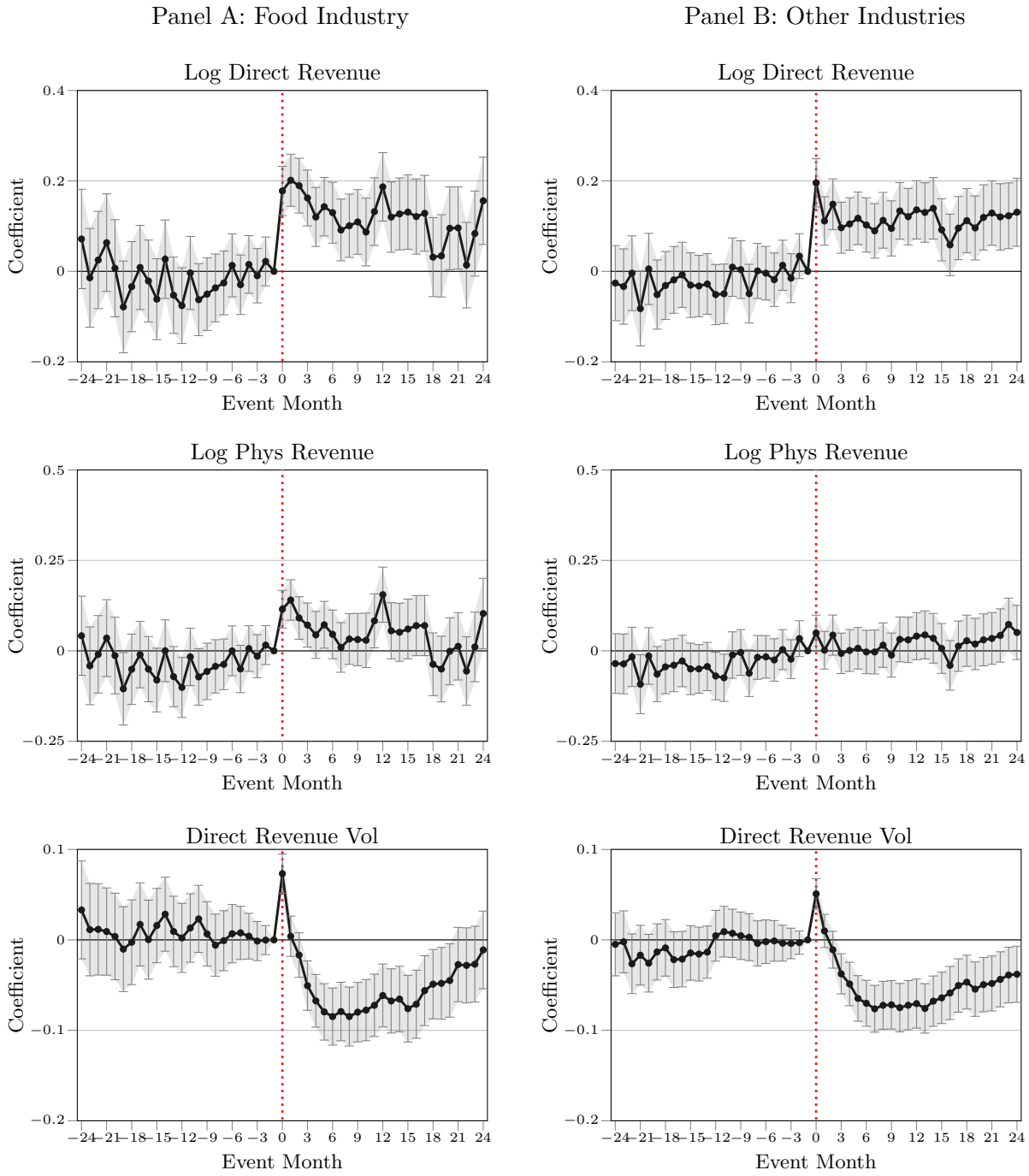
A. Supplementary Figures

Figure IA.1. Trends in E-Commerce Adoption – All States



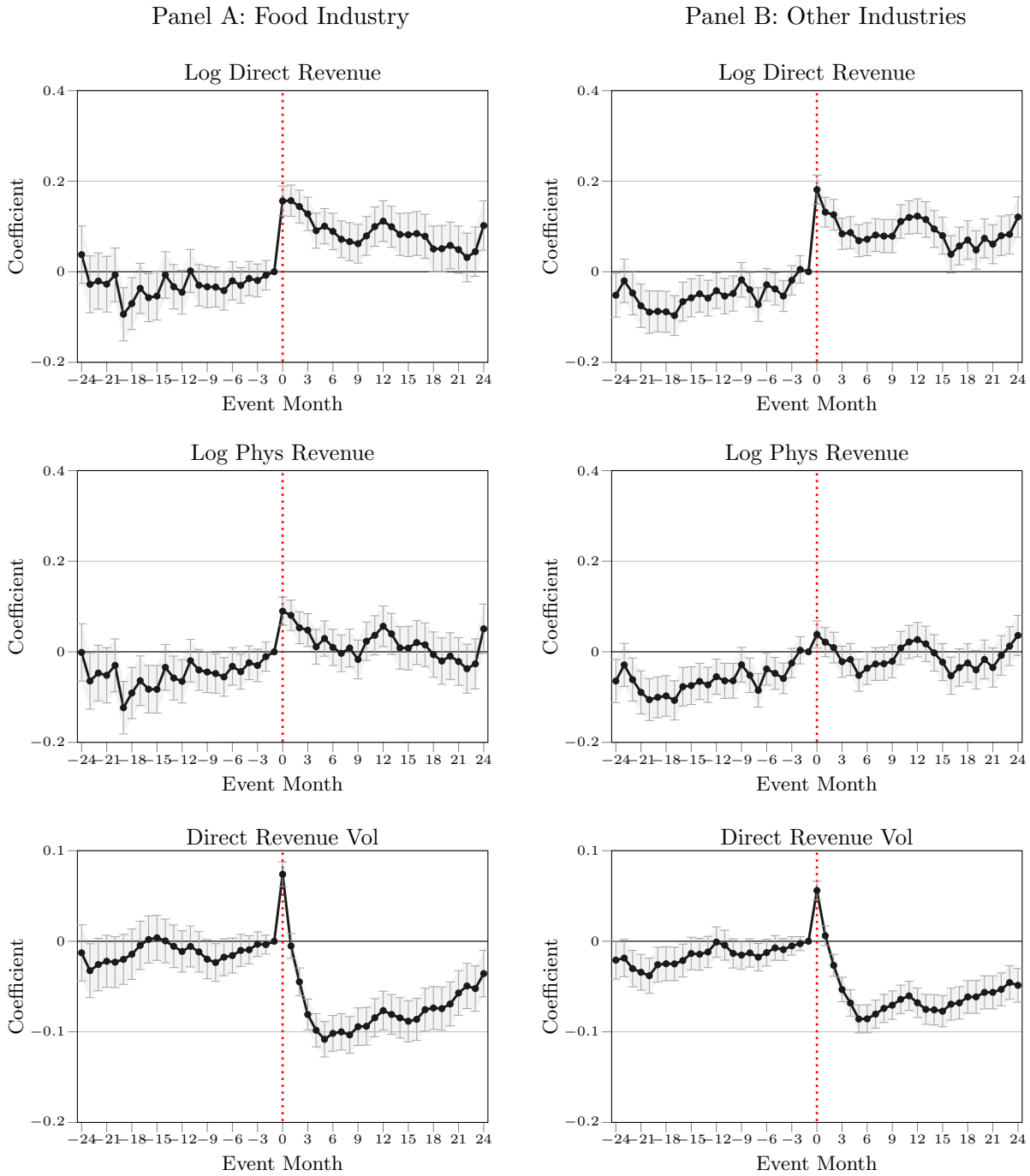
These figures illustrate trends in e-commerce adoption over time. We include merchants in all states in our data, after restrictions described in the main text. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. We measure e-commerce adoption in three ways: the share of transaction amounts coming from e-commerce (black solid line), share of merchants offering e-commerce in each year (blue dotted line), and the number of merchants with their first e-commerce transaction in each year. The 2023 adoption frequency is scaled up by a factor of 365/178 to account for our sample ending on June 28, 2023 rather than the end of year.

Figure IA.2. E-Commerce and Revenue Event Study



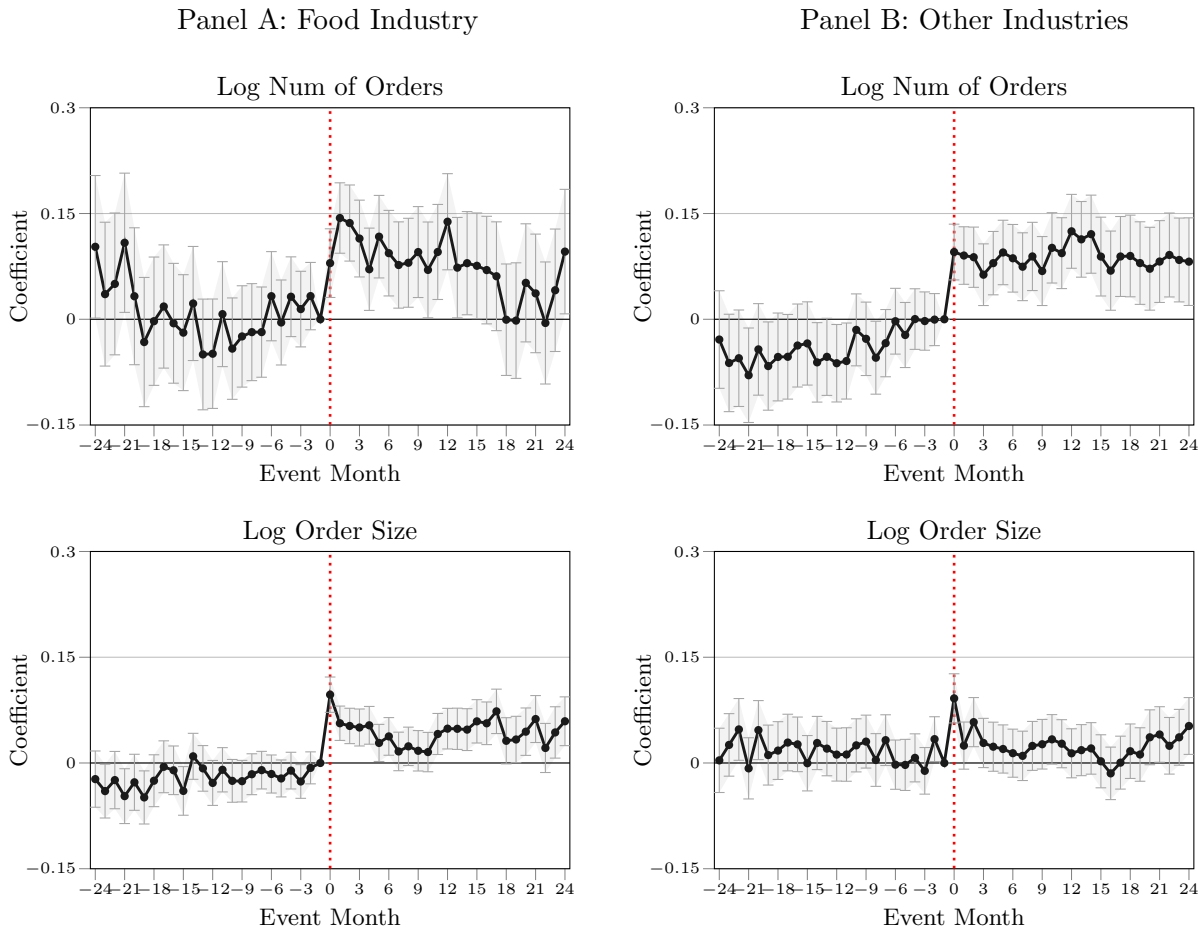
These figures report coefficient estimates for interaction terms between *Treated* and *Event Month* dummies, for main measures of merchant revenue and revenue volatility. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Treated* (1/0) is an indicator that takes the value of 1 if a merchant engages in e-commerce and 0 otherwise. Event Month $k = 0$ is the month of the first e-commerce transaction for the treatment group. The event window is $k = -24$ to $k = 24$, with the reference month $k = -1$. We randomly assign $k = 0$ with the respective event window to the control group. The error bars and shaded areas represent 90% confidence intervals. Standard errors are clustered at the merchant level.

Figure IA.3. E-Commerce and Revenue Event Study – All States



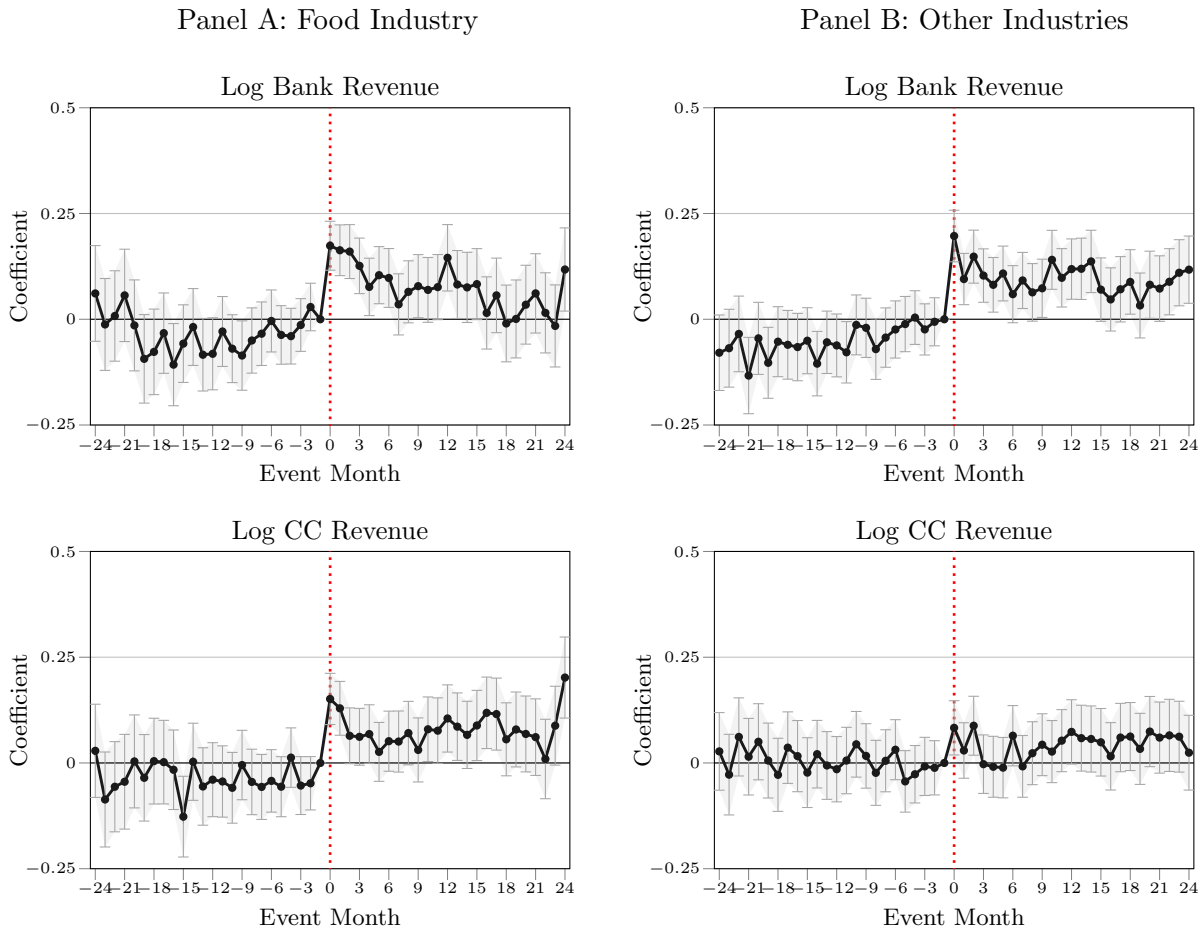
These figures report coefficient estimates for interaction terms between *Treated* and *Event Month* dummies, for for main measures of merchant revenue and revenue volatility. We include merchants in all states in our data, after restrictions described in the main text. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.4. E-Commerce and Revenue Event Study
Number of Orders vs. Order Size



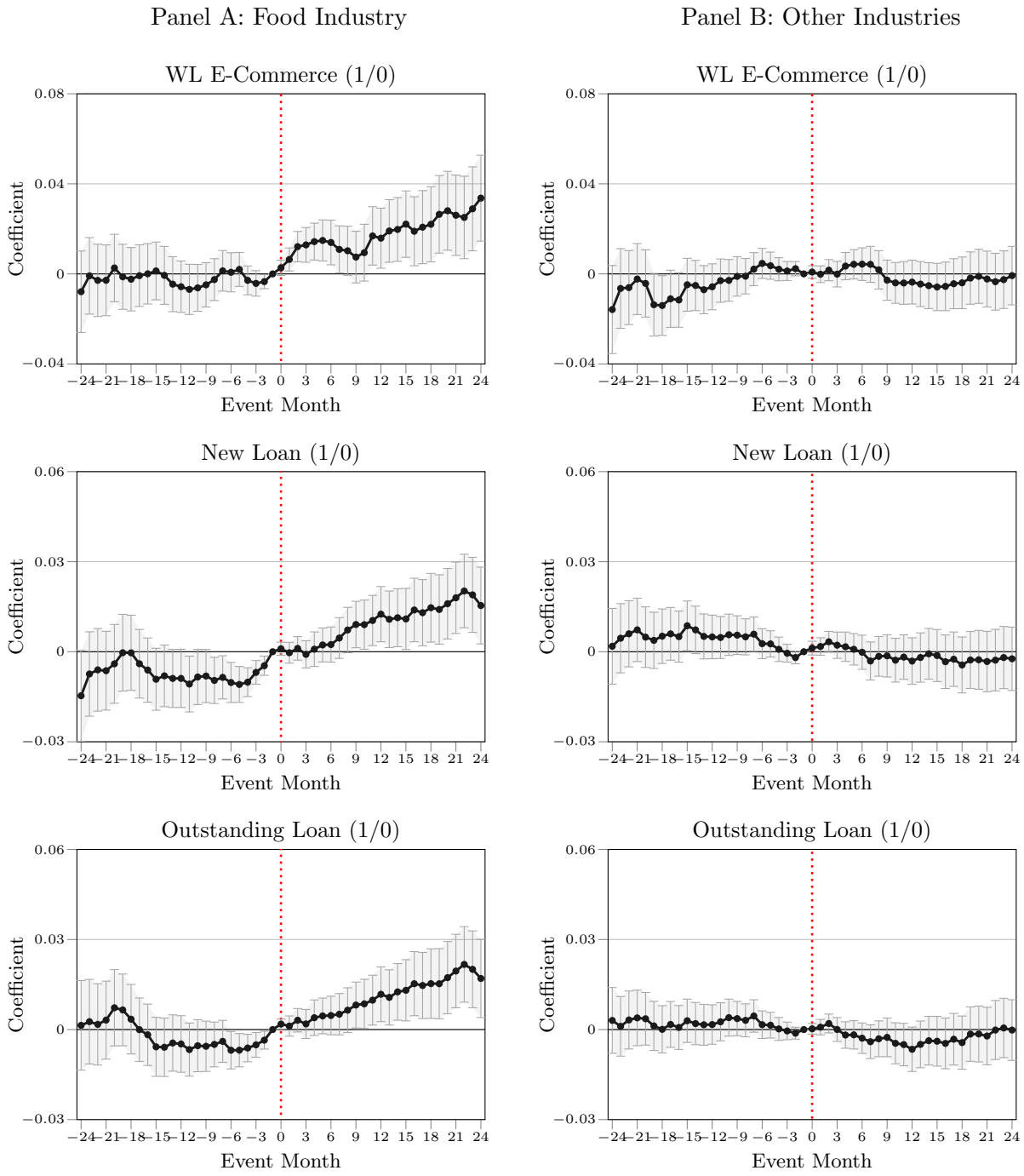
These figures report coefficient estimates for interaction terms between *Treated* and *Event Month* dummies, for additional measures of merchant revenue. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.5. E-Commerce and Revenue Event Study
Bank Account vs. Credit Card Revenue



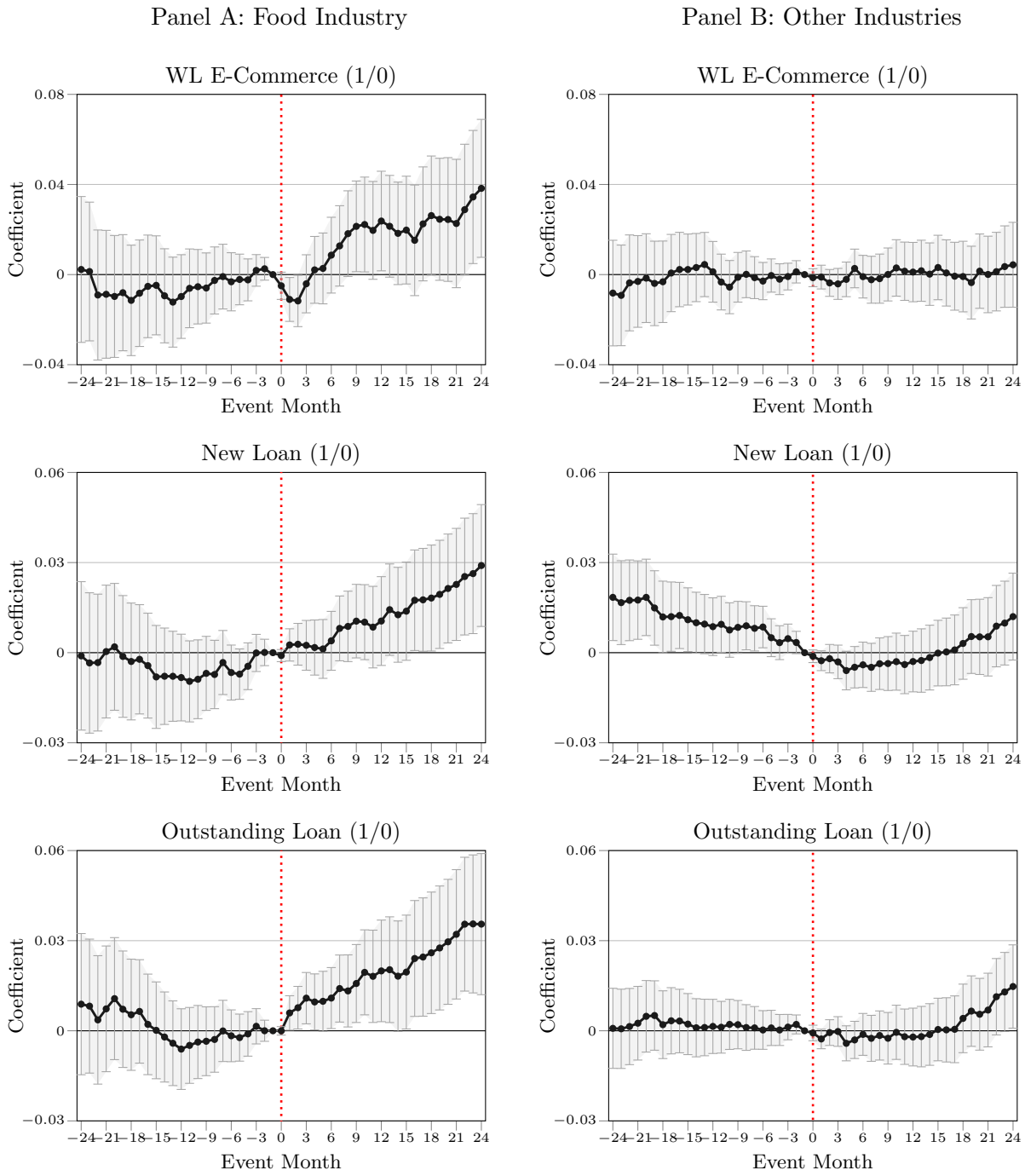
These figures report coefficient estimates for interaction terms between *Treated* and *Event Month* dummies, for the revenue received from bank account versus credit card transactions. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.6. Delivery App Entry, WL E-Commerce, and UCC Loan DiD



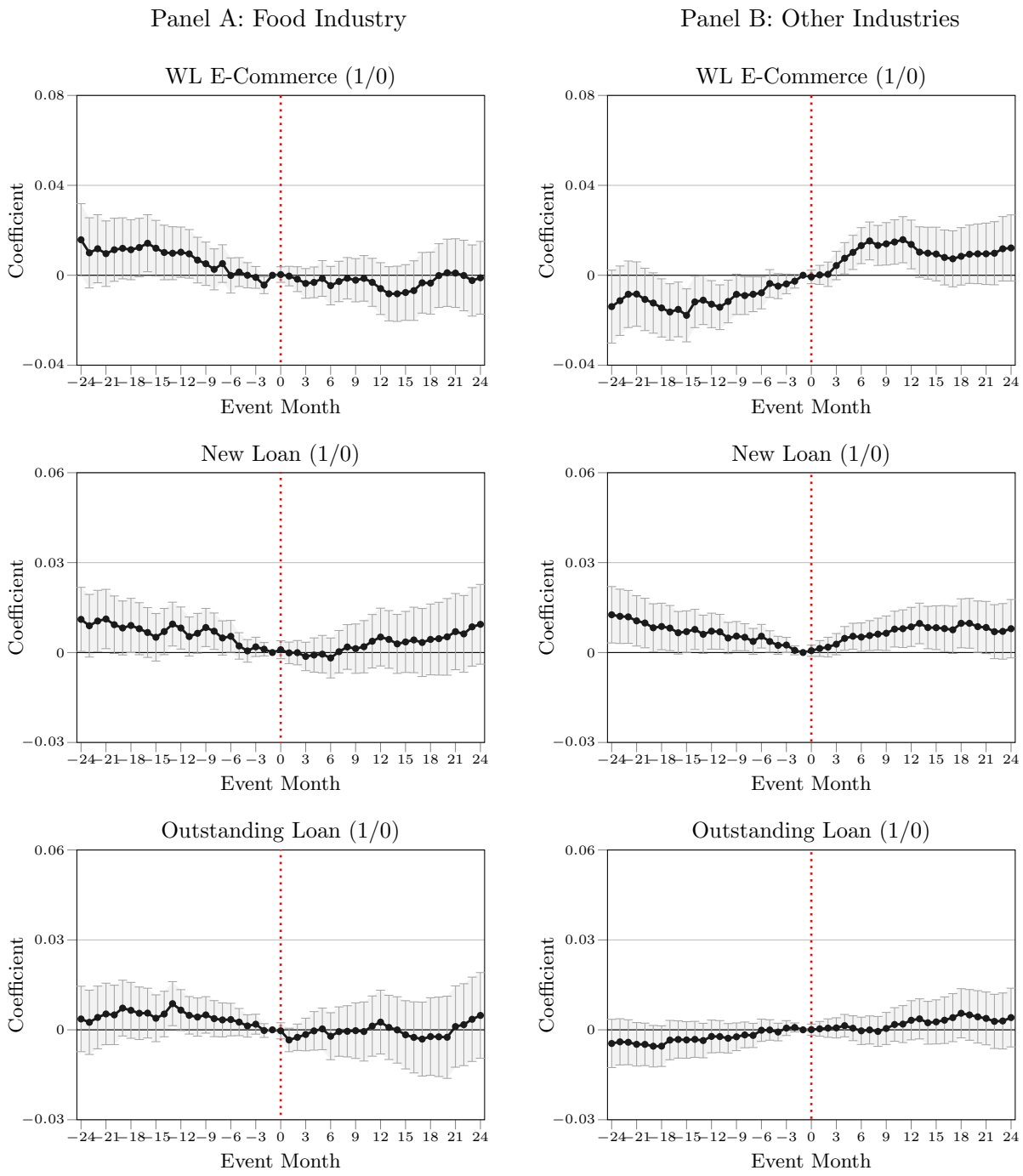
These figures report coefficient estimates for interaction terms between *Delivery App* and *Delivery App Month* dummies, where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.7. Uber Eats Entry, WL E-Commerce, and UCC Loan DiD
Robustness to Propensity Score Matching



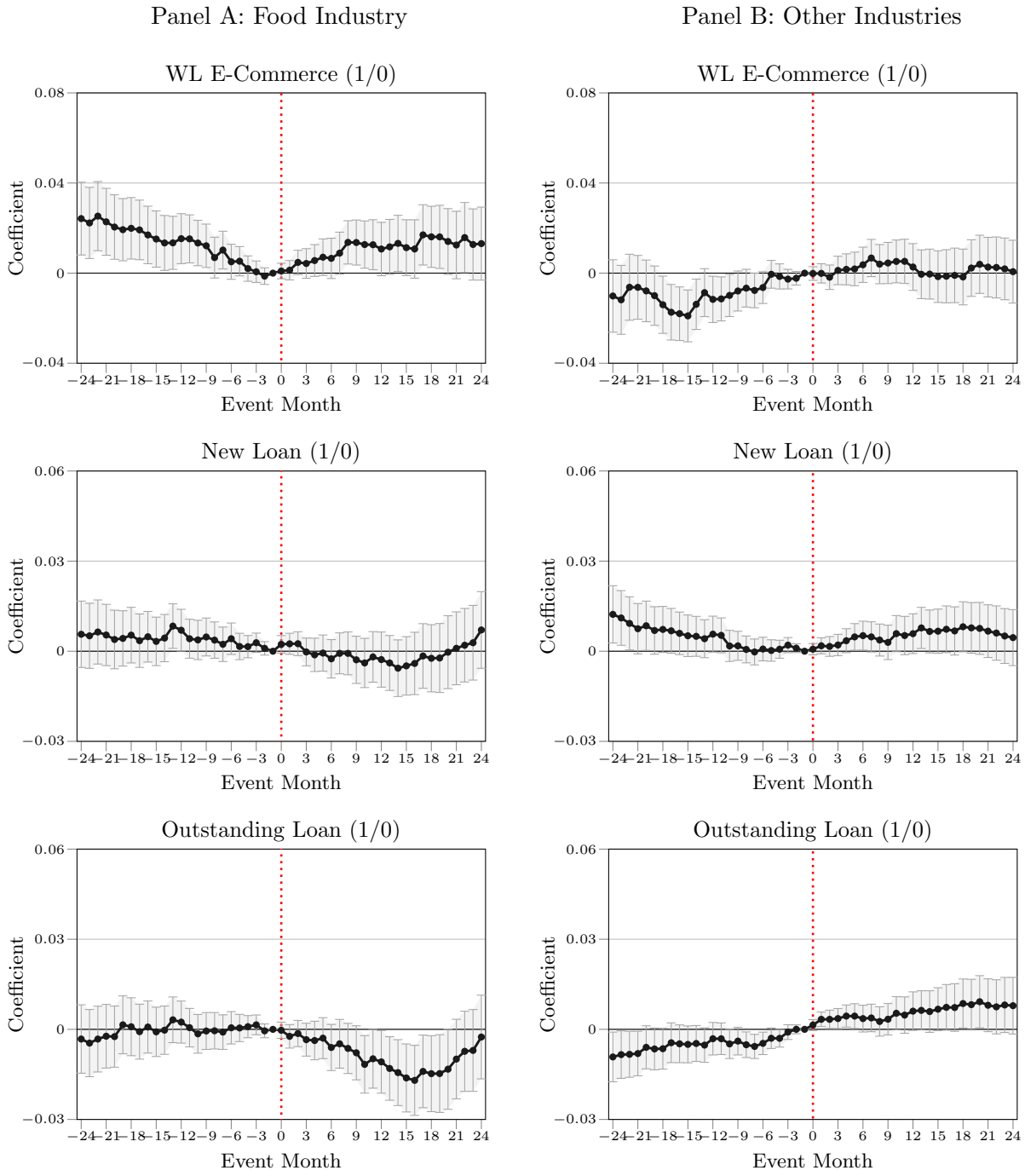
These figures report coefficient estimates for interaction terms between *Uber Eats* and *Uber Eats Month* dummies, where dependent variables are measures of e-commerce and UCC credit to merchants. We keep only merchants in cities of Uber Eats entry and propensity-score-matched cities based on population and housing rents. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.8. UberX Entry Placebo DiD



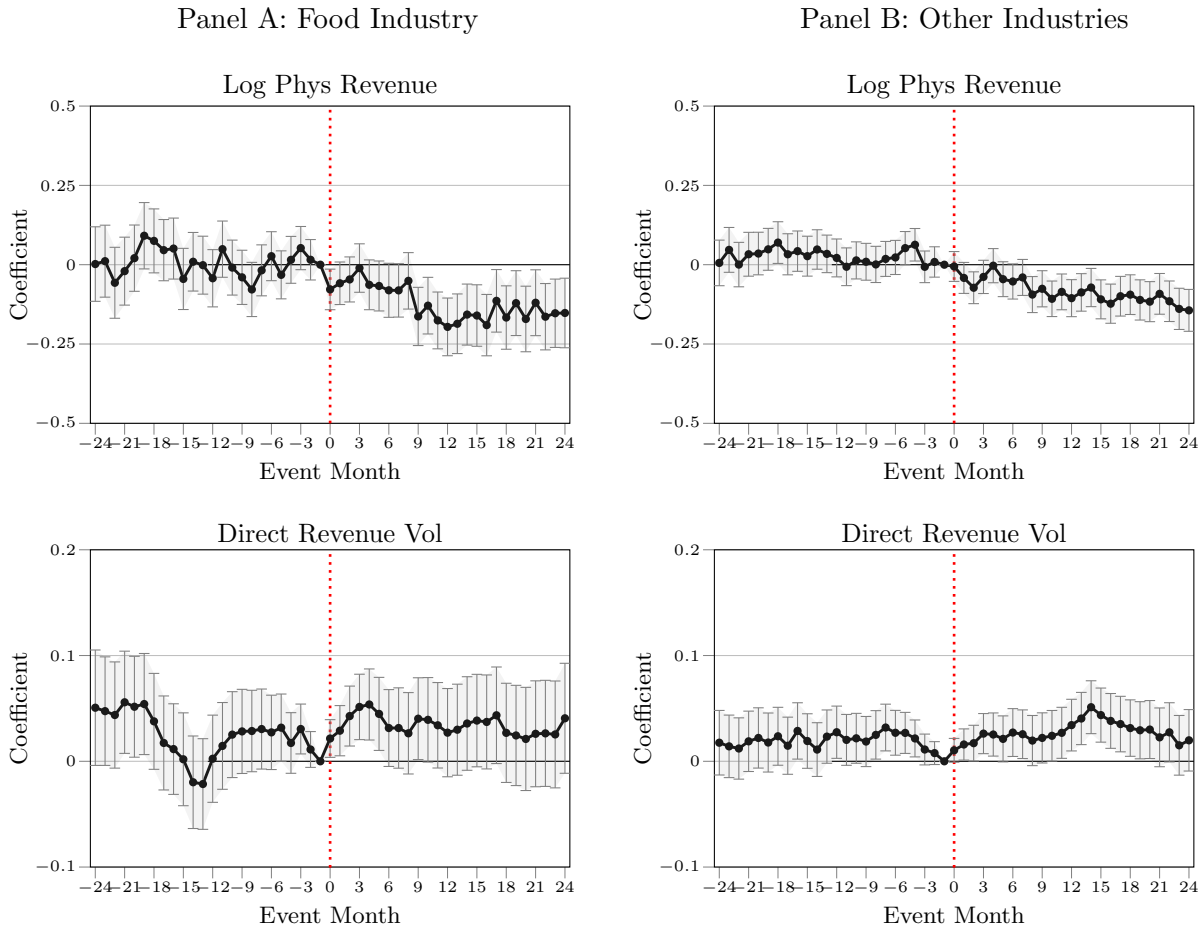
These figures report coefficient estimates for interaction terms between *UberX* and *UberX Month* dummies, where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.9. UberX Entry in Uber Eats Cities Placebo DiD



These figures report coefficient estimates for interaction terms between *Uber Eats* and *UberX Month* dummies, where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was (subsequently) shocked by Uber Eats entry and 0 otherwise. The error bars and shaded areas represent 90% confidence intervals.

Figure IA.10. Uber Eats Entry, Physical Revenue, and Revenue Volatility DiD



These figures report coefficient estimates for interaction terms between *Uber Eats* and *Uber Eats Month* dummies, where dependent variables are measures of merchant physical revenue and revenue volatility. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. The error bars and shaded areas represent 90% confidence intervals.

B. Supplementary Tables

Table IA.I. Summary Statistics: Firms by Industry – All States

This table reports descriptive statistics, split by industry, for merchants in all states in our data, after restrictions described in the main text. The variables are first aggregated at the firm level and then averaged by industry. Variable definitions are provided in Internet Appendix C. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles.

	Num of Firms (#)	Avg Direct Revenue ($/mo)	Avg Num of Orders (#/mo)	Avg Order Size ($)	Avg Pct of CC Orders (%)	Avg Pct of WL E-Comm Orders (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Automotive/Fuel	1,741	2,170	15	211	30.8	1.90
Education	379	2,043	17	191	33.2	13.80
Electronics/Merchandise	1,428	1,259	15	109	28.7	3.85
Entertainment/Recreation	3,843	1,201	17	105	35.2	6.42
Gifts	531	792	12	73	36.6	2.95
Groceries	3,953	804	22	48	29.4	1.62
Healthcare/Medical	3,610	1,213	12	122	34.0	5.85
Home Improvement	1,678	2,107	14	197	39.0	11.27
Office Expenses	182	1,817	13	178	38.7	10.27
Personal/Family	7,907	977	13	88	31.5	3.64
Pets/Pet Care	1,557	1,732	15	128	42.1	2.07
Restaurants	15,286	797	24	38	30.3	5.67
Services/Supplies	1,184	2,620	17	245	29.4	17.23
Subscriptions/Renewals	4	1,632	40	37	47.9	99.54
Travel	854	1,954	17	176	43.0	4.51
Total	44,137	1,137	18	89	32.2	5.25

Table IA.II. Summary Statistics: Revenue – All States

This table reports descriptive statistics for merchants in all states in our data. Columns (1)–(4) describe merchants in the food industry (restaurants & groceries). Columns (5)–(8) describe merchants in all other industries in the data. Variable definitions are provided in Internet Appendix C. Continuous variables are winsorized at the 1^{st} and the 99^{th} percentiles.

		Food Industry				Other Industries			
		mean	median	SD	N	mean	median	SD	N
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All Direct Revenue									
Revenue	($)	751.8	308.2	1,304.1	1,085,859	1,449.3	562.8	2,606.8	1,788,442
Num of Orders	(#)	23	11	36	1,085,859	13	8	18	1,788,442
Order Size	($)	37.5	27.3	37.9	900,930	135.3	69.4	204.8	1,569,266
Pct of CC Orders	(%)	30.1	20.0	31.5	900,930	34.9	27.5	32.6	1,569,266
Pct of WL E-Comm Orders	(%)	4.2	0.0	18.7	900,930	6.0	0.0	21.3	1,569,266
WL E-Comm Revenue									
Revenue	($)	21.9	0.0	127.5	1,085,859	73.8	0.0	365.8	1,788,442
Num of Orders	(#)	0	0	3	1,085,859	0	0	2	1,788,442
Order Size	($)	78.3	36.2	171.8	66,055	225.2	93.2	409.3	176,406
Pct of CC Orders	(%)	41.7	33.3	41.1	66,055	11.2	0.0	27.4	176,406
Physical Revenue									
Revenue	($)	709.4	283.7	1,243.3	1,085,859	1,318.3	505.0	2,371.2	1,788,442
Num of Orders	(#)	22	10	36	1,085,859	12	8	17	1,788,442
Order Size	($)	36.6	27.0	35.6	874,448	130.2	68.1	191.3	1,515,124
Pct of CC Orders	(%)	29.8	20.0	31.6	874,448	36.5	29.4	33.1	1,515,124

Table IA.III. Uber Eats Entry, E-Commerce, and Credit DiD
Robustness to Alternative Control Group

This table reports the results of difference-in-differences (DiD) regressions where the coefficient of interest is the interaction term between *Uber Eats* and *Post* and where dependent variables are measures of e-commerce and UCC credit to merchants. We refine the control group of other industries to focus on local services and non-food retail by excluding merchants in the following industries: Automotive/Fuel, Education, Healthcare/Medical, Office Expenses, Services/Supplies, and Travel. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for the remaining other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Panel A: Food Industry				
Uber Eats (1/0) x Post (1/0)	0.0300***	0.0848***	0.0145**	0.0178***
	(3.26)	(5.66)	(2.19)	(2.70)
Post (1/0)	-0.0115**	0.0764***	-0.00635*	-0.00637*
	(-2.21)	(8.50)	(-1.77)	(-1.72)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	141,997	141,997
R^2	0.758	0.764	0.697	0.863
Panel B: Other Industries				
Uber Eats (1/0) x Post (1/0)	-0.00520	-0.00520	-0.00964*	0.000268
	(-0.71)	(-0.71)	(-1.66)	(0.05)
Post (1/0)	-0.00606	-0.00606	0.00259	-0.00117
	(-1.39)	(-1.39)	(0.74)	(-0.37)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	186,574	186,574	186,574	186,574
R^2	0.851	0.851	0.685	0.895

Table IA.IV. Delivery App Entry, E-Commerce, and Credit DDD

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Delivery App*, *Post*, and *Food* and where dependent variables are measures of e-commerce and UCC credit to merchants. *Delivery App* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by either Uber Eats, DoorDash, or Grubhub entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the first delivery app entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Delivery App (1/0) x Post (1/0) x Food (1/0)	0.0301**	0.176***	0.0225**	0.0164*
	(2.51)	(9.29)	(2.46)	(1.85)
Delivery App (1/0) x Post (1/0)	0.000374	0.000374	-0.00425	-0.00188
	(0.05)	(0.05)	(-0.74)	(-0.35)
Post (1/0)	-0.00291	-0.00291	-0.00165	-0.00137
	(-0.62)	(-0.62)	(-0.44)	(-0.43)
Post (1/0) x Food (1/0)	-0.0213***	0.0368***	-0.00986*	-0.00966*
	(-2.97)	(3.07)	(-1.87)	(-1.90)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	301,459	301,459	301,459	301,459
R^2	0.841	0.813	0.703	0.893

Table IA.V. Uber Eats Entry, E-Commerce, and Credit DDD
Robustness to Propensity Score Matching

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficient of interest is the interaction term between *Uber Eats*, *Post*, and *Food* and where dependent variables are measures of e-commerce and UCC credit to merchants. We keep only merchants in cities of Uber Eats entry and propensity-score-matched cities based on population and housing rents. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats (1/0) x Post (1/0) x Food (1/0)	0.0394***	0.0622***	0.0217**	0.0149
	(2.86)	(3.07)	(2.08)	(1.43)
Uber Eats (1/0) x Post (1/0)	-0.0133*	-0.0133*	-0.0104*	0.00264
	(-1.72)	(-1.72)	(-1.67)	(0.45)
Post (1/0)	-0.00145	-0.00145	0.00501	-0.00304
	(-0.28)	(-0.28)	(1.18)	(-0.76)
Post (1/0) x Food (1/0)	-0.0128	0.0988***	-0.0117*	-0.00585
	(-1.33)	(6.68)	(-1.66)	(-0.81)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	297,240	297,240	297,240	297,240
R^2	0.839	0.826	0.688	0.883

Table IA.VI. Uber Eats Entry, E-Commerce, and Credit DDD
Core vs. Other Markets

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficients of interest are the interaction terms between *Uber Eats Core* or *Uber Eats Other*, *Post*, and *Food* and where dependent variables are measures of e-commerce and UCC credit to merchants. *Uber Eats Core* (1/0) is an indicator that takes the value of 1 if a merchant is located in an announced city of Uber Eats entry and 0 otherwise. *Uber Eats Other* (1/0) is an indicator that takes the value of 1 if a merchant is located in an adjacent city within the same MSA and a 25-mile driving distance of an announced city (a sparse market in Uber's terminology) and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Food* (1/0) is an indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats Core (1/0) x Post (1/0) x Food (1/0)	0.0401***	0.0902***	0.0269**	0.0175
	(2.97)	(4.56)	(2.55)	(1.63)
Uber Eats Other (1/0) x Post (1/0) x Food (1/0)	0.0416***	0.102***	0.0267***	0.0208**
	(2.80)	(4.77)	(2.58)	(2.17)
Uber Eats Core (1/0) x Post (1/0)	-0.0107	-0.0107	-0.00972	0.00306
	(-1.38)	(-1.38)	(-1.54)	(0.49)
Uber Eats Other (1/0) x Post (1/0)	-0.0107	-0.0107	-0.0154**	-0.00627
	(-1.38)	(-1.38)	(-2.56)	(-1.15)
Post (1/0)	-0.00435	-0.00435	0.00488	-0.000108
	(-1.14)	(-1.14)	(1.58)	(-0.04)
Post (1/0) x Food (1/0)	-0.00718	0.0807***	-0.0112**	-0.00623
	(-1.11)	(8.26)	(-2.37)	(-1.33)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	421,497	421,497	421,497	421,497
R^2	0.837	0.822	0.698	0.889

Table IA.VII. UberX Entry Placebo DiD

This table reports the results of difference-in-differences (DiD) regressions where the coefficient of interest is the interaction term between *UberX* and *Post UberX* and where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *UberX* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by UberX entry and 0 otherwise. *Post UberX* (1/0) is an indicator that takes the value of 1 in the month of the UberX entry and 24 months thereafter and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities without UberX entry. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Panel A: Food Industry				
UberX (1/0) x Post UberX (1/0)	-0.0157**	-0.0227***	0.00146	-0.00499
	(-2.39)	(-3.22)	(0.25)	(-0.74)
Post UberX (1/0)	-0.000952	0.0023	-0.00432*	-0.000509
	(-0.31)	(0.58)	(-1.79)	(-0.21)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	136,796	136,796	136,796	136,796
R^2	0.772	0.717	0.672	0.878
Panel B: Other Industries				
UberX (1/0) x Post UberX (1/0)	0.00783	0.00783	-0.00786*	-0.0038
	(1.06)	(1.06)	(-1.72)	(-0.87)
Post UberX (1/0)	-0.0101***	-0.0101***	-0.000575	0.00274
	(-3.43)	(-3.43)	(-0.29)	(1.49)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	297,189	297,189	297,189	297,189
R^2	0.855	0.855	0.724	0.914

Table IA.VIII. UberX Entry in Uber Eats Cities Placebo DiD

This table reports the results of difference-in-differences (DiD) regressions where the coefficient of interest is the interaction term between *Uber Eats* and *Post UberX* and where dependent variables are measures of e-commerce and UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was (subsequently) shocked by Uber Eats entry and 0 otherwise. *Post UberX* (1/0) is an indicator that takes the value of 1 in the month of the UberX entry and 24 months thereafter and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities without UberX entry. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	E-Commerce		UCC Loans	
	WL E-Comm (1/0)	WL + App E-Comm (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Panel A: Food Industry				
Uber Eats (1/0) x Post UberX (1/0)	-0.000122	0.0320***	-0.00299	-0.00816
	(-0.02)	(3.84)	(-0.57)	(-1.41)
Post UberX (1/0)	-0.00450	-0.0176***	-0.00262	0.00208
	(-1.18)	(-3.71)	(-0.83)	(0.62)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	136,796	136,796	136,796	136,796
R^2	0.771	0.717	0.672	0.878
Panel B: Other Industries				
Uber Eats (1/0) x Post UberX (1/0)	-0.00433	-0.00433	-0.00688	0.00911**
	(-0.70)	(-0.70)	(-1.59)	(2.20)
Post UberX (1/0)	-0.00608	-0.00608	0.00105	-0.00271
	(-1.51)	(-1.51)	(0.38)	(-1.03)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	297,189	297,189	297,189	297,189
R^2	0.855	0.855	0.724	0.914

Table IA.IX. Uber Eats Entry and Credit by E-Commerce Adoption Cohort

This table reports the results of difference-in-differences-in-differences (DDD) regressions where the coefficients of interest are the interaction terms between *Uber Eats*, *Post*, and *Early Adopter* or *Late Adopter* and where dependent variables are measures of UCC credit to merchants. Panel A presents the results for the food industry (restaurants & groceries). Panel B presents the results for all other industries. *Uber Eats* (1/0) is an indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise. *Post* (1/0) is an indicator that takes the value of 1 in the month of the Uber Eats entry and 24 months thereafter and 0 otherwise. *Early Adopter* (1/0) is an indicator that takes the value of 1 if a merchant engaged in (white-label or app) e-commerce within 24 months prior to Uber Eats entry and 0 otherwise. *Late Adopter* (1/0) is an indicator that takes the value of 1 if a merchant first engaged in (white-label or app) e-commerce during the month of Uber Eats entry or within 24 months after entry and 0 otherwise. The analysis window is $m = -24$ to $m = 24$. We randomly assign $m = 0$ with the respective event window to cities in the control group. Variable definitions are provided in Internet Appendix C. Standard errors are clustered at the merchant level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. t-statistics are presented in parentheses.

Dependent Variable =	Panel A: Food Industry		Panel B: Other Industries	
	New Loan (1/0)	Outstanding Loan (1/0)	New Loan (1/0)	Outstanding Loan (1/0)
	(1)	(2)	(3)	(4)
Uber Eats (1/0) x Post (1/0) x Early Adopter (1/0)	0.000982	0.0121	0.00264	0.0161
	(0.05)	(0.70)	(0.20)	(1.36)
Uber Eats (1/0) x Post (1/0) x Late Adopter (1/0)	-0.0349*	-0.00848	-0.00109	-0.000744
	(-1.95)	(-0.48)	(-0.05)	(-0.04)
Uber Eats (1/0) x Post (1/0)	0.0171**	0.0142*	-0.0134**	-0.00598
	(2.22)	(1.78)	(-2.36)	(-1.08)
Post (1/0)	-0.0168***	-0.0102**	-0.00316	-0.00242
	(-4.03)	(-2.38)	(-0.80)	(-0.66)
Post (1/0) x Early Adopter (1/0)	0.0265**	0.0177*	0.0273***	0.00812
	(2.27)	(1.66)	(2.83)	(0.99)
Post (1/0) x Late Adopter (1/0)	0.0360***	0.00471	0.0165	0.00518
	(2.70)	(0.37)	(1.25)	(0.42)
Firm FEs	Yes	Yes	Yes	Yes
Month–Year x Ind FEs	Yes	Yes	Yes	Yes
# Observations	141,997	141,997	279,500	279,500
R^2	0.698	0.863	0.699	0.897

C. Variable Definitions

Variable	Definition
Direct Revenue ($)	Dollar amount of direct (physical and white-label e-commerce) transactions with this merchant in the consumer transaction data per month. *Log Direct Revenue* is the natural logarithm of *Direct Revenue*.
Num of Orders (#)	Number of direct transactions with this merchant in the consumer transaction data per month. *Log Num of Orders* is the natural logarithm of *Num of Orders*.
Order Size ($)	Average size of direct transactions with this merchant in the consumer transaction data per month. *Log Order Size* is the natural logarithm of *Order Size*.
Pct of CC Orders (%)	Percent of direct transactions with this merchant via credit cards in the consumer transaction data per month.
Pct of WL E-Comm Orders (%)	Percent of white-label (digital) e-commerce transactions with this merchant in the consumer transaction data per month.
WL E-Comm Revenue ($)	*Direct Revenue*, only white-label e-commerce transactions. *Num of Orders*, *Order Size*, and *Pct of CC Orders* for white-label e-commerce transactions are defined similarly.
Physical Revenue ($)	*Direct Revenue*, only physical transactions. *Num of Orders*, *Order Size*, and *Pct of CC Orders* for physical transactions are defined similarly. *Log Phys Revenue* is the natural logarithm of *Physical Revenue*.
Bank Revenue ($)	*Direct Revenue*, only bank transactions. *Log Bank Revenue* is the natural logarithm of *Bank Revenue*.
CC Revenue ($)	*Direct Revenue*, only credit card transactions. *Log CC Revenue* is the natural logarithm of *CC Revenue*.
Direct Revenue Vol	6-month-rolling-window coefficient of variation of *Direct Revenue*, calculated as the 6-month-rolling-window standard deviation of *Direct Revenue* divided by the 6-month-rolling-window mean of *Direct Revenue*, measured in decimals.
New Loan (1/0)	Dummy that takes the value of 0, switches from 0 to 1 in the month a merchant obtains a UCC loan, and stays at 1 through the event window.
Outstanding Loan (1/0)	Dummy that takes the value of 1 from the month a merchant obtains a UCC loan through the UCC filing expiration (lapse) month and 0 otherwise.
Lender Type	Type of the secured party on a UCC filing, classified based on its name and business description. *New Loan* (1/0) and *Outstanding Loan* (1/0) by lender type only count UCC loans from the respective lender type. Except for banks and non-banks, lender types can overlap (e.g., bank equipment finance units are both banks and equipment finance lenders).
• Banks	Commercial banks and savings institutions, including their subsidiaries, divisions, and vendor programs.
• Non-Banks	All secured parties other than banks.
• FinTechs	FinTech and alternative lenders, neobanks and digital challenger banks, tech-enabled specialty finance and other tech-forward financial services companies, embedded FinTech providers, FinTech partner banks, and digitally inclined ("FinTech-forward") banks.
• Non-Bank FinTechs	FinTechs that are not banks.
• Equipment Finance	Equipment lessors, captive finance arms of manufacturers, bank equipment finance units, and commercial finance companies that mainly finance equipment, as well as equipment manufacturers, dealers, and distributors that take purchase-money security interests in the equipment they sell.

(Continued)

Variable	Definition
Market Entry (1/0)	Indicator that takes the value of 1 in the month a merchant first appears in the consumer transaction data and 0 otherwise.
Market Exit (1/0)	Indicator that takes the value of 1 in the month a merchant last appears in the consumer transaction data and 0 otherwise.
WL E-Comm (1/0)	Indicator for white-label e-commerce adoption that takes the value of 1 in the month of the first white-label e-commerce transaction and stays at 1 thereafter.
WL + App E-Comm (1/0)	Indicator of e-commerce adoption defined similarly to *WL E-Comm* (1/0) but also includes delivery app (Uber Eats, Postmates, DoorDash, Order Online, Grubhub, and Seamless) adoption.
Uber Eats (1/0)	Indicator that takes the value of 1 if a merchant's city was shocked by Uber Eats entry and 0 otherwise.
Uber Eats Core (1/0)	Indicator that takes the value of 1 if a merchant is located in an announced city of Uber Eats entry and 0 otherwise.
Uber Eats Other (1/0)	Indicator that takes the value of 1 if a merchant is located in an adjacent city within the same MSA and a 25-mile driving distance of an announced city (a sparse market in Uber's terminology) and 0 otherwise.
Delivery App (1/0)	Indicator that takes the value of 1 if a merchant's city was shocked by either Uber Eats, DoorDash, or Grubhub entry and 0 otherwise.
UberX (1/0)	Indicator that takes the value of 1 if a merchant's city was shocked by UberX entry and 0 otherwise.
Post (1/0)	Indicator that takes the value of 1 in the month of the Uber Eats entry (the first delivery app entry in tests with *Delivery App*) and 24 months thereafter and 0 otherwise.
Post UberX (1/0)	Indicator that takes the value of 1 in the month of the UberX entry and 24 months thereafter and 0 otherwise.
Food (1/0)	Indicator that takes the value of 1 if a merchant is in the food industry (restaurants & groceries) and 0 otherwise.
E-Comm Adopter (1/0)	Indicator that takes the value of 1 if a merchant engaged in (white-label or app) e-commerce within 24 months of Uber Eats entry and 0 otherwise.
Early Adopter (1/0)	Indicator that takes the value of 1 if a merchant engaged in (white-label or app) e-commerce within 24 months prior to Uber Eats entry and 0 otherwise.
Late Adopter (1/0)	Indicator that takes the value of 1 if a merchant first engaged in (white-label or app) e-commerce during the month of Uber Eats entry or within 24 months after entry and 0 otherwise.
App Adopter (1/0)	Indicator that takes the value of 1 if a merchant adopted delivery app (or, where indicated, Uber Eats) e-commerce within 24 months of Uber Eats entry and 0 otherwise.
Small (1/0)	Indicator that takes the value of 1 if a merchant's direct revenue in the 24 months before Uber Eats entry is below the 20^{th} percentile among food industry merchants and 0 otherwise.
Female (1/0)	Indicator that takes the value of 1 if a merchant has a female among its owners and 0 otherwise.
Non-Alcohol (1/0)	Indicator that takes the value of 1 if a merchant does not primarily engage in the sale of alcohol (e.g., beer store, night club), tobacco (e.g., hookah lounge), or other regulated substances (e.g., medical marijuana) and 0 otherwise.