Digital Adoption, Labor Demand, and Worker Earnings: Evidence from Online Delivery*

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Abstract

This paper studies how firm adoption of digital technologies reshapes labor demand and worker earnings. Linking administrative employer-employee records to restaurants and workers from a major delivery platform and using a matched event-study, I show that adopting restaurants substitute in-house labor hours one-for-one with outsourced platform-worker hours. Earnings losses for incumbent workers are modest because displaced workers reallocate to new formal-sector jobs. Exposed non-adopting restaurants are more likely to close, and their workers experience larger losses. I quantify earnings effects across restaurant and platform workers, showing how platform adoption redistributes earnings across workers and creates income outside traditional restaurant employment.

Keywords: Alternative Work Arrangements, Gig Economy, Technological Change, Outsourcing, Displacement, Informality

JEL Codes J23, J31, J46, O33, L86, J63

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Technological progress profoundly affects both businesses and their employees. Most directly, innovation reshapes labor demand, displacing some jobs while creating new tasks and occupations (Autor et al., 2003; Acemoglu and Autor, 2011; Acemoglu et al., 2024). However, many technologies adopted by firms also reshape demand for goods and services, which in turn affects firms and workers (Bakos, 1997; Dolfen et al., 2023).[1] In these settings, the labor-market consequences of technological change may arise not only within adopting firms, but also through changes in firms' task organization, business-stealing effects on competing establishments, and worker reallocation into newly created jobs.

I study these margins in the context of online-delivery platforms. These platforms allow restaurants to outsource part of their labor input while also changing the demand for their products. As a result, their effects on workers need not be concentrated among incumbent employees at adopting firms. They may instead be distributed across three groups: workers employed at adopting restaurants, workers employed at competing non-adopting restaurants, and workers who enter platform work. Understanding how the effects of this type of firm-side digital adoption are distributed across workers is therefore central to assessing its broader labor-market consequences.

Measuring these effects is empirically challenging. It requires observing both the firms that adopt the technology and the workers who perform the outsourced tasks, which is rarely possible in standard data. In addition, platform adoption is a strategic decision that may correlate with firm-level productivity or demand shocks, making it difficult to distinguish the effects of adoption from the characteristics of the firms that choose to adopt.

To study how online-delivery platforms reshape labor demand and worker earnings in Brazil, I combine a novel data set linking adopting restaurants and platform workers to administrative employer-employee records. These data allow me to observe restaurant adoption of the technology, trace the incumbent workers affected by it, measure substitution toward platform labor, and study spillovers on non-adopting firms. To estimate the causal effects of platform adoption, I exploit the staggered rollout of a large online-delivery platform across regions and compare adopting restaurants—and their workers—to observationally similar restaurants in regions where the platform was not yet available. To account for informal workers, I supplement these data with household surveys.

The evidence shows that the effects of platform adoption extend beyond adopting firms and are distributed across multiple margins. Adopting restaurants substitute in-house non-cooking

[1] Several industries illustrate how technology simultaneously affects labor and product markets. E-commerce has shifted retail toward online operations, altering job composition and forcing some retailers to close (Chava et al., 2024). Streaming services have transformed media consumption patterns (Aguiar and Waldfogel, 2018), while digital banking has reduced bank-teller jobs but increased demand for software-development professionals (Jiang et al., 2025).

labor hours one-for-one with outsourced platform-worker hours. Despite this substantial change in labor demand, incumbent workers at adopting restaurants experience only modest earnings losses because many workers who separate reallocate relatively quickly to new formal-sector jobs. By contrast, non-adopting restaurants exposed to nearby adoption shocks are more likely to contract or close, and their workers experience substantially larger losses. A central implication is that the incidence of platform adoption is shaped not only by adjustment within adopting firms, but also by negative spillovers on competing establishments. I then use these reduced-form estimates in an accounting exercise to assess whether gains to platform workers offset the losses experienced elsewhere in the restaurant sector.

Brazil is a useful setting in which to study the impact of these technologies. The restaurant industry accounts for about 4 percent of formal employment, and the country has a large gig economy, with 1.5 million people working through digital platforms and more than 600,000 of them working on online-delivery platforms (IBGE, 2022). High levels of informality and unemployment imply that delivery platforms may be especially valuable for workers with limited outside options (Ulyssea, 2020). While the mechanisms emphasized in the paper—within-firm substitution, market expansion, and spillovers on competing firms—are likely to arise in other settings, the magnitude and distribution of gains and losses may depend on local labor-market conditions such as informality, unemployment, and workers' outside options.

I begin by developing a stylized framework of firm-side digital adoption in which restaurants produce dine-in and delivery meals using three types of labor: cooks, waiters, and delivery drivers. The framework is designed to capture two forces that can arise more broadly when firms adopt technologies that both change the composition of labor used in production and affect the demand they face. First, there is a *substitution channel*: by making delivery more attractive to consumers, the platform reallocates demand away from dine-in meals and toward delivery meals, shifting labor demand away from in-house service workers and toward delivery workers. Second, there is a *market-expansion channel*: by expanding a restaurant's market reach and reducing consumer search frictions, the platform can increase overall demand for restaurant meals and thereby increase total restaurant employment. The overall effect of platform adoption on employment and wages therefore depends on the relative strength of these two channels.

I first study the market-level effects of platform entry. Following platform entry into a local market, the formal restaurant sector contracts: the number of formal restaurant workers falls by 6 percent and the number of registered establishments declines by 4 percent, while average wages and new restaurant entry remain largely unchanged. These market-level effects reflect not only the response of adopting restaurants, but also displacement of nearby non-adopting competitors.

I then use the granularity of the data to decompose these aggregate effects. Restaurants that adopt the platform are larger and pay higher wages, as reflected in higher AKM firm fixed effects

(Abowd et al., 1999). Comparing adopters to matched restaurants in regions where no delivery platform is available at the time, I find that adopting restaurants reduce the number of in-house workers by 6 percent one year after adoption. This decline is fully accounted for by a one-for-one substitution of non-cooking in-house hours with outsourced platform-driver hours. By contrast, the number of cooks remains stable after adoption, suggesting that, for the average adopting restaurant, the substitution effect dominates the market expansion effect. These results are robust to restricting the sample to early adopters not exposed to the Covid-19 pandemic and to an instrumental-variables specification that instruments platform adoption with platform availability at the microregion level.

The market expansion effect is stronger in some settings than in others. Restaurants in low-density areas typically attract less walk-in traffic (Leonardi and Moretti, 2023) and therefore may benefit more from the platform's ability to expand their customer base. Consistent with this interpretation, restaurants in low-density areas expand their total workforce by 6 percentage points more than restaurants in high-density areas. This increase is driven primarily by an increase in cooks, while the number of non-cooking workers remains broadly unchanged.

Online-delivery platforms also generate economically meaningful spillovers on non-adopting restaurants. To estimate these indirect effects, I study sudden, large increases in the share of nearby restaurants using delivery platforms within a one-kilometer radius of non-adopting establishments. Consistent with the idea that adoption by some restaurants crowds out demand at local competitors (Chava et al., 2024), I find that exposed non-adopters are 5 percentage points more likely to close one year after the shock. These results are also robust to an alternative spillover measure constructed from the market-level contraction of the restaurant sector.

I then turn to workers. Following the job-displacement and outsourcing literature (Goldschmidt and Schmieder, 2017; Jacobson et al., 1993; Bertheau et al., 2023), I define a worker as treated if their employer adopts an online-delivery platform and match them to a control restaurant worker employed in a location where no platform is available at the time. Workers at adopting restaurants experience only modest losses: one year after their employer adopts the platform, their earnings decline by about 1.5 percent and their probability of formal employment falls by 1.5 percentage points. These limited losses reflect relatively rapid reallocation: by the fifth quarter after platform adoption, 75 percent of treated workers who separate from their original employer have found a new formal-sector job. Workers at non-adopting restaurants exposed to nearby adoption shocks experience substantially larger unconditional losses. One year after a large share of nearby restaurants adopt the platform, earnings for these workers decline by 6.6 percent, and they are 3.8 percentage points less likely to be employed than control-group workers. This stronger effect is driven primarily by a higher incidence of displacement—reflecting the greater likelihood that non-adopting restaurants close—rather than by slower reallocation

conditional on displacement.

These findings imply that the incidence of platform adoption extends beyond workers at adopting restaurants and includes losses among workers at exposed non-adopting firms. To assess whether these losses are offset by gains to platform workers, I compute the present discounted net gains or losses per adopting restaurant for three groups: incumbent workers at adopting restaurants, workers affected by spillovers at non-adopting restaurants, and platform workers. This exercise is informative because the reduced-form estimates for restaurant workers alone do not reveal whether platform adoption lowers worker earnings overall or instead redistributes earnings across groups with different outside options. I also account for informally hired restaurant workers (Ulyssea, 2018) by imputing the number of informal workers per formal restaurant using the Brazilian Census and PNAD-C. Per adopting restaurant, incumbent workers at adopting establishments and workers affected by spillovers lose earnings equivalent to 6.2 and 19.5 percent of the pre-platform wage bill, respectively. Under the baseline assumptions, the net gains to platform workers imply a positive aggregate earnings effect, but one that masks substantial redistribution across workers.

The paper contributes to several strands of the literature. First, it relates the study of online-delivery platforms to the broader literature on firm-level technology adoption and its effects on labor demand and product demand. One part of this literature examines how technology reshapes labor demand, including the substitutability between workers and new technologies and the reallocation of labor across occupations (Autor et al., 2003; Acemoglu and Autor, 2011).[2] Firms play a central role in the diffusion of new technologies, which makes firm-level adoption a particularly relevant object of study (Griliches and Corn, 1961; Mokyr, 2003; Bloom et al., 2016; Lindner et al., 2022). Another part studies how technology affects product demand by expanding firms' market reach and altering consumption patterns (Bakos, 1997; Dolfen et al., 2023). These two dimensions of technological change are often studied separately. This paper bridges them by developing a simple framework in which online-delivery platforms affect labor demand through a substitution effect that changes the composition of labor demand within firms and a market expansion effect that raises overall demand. Empirically, I show that the incidence of platform adoption extends beyond adopting firms, generating negative spillovers on competing establishments while creating a new source of earnings outside traditional restaurant employment.

This paper also relates to the growing literature on alternative work arrangements (Katz and Krueger, 2019) and, more specifically, the gig economy (Harris and Krueger, 2015; Fisher, 2022; Garin et al., 2023, 2025).[3] Unlike ride-sharing platforms, which compete directly with self-

[2] Acemoglu et al. (2024) synthesize this literature and provide a framework in which technological change affects labor demand through productivity changes, automation, and the creation of new tasks.

[3] In the United States, gig work has been shown to smooth consumption (Koustas, 2018) and to mitigate some of the costs of job displacement (Jackson, 2022). Degenhardt and Nimczik (2025) show that delivery-platform work is

employed workers, online-delivery platforms are adopted by establishments, allowing firms to outsource part of their labor input while also affecting product demand.[4] By linking data from a major delivery platform to administrative employer-employee records, I am able to track both firms and workers over time, overcoming limitations common to survey and tax data (Garin et al., 2025). The paper also contributes by studying the gig economy in a middle-income country, where high informality and unemployment may make platform work particularly valuable as a source of income and labor-market attachment (Ulyssea, 2018).

2 Theoretical Framework

This section presents a stylized framework for thinking about how firm-adopted technologies affect labor demand when they both reshape product demand and change the composition of labor used by the firm. I develop the framework in the context of online-delivery platforms, which provide a tractable setting in which restaurants produce two goods using three types of labor: cooks (C), waiters (W), and delivery workers (D). Workers are defined by their occupation rather than their skill, reflecting the relatively homogeneous workforce in the restaurant sector. The goal of the model is not to capture every institutional feature of delivery platforms, but to isolate the two main forces that guide the empirical analysis: a reallocation of labor across occupations and a change in the total demand faced by the firm.

2.1 Model Setup

The model features heterogeneous workers defined by their occupations. Cooks and waiters vary in their preferences for firms' non-wage attribute and their supply elasticity. Worker heterogeneity gives rise to imperfect competition in the labor market. Restaurants faces an upward labor supply curve for cooks and waiters which yields a monopsonistic labor market for these occupations.[5] Unlike a competitive market where firms take wages as given, restaurants post wages for cooks and waiters and hire any worker willing to work at that wage. The labor supply of cooks and waiters is given by:

$$ln(C_j) = \lambda_{jC} + \beta_C \, ln(w_C) \tag{1}$$

$$ln(W_j) = \lambda_{jW} + \beta_W \, ln(w_W) \tag{2}$$

associated with lower human-capital investment among refugees.

[4]Delivery platforms are a form of domestic outsourcing that has received limited attention. Most micro-level evidence on outsourcing studies cases in which workers move from an outsourcing firm to a firm providing outsourced services (Goldschmidt and Schmieder, 2017; Daruich et al., 2024) or to temporary-work agencies and contract firms (Deibler, 2023; Drenik et al., 2023; Felix and Wong, 2025). Because of data limitations, there is little evidence on the substitutability between outsourced and in-house workers in core occupations. This paper helps address that gap by observing both in-house and outsourced workers around the adoption of the technology.

[5]This setup is similar to the model proposed in Card et al. (2018).

where $\lambda_{j\{C,W\}}$ represents the firm specific amenities, common to all workers in the occupation, while $\beta_{\{C,W\}}$ is the occupation specific labor supply elasticity. In contrast, delivery workers are hired in a competitive market, such that restaurants are price takers and only decide the quantity of delivery drivers that they would like to employ.[6]

Each restaurant produces two types of goods: in-house meals (Y^I), produced by cooks and waiters and delivery meals (Y^D) produced by cooks and delivery drivers. That is, while waiters and delivery drivers are good specific, cooks are shared across both activities. Specifically the two production are given by:

$$Y_j^I = T_j^I (C^I)^\alpha \; W^{1-\alpha} \tag{3}$$

$$Y_j^D = T_j^D (C^D)^\alpha \; D^{1-\alpha} \tag{4}$$

$$C = C^I + C^D \tag{5}$$

where for simplicity output elasticities (α) are assumed to be the same across types of goods and $T^{\{I,D\}}$ are firm and good specific productivity shifters. Restaurants sell a composite restaurant good Y which aggregates over dine-in and delivery quantities:

$$Y \equiv \left[\omega^{1/\kappa}(Y^I)^{\frac{\kappa-1}{\kappa}} + (1-\omega)^{1/\kappa}(Y^D)^{\frac{\kappa-1}{\kappa}}\right]^{\frac{\kappa}{\kappa-1}} \tag{6}$$

where $\kappa > 1$ is the consumer substitution elasticity across goods, while $\omega \in (0,1)$ represents the baseline consumer preference for dine-in versus delivery. Restaurants compete in a competitive monopolistic market. Each firm faces a negatively sloped demand curve for their composite Y with product demand elasticity of $\epsilon > 1$. The demand curve contains a firm (and location) specific demand shifter P_{0jl}, that allows for firms to set different prices for the same quantities. The product demand function takes the following form:

$$P_{jl} = P_{0jl} Y^{-\frac{1}{\epsilon}} \tag{7}$$

$$P_{jl} \equiv \left[\omega (p^I)^{1-\kappa} + (1-\omega)(\tau \, p^D)^{1-\kappa}\right]^{\frac{1}{1-\kappa}} \tag{8}$$

where p^I is the dine-in price and p^D is the baseline delivery price. Let $\tilde{p}^D \equiv \tau p^D$, where $\tau > 0$ is a revenue-equivalent delivery wedge. The object $\tilde{p}^D$ is the effective delivery price entering both the CES demand system and delivery revenue, so P_{jl} is the corresponding CES price index for the restaurant's composite good. A fall in τ makes delivery relatively cheaper and shifts demand toward delivery meals. Consistent with the setting described in Section 3, I assume that delivery is essentially absent prior to platform adoption, so pre-adoption delivery employment is normalized

[6]Though this assumption is more likely to resemble the delivery worker labor market, especially after delivery platforms become available, none of the following derivations depend on this assumption.

to zero; in the model, this is equivalent to a prohibitively high initial delivery wedge τ, such that $Y^D \approx 0$.

Firms Profit Maximization Problem Restaurants must choose between between the three types of labor. Within a market l restaurants post wages for waiters and cooks and choose the number of delivery workers solving the following profit maximization problem:

$$\max_{w_W, w_C, D, C^I, C^D} R^I + R^D - w_W W - w_C(C^I + C^D) - w_D D$$
$$R^I \equiv p^I Y^I, \qquad R^D \equiv \tilde{p}^D Y^D, \qquad R = R^I + R^D$$

subject to (1)-(7). The first order conditions yield the following wage equations for in-house cooks and waiters (disregarding the j notation for simplicity):

$$w_W = \underbrace{\frac{\beta_W}{1+\beta_W}}_{\text{Markdown}} \underbrace{\frac{\epsilon - 1}{\epsilon} (1-\alpha) \frac{R^I}{W}}_{\text{MRPL}} \tag{9}$$

$$w_C = \frac{\beta_C}{1+\beta_C} \frac{\epsilon - 1}{\epsilon} \alpha \frac{R}{C} \tag{10}$$

The wage equation illustrates the typical monopsonistic setting (Manning, 2011), where the markdown is a function of the labor supply elasticity and the marginal revenue product of labor is a function of the output elasticity of each type of labor. Importantly, the optimal allocation of waiters responds to revenue from in-house meals, while the optimal allocation of cooks responds to total restaurant revenue, including delivery revenue valued at the effective delivery price $\tilde{p}^D = \tau p^D$.

2.2 Impact of online-delivery platforms

Substitution Effect Under this setting, the introduction of online-delivery platforms has two direct effects. First, platforms make delivery more attractive, reallocating consumers away from dine-in meals and toward delivery meals, either through a fall in ω or through a decrease in the delivery friction τ. This change shifts labor demand away from in-house service workers and toward delivery workers. I refer to this reallocation of labor demand across worker types as the *substitution effect.*

Market Expansion Effect The introduction of online-delivery platforms may also affect the total demand for goods produced by the restaurant (P_{0jl}) by allowing restaurants to access new markets that were previously difficult to serve. By increasing restaurant visibility and reducing consumer search costs, platforms may bring new customers to establishments or improve match-

ing between consumers and firms. This effect may be particularly important for restaurants located in low-density areas that typically have less walk-in traffic and therefore may benefit more from the platform's expanded customer base. I refer to this shift in overall demand as the *market expansion effect.*

2.3 Comparative Statics

The changes in labor demand for in-house waiters after a restaurant adopts an online-delivery platform depend on the relative magnitude of the market expansion effect and the substitution effect. Specifically, by defining $s_I = \frac{R^I}{R}$ as the share of restaurant revenue coming from dine-in meals, log-linearizing equation (9) with and without the adoption of the technology, and comparing the two, one arrives at the following expression for the change in labor demand for waiters:

$$\Delta\, ln(W) = \frac{\beta_W}{1+\beta_W}\Bigg[\underbrace{\Delta\, ln\, R}_{\textit{Market expansion effect}} + \underbrace{\Delta\, ln(s_I)}_{\textit{Substitution effect}}\Bigg] \tag{11}$$

Equation (11) shows that the market expansion effect maps directly into changes in the restaurants total revenue after adopting the platform. In contrast the substitution effect is the change in the revenue share of delivery meals respect to in-house meals, conditional on the total output of the restaurant. This change in the share can be a product of either a lower cost of delivery (τ) or a shift in the consumer preference for delivery meals respect to in-house meals (ω).

Taken together, equation (11) provides guidance on the forces behind the impact of delivery platform adoption on the demand for in-house waiters. On the one hand, the substitution effect will most certainly reduce demand for waiters. On the other, the market expansion effect can potentially increase the demand for in-house waiters and will depend on certain characteristics of the adopters, such as their location.[7]

The impact of the adoption of delivery platforms on the number of in-house cooks hired is expressed as follows:

$$\Delta\, ln(C) = \frac{\beta_C}{1+\beta_C}\,\Delta\, ln\, R \tag{12}$$

The change in the number of in-house cooks depends solely on the change in total revenue at the firm. Because the output elasticity of cooks is unaffected by the adoption of the platform (i.e., cooks are equally complementary to waiters and delivery drivers), the change in the number of cooks will be solely determined by the change in their marginal productivity.[8]

[7]The market expansion effect can be microfounded through the lens of a consumer search model, where platforms reduce search costs and these effects are different depending on the agglomeration forces (Vitali, 2022).

[8]Importantly, if online-delivery platforms retain all the additional rents generated by adoption, even if demand

Alternative Models Appendix C1 provides the derivations for the stylized model presented in this section. Appendix C2 relaxes the structure of the production function, demand and labor supply and shows that it is possible to reach a similar decomposition under more general frameworks that also have firms producing two goods and hiring three types of labor. Appendix C3 shows that the same type of decomposition as in equations (11) - (12) arises from assuming a model where restaurants produce one type of good with three types of labor, where delivery platforms affect the relative productivity between in-house waiters and delivery drivers.

3 Setting

Online-delivery platforms operate as intermediaries in a contractual relationship between restaurants, consumers, and workers. Figure I illustrates this relationship: consumers order a product from a restaurant through the app, which then coordinates the delivery by assigning the task to a worker. Under this arrangement, the restaurant is responsible for producing the good, while the worker typically operates as an independent contractor. When a consumer places an order, the platform offers the task to workers who are logged on at the time and are near the restaurant. The first worker to accept the task receives the assignment and earns a fee based on factors such as the distance covered, time spent, and any additional bonuses available at the time.

To work for an online-delivery service in Brazil, individuals must pass a background check and own a smartphone. Deliveries can be made by bicycle, motorbike, or car.[9] According to Brazilian law, these workers are classified as independent contractors, meaning they are not entitled to social security contributions from the company. Consequently, they do not receive benefits such as maternity leave or paid vacations, and they are not subject to the federal minimum wage, which only applies to formal employees. Online-delivery workers are required to pay income taxes on their earnings if they exceed a threshold that applies to all self-employed individuals (in 2023, this threshold was roughly equivalent to two minimum wages). Workers earning above this threshold have 10 percent of their income subject to taxation.[10]

For the context of this paper, I focus on a large online-delivery company operating in Brazil. Since it began operations, the platform has enrolled over 100,000 establishments. It operates in all major cities and holds a significant market share. The company provides two main services to businesses: (1) delivery services, where consumers order through the app and the company coordinates the delivery; and (2) marketplace services, where consumers order through the app,

increases, the revenue may not change. In other words, the revenue in this stylized model reflects the gains in the marginal revenue productivity of labor net of the platform costs.

[9]The majority of deliveries are made by motorbikes. In my sample, 81 percent of workers and 87 percent of deliveries use motorbikes. Bicycles account for 11 percent of deliveries (16 percent of workers), while cars represent 2 percent of deliveries and 3 percent of workers.

[10]This is in contrast with ridesharing workers, 60 percent of whose earnings are taxable. For more details, see: this article.

but the establishment handles the delivery. Fees are charged for both services, though they vary depending on the specific service provided.[11] Restaurants must provide a tax identifier and, therefore, must be formally registered with the government to offer services through the platform.

The flipside of the delivery service industry is the restaurant sector. The restaurant sector represents approximately 4% of the private sector employment in Brazil. As most industries in Brazil, restaurants are exposed to meaningful levels of informality. In 2010—the latest available Census—, informal workers represented 38% of the total employment in the sector. As in most settings, information regarding informal workers is limited which constraints the main analysis of this paper to focus on formal employment and wages. However, in Section 7.5 I discuss how the results differ in low and high informality areas, while in Section 9 I use an imputation method based on information available in the census and household surveys to study the impacts of delivery platforms once considering non-formal workers in the restaurant sector.

The time covered by this paper includes the Covid-19 Pandemic. The pandemic had severe public health consequences in the country, such that by July 2021, Brazil had reported more than 19 million cases and 550,000 deaths (leaving it only behind India and the US). In contrast to other countries in the region, the Brazilian federal government expressed itself actively against a national lockdown and the closure of non-essential services generating disagreement between local authorities and the federal administration. Ultimately, public policy decisions related to social distancing and closure of non-essential businesses were taken at the state and municipal level, such that they varied in speed, intensity and duration.[12] These policies started in March 2020 and were short-lived: by September 2020, essentially no municipalities had active policies regarding the closure of non-essential services (Norden et al., 2021). In section 7.5 I discuss the robustness of my results using a subset of early adopters for which I can estimate the impact of the delivery platform prior to the pandemic.

4 Data

4.1 Delivery Platform Data

The data comes from a major online-delivery company in Brazil. It includes detailed information on establishments enrolled in the app, delivery drivers working through the service, and orders placed between 2018 and 2021. Crucially, both the establishment and worker data con-

[11] The company also employs two types of delivery drivers: (1) independent contractors, who are paid per delivery and make up the majority of the workforce; and (2) drivers affiliated with logistics companies, who are formally employed by an intermediary firm and receive a wage independent of their deliveries. These workers typically work for firms that collaborate with several delivery services and represent a minority of the workforce. For the purposes of this paper, I focus only on independent contractors.

[12] A remarkable exception was the Provisional Measure N.1,045 (Emergency Program to Mantain Employment and Income) which was a short-time work policy that allowed companies to reduce working hours and wages if agreed with the worker or the collective bargaining unit.

tain unique identifiers, allowing me to link them to Brazil's administrative employer-employee database (RAIS). The establishment panel data covers businesses offering delivery through the app. Specifically, it provides monthly information on the number of hours workers spent on deliveries for each business and whether more than 50 percent of the business's revenue on the platform came from deliveries.

The worker panel data includes monthly details on each driver, such as the number of deliveries completed, hours worked and logged in, distance traveled, and earnings (broken down by tips, base earnings, and bonuses). It also tracks the primary municipality where the driver operated, the main mode of transportation used (i.e., bike, motorbike, or car), and identifies workers by their contract with the company.

Descriptive Statistics: Platform Workers Table B2 summarizes the main characteristics and performance of platform-workers in my sample. Column (1) provides a snapshot of the characteristics of platform-workers in the first quarter they worked on the delivery platform.[13] platform-workers are on average 29 years old and predominantly male. Those who are formally employed while working on the platform have close to two years of tenure in their formal job, work full-time, and earn average monthly wages of approximately 1,500 BRL (1.57 times the minimum wage in 2018). Columns (2) and (3) show the platform performance for drivers who, in the quarter prior to starting on the platform, held a formal job and for workers who did not hold a formal job prior to working on the platform, respectively. On average, non-formal app workers earn 13 percent more than formal app workers per month on the platform (646 BRL-2018 vs. 571 BRL-2018). This is explained by non-formal workers working more hours (35 hours vs. 30 hours), spending more time logged on the platform (76 hours vs. 62 hours), completing more deliveries (92 vs. 78), and covering more distance (480 kilometers vs. 426 kilometers).

4.2 Employer-Employee Data: RAIS

The other main data source is the administrative employer-employee dataset from Brazil, called *Relação Anual de Informações Sociais* (RAIS). Employers are required to submit yearly reports to the federal government detailing all formal job contracts established in the previous year. This information is used to calculate various benefits for both workers and firms. Compliance with this reporting requirement is high, as incomplete submissions result in substantial penalties. This dataset allows researchers to follow workers over time and includes the universe of formal workers and their employers—establishments—from 2003 to 2021. The data contains information on the worker, such as average monthly earnings, contracted hours, occupation, age, gender, education, and race. Importantly for the restaurant sector, monthly wages in RAIS also include tips. Each worker is identified by a unique tax identifier: *Cadastro de Pessoas Físicas* (CPF).

[13] Demographic characteristics are only available for workers who at some point in their career held a formal job.

On the employer side, the data includes information on the industry of the employer and the location of the establishment. Each establishment is identified through a unique tax identifier: *Cadastro Nacional de Pessoas Jurídicas* (CNPJ). For my analysis, I deflate earnings to 2018 Reais. I restrict the sample to employment spells where the worker was employed for at least two months and earned at least 20 Reais per month (approximately 5 USD at the time).[14]

Descriptive Statistics: RAIS Column (1) of Table B1 reports summary statistics for restaurant workers in RAIS during 2018.[15] The average formal restaurant worker in RAIS earned 1,574 BRL in 2018, significantly above the minimum wage of 954 BRL-2018.[16] Restaurant workers are older than platform-workers (35 years vs. 30) and are more likely to be female (56 percent vs. 8 percent). In terms of occupations, waiters and cooks represent 77 percent of all restaurant workers, while delivery drivers represented only 2 percent of the restaurant sector workforce during the same period. Columns (2) and (3) of Table B1 show the same statistics for the 2010 Brazilian Census. A key advantage of the census is that it allows observation of informal workers as well. Informal workers in the restaurant sector earn 37 percent less than formal workers, which could be partially driven by differences in occupations. Specifically, informal workers are more likely to be waiters and less likely to be cooks than formal workers in the restaurant sector. Importantly, the delivery sector was essentially non-existent in the restaurant sector in 2010 (1 percent of the workforce for both formal and informal workers).

5 Impact of Delivery Platforms on Labor Markets

A natural starting point for understanding the implications of delivery platforms for firms and workers is to examine how local restaurant markets evolve as the platform becomes available. Figure A2 shows that, once the platform enters a microregion, the share of restaurants using it for delivery increases steadily over time, reaching 15 percent of all formal restaurants in the region three years after entry.[17] To estimate the effects of platform availability formally, I exploit the staggered rollout of the platform across microregions and estimate the following event-study

[14] Formal employment represented 62 percent of all employment in Brazil in 2023. The vast majority of workers earn above the minimum wage (see Figure A6). However, it is possible that some may earn less due to low hours, suspensions, or leaves.

[15] Restaurants are defined as establishments with the two-digit *Classificação Nacional de Atividades Econômicas* (CNAE) code of 56, which includes "Restaurants and other food and beverage services" and "Catering services, buffet, and other prepared food services."

[16] This, along with the fact that restaurants in Brazil predominantly employ full-time workers, contrasts with other countries, such as the US, where part-time work and tips are key components of restaurant sector employment.

[17] This share roughly doubles if I do not require that at least 50 percent of a restaurant's platform revenue comes from delivery services in the first month in which they are on the platform.

specification:

$$Y_{mt} = \beta_0 + \gamma_m + \delta_t + \sum_{k=-7}^{k=5} \beta_k \mathbf{1}\{t = t^*(m) + k\} \times Platform_m + \epsilon_{mt} \tag{13}$$

Here, γ_m are microregion fixed effects, δ_t are time fixed effects, and $Platform_m$ is an indicator equal to 1 for microregions that are ever entered by the platform. The coefficients β_k are the parameters of interest and capture the dynamic effects of platform availability. I exclude microregions where the platform was already available before 2018, because I cannot define an exact entry date, and those where entry occurred between December 2020 and December 2021, to retain a balanced post-entry window. Identification relies on the assumption that, absent platform entry, treated and control microregions would have followed parallel trends.

As a first check, I estimate a logit model of the probability that the platform ever enters a microregion as a function of baseline characteristics of the restaurant sector in 2018 and pre-entry trends:

$$Pr(\text{Platform Ever Available} = 1|X_m) = \Lambda(X_m'\beta) \tag{14}$$

Appendix Table B3 reports the results. Unsurprisingly, larger restaurant sectors, and those with higher average education, are more likely to receive the platform, while average tenure is negatively correlated with entry. By contrast, pre-trends in sector outcomes do not predict platform entry, suggesting that the rollout is related to levels rather than differential trends.

Figure II presents the event-study estimates from equation (13) for total workers (panel a), average monthly wages (panel b), the number of establishments (panel c), and new firm entry in RAIS (panel d) in the restaurant sector.[18] Consistent with the logit evidence, there are no visible differential pre-trends before platform entry. After entry, however, both the number of formal restaurant workers and the number of formal establishments in RAIS decline, indicating that the formal restaurant sector contracts when delivery platforms become available. Five quarters after entry, the number of workers falls by about 400, or 6% of the pre-treatment average, while the number of establishments falls by about 40, or 4% of the pre-treatment average. By contrast, average monthly wages and new restaurant entry remain largely unchanged.[19]

These market-level estimates reflect the combined effects of platform adoption on both adopting and non-adopting restaurants. They capture the aggregate consequences of within-firm reallocation from dine-in service toward delivery, any market expansion among adopters, and potential spillover effects on nearby non-adopters. In the next sections, I use the granularity of the

[18] Appendix A3 shows that the results are robust to the difference-in-differences estimator of Borusyak et al. (2024).

[19] Firms included in RAIS must have a tax identifier and at least one formal employee. Anecdotal evidence suggests that delivery platforms have facilitated the rise of "dark kitchens"—establishments that operate exclusively through delivery without in-house dining (Globo, 2022). If these establishments are informal or never hire formal workers, they will not appear in RAIS.

data to disentangle these forces.

6 Impact of Delivery Platforms on Workers and Firms: Research Design

This section describes the empirical strategy used to estimate the causal effects of online-delivery platforms on restaurant labor demand and on the labor-market outcomes of incumbent restaurant workers.

6.1 Research Design: Adopting Establishments

The ideal experiment for estimating the causal effect of platform adoption on restaurants would randomly assign access to delivery services across establishments. Since such an experiment is not feasible, I exploit the staggered rollout of the platform across Brazil.[20] Comparing restaurants across regions involves a trade-off. On the one hand, the staggered rollout provides quasi-experimental variation in platform availability, preventing establishments from self-selecting into the control group. Moreover, if platform adoption generates spillovers for nearby non-adopters, comparing restaurants within the same local market could violate SUTVA. On the other hand, cross-region comparisons may be confounded by differences in restaurant and market characteristics. To address this concern, my baseline specification uses a matching procedure to construct a balanced sample of adopting restaurants and control establishments that did not have access to the technology. In Section 7.5, I discuss an alternative design that instruments adoption with platform availability.

Matching I define the pool of treated restaurants as those that, at the time of adoption, generate more than 50 percent of their platform revenue through delivery services.[21] Treated restaurants must also be located in microregions where the platform accounts for more than 50 percent of the online-delivery market, in order to minimize multihoming.[22] Given the platform's prominence in Brazil, 99.8 percent of treated establishments satisfy this criterion. To ensure a balanced sample, I further restrict attention to restaurants that had been operating for at least two years before the quarter prior to enrollment and that enrolled at least five quarters before the end of the sample period.

To construct an appropriate comparison group, I use a matching procedure.[23] The potential

[20] Figure A1 shows the staggered rollout of the platform across Brazil. The platform began operating in the largest cities, primarily in the Southeast, and subsequently expanded to other parts of the country without a clear geographic pattern. To operate in a microregion, the platform must satisfy two conditions: it must have a sufficient number of client restaurants willing to use the service, and it must have a sufficiently large driver base to provide coverage during business hours. Both conditions take time to satisfy and help explain why the platform did not expand nationwide all at once.

[21] Among restaurants observed in RAIS at the time of adoption, 47 percent satisfy this criterion, while 60 percent generate more than 50 percent of their platform revenue through delivery in at least one subsequent month.

[22] Information on market share was provided by the platform.

[23] Similar empirical strategies have been used to study worker substitutability (Jäger and Heining, 2022), the

control group consists of restaurants located in labor markets where no online-delivery platform was available up to the time of treatment—neither the platform studied here nor competitors—and where no platform entered for at least five quarters after treatment.[24] Each treated restaurant is matched to a control restaurant within the same cell in the quarter prior to treatment. A control restaurant belongs to the same cell as restaurant j treated in quarter t if it has been open for at least two years and falls in the same quartile of firm size and average monthly wages among restaurants in that year. In addition, to ensure that treated and control restaurants face similar local competition, I compute the number of competing restaurants within a 1-kilometer radius and require treated and control establishments to lie on the same side of the median of this measure.[25] Within each cell, treated and control restaurants are paired on the basis of a propensity score estimated from a logit model that predicts treatment using log firm size in quarters $[t-8, t-1]$, log average monthly wages in quarters $[t-4, t-1]$, firm age, the share of waiters, average tenure, average worker age, the share of female workers, and average worker hours.

Given the high volatility of the restaurant sector (Parsa et al., 2011, 2021), matching on lagged size and wages helps ensure that control restaurants are on similar pre-treatment trajectories as treated establishments. At the same time, matching on lagged outcomes may raise concerns about mean reversion. To mitigate this concern, I do not target wages in three quarters of the pre-treatment period and later show that treated and control restaurants display similar trends even in these untargeted periods. In addition, Section 7.5 presents placebo tests showing that the matching procedure does not mechanically generate treatment effects in the absence of platform adoption.

Econometric Framework I estimate the causal effect of platform enrollment on establishment j by estimating the following event-study model on the matched sample of treated and control establishments:

$$Y_{\{j,i\}t} = \beta_0 + \alpha_{\{j,i\}} + \delta_t + \sum_{k=-7}^{k=5} \theta_k \mathbf{1}\{t = t^*(j)+k\} + \sum_{k=-7}^{k=5} \beta_k \mathbf{1}\{t = t^*(j)+k\} \times Treated_{\{j,i\}} + \epsilon_{\{j,i\}t} \quad (15)$$

Here, Y_{jt} denotes the outcome of interest for restaurant j at time t (for example, establishment size or average monthly wages), α_j are establishment fixed effects, and δ_t are quarter-year fixed effects. $Treated_j$ is an indicator equal to one if restaurant j enrolls on the platform, and $t^*(j)$ is the quarter of first enrollment.[26] The indicators $\mathbf{1}\{t = t^*(j)+k\}$ index event time relative to treatment.

effects of mergers and acquisitions (Arnold, 2019), and the effects of outsourcing (Goldschmidt and Schmieder, 2017; Daruich et al., 2024), among others.

[24] By restricting the control group to establishments that are never treated over the relevant horizon, I avoid using "forbidden controls" (Borusyak et al., 2024).

[25] I normalize the number of competing restaurants by the total number of restaurants in the microregion to account for differences in market size.

[26] In the matched sample, control restaurants are assigned the treatment date of their matched treated restaurant.

The coefficients of interest are the β_k, which capture the causal effect of platform enrollment on the outcome of interest under the parallel-trends assumption. The event-study coefficients are normalized relative to β_{-2}, so the post-treatment coefficients measure effects relative to two quarters prior to enrollment. Standard errors are clustered at the establishment level.

Identification The key identifying assumption is that, absent treatment, the differences in outcomes between treated and control establishments would have remained constant over time. This assumption may be demanding because the decision to enroll on the platform is strategic and may correlate with both potential outcomes and past trends.[27] To address this concern, I match treated restaurants to controls located in microregions where the platform is unavailable. By comparing restaurants in areas with and without access to the platform, I ensure that control establishments are not self-selecting out of treatment. Under this design, the identifying assumption has two components: (i) conditional on the observables used for matching, treated and control restaurants would have followed similar trends; and (ii) common shocks affecting restaurants evolve similarly across treated and control regions.

To support the first component, I exploit the richness of the data. Restaurants are matched on wages, size, workforce composition, and the intensity of local competition. In addition, Figure IV shows that treated and control establishments follow similar pre-treatment trajectories in both average wages and firm size, including in periods not directly targeted by the matching procedure.

Regarding the second component, one concern is that adoption by treated restaurants could induce nearby competitors in the same market to adopt as well, in which case the estimated treatment effects would mix direct and indirect effects. Figure A4, however, shows no discrete increase in the likelihood that nearby restaurants within 1 kilometer adopt the platform after a treated firm enrolls. A related concern is that different microregions may face distinct shocks over time, especially during the Covid-19 pandemic in 2020 and 2021. To address this issue, I present results for restaurants that adopted before January 2019, so that their five-quarter post-treatment window is observed before the pandemic. I also show that the establishment-level treatment effects are robust to the inclusion of state-date fixed effects, which absorb state-specific Covid policies, and are unrelated to the number of Covid cases reported in the municipality of the treated establishment (Chauvin, 2021).

6.2 Research Design: Spillovers

To capture spillover effects on non-adopting restaurants, I define treatment as a sudden, large increase in the share of nearby establishments that enroll on the platform. I restrict the sample

[27] For example, restaurants may enroll in response to a negative demand shock. In that case, wages or employment could decline at enrolling restaurants even in the absence of the platform, biasing the estimates downward. Conversely, if firms enroll after a positive shock that enables them to expand, the estimates would be biased upward.

of potentially exposed restaurants to: (i) establishments that, at the time of the event, had never adopted the platform; (ii) establishments with at least five other restaurants within a 1-kilometer radius; and (iii) establishments that do not belong to a multi-establishment firm in which another location had already enrolled on the platform. Restriction (i) ensures that I study non-adopters at the time of treatment,[28] restriction (ii) guarantees that the restaurant faces a meaningful degree of local competition, and restriction (iii) excludes firms that may be affected by within-firm spillovers generated by adoption at another location.

For each potentially treated establishment j, I compute the share of restaurants within 1 kilometer that are enrolled on the platform in each quarter, denoted by χ_{jt}, and then calculate the change in this share between quarter t and quarter $t-1$: $\Omega_{jt} = \chi_{jt} - \chi_{jt-1}$. Treated restaurants are defined as those exposed to a large local adoption shock, namely establishments in the top 5th percentile of the distribution of Ω. When the same establishment experiences more than one such event, I retain only the first. Appendix Figure A5 plots the distribution of Ω: its mean is 0.01, whereas the top 5th percentile has a mean of 0.11 and a standard deviation of 0.04. Treated establishments are matched to control establishments using the same procedure as in the baseline analysis, and I then estimate the event-study model in equation (15).

6.3 Research Design: Workers

This section describes the empirical strategy used to estimate the effect of online-delivery platforms on the labor-market outcomes of incumbent restaurant workers. As in the establishment-level analysis, I construct the control group using a matching procedure, where potential control workers are drawn from labor markets in which no online-delivery platform was available at the time of treatment or in the following five quarters.

Matching I define treated workers as those employed at a restaurant when that restaurant enrolls on the platform. As in the establishment-level analysis, I restrict attention to workers treated at least five quarters before December 2021. In addition, I require workers to have at least two quarters of tenure at the time their employer adopts the platform.[29] For each treatment cohort (quarter $\times$ year), the pool of potential controls consists of restaurant workers employed in the same period in microregions where no online-delivery platform was available prior to treatment or in the subsequent five quarters.

Each treated worker is matched to a control worker based on characteristics measured in the quarter prior to treatment. Matching is exact on gender, occupation, quartile of firm size, and quartile of average firm monthly wage. As in the firm-level design, treated and control workers

[28] I do not restrict on subsequent adoption in order to avoid conditioning on a post-treatment outcome.

[29] Tenure restrictions are standard in the job-displacement literature (e.g., Jacobson et al., 1993; Schmieder et al., 2023; Bertheau et al., 2023), since they focus attention on workers with a meaningful degree of attachment to the firm.

must also lie on the same side of the median number of restaurants within a 1-kilometer radius in the worker's microregion.[30] Within each cohort, I implement one-to-one matching within each cell using a caliper algorithm (Stepner and Garland, 2017). Matching variables include monthly earnings in $t^* - 4$ to $t^* - 1$ (with a bandwidth of ±200 BRL), age (with a bandwidth of ±2 years), and tenure (with a bandwidth of ±1 quarter).

Econometric Framework I estimate the effect of employer platform adoption on incumbent workers' labor-market outcomes by applying equation (15) at the worker level. In this specification, Y_{it} is the outcome of interest for worker i at time t (for example, earnings or employment), α_i are worker fixed effects, and δ_t are quarter-year fixed effects. I also include a quadratic in age. $Treated_i$ is an indicator for whether worker i is employed by a restaurant that enrolls on the platform, and $t^*(i)$ denotes the date on which that employer adopts. The event-study indicators $\mathbf{1}\{t = t^*(i) + k\}$ index time relative to treatment. The coefficients of interest are the β_k, which capture the effect of employer platform adoption on worker outcomes. As in the establishment analysis, the event-study coefficients are normalized relative to β_{-2}, and standard errors are clustered at the worker level.

Identification relies on the parallel-trends assumption. Although the employer's decision to adopt the platform is plausibly exogenous to any given worker, this assumption cannot be tested directly. Figure VIII, however, shows that treated and control workers display similar pre-treatment trajectories in both targeted and untargeted moments used in the matching procedure.

7 Impact of Online-Delivery Platforms on Restaurants Labor Demand

This section discusses the impact that online-delivery platforms have on restaurants' labor demand.

7.1 Effects of Adoption of the Platform

Descriptive Evidence Columns (1) and (2) of Table I present summary statistics for matched control and treated restaurants in 2017. Workers at both types of establishments have, on average, 11 years of formal education—slightly less than complete high school—and earn approximately 1,500 BRL per month in 2018 reais. Workers at control establishments have somewhat higher tenure (2.6 years versus 1.4 years at treated establishments) and are slightly older (33.4 versus 32.5 years). Both treated and control restaurants predominantly employ Brazilian, full-time workers (97 percent) and have a similar share of female workers (59 percent). The average establishment in the matched sample employs 11 workers, and waiters account for roughly half of the workforce. Columns (3) and (4) report analogous statistics for all potential treated restaurants (including

[30] Occupations are based on the six-digit CBO code (Classificação Brasileira de Ocupações). I group them into four categories: waiters, cooks, administrative, and other. Appendix B9 provides details on this classification.

those that do not match) and for the full universe of restaurants in Brazil. Overall, restaurants that adopt the platform are larger than the average restaurant in Brazil and pay higher monthly wages.[31]

Figure III presents descriptive trends in hours worked for matched establishments that enroll on the platform. Panel (a) shows that hours supplied by in-house workers decline by 27.6 percent between t^*-2 and t^*+5. Part of this decline is offset by greater use of outsourced platform workers, so that total hours worked at the establishment—including app workers—fall by 21.3 percent over the same period. Panel (b) shows that, after enrollment, establishments rely increasingly on platform workers: app workers account for 2.4 percent of total hours in the quarter of enrollment, rising to 8 percent after five quarters. These descriptive patterns are consistent with a reallocation of labor away from in-house service activities and toward outsourced platform delivery. However, these raw trends do not by themselves identify the causal effect of platform adoption, since they do not account for the counterfactual path of adopting establishments. I therefore turn next to the event-study analysis.

Trajectories of treated and control restaurants Figure IV panel (b) plots the trajectories of log average monthly wages paid by treated and control restaurants. The two groups display similar wage trends in the pre-treatment period: average wages are largely stable between $t^* - 7$ and $t^* - 2$ and evolve similarly across treated and control establishments. Between $t^* - 1$ and $t^* + 5$, average wages decline by roughly 20 log points for both groups.[32] To avoid considering any potential anticipatory effects around the moment of adoption, I use $t^* - 2$ as the omitted period in the event-study analysis.

Panel (a) reports the corresponding trajectories for log firm size, measured as the number of formal in-house workers employed by the restaurant. Treated and control establishments exhibit similar levels and trends in the quarters prior to treatment. As with wages, firm size is broadly stable between $t^* - 7$ and $t^* - 2$ and then declines for both groups between $t^* - 1$ and $t^* + 5$. Over this interval, firm size falls by about 18 log points for treated establishments and 12 log points for controls. Unlike wages, however, firm size for treated and control restaurants diverges persistently after enrollment: five quarters after the event, treated establishments are on average

[31]Figure A7 panel (a) plots the distribution of AKM firm fixed effects for restaurants that ever enroll on the platform and those that never do, using the methodology of Abowd et al. (1999). Restaurants that adopt the platform have, on average, firm effects that are 8 log points higher. This suggests that adopters have systematically higher pay policies. AKM firm effects also predict the intensive margin of platform use: Figure A7 panel (b) shows a positive correlation between firm effects and the number of hours platform drivers work for each restaurant. Appendix D6 provides details on the AKM estimation.

[32]It is important to note that this decline refers to real average monthly wages paid to workers employed at the establishment. RAIS reports contracted hours and monthly wages. The fall in average wages for both treated and control establishments is likely related to the short-time work policies introduced during Covid, which allowed firms to reduce hours. Appendix Figure A8 splits the wage trajectories for firms adopting before and after January 2019 and shows that this dip is concentrated among firms exposed to the pandemic.

7 log points smaller than control establishments.

The decline in size for both groups likely reflects, at least in part, the life cycle and high turnover of restaurants, since the sample is restricted to establishments that were open for at least two years before treatment. Panel (c) illustrates the extensive margin: five quarters after the event, approximately 12 percent of restaurants in the sample have closed. I next turn to the regression-based estimates to assess these patterns formally.

Effects of Platform Adoption on Size, Hours and Wages Figure IV panel (d) plots the estimated β_k coefficients from equation (15) for log firm size. The estimates show that treated restaurants reduce the number of in-house workers after adopting the platform, and that this effect is persistent. Five quarters after enrollment, the number of in-house workers at treated firms decreases by 6 percent.

Figure V reports the estimated effect of platform adoption on the log number of hours worked by in-house workers (red line) and on the log number of hours including platform workers (blue line).[33] Consistent with a decline in the use of in-house labor, treated restaurants reduce in-house hours by 7.6 percent five quarters after adoption. This reduction is fully offset by hours supplied by outsourced platform workers, so that total hours worked at the establishment, once app workers are included, remain essentially unchanged. These patterns suggest that platform adoption primarily changes the composition of service labor rather than total labor input at the average adopting restaurant.

Figure IV panel (e) plots the effects of platform adoption on average monthly wages paid by restaurants. The estimates indicate a modest decline in average wages at treated establishments relative to controls. Although somewhat imprecise, they suggest that average wages decrease by about 2 percent between t^*+1 and t^*+4 and recover by t^*+5. On their own, however, these wage effects are difficult to interpret, as they may reflect a combination of changes in occupational composition and wage-setting responses, which I examine in the next section.[34] Panel (f) shows that the effect on restaurant closure is also modest: adoption increases the probability of closure by about 1.5 percent.

[33] App-worker hours are measured as the time platform drivers devote to delivering food from a specific establishment.

[34] A model with worker heterogeneity could also explain the disparity between wages and size through changes in the composition of workers—conditional on the occupational structure of the establishment. Figure A10 panel (a) plots the effect of platform enrollment on the average worker fixed effect at the establishment and finds a null effect. Figure A10 panel (b) uses the matched design to construct establishment-level treatment effects on average worker effects following Schmieder et al. (2023). The estimated effects are flat and close to zero, providing evidence that platform adoption does not change worker composition through sorting.

7.2 Effects by Occupation

This section examines how platform adoption affects labor demand across occupations. Table II presents difference-in-differences estimates for hours and monthly wages by occupation using the methodology of Borusyak et al. (2024). Column (1) reports the effect of platform adoption on the number of in-house waiters. Treated restaurants reduce waiter hours by 5 percent after adoption.

As discussed in Section 4.2, the vast majority of restaurants did not employ in-house delivery drivers before adopting delivery services through the platform. Although the analysis is therefore limited by the sparsity of delivery workers in the pre-treatment period, the point estimates in Column (3) indicate that restaurants that did employ delivery drivers before adoption reduce in-house delivery hours by 8.8 percent.

Column (2) reports the effect of platform adoption on cooks and shows a precisely estimated zero effect on cook hours. Through the lens of the model in Section 2, this null effect is informative because cooks are used in both dine-in and delivery production and therefore respond to changes in total restaurant demand rather than to the composition of demand across the two goods. The fact that cook employment remains unchanged suggests that, for the average adopting restaurant, the market expansion effect is limited. Combined with the evidence that waiter employment declines while total hours worked at the establishment—including platform workers—remain flat, this pattern implies that platform adoption mainly reallocates activity away from dine-in service and toward delivery, reducing the need for in-house waiters while increasing the use of outsourced delivery labor.

Column (4) reports the effect of platform adoption on waiter wages. On average, waiter wages decline by 2.5 percent after adoption. Column (5) reports the effect on cook wages and shows no statistically significant change, consistent with the null effect on cook employment.[35] Overall, the results indicate that, for the average adopting restaurant, the substitution effect dominates the market expansion effect: waiter employment declines, outsourced delivery labor increases, and cook employment remains unchanged. In the next section, I examine heterogeneity across restaurants to assess when the market expansion effect becomes more important.

7.3 Effects by Restaurant Density

So far, the results suggest that the market expansion effect is limited for the average adopting restaurant. The model in Section 2, however, also implies that this effect may vary systematically across locations. If online-delivery platforms reduce consumer search and transportation

[35] Column (6) reports the effect of platform adoption on the wages of delivery workers. The estimates indicate little change in delivery-worker wages, although these estimates should be interpreted with caution given the small sample size.

frictions, restaurants in lower-density areas—which typically attract less walk-in traffic and are located farther from the average consumer—may benefit more from the expansion of market reach than restaurants in denser areas (Vitali, 2022; Leonardi and Moretti, 2023).[36] In the model, cook employment depends only on total restaurant revenue. Therefore, under the assumption that cook labor-supply elasticities do not vary systematically across locations, differential effects on cooks across high- and low-density areas can be interpreted as differences in the strength of the market expansion effect.

To measure local restaurant density, I geocode the universe of formal restaurants in Brazil between 2017 and 2021. For each restaurant, I count the number of nearby restaurants within a 1-kilometer radius and denote this measure by τ_{jt}. I then compute quartiles of τ by microregion and year, and classify restaurants as being in high-density areas if their τ_{jt} is above the median in the quarter before they adopt the platform.[37]

To help interpret the heterogeneous treatment effects, I first examine whether the reallocation from in-house service toward delivery differs systematically across locations. Figure A11 shows that the ratio of delivery to in-house waiter wage bills and hours evolves similarly across high- and low-density areas, suggesting that the substitution effect is not markedly different across the two groups.

I then estimate the effects of platform adoption on firm size and total hours by restaurant density. Figure VI panel (a) shows that restaurants in high-density areas reduce in-house labor substantially more than restaurants in less dense areas. Five quarters after adoption, in-house labor falls by approximately 8 percent in high-density areas, whereas restaurants in low-density areas show essentially no change. Panel (b) shows that these differences remain when platform-worker hours are included. Total hours at restaurants in high-density areas remain essentially unchanged, while total hours at restaurants in low-density areas increase by 6 percent five quarters after adoption.

Table III breaks these effects down by occupation. Column (1) shows that waiter hours at restaurants in high-density areas decline by 7.2 percent after platform adoption. By contrast, Column (3) shows no statistically significant effect on waiter hours in low-density areas. Consistent with the overall hours results, cook hours are unchanged in high-density areas (Column 2), while they increase by 7.6 percent in low-density areas (Column 4). Through the lens of the model, these patterns suggest that the market expansion effect is substantially stronger in less dense areas: there, the increase in total demand is large enough to raise demand for cooks and

[36] Intuitively, restaurants with lower baseline demand stand to gain more from the platform if it allows them to reach customers who would otherwise be too costly to serve. By contrast, restaurants with stronger pre-existing local demand may experience a larger reallocation from dine-in to delivery with a smaller increase in total demand.

[37] Appendix D8 describes the geolocation procedure and reports the share of restaurants that I am able to geolocate in each year.

offset the reallocation away from in-house service. In denser areas, by contrast, the substitution effect dominates and restaurants primarily shift activity from dine-in service toward delivery.

Taken together, these results indicate that the effects of online-delivery platforms on restaurant labor demand depend importantly on location. In particular, platforms appear to provide restaurants in less central areas with an alternative path to expand demand without relying as heavily on the local demand externalities associated with agglomeration (Vitali, 2022; Leonardi and Moretti, 2023).

7.4 Spillovers

Online-delivery platforms may affect not only adopting restaurants but also nearby non-adopters. This can arise if platform adoption by some restaurants reallocates consumer demand away from competing establishments that do not adopt (Chava et al., 2024). Understanding these indirect effects is important for assessing the overall impact of delivery platforms on firms and workers in the restaurant sector.

Following the discussion in Section 6.2, I define an event for non-adopters as a large, sudden increase in the share of restaurants within a 1-kilometer radius that adopt the online-delivery platform. Figure VII panel (a) shows the trends in firm size for firms exposed to a spillover event and their matched pairs. Panel (b) shows the equivalent trajectories for the average monthly wages paid by non-adopting restaurants. Prior to the event, non-adopting firms do not appear to experience a larger negative shock than adopting restaurants (as reported in Figure IV), suggesting that restaurants are not selecting into earlier adoption based on their ex-ante trends.[38]

Figure VII panel (d) plots the estimated effects of the spillover event on the log firm size of non-adopting restaurants. Firm size falls by 3 percent on impact, although this effect is not persistent and is close to zero five quarters later. Panel (e) shows a modest decline in average monthly wages of about 2 percent after five quarters. These effects, however, need to be interpreted jointly with the extensive margin. Panels (c) and (f) show that restaurants exposed to a large local increase in nearby adoption are 5 percent more likely to close within five quarters than their matched controls. Taken together, these results suggest that adoption by some restaurants crowds out demand at nearby non-adopters, increasing their likelihood of contraction and exit.

7.5 Robustness

Availability of the Platform as an Instrument for Adoption Platform adoption is a strategic decision, so unobserved shocks among adopters may bias the baseline matched event-study estimates even when the control group is observationally similar and not yet exposed to the tech-

[38]Recall that I do not condition on post-event adoption of the platform. Approximately 7 percent of the ex-ante non-adopting restaurants adopt the delivery platform within 5 quarters following the spillover event. Table B5 presents the summary statistics for restaurants exposed to spillover events and matched control restaurants.

nology. To address this concern, I instrument for platform adoption using the number of quarters elapsed since the platform first became available in each microregion, exploiting the staggered geographic rollout as a source of plausibly exogenous variation.[39] This IV strategy helps address a central concern with the baseline design: if adopters are positively (negatively) selected on unobservables correlated with subsequent growth, the matching estimator may overstate (understate) the causal effect. At the same time, the IV is not my preferred identifying design because it introduces its own threat to identification. If platform availability directly affects non-adopting restaurants through spillovers—as the evidence in Section 7.4 suggests—then the exclusion restriction is violated, potentially biasing the IV estimates through spillover effects on non-adopters. Details of the IV procedure are reported in Appendix D1.

Table B4 presents the instrumental-variables results. In the first stage, longer exposure to platform availability strongly predicts restaurant adoption. I then estimate 2SLS regressions for 2017–2021 changes in log firm size and log monthly earnings, controlling for pre-period restaurant characteristics and clustering standard errors at the microregion level. I also report in Figure A12 intention-to-treat (ITT) event-study estimates that use platform availability directly as the treatment variable and therefore capture the total effect of rollout on local restaurant outcomes without relying on the exclusion restriction.[40] Importantly, the ITT event-study allows me to assess pre-trends in restaurant outcomes prior to platform entry, providing evidence on the plausibility of the exogeneity assumption underlying the IV strategy.

The IV estimates are broadly consistent with the baseline results, although somewhat more negative. The estimates imply that adoption reduces firm size by approximately 16%, while the effects on earnings are not statistically distinguishable from zero at conventional significance levels. This pattern is informative in two respects. First, the similarity to the baseline estimates suggests that selection into adoption is unlikely to be a first-order concern. Second, the modest downward shift in the IV estimates is consistent with negative spillover effects on non-adopters, which would contaminate the instrument through a violation of the exclusion restriction. Finally, the ITT event-study estimates in Figure A12 show no evidence of differential pre-trends: before platform entry, restaurants in microregions that receive the platform evolve similarly to restaurants in microregions where the platform is not yet available.

Matching A potential concern with matching on pre-treatment outcomes is that the estimated effects may reflect mean reversion rather than the causal effect of platform adoption. This concern is partly mitigated by the fact that the matching procedure leaves some pre-treatment peri-

[39] This research design is similar in spirit to approaches used to study the impact of broadband internet on productivity (Akerman et al., 2015).

[40] This specification is in the same spirit as the market-level regression in Section 5, but it exploits within-establishment variation by including firm fixed effects. As in the market-level analysis, I drop microregions where the platform was already available before 2018 or entered in 2021 in order to retain sufficient post-entry periods.

ods untargeted. To further assess this possibility, Figure A13 replicates the matching design in a placebo exercise: I assign fake treatment dates to a target sample of 15,000 restaurants that never adopted the platform and were never exposed to a spillover event, drawing placebo dates from the same microregion-by-date distribution as the actual treated cohorts.[41] I then match these placebo restaurants to the same control pool used in the baseline analysis using the identical propensity-score matching procedure. Figure A13 shows that the estimated placebo effects on firm size, average worker earnings, and closure are statistically indistinguishable from zero following placebo treatment assignment. These results are consistent with the view that the baseline estimates are not mechanically generated by the matching procedure or by mean reversion in the matched outcomes.

Covid The period of analysis overlaps with the COVID-19 pandemic, which raises concerns about external validity. In particular, if restaurants most negatively affected by the pandemic are also the most responsive to the platform, the estimated effects may partly reflect this unusual environment. To address this concern, I take three approaches. First, using the matched design, I plot in Figure A14 the establishment-level treatment effects on size and wages against the change in Covid-19 cases per capita in the post-treatment period. The figure shows no clear relationship between treatment effects and local Covid incidence. Second, I estimate the effect of platform adoption separately for establishments treated up to five quarters before the pandemic and for those treated during the pandemic. Figure A15 shows that, if anything, establishments adopting before the pandemic reduce in-house size even more. Third, I exploit within-state variation in platform availability and estimate a specification with state-year fixed effects to absorb state-specific policies that may have affected restaurants during Covid (Chauvin, 2021). Figure A16 shows that the results are not sensitive to the inclusion of these fixed effects.

Informality An important feature of the Brazilian setting is the relatively high level of informality compared with developed countries such as the United States and Canada. If restaurants that adopt the platform employ a substantial share of their workforce informally, part of the adjustment in labor demand may not be observed in RAIS, which only covers formal workers. To assess this possibility, I estimate the share of formal workers in the restaurant sector in each municipality using the 2010 Brazilian Census, the latest available census. I then split the matched sample into municipalities above and below the median level of informality. Figure A17 shows that restaurants in municipalities with below-median informality reduce their in-house formal employment by approximately 9 percent after five quarters of adoption. By contrast, restaurants

[41] The final sample in the placebo exercise includes 8,224 restaurants because: (i) I drop observations that are randomly assigned more than one placebo treatment date; (ii) some microregion-date cells do not contain enough never-treated restaurants to reach the target sample size; and (iii) as in the baseline analysis, restaurants must be active in the two years prior to the placebo treatment date.

in municipalities with high informality reduce formal employment by only 2 percent, suggesting that part of the adjustment in those areas may occur through informal labor.[42] When assessing the overall effect of the platform on workers in Section 9, I explicitly account for the possibility that platform adoption also affects informal workers.

Alternative Estimators Following the recent difference-in-differences literature on staggered treatment, Figure A18 shows that the estimated effects of platform adoption on monthly wages and firm size are robust to the estimator proposed by Borusyak et al. (2024). This consistency is expected, since the control establishments are drawn from regions where the platform is not available and therefore are never exposed to treatment (Roth et al., 2023).

Spillovers The spillover design focuses on local effects within a 1-kilometer radius of adopting restaurants. This approach provides a clean source of variation based on discrete jumps in nearby adoption, but broader equilibrium effects may extend beyond that radius. In Section 9.1, I therefore present an alternative spillover calculation based on the market-level employment effects in Section 5. That approach decomposes total market-level employment changes into the component attributable to adopting restaurants and a residual that captures displacement at non-adopting restaurants, thereby allowing for spillover effects that operate beyond the immediate local neighborhood.

8 Restaurant Workers

The results in Section 7 provide evidence that online-delivery platforms reshape restaurant labor demand by reallocating service activity away from in-house workers and toward outsourced delivery labor. To understand the labor-market consequences of this adjustment, it is necessary to examine its effects on workers themselves. This section studies the impact of platform adoption on the labor-market outcomes of incumbent restaurant workers and quantifies how much of the decline in in-house labor demand is borne by those workers. The analysis sets the stage for Section 9, where I assess the overall effects of delivery platforms on worker earnings by combining the impacts on incumbent restaurant workers, workers affected by spillovers, and gig workers.

8.1 Main Results: Effect of Employer Adoption on the Platform

Summary Statistics Table IV reports summary statistics for matched control workers (Column 1), matched treated workers (Column 2), and all potential treated workers, including those who do not match (Column 3), measured in the quarter prior to treatment. Matched treated and control workers earn, on average, 1,500 BRL-2018 per month, work full time, have 11 years of formal education, and are 33 years old. Women represent 59 percent of the matched sample. In terms of

[42] This evidence is suggestive, since treatment effects may also differ across regions for other reasons, including differences in the market expansion effect.

occupations, 67 percent of matched workers are waiters, 18 percent are cooks, and the remaining 15 percent are employed in other occupations. The average tenure of matched workers is 3.7 years. Relative to all potentially treated workers, matched workers have somewhat lower earnings (1,501 versus 1,788 BRL-2018) and a slightly higher share of women and waiters.

Trajectories of Treated and Control Workers Figure VIII panel (a) plots the earnings trajectories of treated and control workers. The two groups display similar earnings levels prior to treatment. After the employer adopts the platform, treated workers experience a somewhat larger decline in formal-sector earnings than control workers, and this gap persists over the subsequent five quarters. In the quarter after the event, treated workers' earnings decline by 11.3 percent, compared with 9.7 percent for control workers. Five quarters after the event, earnings have fallen by 37.1 percent for treated workers and by 35.4 percent for controls.

Panel (c) reports the corresponding trajectories for formal employment. As with earnings, treated and control workers exhibit similar pre-treatment trends and a persistent decline over time. Five quarters after the event, formal employment has fallen by 32.7 percent for treated workers and by 31 percent for control workers. I next turn to the regression-based estimates to quantify these differences formally.

Earnings & Employment Figure VIII panel (b) plots the estimated β_k coefficients from equation (15) for monthly formal-sector earnings. In the quarter following the employer's adoption of the platform, treated workers' earnings decline by 20 BRL-2018, or about 1.5 percent of their pre-event average earnings. This negative effect remains modest and persists over the following quarters, although the estimates become noisier by the fifth quarter.[43] Panel (d) reports the corresponding effects on formal employment. Workers whose employer adopts the delivery platform are 1.5 percentage points less likely to be employed in the formal sector, and this effect also persists over time.

Figure A19 panel (a) reports the cumulative probability that workers leave their pre-event establishment, either for another employer or into non-employment. Workers whose employer adopts the platform are more likely to separate from the firm after the event: in the quarter following adoption, they are 2 percent more likely to leave their employer, and this differential rises to 6 percent after five quarters. These estimates mirror the establishment-level decline in in-house employment documented in Figure IV panel (d). Combined with the modest employment effects in Figure VIII panel (d), they indicate that many workers leave the firm after adoption but reallocate relatively quickly to other formal-sector jobs. Figure A19 panel (b) is consistent with this interpretation: by the fifth quarter after the event, approximately 75 percent of the workers

[43] For context, the negative earnings effect of mass layoffs in Brazil is estimated to be 40 percent (Britto et al., 2022). In Germany, workers exposed to a domestic outsourcing event who remain employed lose approximately 5 percent of their earnings in the year following the event (Goldschmidt and Schmieder, 2017).

who separate from their original employer are employed by a new formal-sector firm. This pattern helps explain why the earnings losses at the worker level are modest relative to the decline in in-house employment at the establishment level.

Transition to Delivery Platform Work The richness of the data also allows me to examine whether workers whose employer adopts the platform are more likely to transition into gig delivery work themselves. This is useful for assessing whether platform income may offset part of the negative effect on formal-sector earnings. Figure A20 plots the estimated effects on the probability of working on the delivery platform. In the quarter following the employer's adoption of the platform, workers are 0.1 percent more likely to start working on the platform. This effect rises only slightly over time, reaching 0.4 percent after five quarters. The small increase in transitions to platform work suggests that incumbent restaurant workers are largely being replaced by a different set of workers, rather than being directly outsourced into gig delivery. I return to the characteristics of these platform workers in Section 9.

8.2 Spillovers on Workers

To characterize the effects of online-delivery platforms on workers more broadly, it is also necessary to study workers employed at restaurants that do not adopt the platform. Section 7.4 showed that nearby adoption generates negative spillovers on non-adopting establishments, increasing their likelihood of contraction and closure. Figure IX panel (b) reports the estimated effects of equation (15) on total earnings for workers employed at non-adopting restaurants exposed to a sharp increase in the share of nearby restaurants on the platform, as defined in Section 6.2.[44] The estimates show that workers at non-adopting restaurants experience larger losses than workers at adopting restaurants. Earnings decline by 3 percent in the quarter following the event and continue to fall thereafter, reaching 6.6 percent five quarters later. Panel (c) shows that these workers also experience substantial employment losses: they are 2.3 percentage points less likely to be employed in the formal sector in the quarter following the event, and 3.8 percentage points less likely after five quarters.

The larger effects on workers at non-adopting restaurants appear to be driven primarily by a higher incidence of displacement, rather than by slower reallocation conditional on job loss. Figure A21 illustrates this mechanism by comparing workers whose employer eventually closes with those whose employer remains open. Panel (a) shows that workers employed at restaurants that remain open (92 percent of the matched sample) experience an earnings decline of approximately 2.6 percent five quarters after the spillover event. By contrast, workers whose employer

[44] Panels (a) and (c) report the raw means for matched treated and control workers. Table B6 presents summary statistics for incumbent restaurant workers whose employer is exposed to a spillover event and their matched controls.

at $t^* - 1$ eventually closes (8 percent of the matched sample) experience a 37 percent decline in earnings over the same horizon. This difference is largely driven by the extensive margin of employment: workers employed at restaurants that eventually close are 40 percent less likely to be employed five quarters after the event.

Taken together, these results suggest that online-delivery platforms affect workers not only through direct adoption by their employer but also through negative spillovers on nearby non-adopting restaurants. These spillover effects are larger than the direct effects of employer adoption, largely because nearby adoption increases the likelihood that non-adopting restaurants close. In the next section, I combine these losses with the gains to platform workers to assess the overall earnings effects of delivery platforms.

9 Winners & Losers

In this section, I use the reduced-form estimates in a partial-equilibrium accounting exercise. The goal is not to recover a fully causal welfare effect of the platform, but to assess whether the measured losses among restaurant workers are plausibly offset by measured earnings gains to platform workers under transparent assumptions. Specifically, I quantify the overall effect of online-delivery platforms on workers' earnings by combining the losses experienced by incumbent restaurant workers with the gains accrued by incoming gig workers. Following Section 8, I distinguish three components: the direct effect on in-house workers at adopting restaurants, the indirect effect on workers at non-adopting restaurants exposed to spillovers, and the effect on platform workers. Unlike a full welfare analysis, which would require evaluating the platform's effects beyond financial outcomes, I focus on earnings—an outcome that is directly comparable across worker groups. Although this narrows the scope of the exercise, it provides a transparent accounting of the trade-offs that delivery platforms generate across workers while minimizing assumptions about welfare.[45]

The reduced-form estimates identify how platform adoption affects worker earnings and employment probabilities for the groups observed in the data. I then combine these estimates with additional assumptions to assess how the resulting gains and losses are distributed across worker groups. This exercise is informative about the incidence of platform adoption, but its aggregate implications depend on three inputs: the outside options of non-formal app workers, the extent of unobserved earnings effects for informal workers, and the definition of spillovers to workers at non-adopting restaurants. I therefore interpret the accounting as a partial-equilibrium bench-

[45] If restaurant and gig jobs offered similar amenities, then under a utility function that depends linearly on earnings—such as the one proposed in Section C1—differences in earnings across jobs would map proportionally into differences in welfare. A similar argument would apply if the value of non-pecuniary job characteristics were small relative to the marginal utility of income. While the first assumption is unlikely to hold given the differing job characteristics, the second may be plausible in this setting given the relatively low income of the workers studied.

mark and report sensitivity analyses for each of these margins.

To study the total effect of online-delivery platforms on workers' earnings, I aggregate the earnings effects associated with each adopting restaurant. Specifically, for each adopting restaurant j, I conduct the following accounting exercise:

$$\underbrace{\pi^{j}_{\text{app}}}_{\substack{\text{Total Wage-Bill Effect} \\ \text{of the Platform}}} = \underbrace{\pi^{j}_{\text{in-house}}}_{\substack{\text{Wage-Bill Effect} \\ \text{on In-House Workers}}} + \underbrace{\pi^{k\neq j}_{\text{spillovers}}}_{\substack{\text{Wage-Bill Effect} \\ \text{from Spillovers}}} + \underbrace{\pi^{j}_{\text{app-workers}}}_{\substack{\text{Wage-Bill Effect} \\ \text{on App Workers}}}. \tag{16}$$

Each component is computed as the total earnings effect for a given group multiplied by the average number of workers in that group affected per adopting restaurant. The first two terms capture the losses borne by restaurant workers. Table V summarizes the corresponding wage-bill effects per adopting restaurant.

Column (1) reports the total earnings losses of in-house restaurant workers after a restaurant adopts the platform. The cumulative present discounted value of these losses over five quarters is 2,878 BRL (in 2018 reais), equivalent to 5.3 percent of the pre-platform wage bill of adopting restaurants. Column (2) reports the same calculation net of social security contributions. As discussed in Section 3, this adjustment is useful when comparing losses for restaurant workers with gains for app workers, who are not required to make these contributions. Net of social security contributions, the loss for in-house workers is equal to 5.0 percent of the pre-platform wage bill.

Column (4) reports the earnings losses generated by spillovers per adopting restaurant. To construct this object, I begin with the per-worker earnings effect of a spillover event estimated in Section 8.2 and rescale it into units of additional nearby adopters using the first-stage relationship between spillover events and nearby platform adoption. I then multiply this rescaled per-worker effect by three objects: the average number of nearby non-adopting restaurants potentially exposed to spillovers, the probability of observing a spillover event under the baseline event-based design, and average restaurant size. This yields an average realized spillover wage-bill loss per adopting restaurant. Appendix D2 provides the exact formula and discusses the interpretation of each term. The cumulative present discounted value of spillover losses over five quarters is 9,281 BRL (in 2018 reais), equivalent to 17.0 percent of the pre-platform wage bill of adopting restaurants. Net of social security contributions, this effect is 15.6 percent (Column 5).

Assessing the gains generated for app workers is more challenging. A substantial share of delivery workers may have been employed informally or unemployed before joining the platform and are therefore not directly observed in RAIS. More generally, app workers have weak attachment to the formal sector. Figure A22 panel (a) shows that seven quarters before their first delivery, 27 percent of drivers held formal jobs. This share declines to 21 percent at the time of

their first delivery and recovers only slightly to 23 percent five quarters later.[46] In addition, quantifying the gains for app workers requires accounting not only for delivery earnings, but also for outside options and work-related costs such as fuel and vehicle maintenance. I use survey evidence and estimates from Callil and Picanço (2023), who report maintenance and transportation costs of 3.7 BRL-2018 per hour worked for motorcycle delivery drivers.

To estimate outside options, I divide app workers into two groups: (i) those who held a formal job in the quarter before starting on the platform and were not laid off, and (ii) those who did not hold a formal job, or were laid off, in the quarter before or during their first quarter on the platform. For the first group, I take a conservative approach and use pre-platform hourly earnings as the outside option.[47] For the second group, whose outside option is not observed directly, I reweight workers in PNAD-C so that their demographic composition matches that of platform workers (DiNardo et al., 1996), and then use the corresponding reweighted wage of non-formal workers in each state and quarter as the relevant outside option. Appendix D3 provides details on this procedure.

Formally, I define the app-worker component for restaurant j as

$$\pi^{j}_{\text{app-workers}} = \sum_{g \in \{F, NF\}} H^{j}_{g} \left(W_g - \zeta_g - \kappa_g \right), \tag{17}$$

where H^{j}_{g} denotes the number of platform hours used by restaurant j from workers in group g, W_g is the hourly earnings of those workers on the platform, ζ_g is their hourly outside option, and κ_g denotes hourly transportation and maintenance costs.

Column (7) of Table V reports the average wage bill generated per adopting restaurant for platform workers who held a formal job prior to working on the app.[48] The cumulative present discounted value of their gross earnings is 5,087 BRL (in 2018 reais), equivalent to 9.3 percent of the pre-adoption wage bill of adopting restaurants. Column (8) nets out outside options and transportation and maintenance costs. Under this adjustment, the cumulative present discounted gain for this group falls to 1,197 BRL, or 2.2 percent of the pre-adoption wage bill.

Column (9) reports the wage bill generated per adopting restaurant for workers who did not hold formal employment, or had been laid off, in the quarter before joining the app. The cumu-

[46] Figure A22 panel (b) plots formal and delivery earnings for app workers who held a formal job in the quarter prior to working on the platform. Consistent with the platform allowing workers to smooth consumption over time, these workers experience a negative income shock in the months before their first delivery. Similar patterns have been documented in the United States (e.g., Koustas, 2018; Jackson, 2022).

[47] This assumption is conservative because it implies that workers who voluntarily transition from the formal sector to the platform and increase their total hours are not hours constrained. If some of these workers were in fact hours constrained, the estimated gains from the platform for this group should be interpreted as a lower bound.

[48] A limitation of the data is that I do not observe which worker delivered for which restaurant at a specific time. Instead, I observe each worker's hourly earnings and the total number of platform hours used by each restaurant in each municipality-month. I therefore impute restaurant-level app-worker earnings using municipality-month averages. Appendix D4 discusses this imputation in detail.

lative present discounted value of their gross wage bill is 25,204 BRL (in 2018 reais), equivalent to 46.1 percent of the pre-adoption wage bill. Column (10) adjusts this amount for transportation and maintenance costs and for the outside options estimated using PNAD-C. Under this baseline measure, the cumulative present discounted gain for this group is 15,892 BRL, or 29.1 percent of the pre-adoption wage bill.

Taken together, and before accounting for informal restaurant workers, the baseline estimates imply that the platform generates a net earnings surplus for workers equivalent to 10.6 percent of the average pre-adoption wage bill of adopting restaurants. Put differently, the gains for app workers—net of their costs and outside options—are 52 percent larger than the combined direct and indirect losses borne by formal restaurant workers.

9.1 Discussion & Limitations

Informal Restaurant Workers The estimates presented so far do not account for the effects of platform adoption on workers employed informally in the restaurant sector. Informality can arise through two margins: workers employed informally by registered restaurants (the intensive margin), and workers employed by fully informal firms that do not have a tax identifier (the extensive margin) (Ulyssea, 2018). To account for the intensive margin, I use the 2010 Brazilian Census and PNAD-C to impute the number of informal workers employed by registered restaurants (see Appendix D5 for details on the imputation procedure and a sensitivity analysis for the assumed earnings effects of informal workers). Importantly, PNAD-C provides information on informal workers, but not on informal firms. I estimate that, on average, 25 percent of the workforce in registered restaurants in my sample is informal. Column (3) shows that the estimated earnings losses for informal workers at adopting restaurants are equivalent to 1.2 percent of the pre-platform wage bill. In Column (6), I estimate the impact on informal workers employed by registered restaurants that are affected by spillovers, using the same methodology applied to in-house workers. The implied effect is a loss equivalent to 3.9 percent of the pre-platform wage bill of adopting restaurants. Incorporating these imputed effects for informal workers reduces, but does not overturn, the main conclusion: under the baseline estimates, the total earnings effect of the platform on workers remains positive at 5.7 percent of the pre-platform wage bill.

A limitation of the data, however, is that I cannot account for the extensive margin of informality. In particular, I do not observe the number, size, or location of fully informal restaurant firms, which are likely smaller on average than registered firms (Ulyssea, 2018). These firms cannot adopt the platform, but they may nonetheless be affected by spillovers if nearby adopting restaurants divert consumer demand away from them. Because my spillover calculations only include workers at registered firms, any earnings losses borne by workers at fully informal firms are omitted. In that sense, the estimated spillover losses should be interpreted as applying to the

registered sector and may understate total spillover losses in the restaurant sector as a whole. To assess the magnitude required for this omission to overturn the baseline result, I compute a break-even benchmark. Spillover effects on workers at fully informal firms would need to amount to 36.5 percent of the estimated spillover effect on formal workers at registered firms for the total effect of the platform on worker earnings to break even.[49]

Effects by Gender As reported in Table B2, the vast majority of delivery drivers are men, whereas about 60% of workers in Brazil's restaurant sector are women. This compositional difference implies that the aggregate earnings effects of delivery platforms are highly heterogeneous by gender. Table B7 reports the effects on in-house workers, spillover workers, and platform workers separately for men and women. A clear contrast emerges: men experience gains equivalent to 14.7% of the pre-platform restaurant wage bill, whereas women experience losses equivalent to 13.7%.[50] Explaining the low participation of women among platform drivers is beyond the scope of this paper. However, future work should examine the sources of this participation gap, which may reflect differences in labor supply elasticities, job amenities, or safety concerns across genders (Caldwell and Oehlsen, 2023).

Alternative Outside Options The outside options estimated using PNAD-C provide a reasonable benchmark; however, the outside options of gig workers may not be fully captured by survey data. Figure A23 therefore presents the total earnings effect of the delivery platform as a function of the outside option of non-formal app workers, expressed as the ratio of their outside option to the earnings of platform workers who transition from the formal sector (Φ). The figure highlights several informative scenarios. If delivery workers without formal jobs had an outside option of zero (for example, unemployment), the implied total worker-earnings surplus would be approximately 13.75 percent of the pre-platform wage bill. At the other extreme, if all non-formal app workers had the same outside option as formal workers, the platform would generate a worker-earnings deficit of 12.77 percent of the pre-platform wage bill. The break-even point occurs when the outside option of non-formal app workers is equal to 52 percent of the outside option of formal workers, in which case the implied worker-earnings surplus is zero.

Spillovers The baseline spillover calculation (Table V, columns 4–6) relies on a rescaling procedure that maps the worker-level spillover earnings coefficient estimated within a 1-kilometer radius into a per-adopting-restaurant effect. I implement a sensitivity analysis using a bounding

[49] This benchmark implies that, if the per-worker earnings effect were the same in formal and informal firms and average firm size were similar across the two groups, there would need to be 0.36 informal firms affected by spillovers for each formal firm affected for the aggregate worker-earnings effect to break even. Given that the average share of informal restaurant workers in my sample is 25 percent, this scenario appears unlikely.

[50] The sum of the gender-specific effects may differ from the overall total because both worker-earnings effects and outside options are estimated separately by gender.

exercise based on an additive decomposition of market-level employment flows. Specifically, I first estimate the total change in restaurant-sector employment at each event time τ using the market-level regression in Figure II, panel (a). I then compute the component attributable to adopting restaurants by convolving the flow of new adopters at each period $s \leq \tau$ with the estimated firm-size effect at lag $\tau - s$, evaluated at the baseline firm size of non-adopters. The residual—defined as total sector employment change minus the adopter-driven component—captures displacement at non-adopting restaurants. Dividing this residual by the number of new adopters yields the number of workers displaced per adopter.

I implement two versions of this exercise. In version (i), I use the displacement rate estimated at $\tau = 0$ as a constant rate in all periods, motivated by the fact that the initial per-adopter displacement rate is less likely to be contaminated by interactions with earlier adoption cohorts. In version (ii), I allow the displacement rate to vary by event time. I then obtain the aggregate earnings effect by convolving these per-adopter displacement rates with the spillover worker-level earnings coefficients.

Table B8 reports the results. Both versions imply more negative spillover effects than the baseline. The cumulative present discounted value of spillover earnings losses is -21.2% of the pre-platform wage bill per adopter under version (i) and -30.4% under version (ii), compared with -15.6% in the baseline specification. I interpret these estimates as an upper bound, in absolute value, on spillover effects. The baseline rescaling procedure restricts spillovers to firms within a 1-kilometer radius and may therefore miss effects on more distant firms, but it benefits from a cleaner source of identification based on discrete local variation in nearby adopters. By contrast, the alternative approach captures market-wide displacement—including effects beyond the local radius—but attributes the entire residual decline in sector employment to spillovers from adopters, which may overstate the spillover effect if part of the sector-level decline reflects other forces, such as structural change in the sector.

Amenities The exercise in this section focuses on the monetary value created and destroyed by delivery platforms, abstracting from non-pecuniary job characteristics that may also shape worker welfare. If platform work offers valued flexibility (Mas and Pallais, 2020), the gains for app workers may exceed those measured here. Conversely, if working on the platform entails greater health risks, safety concerns, or job insecurity (Jarosch, 2023), focusing on earnings alone may overstate its value. To provide a benchmark for these non-pecuniary dimensions, I estimate the value of platform work using the PageRank approach of Sorkin (2018). Specifically, I treat the platform in each microregion as an employer-like destination and use transitions between formal-sector jobs and platform work for workers whose main source of income is the platform to infer the relative value of working on the platform. This revealed-preference measure therefore captures the value of platform work for workers who move between the formal sector and gig

work, rather than the value of employment at the platform company itself. Appendix D7 provides details on the estimation.[51]

Figure A25 shows that firm value is positively correlated with pay policies, as measured by AKM firm effects, both for restaurants (panel a) and for all establishments (panel b). Figure A24 plots the distribution of V_j^e across Brazilian restaurants for which value can be estimated, representing approximately 30 percent of all formal establishments. The red dashed line marks the platform's value. Platform work ranks at the 90th percentile of V_j^e among formal restaurant jobs in Brazil, suggesting that the welfare effects of delivery platforms may extend beyond earnings alone.

Limitations The approach used to assess the costs and benefits of the platform focuses on its partial-equilibrium impact and does not capture the broader reallocation effects that the platform may generate over time. For example, platform work may serve as a stepping stone for non-formal workers (Booth et al., 2002; Jahn and Rosholm, 2014), potentially facilitating entry into the formal sector. Alternatively, delivery platforms may redirect workers toward less advantageous career paths by sending negative signals to employers (Neumark, 2018) or by reducing job-search effort (Le Barbanchon et al., 2024). Likewise, in-house workers displaced by outsourced delivery labor may reallocate to other firms (Dustmann et al., 2022), potentially affecting their long-run careers in ways that the time horizon of the data cannot capture. These broader effects are beyond the scope of this paper but are important for understanding the overall labor-market consequences of gig platforms.

A second limitation is that, although the platform studied here accounts for a large share of the delivery market, workers may combine it with other platforms (Caldwell and Oehlsen, 2023) or with other forms of informal work, which I cannot observe. If workers are able to generate additional income because the platform offers flexibility, my estimates may understate the gains to app workers. Conversely, if platform work crowds out access to higher-paying jobs, the gains to app workers may be overstated.

10 Conclusion

Over the past decade, online-delivery platforms have expanded rapidly around the world, reshaping the relationship between restaurants and workers. This paper studies the effects of online-delivery platforms on firms and workers in Brazil. Using a novel data set that combines administrative employer-employee records with platform-worker data, I show that platform adoption substantially changes restaurant labor demand. Adopting restaurants reduce in-house waiter

[51] A limitation of this design is that it is informative only for the subset of workers who move between the formal sector and the platform and for whom the platform is the main source of income while working on it. If these movers have above-average utility for platform work, the resulting estimates may be upward biased.

employment and substitute toward outsourced delivery labor. However, the earnings losses for incumbent workers at adopting restaurants are modest, largely because many of those who separate reallocate relatively quickly to new formal-sector jobs. At the same time, platform adoption generates negative spillovers on non-adopting restaurants, increasing their likelihood of contraction and closure.

I then assess the overall effect of delivery platforms on workers' earnings by comparing the losses experienced by restaurant workers with the gains accrued to platform workers. Accounting for both gig workers and informal workers in the restaurant sector, I find that, under the baseline assumptions, the estimates imply a worker-earnings surplus equal to 5.7 percent of the average pre-platform wage bill of adopting restaurants. These gains are driven primarily by workers who did not hold formal employment when they began platform work.

The results highlight that the effects of delivery platforms are not uniform across workers. On the one hand, platform adoption displaces some incumbent restaurant workers and generates negative spillovers on non-adopters. On the other hand, it creates substantial earnings gains for workers with weak attachment to the formal sector. These findings suggest that policy discussions around gig platforms should account for both margins. In particular, focusing only on the losses borne by traditional employees would miss an important part of the picture in settings such as Brazil, where high informality and unemployment make platform work a potentially valuable source of earnings for workers with limited outside options.

More broadly, the paper shows that the labor-market effects of firm-side digital adoption cannot be assessed by looking only at workers in adopting firms. Technologies of this kind may reshape labor demand within adopters, generate spillovers on competing firms, and create new earnings opportunities outside traditional employment. While these margins are likely to arise in many settings, the quantitative importance of each depends on local labor-market institutions and outside options. In Brazil, high informality and limited formal-sector opportunities make platform work relatively valuable for many workers, which likely amplifies the gains measured in the accounting exercise.

A limitation of the paper is that it cannot fully characterize the non-wage gains and losses associated with platform work. Future research should investigate the non-pecuniary aspects of gig work and how these platforms affect overall worker welfare. A particularly important direction is to study dimensions such as health risks, job flexibility, and other amenities valued by workers. In addition, this paper does not address the long-run effects of platform work on workers' careers. Given the large share of informal and unemployed workers entering delivery platforms, future research should examine whether these jobs serve as stepping stones into the formal sector or instead limit workers' access to higher-quality opportunities over time.

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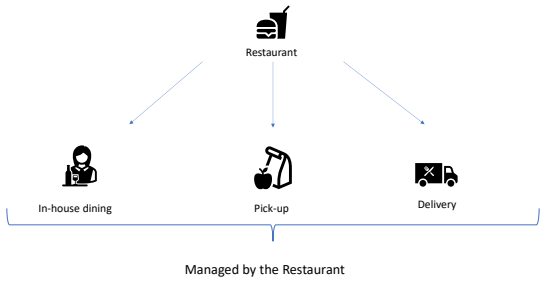
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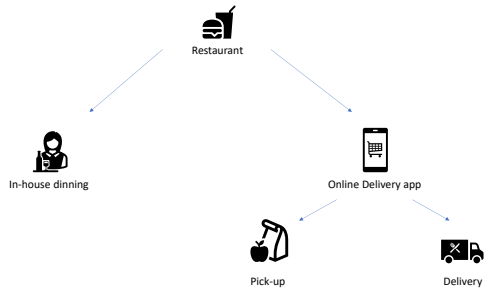
Figures

Figure I: Restaurant Structure before and after online-delivery platforms

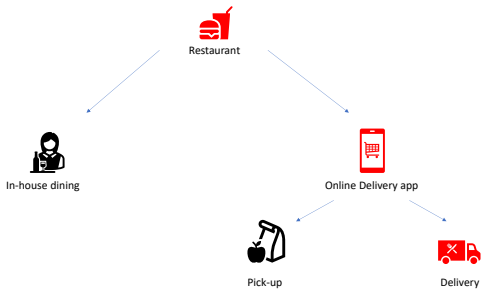
(a) Restaurant Structure Previous to online-delivery Platforms



(b) Restaurant Structure After online-delivery Platforms

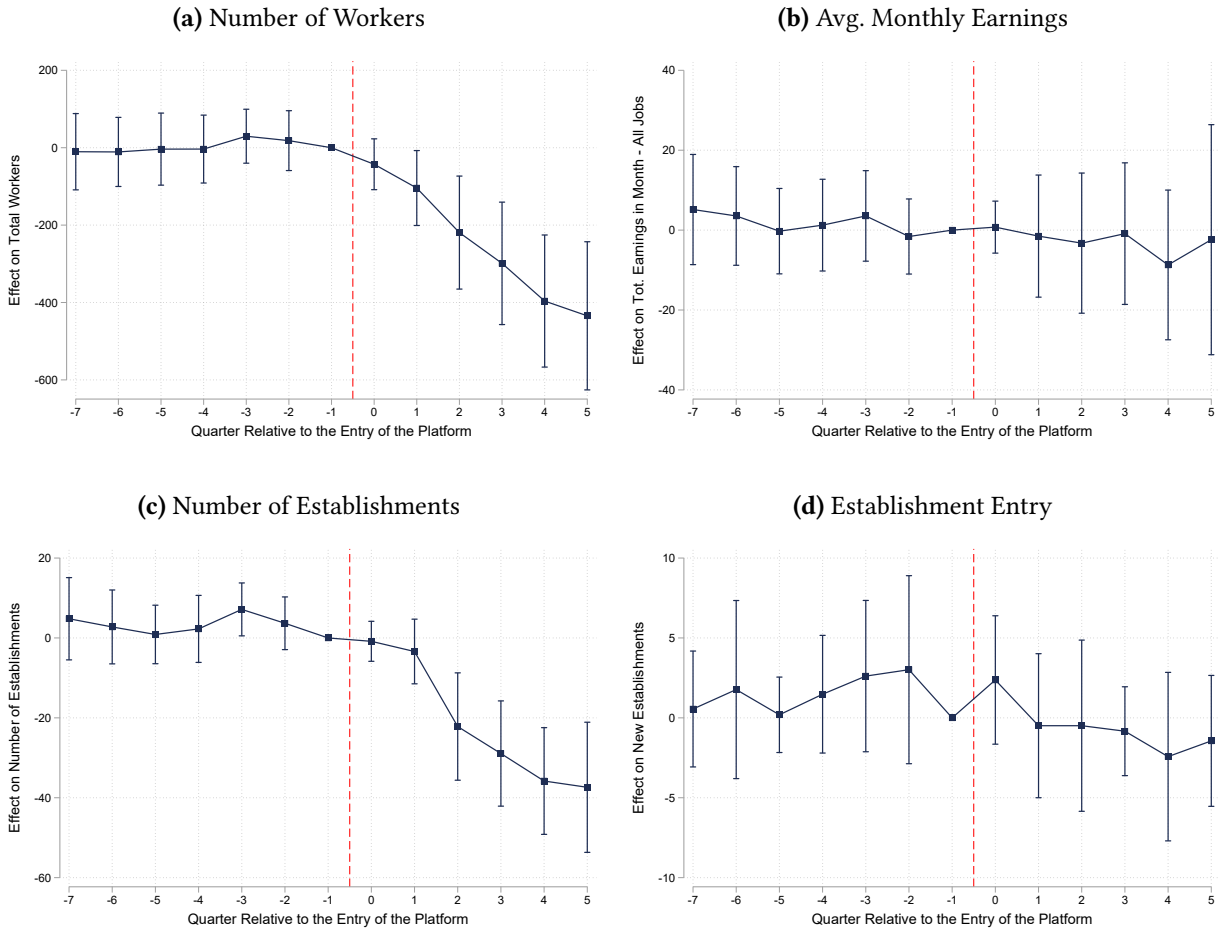


(c) Main Relationship Studied in the Paper



Notes: The figure illustrates the structure of restaurants before and after the adoption of online-delivery platforms. Panel (a) shows the structure of restaurants before the adoption of online-delivery platforms. When not enrolled in delivery platforms, restaurants usually operate in-house dinning, pick-ups and deliveries (if offered). Panel (b) and panel (c) show the structure of restaurants after the adoption of online-delivery platforms. When restaurants start offering services through online-delivery platforms, these platforms often take over the pick-up and deliveries of the restaurants. In this paper I focus on the impact of online-delivery platforms that offer delivery services, as highlighted in red in panel (c).

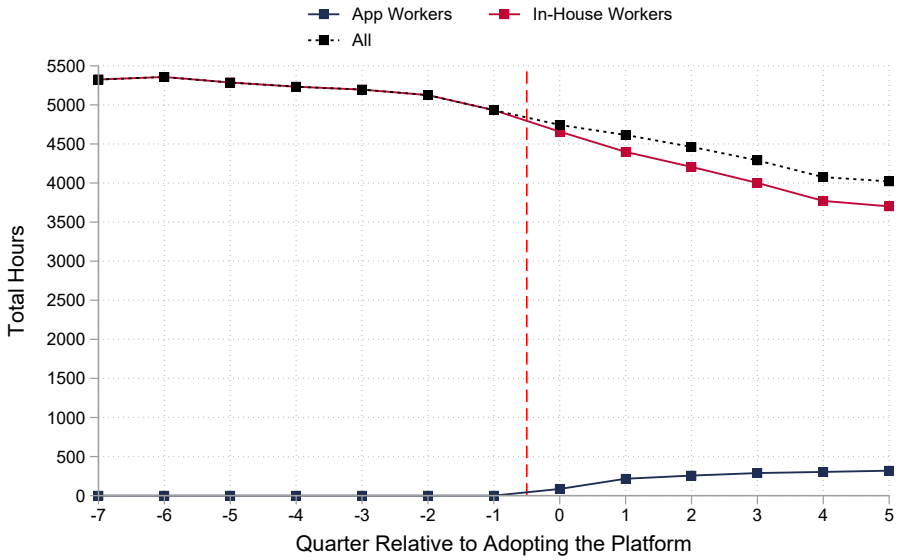
Figure II: Effects of Delivery Platform Entry on Restaurant Sector



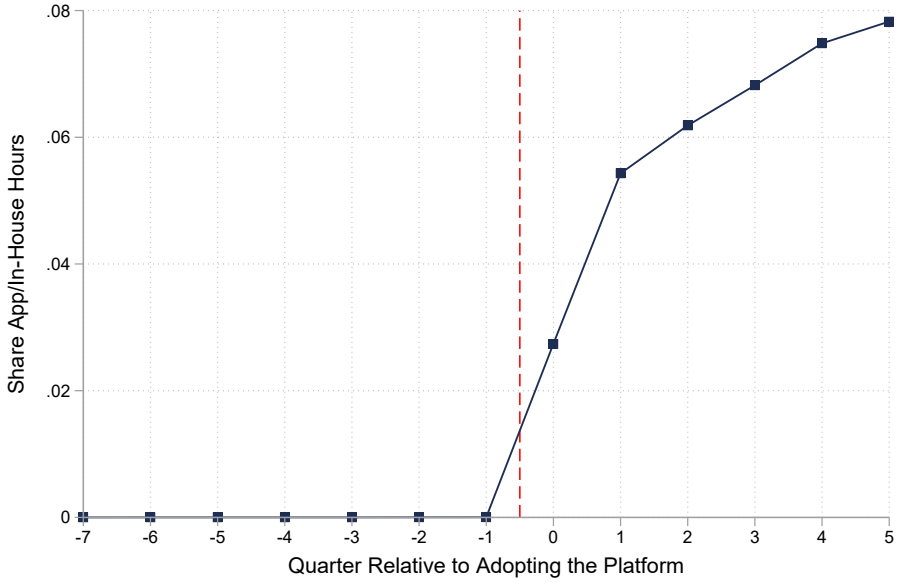
Notes: This figure presents estimates obtained after fitting equation (13) on total number of workers in the restaurant sector (panel a), average monthly earnings of restaurant workers (panel b), total number of establishments in restaurant sector (panel c) and establishment entry (panel d). The regressions are weighted by the number of restaurant workers in each microregion 2017 and controls for covid cases per capita at each date in each microregion. Establishment entry is defined as the first time that the establishment appears in RAIS. The mean number of restaurant workers prior to platform entry is 6,398 (SD 4,480), the mean earnings are 1,585 (SD 234), the mean number of establishments is 868 (SD 501), and the mean quarterly new establishments prior to treatment is 39 (SD 26). The regression excludes microregions where the platform had already entered prior to 2018. Standard errors are clustered at the microregion level.

Figure III: Trends in hours Hired for In-House and Platform Workers

(a) Total Hours by Type of Labor



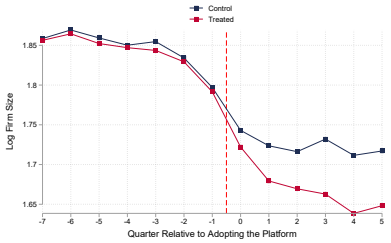
(b) Share of Outsourced Labor



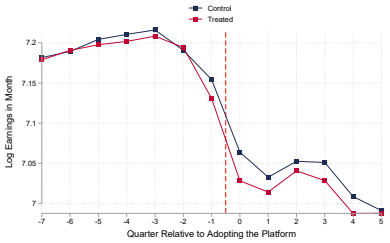
Notes: Panel (a) reports the total hours per quarter hired by restaurants that offer delivery services through the platform and match to a control restaurant. The red line reports the average quarterly hours for workers hired formally by these restaurants (in-house workers). The blue line reports the average quarterly hours that delivery platform-workers worked for these restaurants (app workers). The solid black line is the sum of in-house workers and app workers hours. Panel (b) shows the share of average quarterly hours that delivery platform-workers contributed to these restaurants, relative to the total hours worked by both in-house employees and app workers. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time.

Figure IV: Effects of Online-Delivery Platform Adoption on In-House Size, Wages and Closure

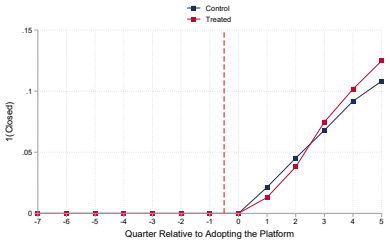
(a) Log In-House Size of Restaurants



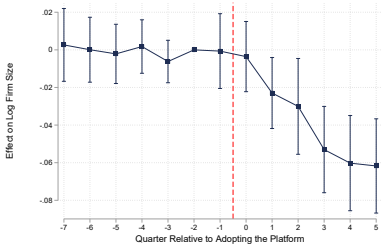
(b) Log Avg. Wage Paid by Restaurants



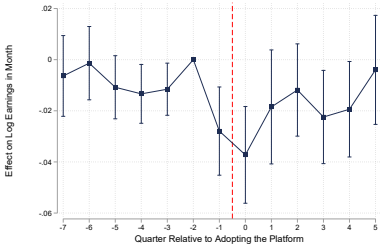
(c) Restaurant Closure



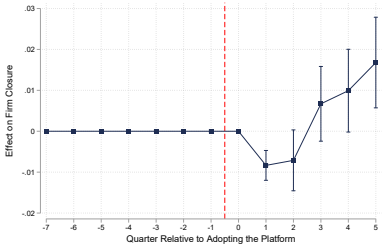
(d) Event-Study: Log In-House Size



(e) Event-Study: Log Avg. Monthly Wages

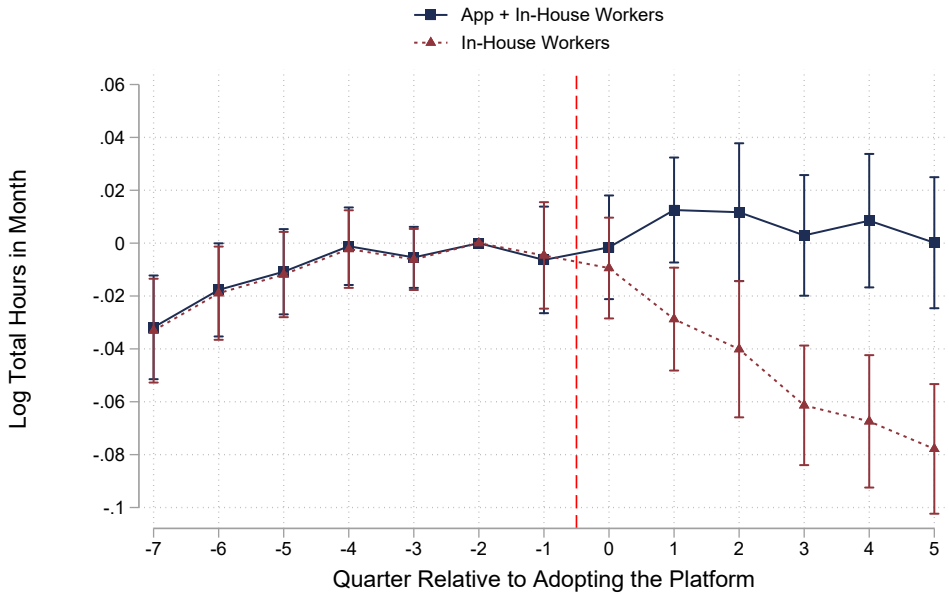


(f) Event-Study: Closure



Notes: Panels (a)-(c) reports the trajectories of the logarithm of the size of the restaurants—as measured by the number of workers hired in a quarter— (Panel a), average paid wages (Panel b), and closure of restaurants that offer delivery services through the online platform and their matched-controls. Panels (d)-(f) report the corresponding event-study estimates obtained after fitting equation (15). The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. Average monthly wages in a quarter are constructed by taking the quarterly average of all the monthly wages reported in RAIS by the establishment in the corresponding quarter. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the establishment level.

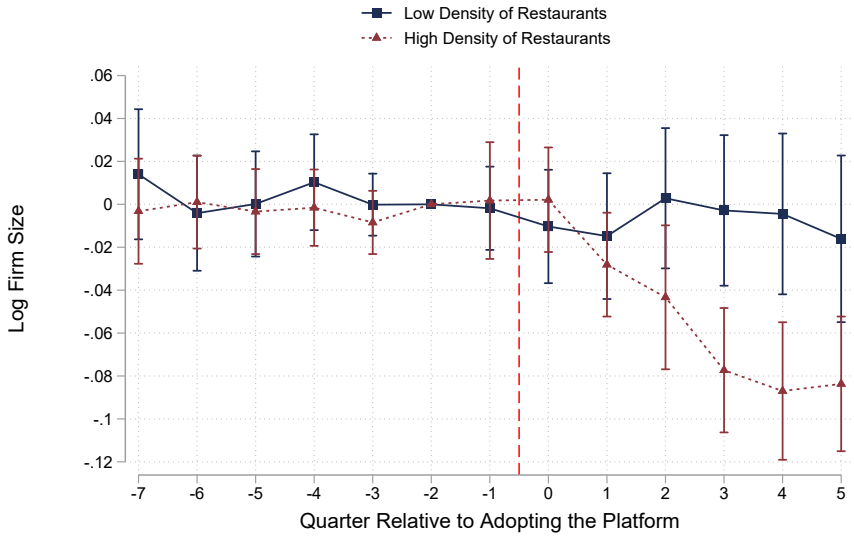
Figure V: Effects of Online-Delivery Platform Adoption on Total Hours Hired



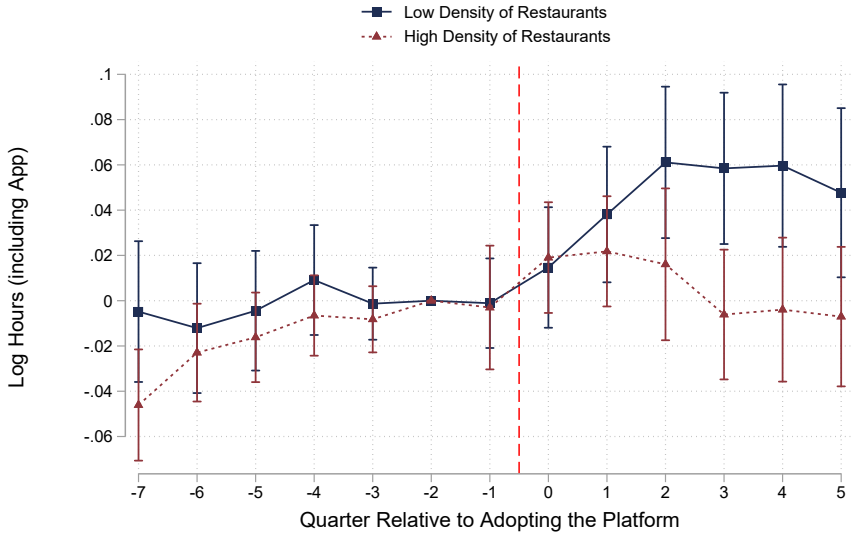
Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on log total hours hired quarterly for workers hired formally (the red dashed line) and log total hours hired quarterly when accounting for formal workers and platform-workers (the solid blue line). The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. The panel reports 95% confidence intervals based on standard errors clustered at the establishment level.

Figure VI: Effects of Online-Delivery Platform Adoption on Total Hours Hired and In-House Size by Restaurant Density

(a) Log In-House Size of Restaurants



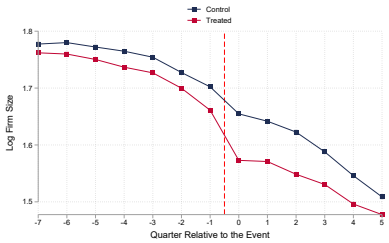
(b) Log Total Hours Hired



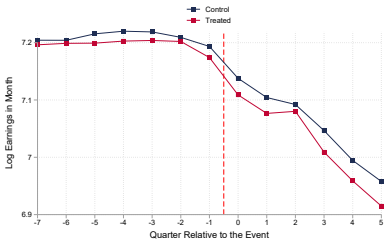
Notes: Panel (a) reports the corresponding event-study estimates obtained after fitting equation (15) on log restaurant size as defined by the average number of formal workers hired by the establishment. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. The red dashed line reports the estimates for restaurants that are above the median of restaurant density within their microregion. The solid blue line reports the estimates for restaurants that are below the median of restaurant density within their microregion. Restaurant density is calculated as the number of restaurants that are located in a 1 kilometer radius of each restaurant (τ). The median of restaurant density is calculate using the distribution of τ corresponding to the microregion of each restaurant in each quarter. The density assigned to each restaurant corresponds to the τ calculated using the quarter prior to the first quarter in which the treated restaurant of the pair started offering delivery services through the platform. Panel (b) reports the corresponding event-study estimates obtained after fitting equation (15) on log total hours hired in the quarter when accounting for workers hired formally and workers working through the platform. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. Both panels report 95% confidence intervals based on standard errors clustered at the establishment level.

Figure VII: Spillover Effects of Platform Adoption on Non-Adopting Restaurants

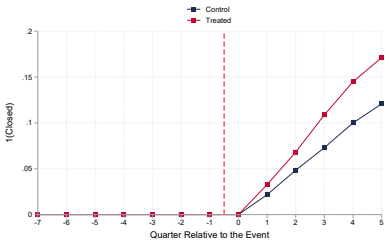
(a) Log In-House Size of Restaurants



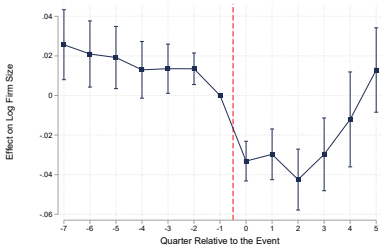
(b) Avg. Wage Paid by Restaurants



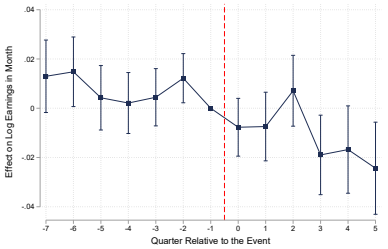
(c) Restaurant Closure



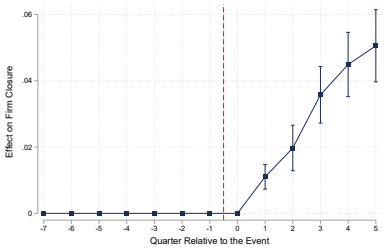
(d) Event-Study: Log In-House Size



(e) Event-Study: Log Avg. Monthly Wages



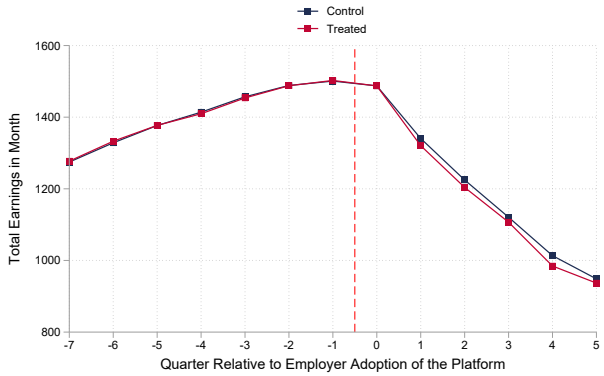
(f) Event-Study: Closure



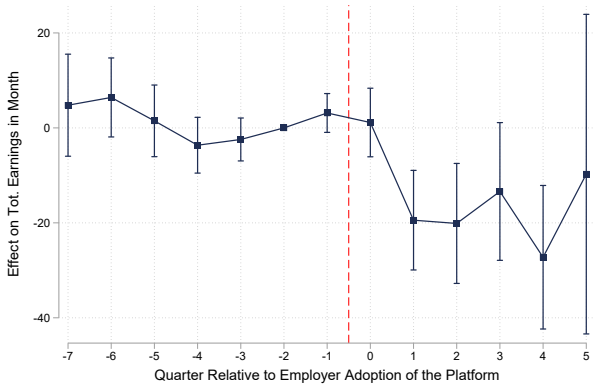
Notes: This figure reports event study estimates for restaurants that are exposed to a spillover event. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. Panels (a)-(c) report the trajectories of the logarithm of the size—as measured by the number of workers hired in a quarter— (Panel a), average paid wages (Panel b), and closure (Panel c) of restaurants exposed to a spillover event and their matched-controls. Panels (d)-(f) report the corresponding event-study estimates obtained after fitting equation (15). A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. The x-axis reflects the quarter relative to the quarter in which the spillover event occurs. The date of closure is defined as the last quarter in which the establishment reported having workers to RAIS (previous to the last quarter available in my sample). All event-study panels report 95% confidence intervals based on standard errors clustered at the establishment level.

Figure VIII: Effects of Employer Adoption of Online-Delivery Platform on Worker Earnings and Employment

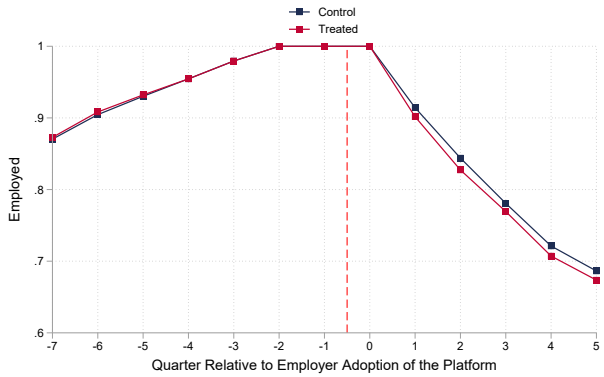
(a) Avg. Monthly Earnings



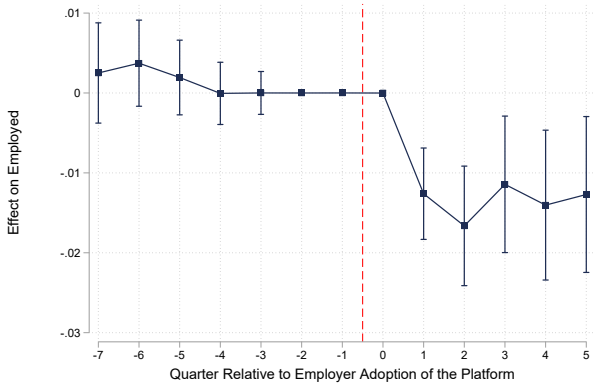
(b) Event-Study Estimates on Earnings



(c) Employment



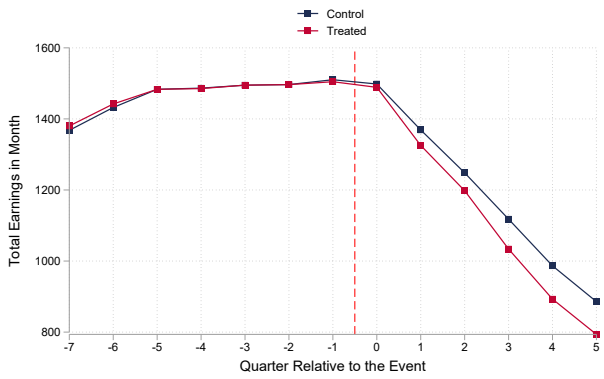
(d) Event-Study Estimates on Employment



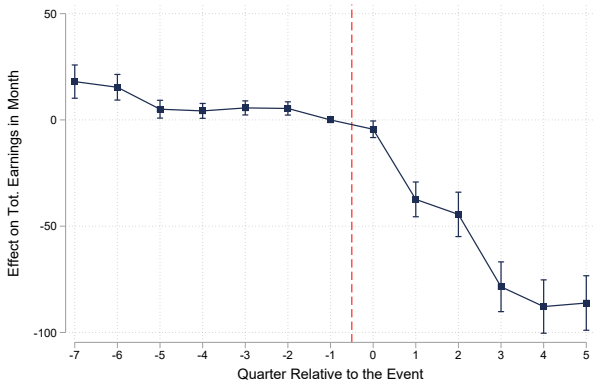
Notes: Panel (a) and Panel (c) reports the trajectories of total earnings and employment of restaurant workers whose employer starts offering delivery through the platform and their matched control workers. Panel (b) and Panel (d) report the corresponding event-study estimates obtained after fitting equation (15) on the quarterly average of monthly earnings (Panel b) or employment (panel d). The mean earnings for treated workers the quarter prior to treatment is 1,488 BRL-2018. Average monthly earnings reflects the quarterly average of the monthly formal wages that a worker earns in the corresponding quarter. If the worker did not hold a formal job during the period, the earnings are equal to 0. The x-axis reflects the quarter relative to the first quarter in which the employer of the treated worker offered delivery services through the platform for the first time. Quarter 0 is the quarter when the restaurant that employs the treated worker starts offering delivery services through the platform. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the worker level.

Figure IX: Spillover Effects of Platform Adoption on Restaurant Workers

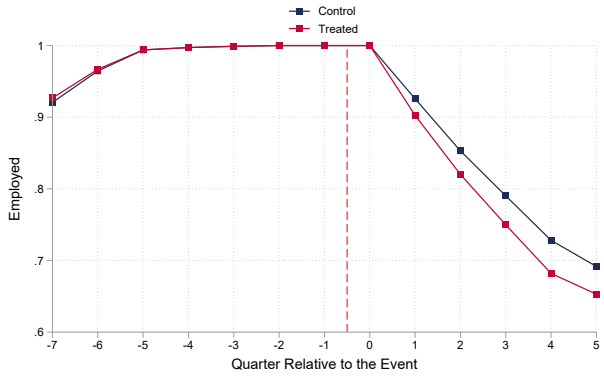
(a) Avg. Monthly Earnings



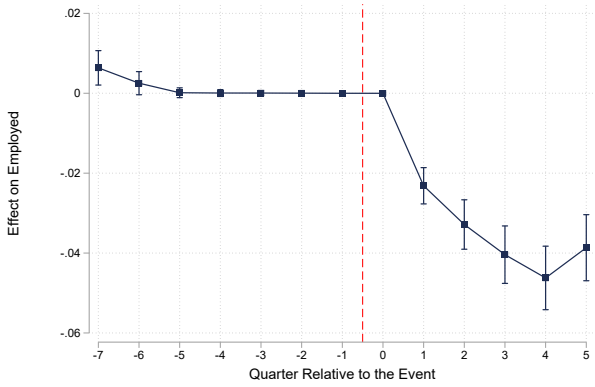
(b) Event-Study Estimates on Earnings



(c) Employment



(d) Event-Study Estimates on Employment



Notes: This figure reports event study estimates for workers who are employed at restaurants that are exposed to a spillover event. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. Panel (a) and Panel (c) reports the trajectories of total earnings and employment of restaurant workers whose employer is exposed to a spillover event and their matched control workers. Panel (b) reports the corresponding event-study estimates obtained after fitting equation (15) on on the quarterly average of monthly earnings. The mean earnings for treated workers the quarter prior to treatment is 1,496 BRL-2018. Average monthly earnings reflects the quarterly average of the monthly formal wages that a worker earns in the corresponding quarter. If the worker did not hold a formal job during the period, the earnings are equal to 0. Panel (d) presents the estimates for employment. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. The x-axis reflects the quarter relative to the quarter in which the spillover event occurs. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the worker level.

Tables

Table I: Summary Statistics for Adopting and Control Restaurants

	(1) Matched Control	(2) Matched Treated	(3) Potential Treated	(4) All Restaurants
Years of Education	11.02	11.29	11.28	11.01
	(1.38)	(1.27)	(1.31)	(1.62)
Tenure (in years)	2.58	1.43	1.46	2.04
	(1.78)	(1.92)	(1.99)	(2.43)
Monthly Wage (2018 - R$)	1,484	1,530	1,522	1,438
	(413)	(571)	(552)	(508)
Age	33.38	32.48	32.68	34.95
	(7.02)	(6.94)	(7.10)	(8.42)
Share of Brazilians	1.00	0.99	0.99	0.99
Female	0.59	0.59	0.59	0.60
Full-Time	0.97	0.97	0.97	0.97
Establishment Size	11.34	11.50	11.86	7.36
	(24.75)	(22.83)	(53.59)	(34.84)
Share of Waiters	0.48	0.50	0.52	0.49
	(0.32)	(0.31)	(0.33)	(0.36)
Number of Establishments	7,836	14,862	15,933	296,263

Notes: Column 2 presents the summary statistics of the restaurants that offer delivery services through the platform and for whom I can find a matched control restaurant. These summary statistics are calculated in 2017. Potential matched control restaurants are those who in $t_j^* - 1$ do not have platform delivery services available in their microregion (and the platform does not start offering services the following five quarters). Additionally, these control restaurants must have been open for at least two years, Treated restaurants are matched to controls that belong to the same quartile of firm size, the same quartile of average earnings, and the same side of the median of the number of restaurants within a 1-kilometer radius. (with respect to the distribution of restaurants in their corresponding microregion). A propensity score matching based on log size in $t^* - 8$ to $t^* - 1$, log average monthly wages paid in quarters $t^* - 4$ to $t^* - 1$, firm age, share of waiters, average tenure, age of workers, share of female workers and average hours of workers is conducted to assign exactly one matched control restaurant to each treated restaurant within each cohort of treatment. Column 1 reports the characteristics of matched control restaurants, and Column 3 reports the summary statistics for the entire set of restaurants that offer delivery services through the platform identified in RAIS. Column 4 reports the summary statistics for all restaurants found in RAIS in 2017. Restaurants are defined as establishments that have a CNAE two digit code equal to 56. Wages are expressed in real terms (2018 CPI). All statistics are person-month-weighted, and standard deviations are reported in parentheses.

Table II: Effect of Platform Adoption on Labor Demand per Occupation

	(1) Hours: Waiters	(2) Hours: Cooks	(3) Hours: Delivery	(4) Log Wage Waiters	(5) Log Wage Cooks	(6) Log Wage Delivery
β**(Diff-in-Diff)**	-45.7***	3.6	-2.2	-0.025***	-0.011	-0.003
	(10.2)	(5.0)	(1.5)	(0.007)	(0.008)	(0.024)
Mean prior to Adoption	913.2	351.1	24.9	7.1	7.2	7.2
Observations	379,254	379,254	379,254	292,984	207,408	29,466

Notes: The table reports the corresponding difference-in-difference estimates obtained after fitting equation (15) on the hours hired of waiters (column 1), cooks (column 2), in-house delivery drivers (3) and their respective log wages (column 4, 5 and 6) at each establishment j using the estimator from Borusyak et al. (2024). Hours are defined as the average monthly hours hired of workers for each occupation after applying the restrictions described in Section 3. Average monthly wages in a quarter are constructed by taking the quarterly average of all the monthly wages reported in RAIS by the establishment in the corresponding quarter. Wages are expressed in real terms (2018 CPI). Standard errors clustered at the establishment level. *** is significant at the 0.01 level, ** is significant at the 0.05 level, and * is significant at the 0.1 level.

Table III: Heterogeneous Effects of Platform Adoption on Labor Demand per Occupation

	Panel A: Restaurants at High Density Areas		**Panel B: Restaurants at Low Density Areas**	
	(1)	(2)	(3)	(4)
	Hours Hired: Waiters	Hours Hired: Cooks	Hours Hired: Waiters	Hours Hired: Cooks
β**(Dif-in-Diff)**	-68.77***	-5.19	0.20	25.45**
	(12.11)	(5.60)	(19.95)	(10.36)
Mean prior to App Adoption	947.85	359.89	820.34	333.17
Observations	242,794	242,794	110,676	110,676

Notes: The table reports the corresponding difference-in-difference estimates obtained after fitting equation (15) on the hours hired of waiters and cooks at each establishment j using the estimator from Borusyak et al. (2024). The table reports the heterogeneity by restaurant density. Columns (1) and (2) present the results for above the median restaurant density, while columns (3) and (4) present the results for below the median of density. Restaurant density is calculated as the number of restaurants that are located in a 1 kilometer radius of each restaurant (τ). The median of restaurant density is calculate using the distribution of τ corresponding to the microregion of each restaurant in each quarter. The density assigned to each restaurant corresponds to the τ calculated using the quarter prior to the first quarter in which the treated restaurant of the pair started offering delivery services through the platform. Hours are defined as the average monthly hours hired of workers for each occupation after applying the restrictions described in Section 3. Standard errors clustered at the establishment level. *** is significant at the 0.01 level, ** is significant at the 0.05 level, and * is significant at the 0.1 level.

Table IV: Summary Statistics for Workers whose Employer Adopts the Delivery Platform and Control Workers

	(1) Matched Control Workers	(2) Matched Treated Workers	(3) Potential Treated Workers
Years of Education	10.92	11.07	11.42
	(2.18)	(1.87)	(2.05)
Avg Tenure (in years)	3.68	3.68	2.91
	(1.35)	(1.35)	(1.58)
Avg Monthly Earnings (in 2018-R$)	1,501	1,503	1,789
	(377)	(389)	(1,475)
Age	32.51	32.45	33.14
	(9.96)	(9.99)	(11.10)
Share of Brazilians	1.00	0.99	0.99
Female	0.59	0.59	0.51
Hours	43.13	41.29	41.69
	(3.22)	(6.95)	(6.66)
Delivery	0.01	0.01	0.01
Kitchen	0.18	0.18	0.18
Waiters	0.67	0.67	0.52
Other	0.14	0.14	0.29
Number of Workers	15,283	19,610	132,861

Notes: Column 2 presents the summary statistics of workers working at restaurants when they start offering delivery services through the platform and for whom I can find a matched control worker. These summary statistics are calculated the quarter prior to the employers adoption of the platform, that is, $t^* - 1$. Potential matched control workers are those who in $t_j^* - 1$ are working at a restaurant that is located in a microregion where there are no platform delivery services available (and the platform does not start offering services the following five quarters). Additionally, these control workers must have the same gender, occupation as their corresponding treated worker and their employer must belong to the same quartile of wage, size and median of number of restaurants within a radius of 1 kilometers (with respect to the distribution of restaurants in their corresponding microregion). A caliper matching method based on earnings in $t^* - 1$ to $t^* - 4$, age and tenure is then conducted to assign exactly one matched control worker for each treated worker without replacement (Stepner and Garland, 2017). Column 1 reports the characteristics of matched control workers, and Column 3 reports the summary statistics for the entire set of workers that work at a restaurant that offer delivery services through the platform identified in RAIS. Restaurants are defined as establishments that have a CNAE two digit code equal to 56. Occupations are based on the 6 digit *Classificação brasileira de ocupações* (CBO). Wages are expressed in real terms (2018 CPI). Standard deviations are reported in parentheses.

Table V: Total Wage Bill Effect of Platform Adoption

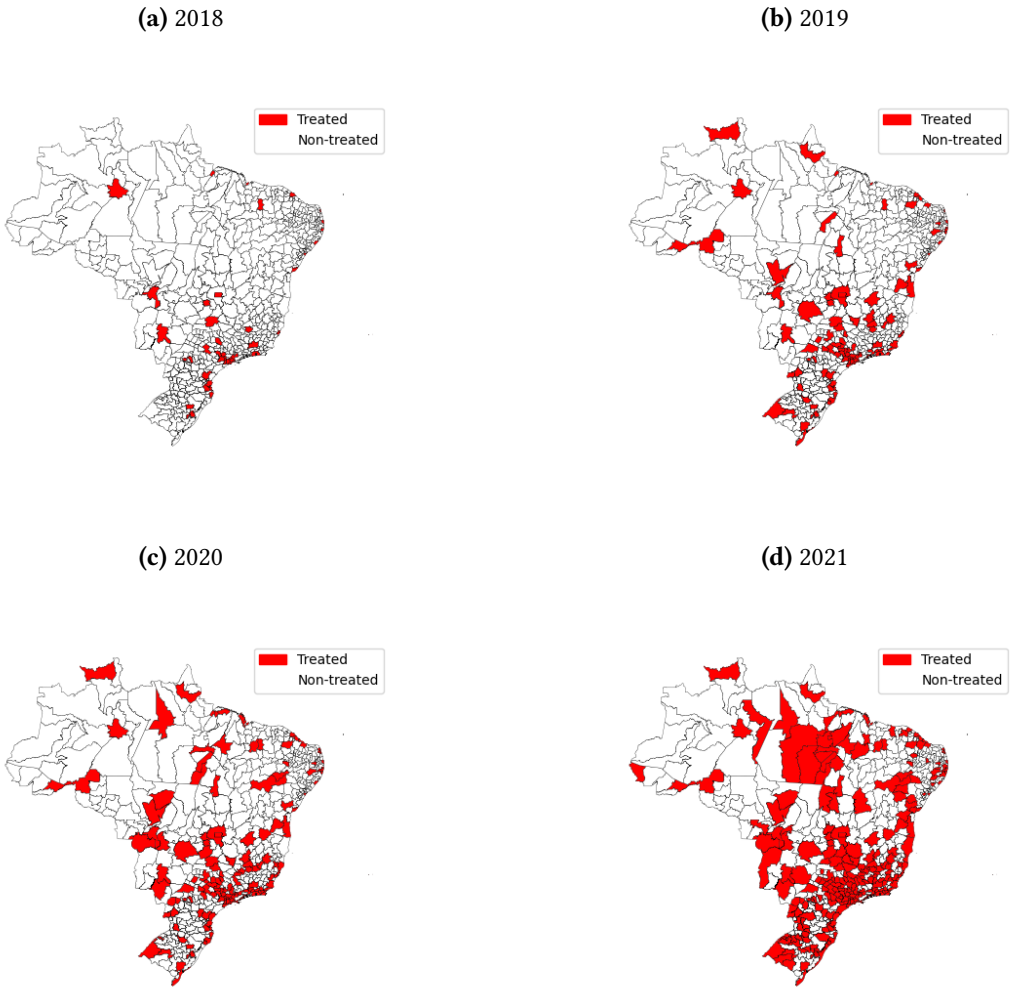
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	In-House Restaurant Workers (Formal)		**In-House Restaurant Workers (Informal)**	**Spillovers (Formal)**		**Spillovers (Informal)**	**App Workers (From Formal Sector)**		**App Workers (From Non-Formal Sector)**	
	Gross Wage	Net Wage		Gross Wage	Net Wage		Wage Bill App	Wage Bill Net of Opportunity & Transportation Costs	Wage Bill App	Wage Bill Net of Opportunity & Transportation Costs
Quarter 0	38	19	4	-46	-43	-11	330	88	1,494	973
	(121)	(108)	(25)	(20)	(18)	(4)	(11)	(4)	(45)	(31)
Quarter 1	-652	-616	-143	-354	-327	-81	771	190	3,661	2,351
	(167)	(152)	(35)	(41)	(37)	(9)	(25)	(7)	(96)	(64)
Quarter 2	-675	-633	-147	-509	-471	-117	869	199	4,317	2,717
	(213)	(194)	(45)	(62)	(56)	(14)	(29)	(8)	(114)	(74)
Quarter 3	-449	-429	-100	-1,409	-1,293	-320	1,076	269	5,290	3,381
	(237)	(216)	(50)	(107)	(98)	(24)	(35)	(9)	(128)	(85)
Quarter 4	-914	-845	-196	-2,856	-2,620	-649	1,111	260	5,548	3,478
	(237)	(216)	(50)	(240)	(219)	(54)	(31)	(9)	(120)	(78)
Quarter 5	-327	-322	-75	-4,571	-4,184	-1,037	1,115	233	5,820	3,572
	(568)	(508)	(118)	(502)	(459)	(114)	(30)	(7)	(123)	(77)
Cumulative PDV	-2,878	-2,731	-634	-9,281	-8,513	-2,109	5,087	1,197	25,204	15,892
	(974)	(882)	(205)	(788)	(719)	(178)	(135)	(33)	(519)	(333)
Percentage of Pre-Platform Wage Bill	-5.3%	-5.0%	-1.2%	-17.0%	-15.6%	-3.9%	9.3%	2.2%	46.1%	29.1%

Notes: This table reports the estimated wage bill surplus generated by the platform for each component of equation (16). Column 1 presents the effects on in-house restaurant workers whose employer starts offering delivery services through the platform. Each quarter report the corresponding event-study estimates obtained after fitting equation (15) on monthly earnings, converting the coefficient to quarterly and multiplying it by the average firm size of restaurants that adopt the platform in $t^* - 2$. Column 2 presents the same estimates but using the wages net of social security contributions. Column 3 presents the estimated effects for informal workers at adopting restaurants. The number of informal workers is imputed to each establishment using the average share of informal workers in the restaurant sector in each municipality estimated using the 2010 Census. The earnings of informal workers are imputed to each establishment using the ratio of informal to formal wages in the restaurant sector in each state-quarter estimated using the PNAD-C. Column 4 presents the estimated coefficients for workers that are employed at restaurants that are exposed to a spillover event. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. To estimate a per adopting restaurant effect of spillovers, the earnings effects of workers exposed to spillover events are re-scaled by the effect of spillover on restaurant adoption and then scaled by the average restaurant size in my sample and by the probability of a restaurant being exposed to a spillover event as described in Appendix D2. Column 5 and Column 6 present the net wage effect and the spillover effects on informal workers estimated using the same methodology as for in-house workers. Column 7 reports the total earnings gained by workers who held a formal job the quarter prior to working on the platform (and were not laid-off). Column 8 reports the total earnings gained for these workers net of their outside option and their transportation costs. The outside option is equivalent to the average per hour wage that workers held prior to working on the platform. Transportation costs are based on estimates from Callil and Picanço (2023). Column 9 and 10 report the same estimates for workers that did not hold a formal job the quarter prior to working on the app. The outside option for workers who transition from non-formal status is imputed using a regression-adjusted earnings for non-formal workers within the same state-quarter using the PNAD-C. PDV means present discounted value which was calculated using the average quarterly inflation of Brazil between 2019 and 2021 (0.012). Pre-platform wage bill is the average quarterly wage bill of restaurants that adopt the platform in $t^* - 2$. For all columns in the table, the pre-adoption wage bill is calculated including the inputed informal workers. Wages are expressed in real terms (2018 CPI). Standard errors were bootstraped.

Appendix

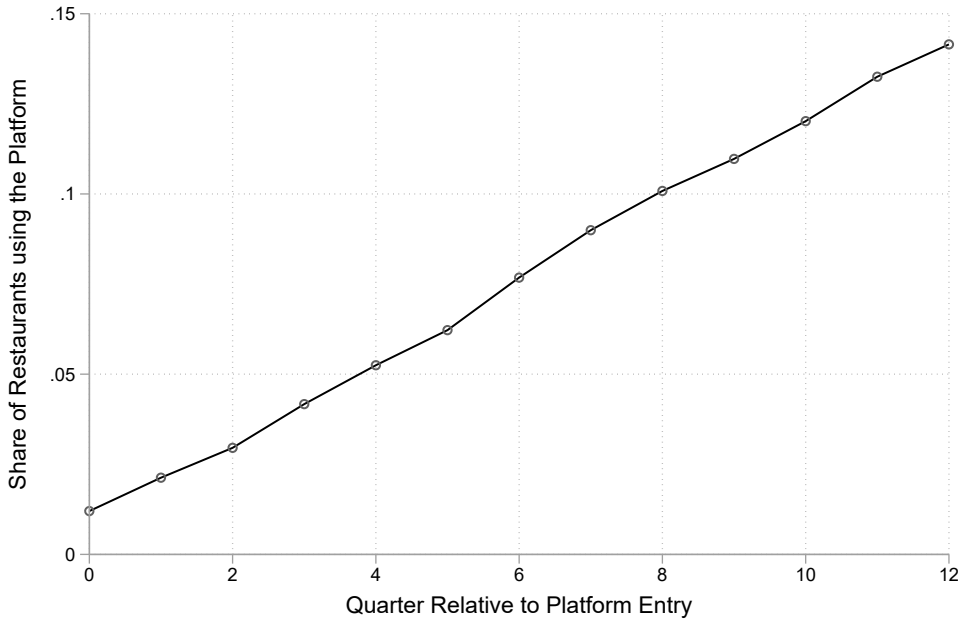
A Appendix: Figures

Figure A1: Rollout of the Platform



Notes: This figure reports the microregions in which restaurants offered services through the delivery platform in each year. Treated means that at least one establishment offered a delivery service through the platform. Panel (a) shows the presence of the platform in 2018, panel (b) in 2019, panel (c) in 2020 and panel (d) in 2021.

Figure A2: Share of Restaurants on the Platform



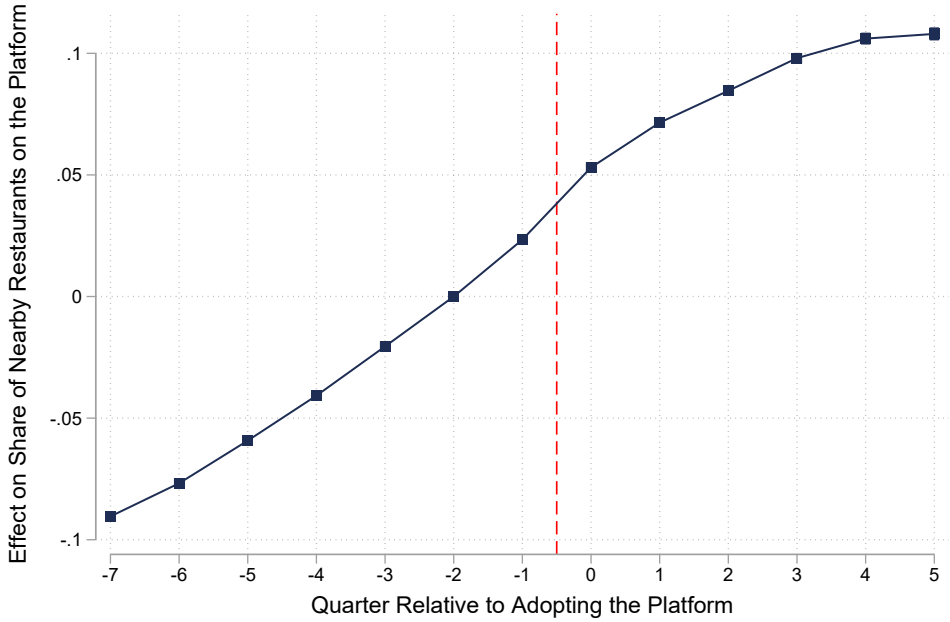
Notes: This figure reports the share of restaurants that adopt the platform (y-axis) respect to the the moment in which the platform becomes available in the microregion (x-axis). A restaurant is considered to adopt the platform if more than 50 percent of their revenues in the platform come through delivery services the first month in which they enroll. Observations are not weighted. T^*0 represents the first quarter in which a restaurant in the microregion starts offering delivery services through the platform.

Figure A3: Effects of Delivery Platform Entry on Restaurant Sector using Borusyak et al. (2024) estimator



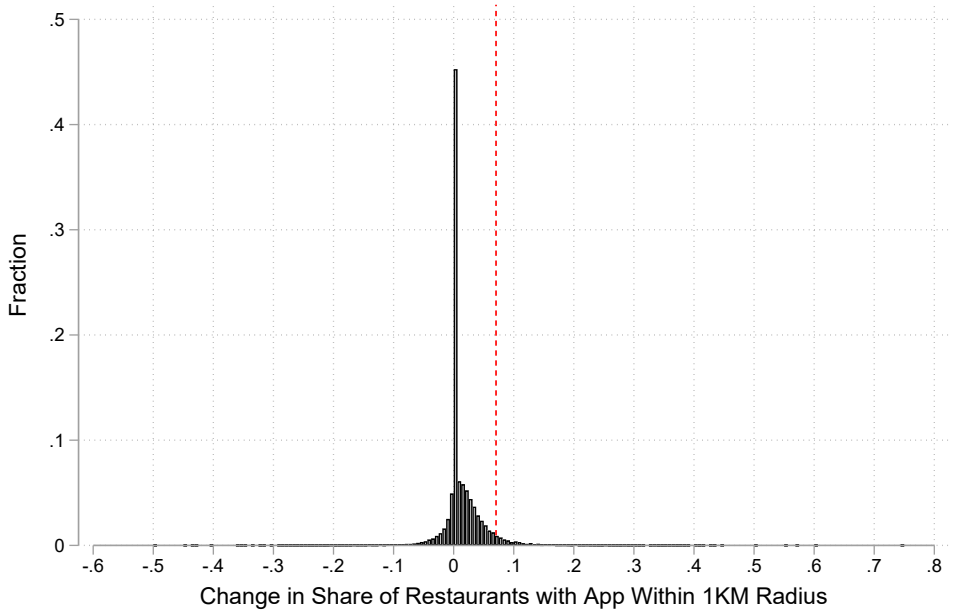
Notes: This figure presents estimates obtained after fitting equation (13) using the estimator developed by Borusyak et al. (2024) on total number of workers in the restaurant sector (panel a), average monthly earnings of restaurant workers (panel b), total number of establishments in restaurant sector (panel c) and establishment entry (panel d). The regressions are weighted by the number of restaurant workers in each microregion 2017 and controls for covid cases per capita at each date in each microregion. Establishment entry is defined as the first time that the establishment appears in RAIS. The mean number of restaurant workers prior to platform entry is 6,398 (SD 4,480), the mean earnings are 1,585 (SD 234), the mean number of establishments is 868 (SD 501), and the mean quarterly new establishments prior to treatment is 39 (SD 26). The regression excludes microregions where the platform had already entered prior to 2018. Standard errors are clustered at the microregion level.

Figure A4: Restaurants within 1 Kilometer Adopting the Platform



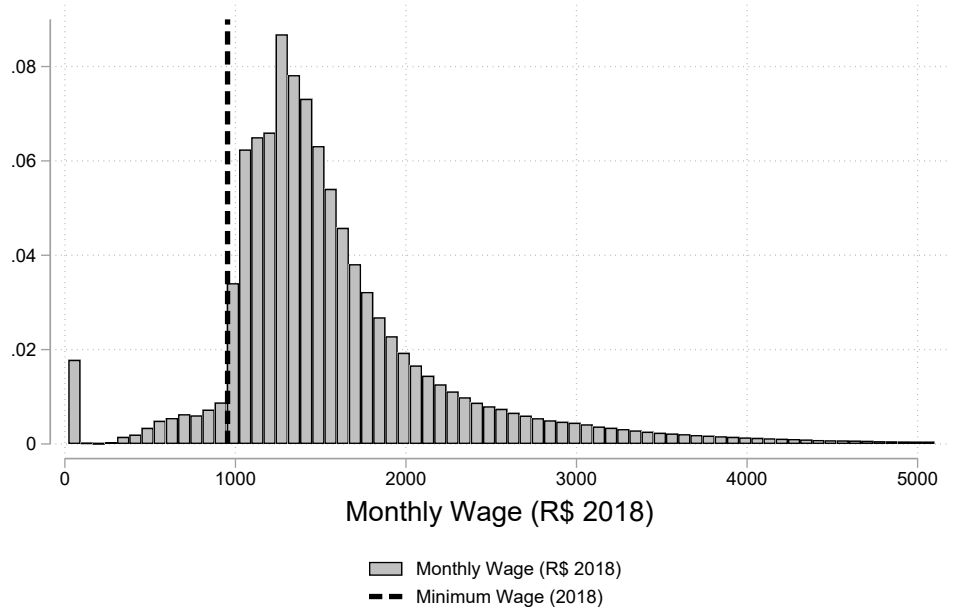
Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on the share of restaurants within a 1 kilometer radius that offer delivery services through the platform. The mean share of restaurant within 1 kilometer using the platform the quarter prior to treatment is 0.06. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. The panel reports 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A5: Distribution of Ω



Notes: This figure reports the histogram of the quarterly difference in the share of restaurants that offer delivery services through the platform within a 1 kilometer radius of each restaurant (Ω). The distribution is calculated using restaurants that have at least 5 restaurants within a 1 kilometer radius. A restaurant is considered to offer delivery services through the platform if they sold at least one good through the platform using the delivery services of the platform in the quarter. The red dashed line shows the 95th percentile of the distribution. Restaurants are defined as establishments that have a CNAE two digit code equal to 56.

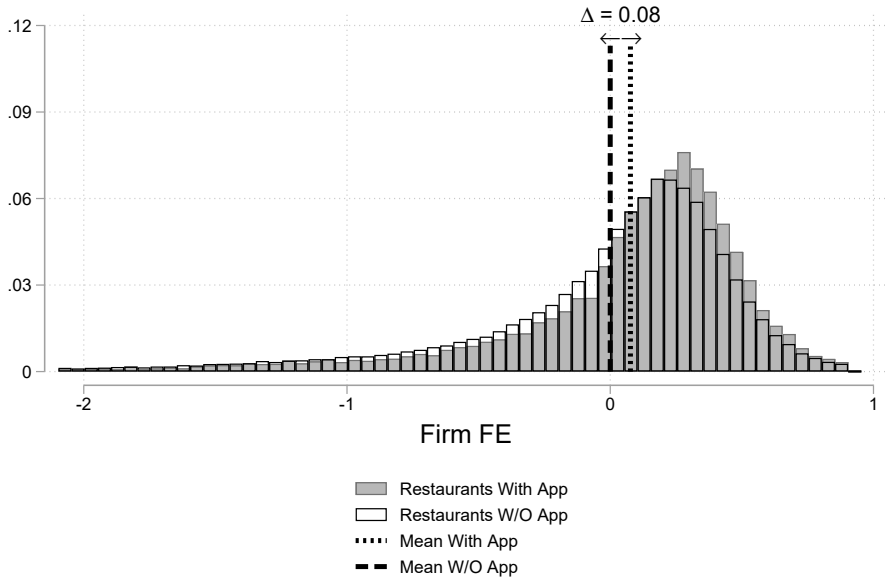
Figure A6: Distribution of Wages of Restaurant Workers (2018)



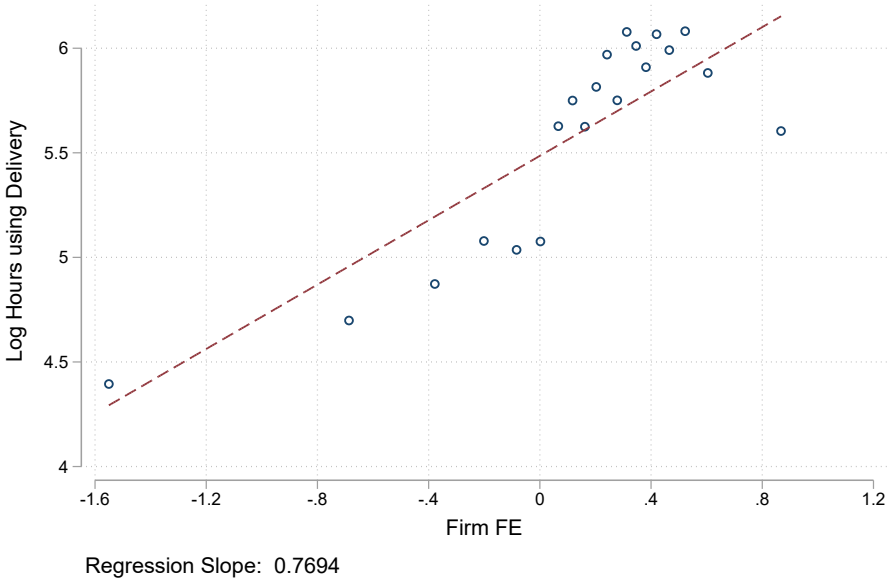
Notes: This figure reports the histogram of wages for restaurant workers in 2018. Restaurants are defined as establishments that have a CNAE two digit code equal to 56. The vertical black dashed line shows the minimum wage in 2018 (954 BRL). Wages are winsorized at the 99 percentile and are expressed in real terms (2018 CPI).

Figure A7: AKM Firm FE and usage of the Platform

(a) Distribution of AKM firm FE for Adopters and Non-Adopters



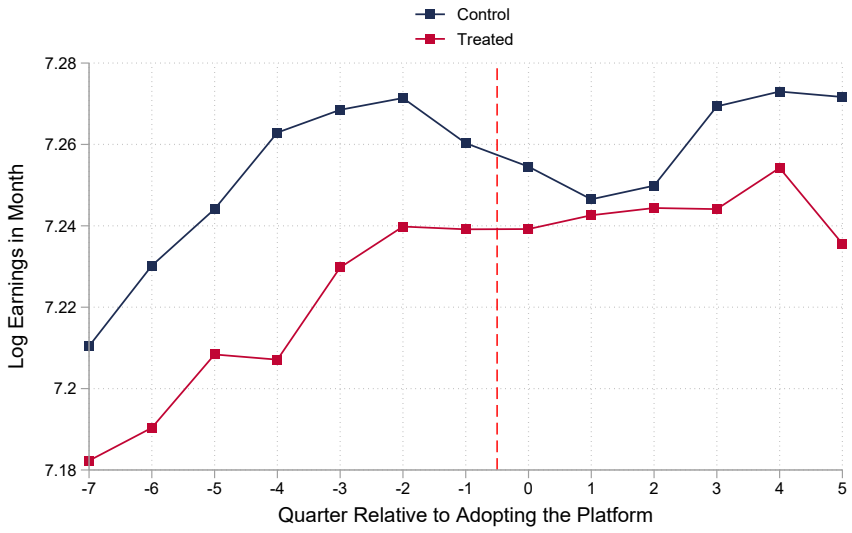
(b) Intensive Margin of Platform and AKM Firm FE



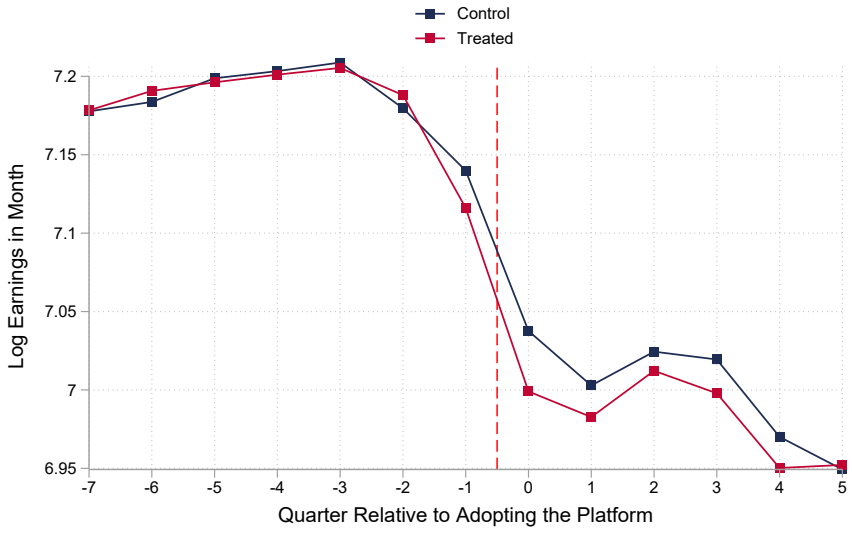
Notes: Panel (a) reports the histogram of AKM workplace effects for restaurants in Brazil. It plots the selection into offering deliveries through an online platforms. The figure plots the histogram of AKM firm effects for restaurants that at some point in time offered deliveries through the delivery platform and restaurants that never offered services through the app. The distributions fo AKM firm effects are normalized such that the average workplace effect in the group of firms that never offered services through online-delivery platforms is zero. The distribution for restaurants that offer services through the platform is shifted to the right by 8 log points, indicating that firms with higher wage policies for formal workers are more likely to offer services through the platform. Panel (b) plots a binned scatter plot of the logarithm of the total hours reported in my sample that platform-workers worked as delivery drivers through the platform for restaurants, plotted against the AKM firm effects of restaurants that offer services through the app (slope is 0.77; SE 0.03). The hours worked at each firm through the delivery platform cover the years 2018 to 2021. Estimated firm effects are restricted to establishment in the largest connected set that, at any point in my sampling window, offered services through delivery platforms. The AKM specification was estimated using data from RAIS between the years 2012 and 2018. Restaurants are defined as establishments that have a CNAE two digit code equal to 56.

Figure A8: Trends in Monthly Wages paid by Treated and Control Restaurants Before and After COVID-19

(a) Avg. Monthly Wages for Restaurants Adopting Prior to January 2019

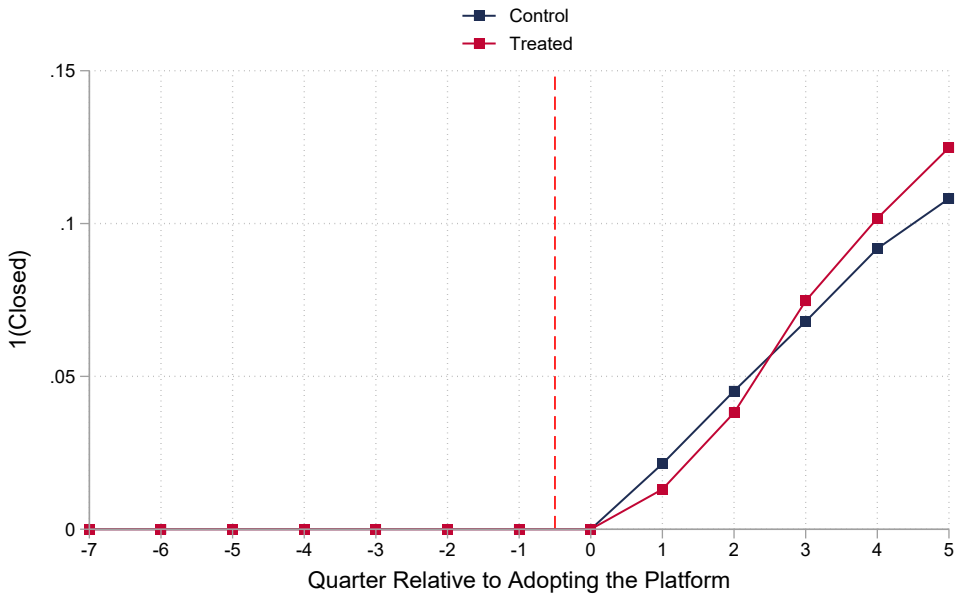


(b) Avg. Monthly Wages for Restaurants Adopting After January 2019



Notes: This figure reports the trajectories of establishment average of the monthly wages paid for matched restaurants that offered delivery services through the platform and matched control restaurants. Panel (a) presents the trajectories for those that adopt the platform prior to January 2019 and therefore are not exposed to COVID-19 up to 5 quarters after their adoption. Panel (b) presents the trajectories for those adopting after January 2019. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. Potential matched control restaurants are those who in $t_j^* - 1$ do not have platform delivery services available in their microregion (and the platform does not start offering services the following five quarters). Additionally, these control restaurants must have been open for at least two years, Treated restaurants are matched to controls that belong to the same quartile of firm size, the same quartile of average earnings, and the same side of the median of the number of restaurants within a 1-kilometer radius. (with respect to the distribution of restaurants in their corresponding microregion). A propensity score matching based on log size in $t^* - 8$ to $t^* - 1$, log average monthly wages paid in quarters $t^* - 4$ to $t^* - 1$, firm age, share of waiters, average tenure, age of workers, share of female workers and average hours of workers was conducted to assign exactly one matched control restaurant to each treated restaurant within each cohort of treatment. Restaurants are defined as establishments that have a CNAE two digit code equal to 56.

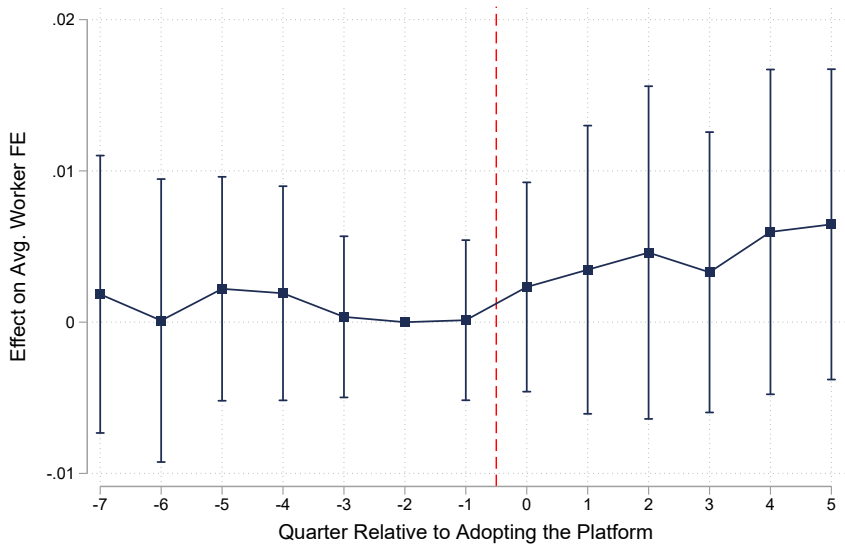
Figure A9: Trends in Closure for Adopting and Control Restaurants



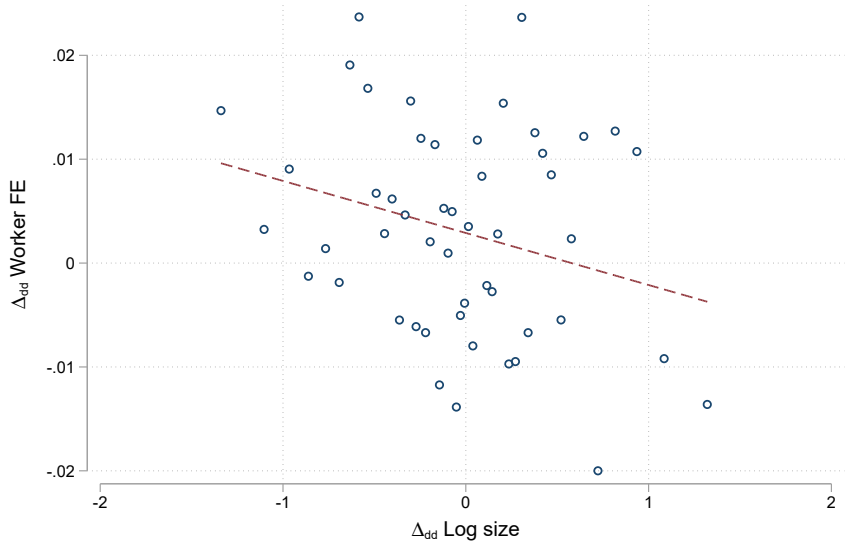
Notes: This figure reports the trajectories of establishment closure for matched restaurants that offered delivery services through the platform and matched control restaurants. The date of closure is defined as the last quarter in which the establishment reported having workers to RAIS (previous to the last quarter available in my sample). The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. Potential matched control restaurants are those who in $t_j^* - 1$ do not have platform delivery services available in their microregion (and the platform does not start offering services the following five quarters). Additionally, these control restaurants must have been open for at least two years, Treated restaurants are matched to controls that belong to the same quartile of firm size, the same quartile of average earnings, and the same side of the median of the number of restaurants within a 1-kilometer radius. (with respect to the distribution of restaurants in their corresponding microregion). A propensity score matching based on log size in $t^* - 8$ to $t^* - 1$, log average monthly wages paid in quarters $t^* - 4$ to $t^* - 1$, firm age, share of waiters, average tenure, age of workers, share of female workers and average hours of workers was conducted to assign exactly one matched control restaurant to each treated restaurant within each cohort of treatment. Restaurants are defined as establishments that have a CNAE two digit code equal to 56.

Figure A10: Effect of Platform Adoption on Avg. Worker Fixed Effects at Restaurants

(a) Avg. AKM Worker Fixed Effects



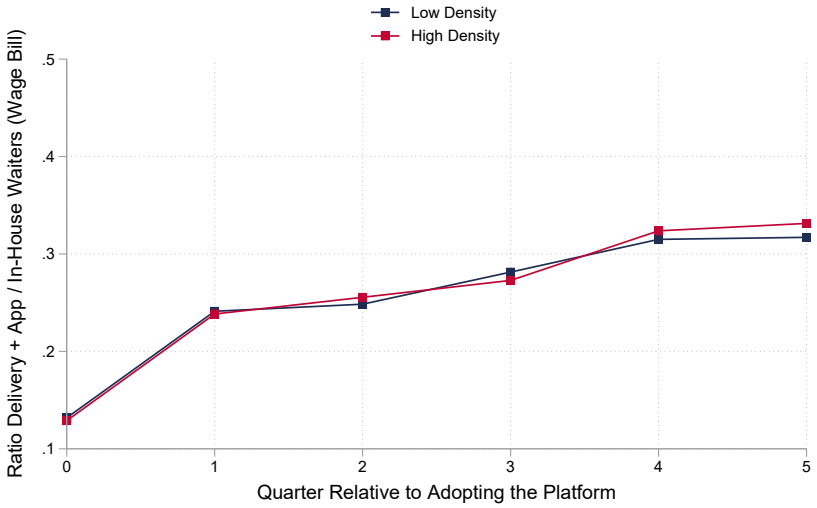
(b) Relation Between Log In-House Size Treatment Effects and Avg. AKM Worker Fixed Effects



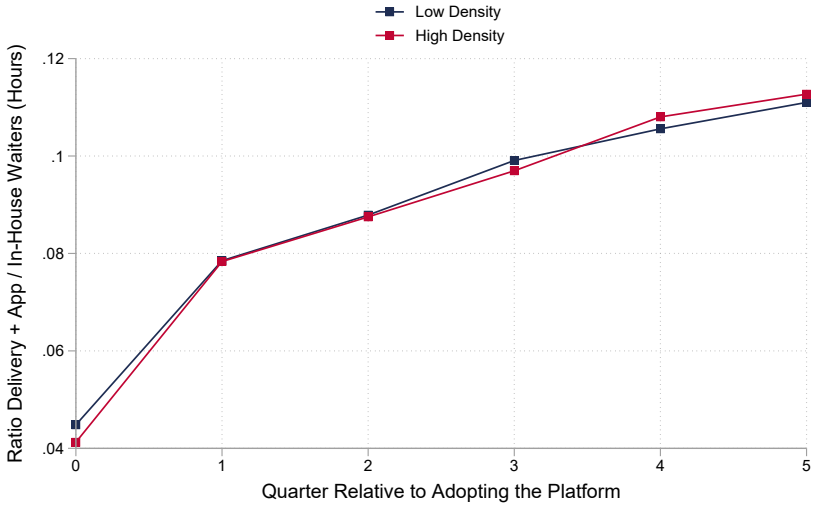
Notes: This figure reports the effect of the adoption of the platform on sorting at the establishment level. Panel (a) reports the corresponding event-study estimates obtained after fitting equation (15) on the average AKM worker effect at the establishment level. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. The panel reports 95% confidence intervals based on standard errors clustered at the establishment level. Panel (b) displays a binscatter plot of the establishment level difference-in-difference estimates of the adoption of the platform on the average AKM worker effects plotted against the effects on log in-house establishment size (slope -0.005, SE 0.003). Δ_{dd} represents the establishment level treatment effect estimate and is equivalent to $\Delta_{dT} - \Delta_{dC}$ where Δ_{dT} and Δ_{dC} are the change before (-7 to -1 quarters) and after (0 to 5 quarters) the adoption of the platform for the treated and control establishment of the matched pair. The AKM specification was estimated using data from RAIS between the years 2012 and 2018. For workers out of the AKM sample, I imputed their worker effects using the corresponding firm effects, year effects, and age. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3.

Figure A11: Ratio of Delivery Workers and Service Workers at Treated Establishments by Density

(a) Wage Bill



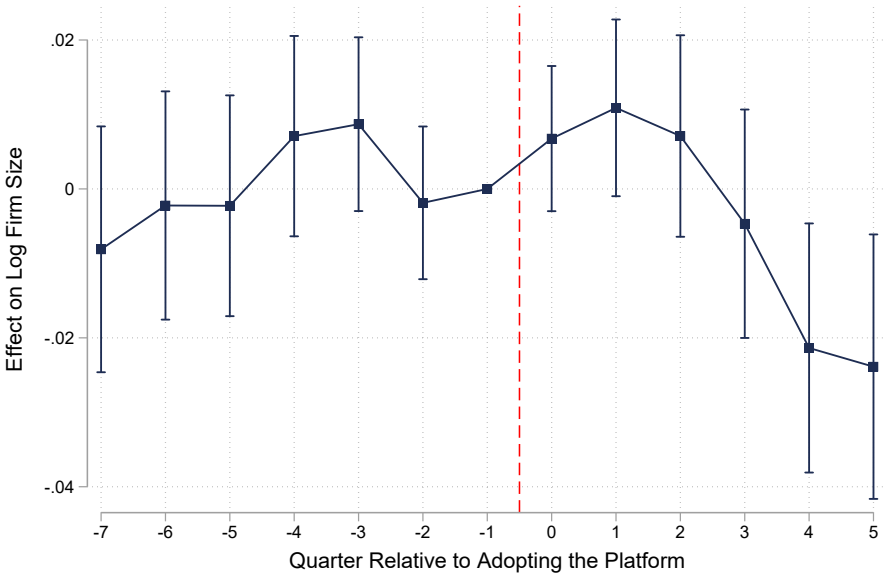
(b) Hours



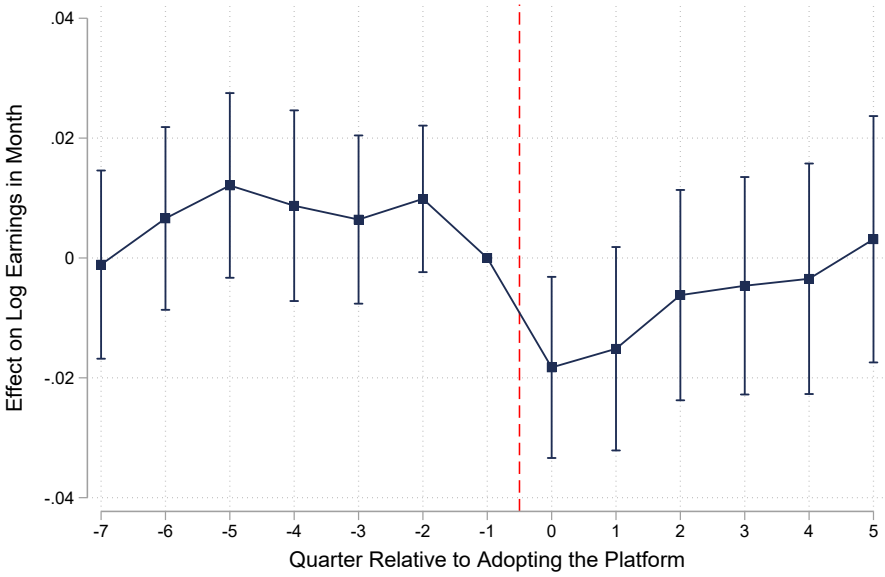
Notes: This figure reports the ratio of delivery drivers (both in-house and platform drivers) to in-house waiters wage bill (panel a) and hours (panel b) at treated establishments after the restaurants starts offering services through the platform. The ratio in panel (a) is calculated by dividing the quarterly wage bill of delivery drivers by the quarterly wage bill of in-house waiters. The red dashed line reports the estimates for restaurants that are above the median of restaurant density within their microregion. The solid blue line reports the estimates for restaurants that are below the median of restaurant density within their microregion. Restaurant density is calculated as the number of restaurants that are located in a 1 kilometer radius of each restaurant (τ). The median of restaurant density is calculate using the distribution of τ corresponding to the microregion of each restaurant in each quarter. The density assigned to each restaurant corresponds to the τ calculated using the quarter prior to the first quarter in which the treated restaurant of the pair started offering delivery services through the platform. The ratio in panel (b) is calculated by dividing the total hours hired of delivery drivers by the total number of hours hired of in-house waiters. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. Workers ocucpations are defined using the 6 digit *Classificação brasileira de ocupações* (CBO). Appendix Table B9 presents how occupations were classified using the CBO reported in RAIS.

Figure A12: Intention-To-Treat: Platform availability on Restaurant In-House Size and Wages

(a) Log In-House Firm Size



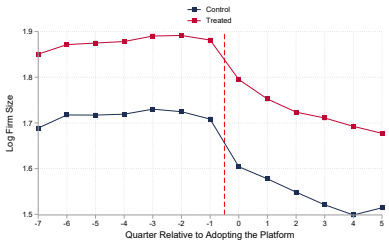
(b) Log Monthly Wages



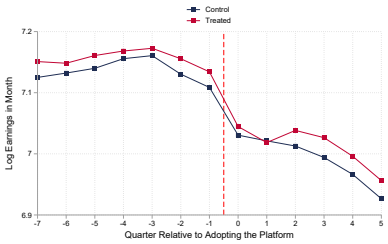
Notes: This figure reports the corresponding event-study estimates obtained after regressing each outcome (logarithm of firm size and logarithm of the average monthly wages) on event dummies related to the time of entry of the platform to the micro-region, restaurant fixed effects and quarter-year fixed effects. The x-axis reflects the quarter relative to the first quarter in which the platform becomes available in the microregion. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. Average monthly wages in a quarter are constructed by taking the quarterly average of all the monthly wages reported in RAIS by the establishment in the corresponding quarter. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A13: Placebo Test: Effects of Online-Delivery Platform Adoption on In-House Size, Wages and Closure

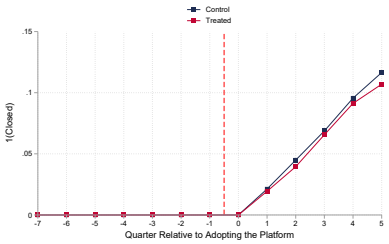
(a) Log In-House Size of Restaurants



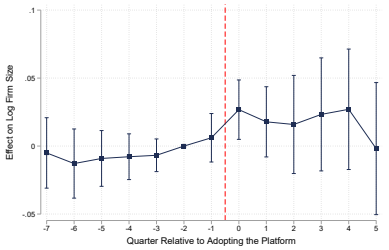
(b) Log Avg. Wage Paid by Restaurants



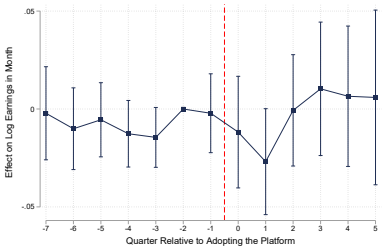
(c) Restaurant Closure



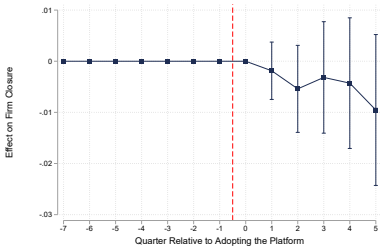
(d) Event-Study: Log In-House Size



(e) Event-Study: Log Avg. Monthly Wages



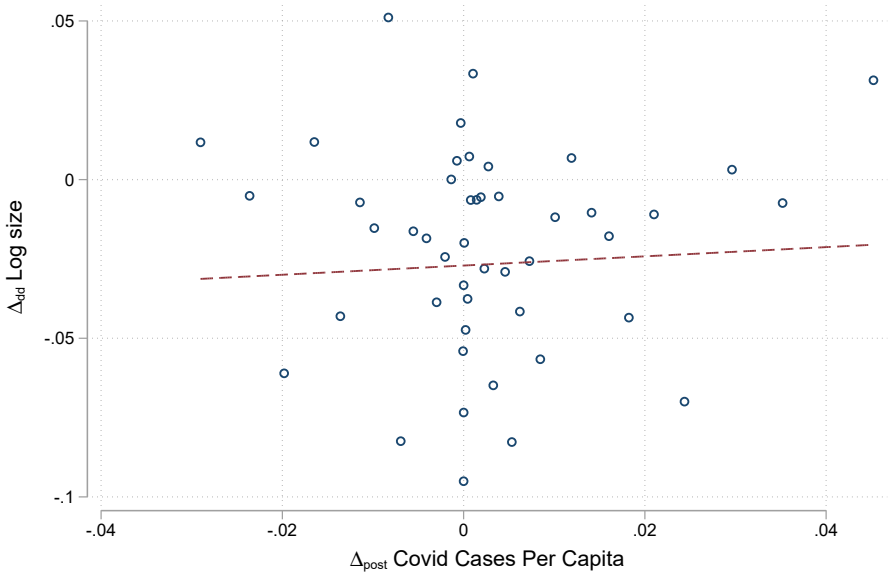
(f) Event-Study: Closure



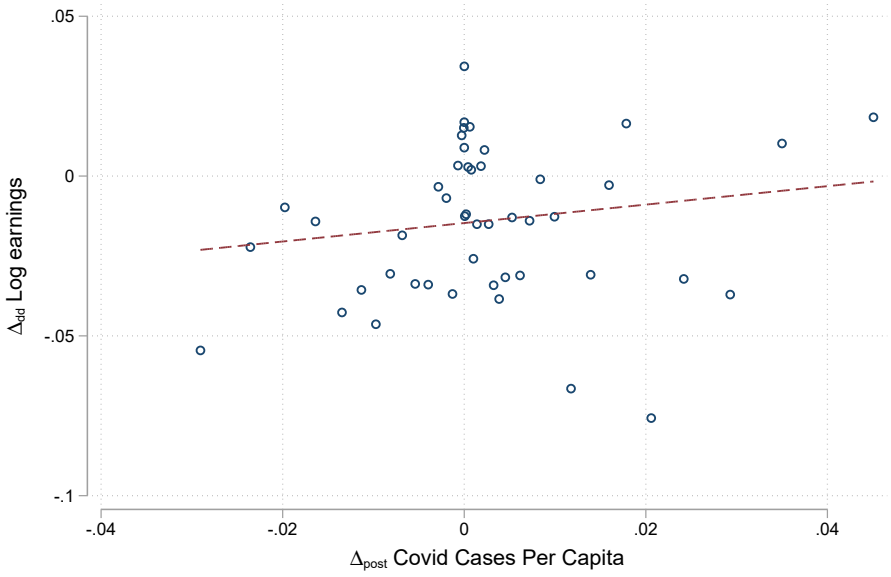
Notes: Panels (a)–(c) report the trajectories of the logarithm of restaurant size—as measured by the number of workers hired in a quarter—(panel a), average paid wages (panel b), and restaurant closure (panel c) for placebo restaurants and their matched controls. Placebo restaurants are restaurants that neither adopt the platform nor are exposed to a spillover event. Panels (d)-(f) report the corresponding event-study estimates obtained after fitting equation (15). The x-axis reflects the quarter relative to the fake treatment date assigned to the restaurant. Fake treatment dates were assigned following the same microregion-by-date distribution as the adopting restaurants in the baseline analysis. Placebo restaurants are restaurants that are neither adopters or exposed to a spillover event. Control restaurants are collected from the same pool as the baseline analysis and matched following the same procedure. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. Average monthly wages in a quarter are constructed by taking the quarterly average of all the monthly wages reported in RAIS by the establishment in the corresponding quarter. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A14: Establishment level treatment effect and COVID-19 Cases

(a) Log In-House Firm Size

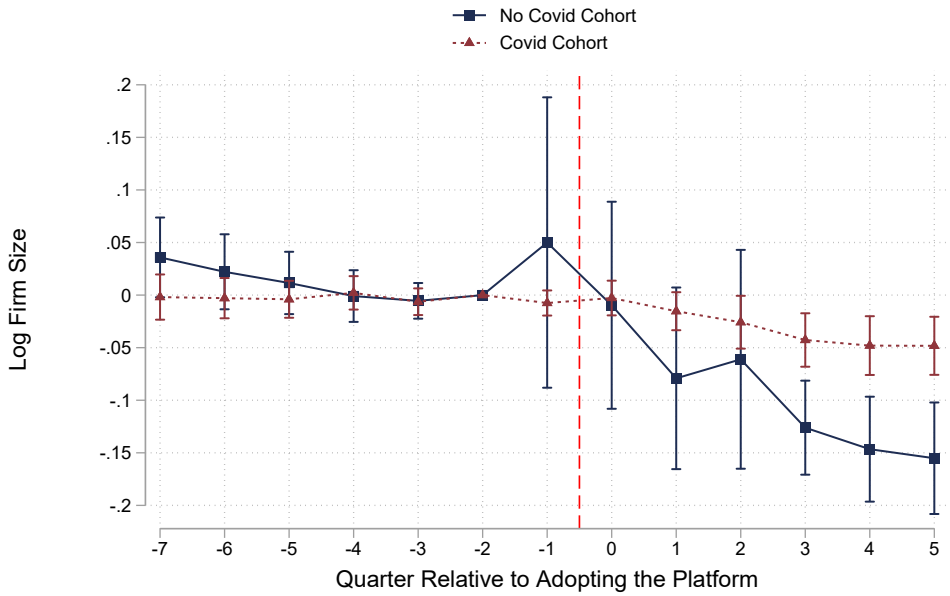


(b) Log Monthly Wages



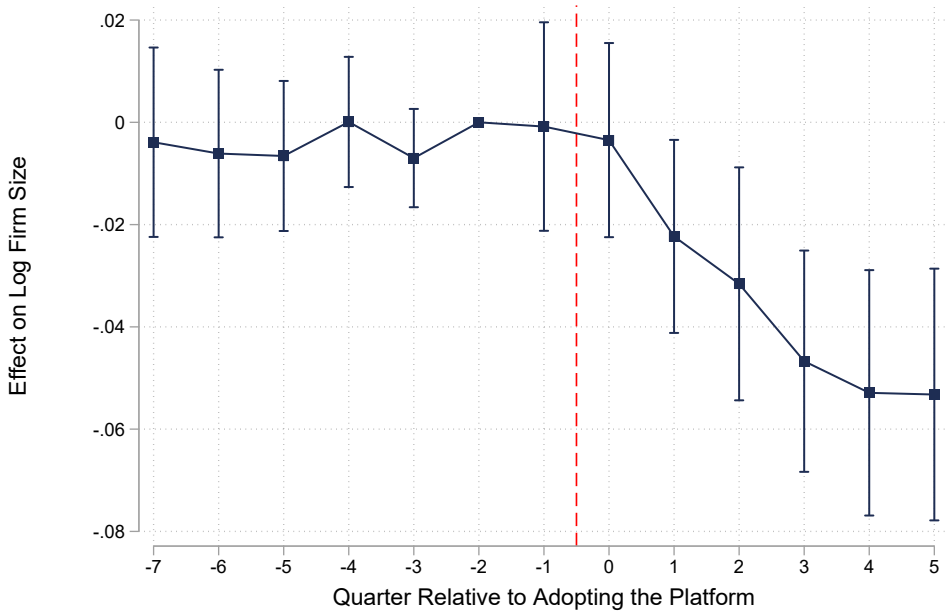
Notes: This figure displays a binscatter plot of the establishment-level difference-in-difference estimates of the adoption of the platform on log in-house establishment size (panel a) and log average monthly wages paid by restaurants (panel b), plotted against the treated-minus-control difference in average Covid-19 cases per capita in the municipalities of the matched establishments, where the average is taken over post-treatment quarters 0 to 5. Δdd denotes the establishment-level difference-in-difference estimate and is equal to $\Delta d_T - \Delta d_C$, where Δd_T and Δd_C are the changes between the pre-treatment period (quarters -7 to -1) and the post-treatment period (quarters 0 to 5) for the treated and control establishments in each matched pair. Covid cases per capita are calculated as the total covid cases reported in a quarter divided by the population of the municipality. Panel (a) has a linear slope of 0.145 (SE 0.357). Panel (b) has a linear slope of 0.289 (SE 0.246). A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. Average monthly wages in a quarter are constructed by taking the quarterly average of all the monthly wages reported in RAIS by the establishment in the corresponding quarter. Wages are expressed in real terms (2018 CPI).

Figure A15: Effect of Platform Adoption on Log Firm In-House Size by COVID-19 period



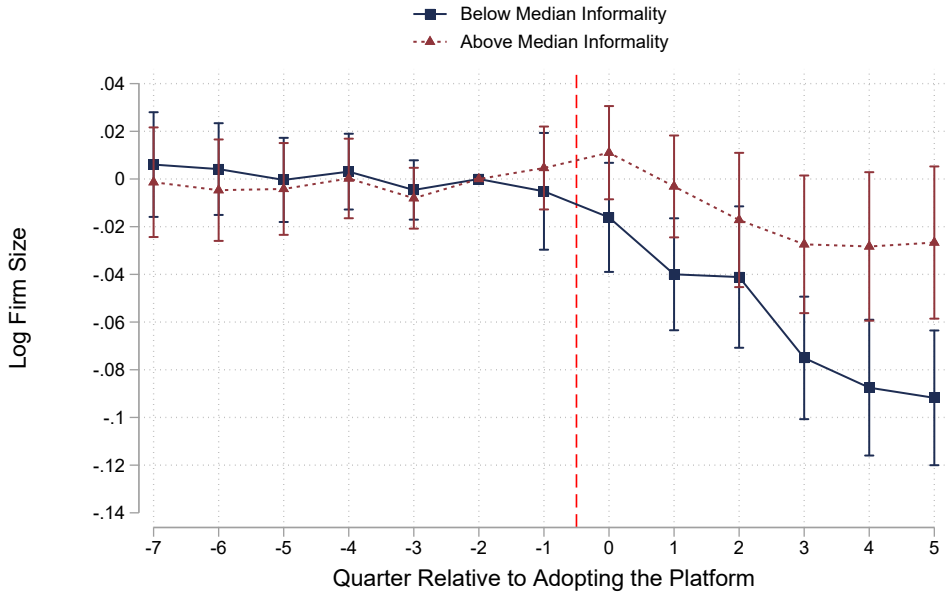
Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on the logarithm of in-house restaurant size. The blue line represents cohort of restaurants that started offering delivery services through the platform before the first quarter of 2019. That is, this quarter had five full quarters after offering services through the platform before the Covid-19 pandemic started. The red dashed line represents the cohort of restaurants that started offering delivery services through the platform between the second quarter of 2920 and the last quarter of 2020. This cohort started offering delivery services through the platform during the pandemic and I can observe at least four quarters after they adopted the platform. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. The panel reports 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A16: Effect of Platform Adoption on Log Firm In-House Size with State × Year Fixed Effects



Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on the logarithm of in-house restaurant size when including state by year fixed effects. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. The panel reports 95% confidence intervals based on standard errors clustered at the establishment level.

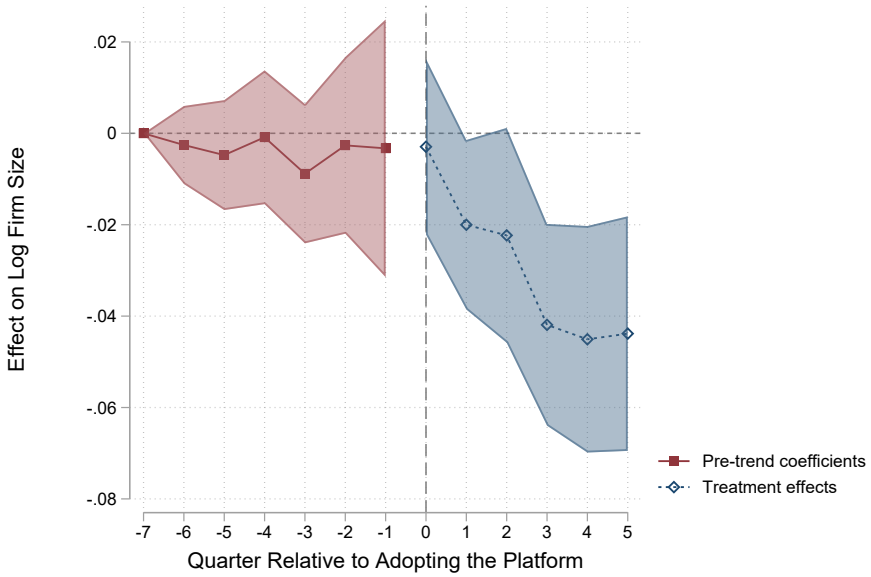
Figure A17: Effect of Platform Adoption on Log Firm Size By Informality of the Municipality of the Establishment



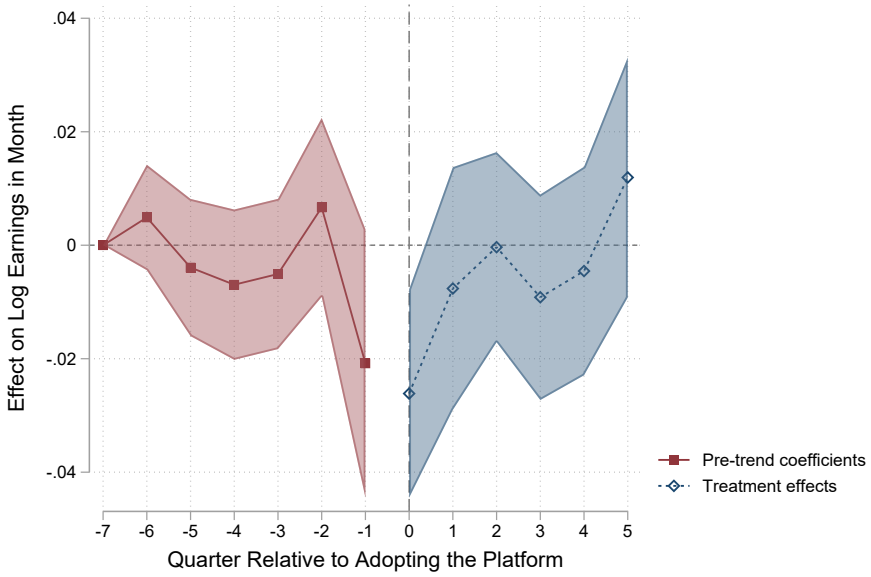
Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on the logarithm of in-house restaurant size. The plot presents the estimated coefficients by the levels of informality in the restaurant sector at the municipality of each restaurant as estimated using the 2010 Brazilian Census. Informality is calculated as the share of informal workers in the restaurant sector in each municipality. The median of informality is calculated using the distribution of informality of restaurants that adopt the platform and match to a control group. Control restaurants are assigned the level of informality of their matched treated restaurant. The blue line presents the estimated coefficients for treated restaurants located in municipalities that have above the median levels of informality. The red dashed line presents the estimated coefficients for treated restaurants located in municipalities that have below the median levels of informality. The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. The panel reports 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A18: Effect of Adopting the Platform using Borusyak et al. (2024) estimator

(a) Log In-House Firm Size



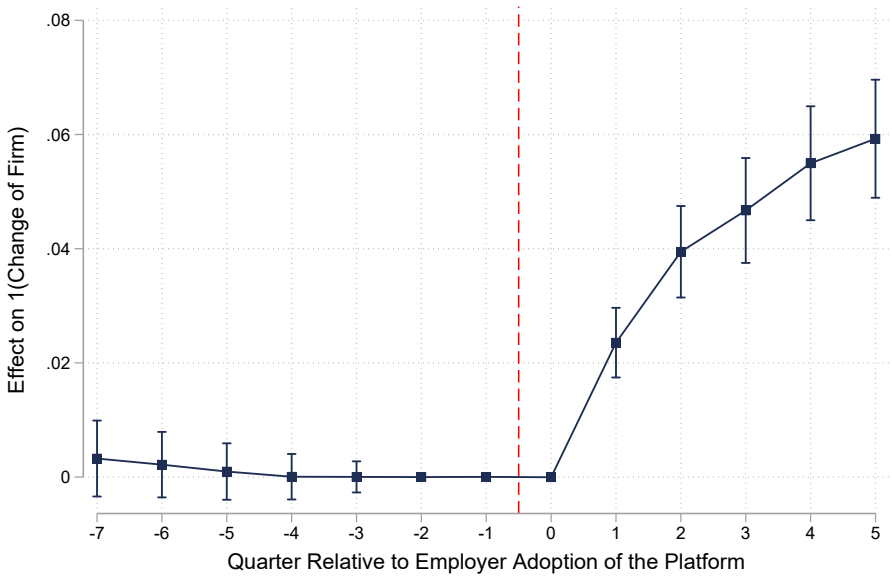
(b) Log Monthly Wages



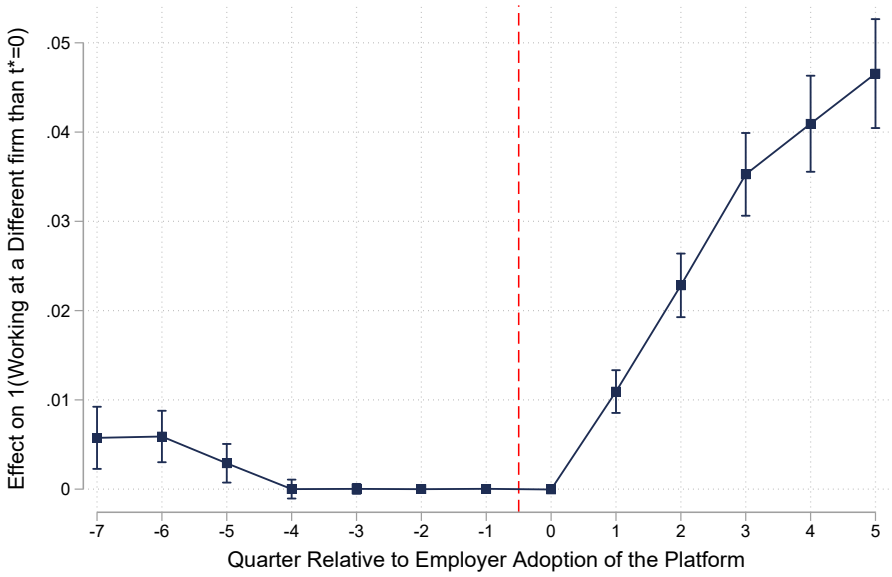
Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on the logarithm of firm size (panel a) and the logarithm of the average monthly wages paid by restaurants (panel b) using the estimator proposed by Borusyak et al. (2024). The x-axis reflects the quarter relative to the first quarter in which each restaurant offered delivery services through the platform for the first time. Quarter 0 is the quarter when the treated restaurant starts offering delivery services through the platform. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. Quarterly size is constructed by taking the average of the number of workers employed formally each month in the quarter by the establishment after applying the restrictions described in Section 3. Average monthly wages in a quarter are constructed by taking the quarterly average of all the monthly wages reported in RAIS by the establishment in the corresponding quarter. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the establishment level.

Figure A19: Effect of Employer Platform Adoption on Worker Probability of Changing Firm

(a) Likelihood of Leaving Employer from t*=0 (Non-Employment or New-Employer)

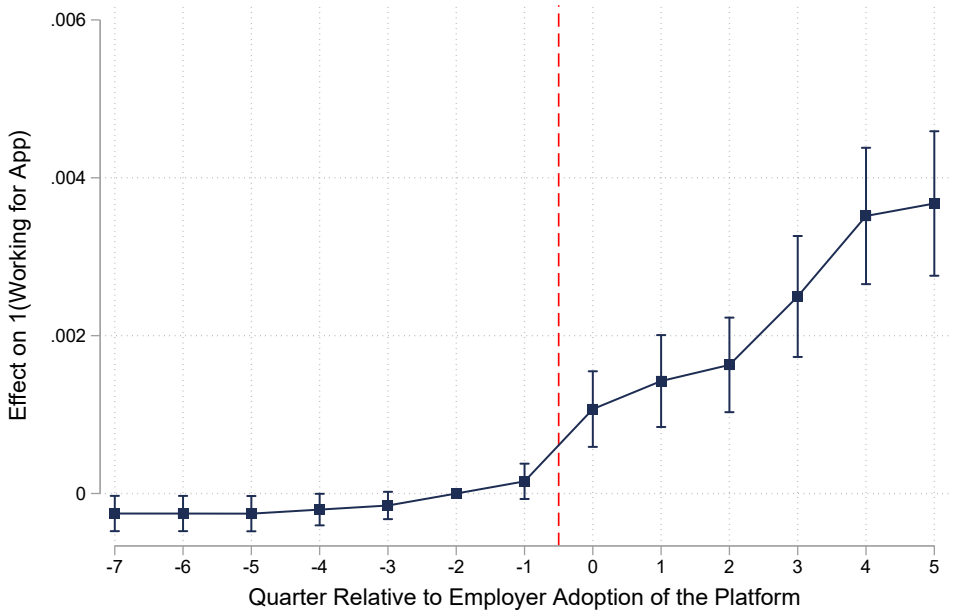


(b) Likelihood of Working for a different Employer than t*=0



Notes: Panel (a) reports the corresponding event-study estimates obtained after fitting equation (15) on a dummy equal to one if individual i in time t no longer works for the employer they had in $t^* - 1$ (either because of non-employment or they are employed at another establishment). Panel (b) reports the corresponding event-study estimates obtained after fitting equation (15) on a dummy equal to one if individual i is employed by a different employer than the one they had in $t^* - 1$. The employer in a quarter is defined as the employer that pays the highest total wages in a quarter (the dominant employer of the quarter). The x-axis reflects the quarter relative to the first quarter in which the employer of the treated worker offered delivery services through the platform for the first time. Quarter 0 is the quarter when the restaurant that employs the treated worker starts offering delivery services through the platform. Both panels report 95% confidence intervals based on standard errors clustered at the worker level.

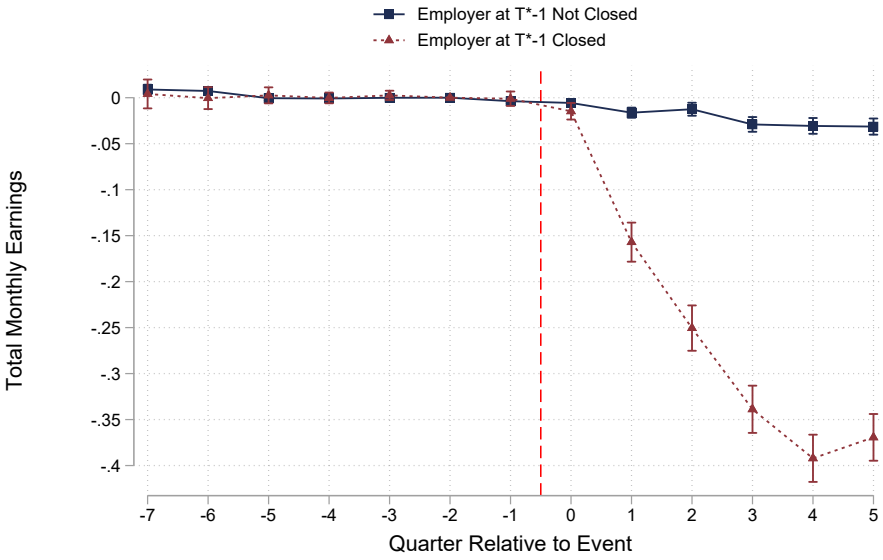
Figure A20: Effect of Employer Platform Adoption on Worker Probability of Working at Delivery Platform



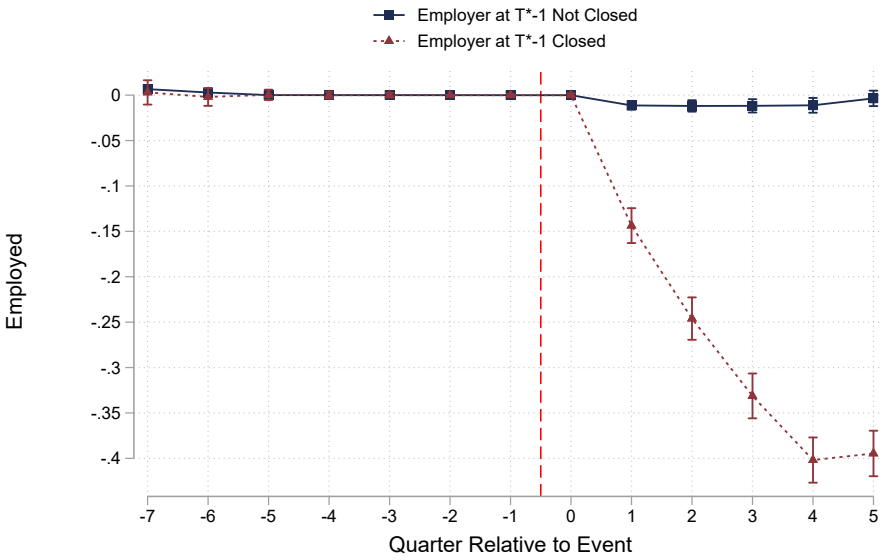
Notes: This figure reports the corresponding event-study estimates obtained after fitting equation (15) on a dummy equal to one if individual i in time t works in the platform. Employment at time t in the platform is defined as doing at least one delivery in quarter t. The x-axis reflects the quarter relative to the first quarter in which the employer of the treated worker offered delivery services through the platform for the first time. Quarter 0 is the quarter when the restaurant that employs the treated worker starts offering delivery services through the platform. The panel reports 95% confidence intervals based on standard errors clustered at the worker level.

Figure A21: Spillover Effects of Platform Adoption on Restaurant Workers by Employer Closure

(a) Total Earnings



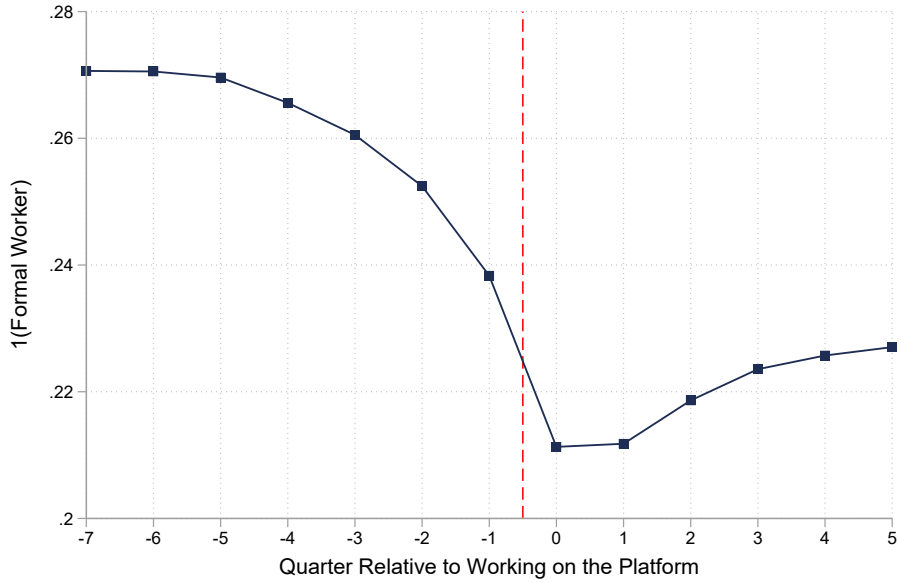
(b) Employment



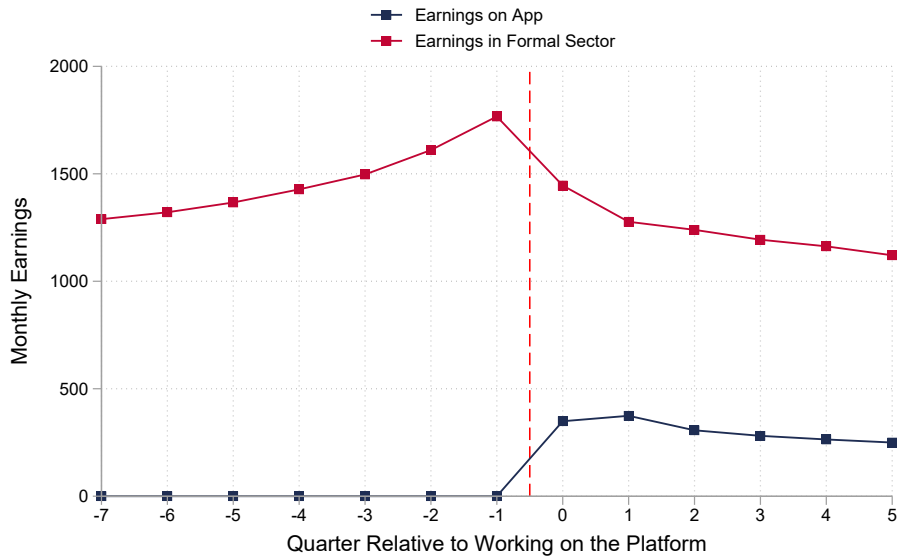
Notes: This figure reports event study estimates for workers who are employed at restaurants that are exposed to a spillover event. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. Panel (a) reports the corresponding event-study estimates obtained after fitting equation (15) on on the quarterly average of monthly earnings. Average monthly earnings reflects the quarterly average of the monthly formal wages that a worker earns in the corresponding quarter. If the worker did not hold a formal job during the period, the earnings are equal to 0. The red dashed line shows the estimates for workers whose employer at $t^* - 1$ closes within the period t^*0 and $t^* + 5$. The black line represents the estimates for workers whose employer at $t^* - 1$ does not close the following five quarters after the event. Panel (b) presents the estimates for employment. A formal worker is defined to be employed in a quarter if they have at least one day of work recorded in RAIS. The x-axis reflects the quarter relative to the quarter in which the spillover event occurs. Wages are expressed in real terms (2018 CPI). Both panels report 95% confidence intervals based on standard errors clustered at the worker level.

Figure A22: Trends of Formal Employment and Earnings for Platform Workers

(a) Formal Employment

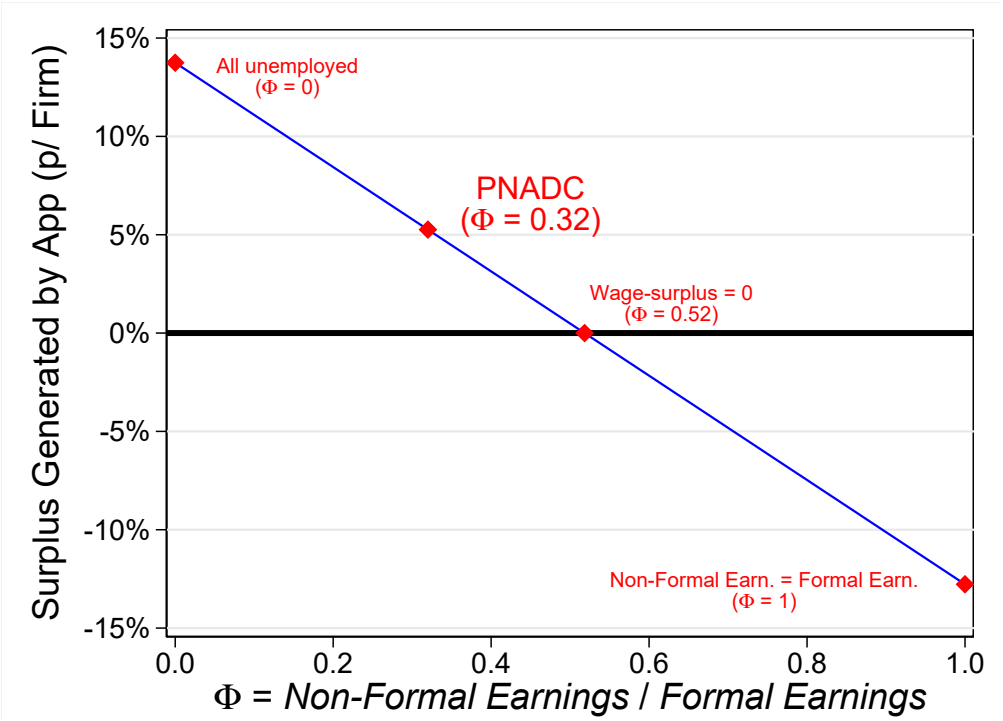


(b) Total Earnings Conditional on Worker Employed in $t^* - 1$



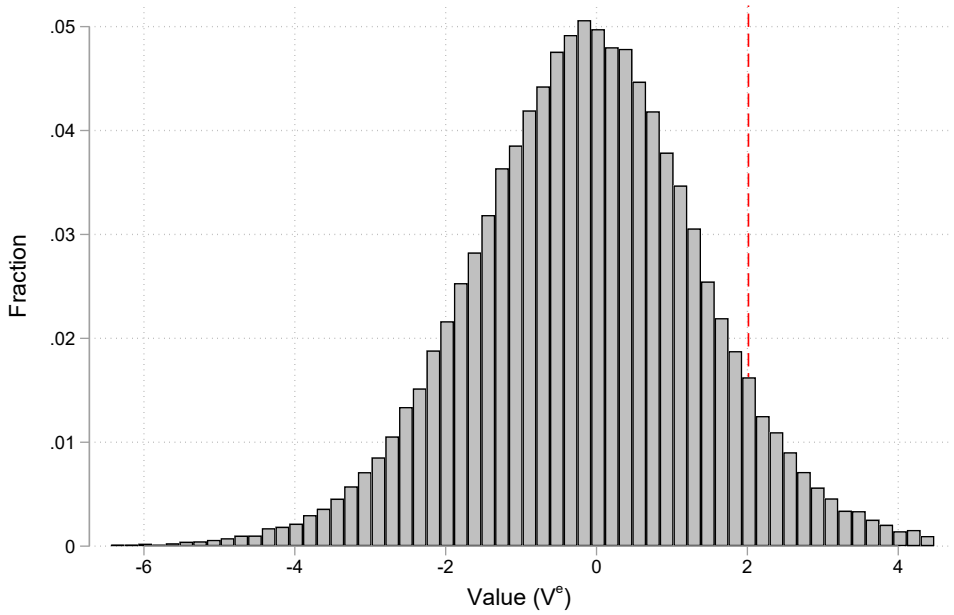
Notes: Panel (a) presents the trajectories of formal employment for workers who at some point in time work as delivery workers in the platform. A worker is defined as a platform-worker if they did at least one delivery through the platform. The worker is considered to be formally employed in quarter t if they are employed in the quarter for at least one day and they're employment is recorded in RAIS. The x-axis reflects the quarter relative to the first quarter in which the individual i worked for the first time on the platform. Panel (b) reports the monthly earnings made in formal employment (red line) and on the platform (blue line) for workers that held a formal employment the quarter prior to working on the platform (and were not laid-off either in $t^* - 1$ or in t^*0). Wages are expressed in real terms (2018 CPI).

Figure A23: Total Wage Bill Effect from Platform Adoption as a Function of Non-Formal platform-workers Outside Option



Notes: This figure reports the estimated total wage bill surplus generated by the platform as estimated using equation (16). The figure plots the wage bill surplus as a function of different outside options for app workers that did not hold a formal employment prior to working on the platform. Workers are considered to not have a formal employment prior to working on the platform if they did not have a formal job the quarter prior to working on the app, or held a formal job but were laid-off the quarter prior (or the first quarter) to their first spell on the platform. The x-axis expresses the outside option for these workers as a share of the average hourly wage that platform-workers that did have a formal employment prior to working on platform earned prior to working on the platform (Φ). The y-axis represents the surplus generated by each restaurant that adopts the platform as a percentage of the average quarterly wage bill of adopting restaurants in t*-2. The figure highlights four different points. All unemployed means that the outside option of platforms workers that did not hold a formal employment prior to working on the platform is 0. PNADC is the estimated outside option using the Brazilian household survey (PNAD-C). Wage-surplus equal 0 is the outside option for non-formal workers that makes the total wage bill surplus of the platform equal to 0. Non-Formal earnings equal to formal earnings means that the outside option of non-formal workers is the same as the outside option of formal workers.

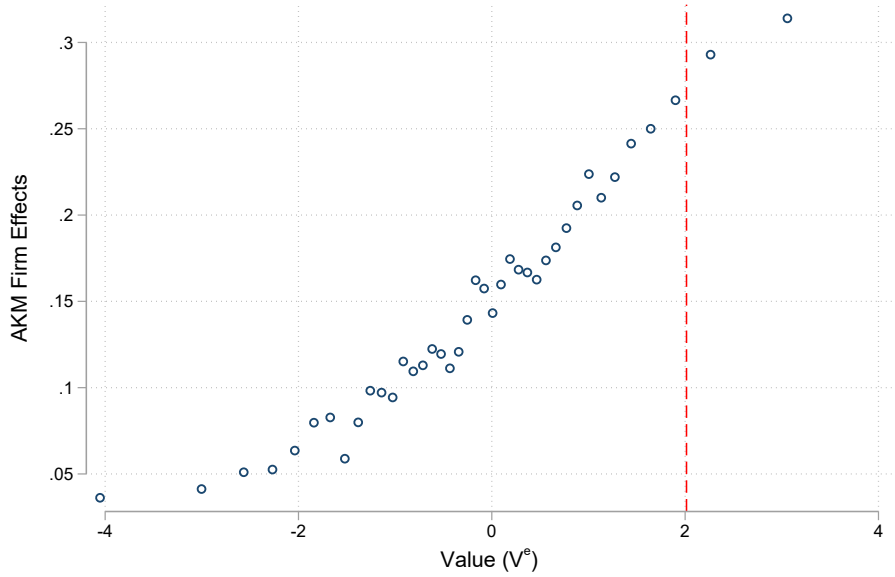
Figure A24: Histogram of Values (V_j^e) for all Restaurants



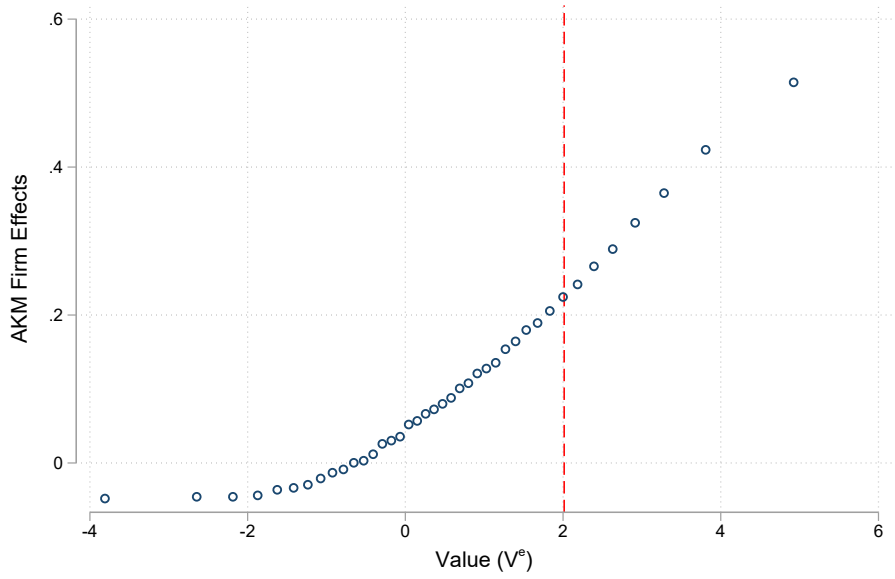
Notes: This histogram reports the distribution of V_j^e for all restaurants in the sample for whom it was possible to estimate the value (30%). V_j^e was estimated using the algorithm developed by Sorkin (2018) and represents the value of a firm. The average V_j^e in the sample is -0.158. The red dashed line represents the V_j^e of the delivery platform (2.014). This places the delivery platform in the percentile 93 of the distribution. Values V_j^e are winsorized at the 0.05 and 99.5 level.

Figure A25: Relationship Between Values (V_j^e) and Earnings

(a) Restaurants

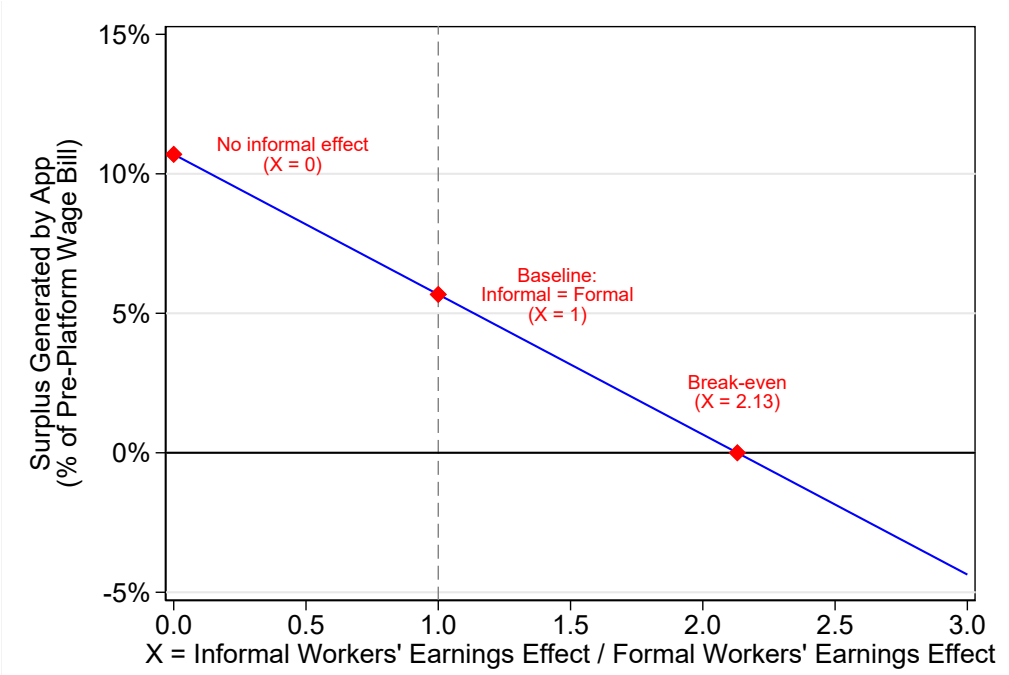


(b) All Establishments



Notes: This figure presents a binscatter plot of the relationship between the value of the AKM firm effects and firm V_j^e. Panel (a) presents the relationship for restaurants. Panel (b) presents the relationship for all establishments in the sample. The value of the firm V_j^e was estimated using the algorithm developed by Sorkin (2018) and represents the value of a firm. The red dashed line represents the V_j^e of the delivery platform (2.014). Both AKM firm effects and Values V_j^e are winsorized at the 0.05 and 99.5 level. The Binscatter plots 40 bins.

Figure A26: Total Wage Bill Effect from Platform Adoption as a Function Earnings Effect of Informal Workers



Notes: This figure reports the estimated total wage bill surplus generated by the platform as estimated using equation (16). The figure plots the wage bill surplus as a function of different earnings effects for informal workers. The x-axis expresses the earnings effect of informal workers share of earnings effects of formal workers (X). The y-axis represents the surplus generated by each restaurant that adopts the platform as a percentage of the average quarterly wage bill of adopting restaurants in t*-2. The figure highlights three different points. No informal effect means that the impact on earnings for informal workers of employer adoptoin (and spillovers) is 0. The baseline assumption is that the impact on earnings of informal workers is the same as those for formal workers. The break-even point represents the point in which the wage-surplus is equal to 0.

B Appendix: Tables

Table B1: Summary Stats Restaurant Workers (RAIS & Census)

	(1)	(2)	(3)
	RAIS	Census (2010)	
		Formal Workers	Informal Workers
Monthly Wage (2018 - R$)	1,574	1,353	842
	(686)	(2,831)	(860)
Age	34.84	33.20	31.37
	(11.50)	(11.29)	(13.35)
Female	0.56	0.56	0.60
Hours	42.84		
	(4.35)		
Share of Black Workers	0.06	0.09	0.09
Delivery	0.02	0.01	0.02
Kitchen	0.24	0.43	0.34
Waiters	0.53	0.29	0.43
Other	0.21	0.27	0.21
Number of Workers	1,795,250	94,559	59,115

Notes: Column 1 presents the summary statistics of restaurant workers in 2018 using data from RAIS. These statistics are weighted at the individual level. Column (2) and Column (3) present statistics for restaurant workers using the 2010 Brazilian Census (the latest census available). A worker is considered to be formal if they respond that they have a *Carteira de trabalho assinada* in their job. An individual is considered to work in a restaurant if they are working for an establishment that has a two digit CNAE industry code equal to 56. Definitions of occupations are presented in Table B9.

Table B2: Summary Stats for Platform Workers

	Summary Stats	Performance on the App	
	App Workers	Worker Formal Before App	Worker Non-Formal Before App
	(1)	(2)	(3)
Demographics:			
Age	29.5		
	(8.0)		
Female	0.08		
Monthly Earnings in Formal Job (Unconditional)	358		
	(867)		
Monthly Earnings in Formal Job (Conditional on Employment)	1,583		
	(1,177)		
Weekly Hours in Formal Job	41.7		
	(6.5)		
Tenure in Formal Job (in Years)	2.16		
	(2.9)		
Education (Years)	11.6		
	(1.7)		
Full Time	0.94		
Brazilian	1.0		
Workers in RAIS at some point	0.57		
Performance on App:			
Monthly Earnings (Tips)		10.2	11.2
		(19.0)	(20.8)
Monthly Earnings (Trip)		571.2	646.6
		(727.3)	(810.5)
Monthly Earnings (Promotions)		51.7	47.8
		(92.2)	(88.7)
Monthly Hours Online		62.0	75.9
		(62.9)	(73.0)
Monthly Hours Worked		30.1	34.9
		(36.8)	(41.6)
Monthly Total Deliveries		79.5	91.8
		(104.4)	(117.3)
Monthly Total Distance (KMs)		425.9	480.0
		(593.5)	(660.0)
Number of Workers	624,141	143,996	480,145

Notes: Column 1 presents the summary statistics of individuals that at some point in time work as delivery workers in the platform. Statistics reflect the demographics at the first quarter in which the individual worked for the platform. Demographic information is only available for individuals who at some point in their career hold a formal employment. Unconditional formal earnings represents earnings that include zeros for individuals that are not employed in the formal sector. Workers in RAIS at some point, are workers that at some point in my sample hold a formal employment as reported in RAIS. Column 2 reports the performance on the platform for workers that the quarter prior to joining the platform held a formal employment (and were not laid-off either the quarter prior to joining the platform or the quarter in which they joined the platform). Column 3 reports the performance for workers that the quarter prior to joining the platform did not hold a formal employment. Hours online are hours in which the individual had the platform open (either in the background or actively using it). Hours worked are hours in which the individual was actively doing a delivery. Standard deviations are reported in parentheses.

Table B3: Predictors of Platform Entry

	1 (Platform Entered Microregion)
Total Workers	0.0000954***
	(0.0000216)
Total Firms	-0.000251
	(0.000266)
Share of Female	2.91300
	(4.50000)
Share of Black	7.87900
	(5.99600)
Avg. Wages	-0.0000661
	(0.000575)
Avg. Years of Education	1.95300***
	(0.64800)
Avg. Tenure	-0.04170**
	(0.01680)
Avg. Age	0.15900
	(0.17200)
Avg. Hours Hired	0.00306
	(0.13400)
Growth of Total Workers	-2.49600
	(3.00200)
Growth Total Firms	3.43200
	(6.13900)
Growth Female Share	-1.77900
	(5.65300)
Growth Black Share	0.32900
	(0.84300)
Growth Avg. Wages	1.24200
	(5.10300)
Observations	539

Notes: This table presents the estimates for a logit regression of platform availability in a microregion on covariates. Platform availability is defined as the platform ever entering a micro region in th period covered by the sample (2018-2021). Covariates are based on 2018 averages. Standard errors are reported in parentheses.

Table B4: Effects of Online-Delivery Platform Adoption: Instrumental Variable

	(1) Δ Log Firm Size	(2) Δ Log Avg. Wages
OLS:		
Platform Adoption	0.00944	0.00972
	(0.0109)	(0.00631)
Reduced Form:		
Platform Availability	-0.00284***	0.00122*
	(0.000935)	(0.000675)
2SLS:		
Platform Adoption	-0.161***	0.0733*
	(0.0572)	(0.0410)
Fstat	320.063	293.933
Observations	13,112	13,112

Notes: This table presents the OLS, reduced form and instrumental variable estimates for the effects of platform adoption on changes in establishment log firm size (column 1) and changes in establishment log average wages (column 2). Details on the estimation procedure are described in Appendix D1. Changes in outcomes are differences between 2021 averages and 2017. Platform adoption is a binary variable equal to 1 if the platform was ever adopted by the restaurant. Platform availability is the amount of months the platform was available in the microregion between January 2018 and December 2021. Microregions where the platform was available prior to 2018 or where it entered in 2021 were dropped. Fstat referes to the Kleibergen-Paap rk Wald F statistic. Outcomes are winsorized at the 2-98 percentiles. Standard errors are clustered at the microregion level and reported in parenthesis.

Table B5: Summary Statistics for Restaurants Exposed to Spillovers and Control Restaurants

	(1) Matched Control Restaurant	(2) Matched Spillover Restaurants	(3) Potential Spillover Restaurants
Years of Education	10.88	11.05	11.03
	(1.53)	(1.40)	(1.50)
Tenure (in years)	3.02	3.01	2.92
	(2.10)	(2.22)	(2.29)
Monthly Earnings (in 2018-R$)	1,501	1,521	1,488
	(410)	(425)	(475)
Age	35.56	35.03	35.28
	(7.19)	(7.51)	(8.10)
Share of Brazilians	1.00	0.99	0.99
Female	0.60	0.60	0.61
Hours	43.14	43.18	43.22
	(2.69)	(2.46)	(2.54)
Full-Time	0.97	0.98	0.98
Establishment Size	10.86	10.96	9.32
	(21.84)	(56.57)	(50.18)
Share of Waiters	0.46	0.48	0.48
	(0.32)	(0.32)	(0.34)
Number of Establishments	9,998	19,885	34,082

Notes: Column 2 presents the summary statistics for restaurants who are exposed to a spillover event and for whom I can find a matched control restaurant. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. These summary statistics are calculated in 2017. Potential matched control restaurants are those who in $t_j^* - 1$ do not have platform delivery services available in their microregion (and the platform does not start offering services the following five quarters). Additionally, these control restaurants must have been open for at least two years, Treated restaurants are matched to controls that belong to the same quartile of firm size, the same quartile of average earnings, and the same side of the median of the number of restaurants within a 1-kilometer radius. (with respect to the distribution of restaurants in their corresponding microregion). A propensity score matching based on log size in $t^* - 8$ to $t^* - 1$, log average monthly wages paid in quarters $t^* - 4$ to $t^* - 1$, firm age, share of waiters, average tenure, age of workers, share of female workers and average hours of workers is conducted to assign exactly one matched control restaurant to each spillover restaurant. Column 1 reports the characteristics of matched control restaurants, and Column 3 reports the summary statistics for the entire set of restaurants that are exposed to spillover events and are identified in RAIS. Restaurants are defined as establishments that have a CNAE two digit code equal to 56. Wages are expressed in real terms (2018 CPI). All statistics are person-month-weighted, and standard deviations are reported in parentheses.

Table B6: Summary Statistics for Workers Exposed to Spillover Events and Control Workers

	(1) Matched Control Workers	(2) Matched Treated Workers	(3) Potential Treated Workers
Years of Education	10.73	10.83	11.09
	(2.33)	(2.14)	(2.29)
Avg Tenure (in years)	4.04	4.04	3.11
	1.26	(1.26)	(1.62)
Avg Monthly Earnings (in 2018-R$)	1,510	1,504	1,641
	(364)	(372)	(909)
Age	36.04	36.03	36.43
	(10.26)	(10.26)	(11.66)
Share of Brazilians	1.00	0.99	0.99
Female	0.66	0.66	0.58
Hours	43.21	43.21	42.80
	(3.01)	(3.04)	(4.43)
Delivery	0.01	0.01	0.02
Kitchen	0.25	0.25	0.25
Waiters	0.60	0.60	0.47
Other	0.14	0.14	0.26
Number of Workers	21,371	29,154	160,648

Notes: Column 2 presents the summary statistics for workers who are employed at restaurants that are exposed to a spillover event and for whom I can find a matched control worker. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. These summary statistics are calculated the quarter prior to the spillover event, that is, $t^* - 1$. Potential matched control workers are those who in $t_j^* - 1$ are working at a restaurant that is located in a microregion where there are no platform delivery services available (and the platform does not start offering services the following five quarters). Additionally, these control workers must have the same gender, occupation as their corresponding treated worker and their employer must belong to the same quartile of wage, size and median of number of restaurants within a radius of 1 kilometers (with respect to the distribution of restaurants in their corresponding microregion). A caliper matching method based on earnings in t^*-1 to t^*-4, age and tenure is then conducted to assign exactly one matched control worker for each treated worker without replacement (Stepner and Garland, 2017). Column 1 reports the characteristics of matched control workers, and Column 3 reports the summary statistics for the entire set of workers that work at a restaurant exposed to a spillover event. Restaurants are defined as establishments that have a CNAE two digit code equal to 56. Occupations are based on the 6 digit *Classificação brasileira de ocupações* (CBO). Wages are expressed in real terms (2018 CPI). Standard deviations are reported in parentheses.

Table B7: Total Wage Bill Effect of Platform Adoption by Gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	In-House Restaurant Workers Net Earnings (Formal)		In-House Restaurant Workers Net Earnings (Informal)		Spillovers Net Earnings (Formal)		Spillovers Net Earnings (Informal)		App Workers Net Earnings (From Formal)		App Workers Net Earnings (From Non-Formal)	
	Women	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women	Men
Quarter 0	-208	214	-49	51	-39	-3	-10	-1	5.9	166	40	733
	(44)	(40)	(10)	(10)	(13)	(12)	(3)	(3)	(0.6)	(1)	(0)	(2)
Quarter 1	-664	15	-158	4	-203	-122	-50	-30	14.0	369	103	1,761
	(10)	(80)	(2)	(19)	(27)	(25)	(7)	(6)	(0.6)	(21)	(7)	(87)
Quarter 2	-539	-126	-128	-30	-246	-239	-61	-59	15.6	396	129	2,016
	(236)	(5)	(56)	(1)	(41)	(37)	(10)	(9)	(0.8)	(6)	(4)	(42)
Quarter 3	-361	-84	-86	-20	-678	-652	-168	-162	23.6	525	160	2,515
	(146)	(74)	(35)	(17)	(67)	(68)	(17)	(17)	(1.0)	(4)	(4)	(40)
Quarter 4	-475	-397	-113	-95	-1,419	-1,267	-352	-314	20.8	505	172	2,577
	(15)	(228)	(3)	(54)	(143)	(148)	(35)	(37)	(0.1)	(14)	(3)	(36)
Quarter 5	42	-385	10	-92	-2,230	-2,116	-553	-524	18.8	474	180	2,641
	(108)	(305)	(26)	(72)	(281)	(279)	(70)	(69)	(0.4)	(8)	(2)	(20)
Cumulative PDV	-2,151	-716	-513	-171	-4,588	-4,188	-1,137	-1,038	95.3	2,351	756	11,814
	(406)	(610)	(96)	(145)	(459)	(452)	(114)	(112)	(0.1)	(50)	(19)	(221)
Percentage of Pre-Platform Wage-Bill	-3.9%	-1.3%	-0.9%	-0.3%	-8.4%	-7.7%	-2.1%	-1.9%	0.2%	4.3%	1.4%	21.6%

Notes: This table reports the estimated wage bill surplus generated by the platform for each component of equation (16) by gender. Columns 1-4 present the effects on the net earnings of in-house restaurant workers whose employer starts offering delivery services through the platform (for formal and inputed informal workers, respectively). Each quarter report the corresponding event-study estimates obtained after fitting equation (15) on monthly earnings, converting the coefficient to quarterly and multiplying it by the average firm size of restaurants that adopt the platform in $t^* - 2$. The number of informal workers is imputed to each establishment using the average share of informal workers in the restaurant sector in each municipality estimated using the 2010 Census. The earnings of informal workers are imputed to each establishment using the ratio of informal to formal wages in the restaurant sector in each state-quarter estimated using the PNAD-C. Columns 5-8 present the estimated coefficients for formal and informal workers that are employed at restaurants that are exposed to a spillover event. A spillover event is defined as a restaurant that is exposed to a large increase in the share of restaurants within a 1 kilometer radius that start offering services through the platform. As described in Section 6.2, a large increase is defined as the top 5 percentile of the distribution of quarterly changes in the share of nearby restaurants that adopt the platform. To estimate a per adopting restaurant effect of spillovers, the earnings effects of workers exposed to spillover events are re-scaled by the effect of spillover on restaurant adoption and then scaled by the average restaurant size in my sample and by the probability of a restaurant being exposed to a spillover event as described in Appendix D2. Columns 9-12 report the total earnings gained for workers who work on the platform net of their outside option and their transportation costs. The outside option for workers who transition from the formal sector is equivalent to the average per hour wage that workers held prior to working on the platform. The outside option for workers who transition from non-formal status is imputed using a regression-adjusted earnings for non-formal workers within the same state-quarter using the PNAD-C. Transportation costs are based on estimates from Callil and Picanço (2023). PDV means present discounted value which was calculated using the average quarterly inflation of Brazil between 2019 and 2021 (0.012). Pre-platform wage bill is the wage bill of restaurants that adopt the platform in $t^* - 2$. For all columns in the table, the pre-adoption wage bill is calculated including the inputed informal workers. Wages are expressed in real terms (2018 CPI). Standard errors are in parentheses and were bootstraped.

Table B8: Robustness: Alternative Spillover Measures

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline		**Constant WDA (T*0)**		**Varying WDA**	
	Spillovers (Formal)	Spillovers (Informal)	Spillovers (Formal)	Spillovers (Informal)	Spillovers (Formal)	Spillovers (Informal)
Quarter 0	-43	-11	-61	-15	-61	-15
	(18)	(4)	(58)	(14)	(58)	(14)
Quarter 1	-327	-81	-573	-142	-598	-148
	(37)	(9)	(449)	(111)	(405)	(100)
Quarter 2	-471	-117	-1,181	-293	-1,488	-369
	(56)	(14)	(924)	(229)	(653)	(162)
Quarter 3	-1,293	-320	-2,248	-557	-3,302	-818
	(98)	(24)	(1,754)	(434)	(1,127)	(279)
Quarter 4	-2,620	-649	-3,442	-853	-4,943	-1,225
	(219)	(54)	(2,672)	(662)	(1,402)	(347)
Quarter 5	-4,184	-1,037	-4,610	-1,142	-7,030	-1,742
	(459)	(114)	(3,577)	(886)	(1,886)	(467)
Cumulative PDV	-8,513	-2,109	-11,574	-2,868	-16,627	-4,119
	(719)	(178)	(8,993)	(2,228)	(4,889)	(1,211)
Percentage of Pre-Platform Wage Bill	15.6%	3.86%	21.2%	5.26%	30.47%	7.55%

Notes: This table reports alternative estimates of the spillover component of equation (16) using a market-level decomposition of employment flows. Columns 1-2 reproduce the baseline spillover estimates for formal and informal workers (columns 5-6 of Table V), which recover per-adopter displacement by rescaling the worker-level earnings coefficient within a 1 kilometer radius as described in Appendix D2. Columns 3-6 present estimates from an alternative approach that decomposes total market-level employment changes in the restaurant sector (Figure II, panel a) into the component attributable to adopting restaurants and a residual capturing displacement at non-adopting restaurants as described in Section 9.1. The adopter component is computed by convolving the flow of new adopters at each period with the estimated firm-size effect from the adopting restaurant event study, evaluated at the baseline firm size of non-adopters. The residual, divided by the number of new adopters, yields the workers displaced per adopter (WDA). Columns 3-4 use a constant WDA estimated at $\tau = t^*0$ for all periods, on the grounds that the initial displacement rate is free from confounding interactions with prior adoption cohorts. Columns 5-6 allow the WDA to vary by period. In both versions, the aggregate earnings effect is obtained by convolving the per-adopter displacement rates with the spillover worker earnings coefficients. Informal workers are imputed using the average share of informal workers in the restaurant sector in each municipality from the 2010 Census, and their earnings are imputed using the ratio of informal to formal wages in each state-quarter from the PNAD-C. PDV means present discounted value calculated using the average quarterly inflation of Brazil between 2019 and 2021 (0.012). Wages are expressed in real terms (2018 CPI). Standard errors are in parentheses and were bootstrapped.

Table B9: Crosswalk of Occupations from CBO to Waiters, Cooks and Delivery Workers

CBO Code	CBO Definition	Occupation Crosswalk
513405	Waiter	Waiter
513435	Snack Bar Attendant	Waiter
513505	Food Service Assistant	Waiter
513425	Pantry Worker	Waiter
521110	Retail Salesperson	Waiter
421125	Cashier	Waiter
513420	Bartender	Waiter
422105	Receptionist	Waiter
513415	Busser (Assistant Waiter)	Waiter
141510	Restaurant Manager	Waiter
271105	Head Chef	Cook
513205	General Cook	Cook
513215	Industrial Cook	Cook
513605	Barbecue Cook	Cook
513610	Pizza Maker	Cook
513615	Sushi Chef	Cook
841408	Food Preserver (Conservation of Food)	Cook
841416	Meat Cooker	Cook
841420	Fruit and Vegetable Cooker	Cook
848315	Pasta Maker	Cook
848310	Confectioner	Cook
848305	Baker	Cook
519110	Motorcyclist for Document and Small Parcel Delivery	Delivery
782310	Van or Similar Vehicle Driver	Delivery
782305	Passenger Car Driver	Delivery
782510	Truck Driver (Regional and International Routes)	Delivery
783225	Driver's Assistant	Delivery
782820	Pedal Vehicle Driver	Delivery

C Appendix: Model

C1 Main Model: Derivations

This section presents the derivations of the model discussed in Section 2. The model is a job-posting model that builds on Card et al. (2018). I start by providing a microfoundation for the labor supply of each type of worker. I then derive the profit-maximization problem for restaurants, and finally derive the comparative statics for waiters and cooks.

There are J firms in the economy and three types of workers: cooks (C), waiters (W), and delivery drivers (D). Each firm j posts wages for cooks and waiters, while delivery drivers are price takers in a competitive market. These wages are observed at no cost by workers, and restaurants hire any worker willing to work at the posted wage. Each firm offers a unique workplace environment for each type of worker, and workers have heterogeneous preferences over these environments. Specifically, the indirect utility of worker i from working at firm j is given by:

$$u_{i\aleph j} = \beta_{\aleph} \ln(w_{\aleph j}) + a_{\aleph j} + \varepsilon_{i\aleph j} \tag{C18}$$

where $\aleph = \{C, W\}$, $a_{\aleph j}$ is a restaurant-specific amenity common to each type of worker $\aleph$, and $\varepsilon_{i\aleph j}$ is an idiosyncratic worker-specific taste shock. As in Card et al. (2018), I assume that $\varepsilon_{i\aleph j}$ is distributed as a standard type-I extreme value distribution. Following McFadden (1972), the probability of a worker of type $\aleph$ working at firm j is given by:

$$p_{\aleph j} = \frac{\exp\left(\beta_{\aleph} \ln(w_{\aleph j}) + a_{\aleph j}\right)}{\sum_{k=1}^{J} \exp(\beta_{\aleph} \ln(w_{\aleph k}) + a_{\aleph k})} \tag{C19}$$

When the number of firms J is sufficiently large—that is, abstracting from strategic interactions—the probability of a worker of type $\aleph$ working at firm j can be approximated by:

$$p_{\aleph j} \approx \tilde{\lambda}_{\aleph} \exp\left(\beta_{\aleph} \ln(w_{\aleph j}) + a_{\aleph j}\right) \tag{C20}$$

where $\tilde{\lambda}_{\aleph}$ is constant across firms. Taking logs gives the labor supply presented in Section 2:

$$\ln(\aleph_j) = \lambda_{j\aleph} + \beta_{\aleph} \ln(w_{j\aleph}) \tag{C21}$$

where $\lambda_{j\aleph} = \ln(\tilde{\lambda}_{\aleph}) + a_{\aleph j}$.

Each restaurant produces two goods: (i) dine-in meals (Y^I) and (ii) delivery meals (Y^D). The production process of each good is given by:

$$Y_j^I = T_j^I (C^I)^{\alpha} W^{1-\alpha} \tag{C22}$$

$$Y_j^D = T_j^D (C^D)^{\alpha} D^{1-\alpha} \tag{C23}$$

$$C = C^I + C^D \tag{C24}$$

where, for simplicity, the output elasticity α is assumed to be the same across goods, and $T^{\{I,D\}}$ are Hicks-neutral productivity shifters.

The composite restaurant good Y aggregates dine-in and delivery quantities according to

$$Y \equiv \left[\omega^{1/\kappa}(Y^I)^{\frac{\kappa-1}{\kappa}} + (1-\omega)^{1/\kappa}(Y^D)^{\frac{\kappa-1}{\kappa}}\right]^{\frac{\kappa}{\kappa-1}} \tag{C25}$$

with $\kappa > 1$ representing the elasticity of substitution across goods, while $\omega \in (0,1)$ captures the baseline preference for dine-in relative to delivery.

Define the effective delivery price as

$$\tilde{p}^D \equiv \tau p^D,$$

where τ is a revenue-equivalent delivery wedge. I use $\tilde{p}^D$ as the relevant delivery price in both the CES demand system and delivery revenue.

Next, define the CES price index for the restaurant's composite good:

$$P \equiv \left[\omega(p^I)^{1-\kappa} + (1-\omega)(\tilde{p}^D)^{1-\kappa}\right]^{\frac{1}{1-\kappa}} \tag{C26}$$

Standard CES demand implies that, conditional on total restaurant demand Y, the within-restaurant demands are:

$$Y^I = \omega\left(\frac{p^I}{P}\right)^{-\kappa} Y \tag{C27}$$

$$Y^D = (1-\omega)\left(\frac{\tilde{p}^D}{P}\right)^{-\kappa} Y \tag{C28}$$

Defining the within-restaurant dine-in quantity share as

$$q_I \equiv \frac{Y^I}{Y},$$

one reaches the following log-odds ratio:

$$\ln\left(\frac{Y^D}{Y^I}\right) = \ln\left(\frac{1-\omega}{\omega}\right) - \kappa \ln\left(\frac{\tilde{p}^D}{p^I}\right) = \ln\left(\frac{1-\omega}{\omega}\right) - \kappa \ln\left(\frac{\tau p^D}{p^I}\right).$$

Thus, if adoption of delivery platforms lowers either ω or τ, the relative quantity of delivery rises.

Define revenue components as

$$R^I \equiv p^I Y^I, \qquad R^D \equiv \tilde{p}^D Y^D, \qquad R \equiv R^I + R^D = PY.$$

To derive the first-order conditions, it is helpful to note that the CES aggregator implies

$$\frac{\partial Y}{\partial Y^I} = \omega^{1/\kappa}\left(\frac{Y}{Y^I}\right)^{1/\kappa} = \frac{p^I}{P}. \tag{C29}$$

Using the chain rule, one then obtains:

$$\frac{\partial R}{\partial Y^I} = \frac{\partial R}{\partial Y}\frac{\partial Y}{\partial Y^I} \tag{C30}$$

Using the inverse demand function $P = P_0 Y^{-1/\epsilon}$, it follows that

$$\frac{\partial R}{\partial Y} = \frac{\epsilon - 1}{\epsilon} P, \tag{C31}$$

so that

$$\frac{\partial R}{\partial Y^I} = \left(\frac{\epsilon - 1}{\epsilon} P\right)\frac{p^I}{P} = \frac{\epsilon - 1}{\epsilon} p^I. \tag{C32}$$

Using the Cobb-Douglas production function, the marginal physical product of waiters is

$$\frac{\partial Y^I}{\partial W} = (1 - \alpha)\frac{Y^I}{W}. \tag{C33}$$

Therefore, the marginal revenue product of waiters is

$$\text{MRP}_W = \frac{\epsilon - 1}{\epsilon} p^I \frac{\partial Y^I}{\partial W} = \frac{\epsilon - 1}{\epsilon}(1 - \alpha)\frac{R^I}{W}. \tag{C34}$$

Using the labor supply equation, one arrives at the first-order condition described in the paper:

$$w_W = \frac{\beta_W}{1 + \beta_W}\frac{\epsilon - 1}{\epsilon}(1 - \alpha)\frac{R^I}{W}. \tag{C35}$$

The first-order condition for cooks can be derived similarly, using the fact that cooks are hired at a single wage w_C and therefore must satisfy

$$\frac{\epsilon - 1}{\epsilon}\alpha\frac{R^I}{C^I} = \frac{\epsilon - 1}{\epsilon}\alpha\frac{R^D}{C^D} = \frac{1 + \beta_C}{\beta_C} w_C.$$

This implies the cook-allocation rule:

$$\frac{C^I}{C^D} = \frac{R^I}{R^D} \qquad \Rightarrow \qquad C^I = C\frac{R^I}{R^I + R^D}, \qquad C^D = C\frac{R^D}{R^I + R^D}.$$

and the wage first-order condition for cooks collapses to

$$w_C = \frac{\beta_C}{1 + \beta_C}\frac{\epsilon - 1}{\epsilon}\alpha\frac{R^I + R^D}{C}. \tag{C36}$$

Finally, the comparative statics equations (11)–(12) can be derived by taking logs and differences of the wage equation and plugging in the labor supply function. Using the waiter wage condition as an example:

$$\ln w_W = \text{const} + \ln R^I - \ln W \tag{C37}$$

Taking log differences of the labor supply equation gives

$$\Delta \ln W = \beta_W \Delta \ln w_W. \tag{C38}$$

Combining equations (C37) and (C38) yields

$$\Delta \ln W = \beta_W (\Delta \ln R^I - \Delta \ln W) \qquad \Rightarrow \qquad \Delta \ln W = \frac{\beta_W}{1+\beta_W} \Delta \ln R^I. \tag{C39}$$

Now define the dine-in revenue share as

$$s_I \equiv \frac{R^I}{R}.$$

Using the identity $R^I = s_I R$, it follows that

$$\Delta \ln R^I = \Delta \ln R + \Delta \ln s_I.$$

Therefore, the decomposition in equation (11) is

$$\Delta \ln W = \frac{\beta_W}{1+\beta_W} \left[\Delta \ln R + \Delta \ln s_I \right].$$

The decomposition for cooks follows analogously.

C2 Model generalization: two goods, three types of labor

This section generalizes the stylized model proposed in Section 2 relaxing the functional forms of the labor supply, product demand and production function.

Labor Supply For $\mathcal{L} \in \{W, C\}$, let the firm face an inverse labor supply curve

$$w_{\mathcal{L}} \;=\; \psi_{\mathcal{L}}(L_{\mathcal{L}};\; a_{\mathcal{L}}), \quad \psi'_{\mathcal{L}}(.) > 0$$

where $a_{\mathcal{L}}$ are firm and occupation specific amenities. Given this general form, define the finite local labor supply elasticity as:

$$\eta_{\mathcal{L}} \;\equiv\; \frac{\partial ln\; L_{\mathcal{L}}}{\partial ln\; w_{\mathcal{L}}} \;=\; \Big(\frac{\partial ln\; w_{\mathcal{L}}}{\partial ln\; L_{\mathcal{L}}}\Big)^{-1}$$

Using this local elasticity, the marginal expenditure on labor satisfies

$$\text{ME}_{\mathcal{L}} \equiv \frac{d(w_{\mathcal{L}}\; L_{\mathcal{L}})}{d\; L_{\mathcal{L}}} = w_{\mathcal{L}}\Big(1 \;+\; \frac{1}{\eta_{\mathcal{L}}}\Big)$$

Production Let the restaurants produce two goods:

$$Y^I \;=\; F^I(C^I,\; W), \qquad Y^D \;=\; F^D(C^D,\; D), \qquad C^I \;+ C^D \;=\; C$$

where F^I, F^D are increasing and concave functions. Additionally, assume that the production functions have constant returns to scale within each channel. Then one can define the local output elasticities as:

$$\alpha_W^I \equiv \frac{\partial ln\ F^I}{\partial ln\ W}, \qquad \alpha_C^I \equiv \frac{\partial ln\ F^I}{\partial ln\ C^I}, \qquad \alpha_C^D \equiv \frac{\partial ln\ F^D}{\partial ln\ C^D}$$

Under constant returns to scale, we have the following identity

$$\frac{\partial Y^I}{\partial W} = \alpha_W^I \frac{Y^I}{W}, \qquad \frac{\partial Y^I}{\partial C^I} = \alpha_C^I \frac{Y^I}{C^I}, \qquad \frac{\partial Y^D}{\partial C^D} = \alpha_C^D \frac{Y^D}{C^D}$$

Demand Define a demand environment such that the marginal revenue per unit of each output can be expressed in revenue terms. A convenient general way is to define a channel-specific marginal revenue factor:

$$MR^I \equiv \frac{\partial R}{\partial Y^I} \ = \ \mu_I \frac{R^I}{Y^I}, \qquad MR^D \equiv \frac{\partial R}{Y^D} = \mu_D \frac{R^D}{Y^D}$$

where $\mu_I,\ \mu_D \ \in \ (0, 1]$ summarize demand curvature/markups. In a nested CES with top-level isoelastic demand, these $\mu's$ are simple functions of elasticities; in a more general demand system they are still well-defined locally.

General First Order Conditions Profit maximization yields the following expression for waiters:

$$ME_W \ = \ MR^I \ \frac{\partial Y^I}{\partial W}$$

which can be re-expressed the following way (after plugging in the general forms):

$$w_W\left(1 \ + \ \frac{1}{\eta_W}\right) \ = \ \left(\mu_I \ \frac{R^I}{Y^I}\right) \left(\alpha_W^I \frac{Y^I}{W}\right) \ = \ \mu_I \ \alpha_W^I \ \frac{R^I}{W}$$

$$w_W \ = \ \frac{\eta_W}{1 + \eta_W} \ \mu_I \ \alpha_W^I \ \frac{R^I}{W}$$

Relating W and w_W using the local elasticity η_W and taking log differentials one reaches the following expression:

$$dln\ W = \frac{\eta_W}{1 + \eta_W}\left[dln\ R^I + dln(\mu_I\ \alpha_W^I)\right]$$

where under the assumption that μ_I and α_W^I are approximately stable around adoption, we reach a decomposition with the same structure as equation (11) in Section 2:

$$d\ln W \approx \frac{\eta_W}{1 + \eta_W} d\ln R^I = \frac{\eta_W}{1 + \eta_W}\left[d\ln R + d\ln s_I\right],$$

where $s_I \equiv R^I/R$ is the dine-in revenue share. The decomposition for cooks can be derived in a similar way.

C3 Model with one good and three types of labor

This section presents an alternative to the model presented in Section 2. The main differnece with the model presented in the main section of the paper is that here restaurants produce one output (instead of two). In this model, platform adoption affects the relative productivity of delivery drivers respect to waiters, who are combined through a CES function to produce the unique good. Similar to the main model, this model is a job posting model that builds on Card et al. (2018).

The production function of each restaurant is given by:

$$Y_j = T_j \, C_j^{\alpha} \, S_j^{1-\alpha} \tag{C40}$$

$$S_j = [\theta W^{\rho} + (1-\theta) D^{\rho}]^{\frac{1}{\rho}} \tag{C41}$$

where cooks and service workers (S) are complements in production and the output elasitcity is defined by α. Within the service sector, waiters (W) and delivery drivers (D) operate through a CES function with elasticity of substitution equal to $\sigma = (1-\rho)^{-1}$ and relative productivity equal to θ. The profit maximization problem for restaurant j is given by:

$$\max_{w_W, w_C, D} P_{jl} Y_j - w_W W - w_C C - w_D D \tag{C42}$$

subject to the following constraints:

$$ln(W) = \lambda_{jW} + \beta \, ln(w_{jW}) \tag{C43}$$

$$ln(C) = \lambda_{jC} + \beta \, ln(w_{jC}) \tag{C44}$$

$$P_{jl} = P_{0jl} Y_j^{-\frac{1}{\epsilon}} \tag{C45}$$

where w_W and w_C are the wages of waiters and cooks respectively and P_{0jl} is the firm specific demand shifter for the good produced by firm j. Motivated by marignal number of delivery drivers in the data previous to online-delivery platforms, I start by assuming that pre-delivery platforms the $\theta \approx 1$. That is, that the productivity delivery drivers was essentially 0 previous to delivery platforms, and so restaurants would not hire them (neither through platforms or formally). Dropping the j subscripts for simplicity, the first-order condition with respect to the posted wage of

waiters is:

$$\frac{\partial \Pi}{\partial w_W} = [P(Y) + YP'(Y)] \frac{\partial Y}{\partial W} \frac{\partial W}{\partial w_W} - \left(W + w_W \frac{\partial W}{\partial w_W} \right) = 0. \tag{C46}$$

Using the inverse demand function $P = P_0 Y^{-1/\epsilon}$, we have

$$P(Y) + YP'(Y) = \frac{\epsilon - 1}{\epsilon} P(Y).$$

Using the labor-supply equation $\ln W = \lambda_W + \beta \ln w_W$, we also have

$$\frac{\partial W}{\partial w_W} = \beta \frac{W}{w_W}.$$

Next, note that

$$Y = TC^{\alpha} S^{1-\alpha}, \qquad S = \left[\theta W^{\rho} + (1 - \theta) D^{\rho} \right]^{1/\rho}.$$

Therefore,

$$\frac{\partial Y}{\partial W} = TC^{\alpha} (1 - \alpha) S^{-\alpha} \frac{\partial S}{\partial W}, \tag{C47}$$

where

$$\frac{\partial S}{\partial W} = \theta W^{\rho - 1} \left[\theta W^{\rho} + (1 - \theta) D^{\rho} \right]^{\frac{1}{\rho} - 1} = \theta W^{\rho - 1} S^{1 - \rho}. \tag{C47'}$$

Combining these expressions yields

$$\frac{\partial Y}{\partial W} = Y (1 - \alpha) \theta \frac{1}{S} \left(\frac{W}{S} \right)^{\rho - 1}. \tag{C47"}$$

Define revenue as

$$R \equiv PY = P_0 Y^{\frac{\epsilon - 1}{\epsilon}}.$$

Substituting (C46) and (C47") into the first-order condition and rearranging yields a wage equation with the same structure as equation (9) in Section 2:

$$w_W = \frac{\beta}{1 + \beta} \frac{\epsilon - 1}{\epsilon} \frac{R}{S} \theta (1 - \alpha) \left(\frac{W}{S} \right)^{\rho - 1}. \tag{C48}$$

Similarly, using the fact that that:

$$\frac{\partial Y}{\partial C} = \alpha C^{\alpha - 1} S^{1 - \alpha} = \frac{Y}{C} \alpha \tag{C46}$$

We again obtain a structure similar to equation (10) in Section 2:

$$w_C = \frac{\beta}{1 + \beta} \frac{\epsilon - 1}{\epsilon} \frac{R}{C} \alpha \tag{C47}$$

To derive the comparative statics for waiters and cooks, define the change in X between the pre- and post-platform periods as ΔX. Taking logs, plugging in the labor supply of waiters, and subtracting the pre-platform first-order condition from the post-platform first-order condition for waiters yields:

$$\Delta \ln P_{0j} + \frac{\epsilon - 1}{\epsilon} \Delta \ln Y + \frac{1 - \sigma}{\sigma} \Delta \ln S + \Delta \ln \theta = \left(\frac{1}{\beta} + \frac{1}{\sigma} \right) \Delta \ln W \tag{C48}$$

where I am using the fact that firm-level amenities λ_j do not vary with platform adoption, and

that the labor supply elasticity is also fixed. Define revenue per service worker as

$$V_S \equiv \frac{R}{S}.$$

Using the identity

$$\Delta \ln V_S = \Delta \ln R - \Delta \ln S,$$

equation (C48) can be rewritten in a form analogous to equation (11):

$$\Delta \ln W = \frac{\beta\sigma}{\beta+\sigma}\left[\Delta \ln(V_S) + \frac{1}{\sigma}\Delta \ln(S) + \Delta \ln(\theta)\right] \tag{C49}$$

Equation (C49) highlights two channels. The first two terms,

$$\Delta \ln(V_S) + \frac{1}{\sigma}\Delta \ln(S),$$

capture the market-expansion effect, while $\Delta \ln(\theta)$ captures the substitution effect through changes in the relative productivity of waiters. The comparative statics for cooks can be derived in a similar way.

D Appendix: Empirics

D1 Robustness: Availability of the Platform as an Instrument for Adoption

As discussed in Section 6, the adoption of delivery platforms is based on a strategic decision of restaurants. If these decisions are based on unobservable firm level shocks, then it could be that the baseline matched difference-in-difference design could be biased. In this section, I present an alternative research design to estimate the impact of delivery platforms on restaurants based on an instrumental variable approach where I instrument platform adoption with the availability of the platform. The starting point is to define the outcome of interest which is defined as: $Y_j = \Delta\ (\ln Y_{j_{21}} - \ln Y_{j_{17}})$, that is, the difference between log firm size (wages) in 2021 and log firm size (wages) in 2017. With this outcome at hand, I then estimate the following two stage least square model:

$$Y_j = \beta\ D_j + \delta\ X_j + u_j \tag{D50}$$

$$D_j = \pi_1\ Z_j + \pi_2 X_j + v_j \tag{D51}$$

Where D_j is an endogenous binary variable equal to 1 when firm j adopted the technology, while Z_j is an exogenous variable equal to the amount of months in which the platform was available in $j's$ microregion between January 2018 and December 2021. Under this specification, the vector

X includes firm level average of tenure, age, hours, firm age, share of service workers, share of female workers, share of restaurants within a 1 kilometer radius, quartile of firm wage, and quartile of firm size, all in 2017. Standard errors are clustered at the microregion level.

Within this framework, the panel data regression (13) presented in Section 5 can be interpreted as the intention-to-treat effects of the availability of delivery platforms in a labor market, where date and microregion indicators absorb common time shocks and unobersvable restaurant productivity determinants that are fixed at the microregion level. Furthermore, Figure A12 implements the intention-to-treat regression at the firm level (including firm fixed effects) to check for pre-trends and finds no evidence of differential trends across locations with and without platform.

As in the classical IV design, under regular assumptions (relevance, exogeneity and exclusion) the IV will identify the local average treatment effect for compliers (i.e. restaurants whose adoption is moved by availability timing), similar to Akerman et al. (2015). However, given the discussion in Section 7.4 regarding the spillover effects of delivery platforms on non-adopting restaurants, the exclusion restriction is likely to be violated. Specifically, to see this suppose platform availability has the following impact on firm level outcomes (beyond its effect on Y through D):

$$u_i = \lambda Z_i + \epsilon \qquad \text{with } \lambda \neq 0 \tag{D52}$$

$$E[u|Z] \neq 0 \tag{D53}$$

Given the results discussed in section 7.4, it is safe to assume that $\lambda < 0$ for firm size while it will be closer to 0 for average wages. Then with one endogenous regressor and one instrument, the IV/Wald estimand becomes:

$$\hat{\beta}^{IV} = \frac{\text{COV}(Z,\ Y)}{\text{COV}(Z,\ D)} = \beta + \frac{\lambda}{\pi}, \tag{D54}$$

Where π is the first-stage effect of availability on adoption. From here, it is straightforward to see that under the assumption that the average treatment effect on compliers is similar to the effect on the average treated firm, the negative spillover effects on firm size will imply that $\hat{\beta}^{IV} < \hat{\beta}^{DiD}$. This is precisely what is found in Table B4.

D2 Procedure to Rescale Spillover Effects

This subsection describes how I rescale the worker-level spillover estimates from Section 8.2 to construct the spillover-loss objects reported in Columns 4–6 of Table V. The object recovered is an average realized spillover wage-bill loss per adopting restaurant under the baseline event-based design.

I begin by defining a spillover event as

$$\mathbf{1}\{\text{Spillover}_{jt}\} = \mathbf{1}\left[\Omega_{jt} > 95\text{th percentile of } \Omega\right], \tag{D55}$$

where

$$\Omega_{jt} = \chi_{jt} - \chi_{jt-1},$$

and χ_{jt} is the share of restaurants within a 1-kilometer radius of restaurant j that are enrolled in the platform in quarter t.

I then estimate the effect of spillover events on workers' earnings, as discussed in Section 8.2, using the following regression:

$$Y_{it} = \beta_0 + \alpha_i + \delta_t + \sum_{k=-7}^{5} \theta_k \mathbf{1}\{t = t^*(i) + k\} + \sum_{k=-7}^{5} \beta_k \mathbf{1}\{t = t^*(i) + k\} \times \mathbf{1}\{\text{Spillover}_i\} + \epsilon_{it}. \tag{D56}$$

The coefficient β_k captures the per-worker earnings effect of a spillover event k quarters after exposure. To express this effect in units of nearby adopting restaurants, I next estimate:

$$Z_{jt} = \gamma_0 + \alpha_j + \delta_t + \sum_{k=-7}^{5} \gamma_k \mathbf{1}\{t = t^*(j) + k\} \times \mathbf{1}\{\text{Spillover}_j\} + \epsilon_{jt}, \tag{D57}$$

where Z_{jt} is the number of nearby restaurants within 1 kilometer that adopt the platform, and γ_k measures how many additional nearby adopters are associated with a spillover event.

I then define the rescaled spillover effect as

$$\beta_k^{\text{rescaled}} = \frac{\beta_k}{\gamma_k}. \tag{D58}$$

This object can be interpreted as the per-worker earnings effect associated with one additional nearby adopting restaurant among establishments exposed to a spillover event.

To translate this per-worker effect into a per-adopter wage-bill effect, I multiply by three additional terms. First, I multiply by $N_{\text{Restaurants non-adopt}}$, the median number of nearby non-adopting restaurants in the sample, to capture the number of establishments potentially affected around a representative adopter. Second, I multiply by $\Pr(\text{event})$, the unconditional probability that a non-adopting restaurant experiences a spillover event under the baseline design. Because a spillover event is defined as a top-5% increase in nearby adoption, this probability equals 0.05 by construction. This term is therefore a frequency adjustment implied by the event-based design. Third, I multiply by $\overline{L}$, the average restaurant size, to convert the worker-level effect into a wage-bill effect.

The resulting spillover-loss object is

$$\pi_k^{\text{spillover}} = \beta_k^{\text{rescaled}} \times N_{\text{Restaurants non-adopt}} \times \Pr(\text{event}) \times \overline{L}. \tag{D59}$$

Equation (D59) should be interpreted as an average realized spillover wage-bill loss per adopt-

ing restaurant under the baseline event-based design. Because the spillover regression identifies the effect of large exposure shocks, the term Pr(event) converts the event-level effect into an unconditional average by weighting it by the empirical probability that a non-adopting restaurant is exposed to such a shock. This object should therefore not be interpreted as the full structural spillover effect of an additional adopter on all nearby non-adopting restaurants.

Finally, because the baseline design focuses on large discrete increases in nearby adoption, this procedure may understate total spillovers if smaller increases in nearby adoption also reduce earnings at non-adopting restaurants. For this reason, Table B8 reports alternative spillover calculations based on a market-level decomposition.

D3 Estimation of Outside Option for Delivery Drivers Transitioning from Non-Formality

To estimate the outside option of delivery drivers who transition from either unemployment or informality, I use the Brazilian household survey PNAD-C. Given potential inconsistencies across administrative earnings (reported in RAIS) and self-reported earnings from PNAD-C, I use PNAD-C to estimate the earnings ratio between non-formal and formal workers that follow the same distribution of demographics as platform workers. The I multiply the corresponding ratio of each quarter and state to the average outside option of platform workers that are transitioning from the formal sector.

To compute the earnings ratio between non-formal and formally employed workers in PNAD-C, I start by restricting the sample of PNAD-C workers that are between 18 and 65. For employed workers, I keep individuals in PNAD-C who are employed in the non-agricultural sector and have one job. Within employed workers, I define a formal worker if they answered having a *Carteira de trabalho assinada* in their job, and informal if they don't. I define unemployed individuals as those that did not hold a job in the week of reference of the survey but answered that they were actively searching for a job.

I re-weight the sample of PNAD-C workers to match the distribution of age, education, race and gender of my sample of delivery drivers using the procedure developed by DiNardo et al. (1996). Specifically, the reweighting factor $\hat{\Psi}(X)$ is constructed as follows:

$$\hat{\Psi}(X) = \frac{\hat{Pr}(D_{\text{App}} = 1|X)/\hat{Pr}(D_{\text{App}} = 1)}{\hat{Pr}(D_{\text{App}} = 0|X)/\hat{Pr}(D_{\text{App}} = 0)}$$

where $Pr(D_{\text{App}} = 0|X) = 1 - Pr(D_{\text{App}} = 1|X)$ and $\hat{Pr}(D_{\text{App}} = 1|X)$ is the estimated probability of being a platform worker estimated through a logit model. Once the PNAD-C sample is reweighted to match de distribution of demographics of platform workers, I calculate the earnings ratio between non-formal (informal and unemployed) and formal employed workers at the

state-quarter level and multiply the ratio by the outside option of delivery drivers transitioning from the formal sector. The average re-weighted wage ratio of non-formal to formal workers in my sample is 0.32.

D4 Imputation of Wage on Delivery Platform per Restaurant

A limitation of my data is that I do not observe which worker delivered for each restaurant at each moment in time. Therefore I can not compute the exact wage bill generated for platform-workers at each restaurant. I instead observe the earnings that each app worker makes per hour at each month/municipality and the total number of hours that restaurants used delivery services from the platform at each month/municipality. I overcome this limitation by calculating the average per hour earnings on the platform of workers in each month and municipality (weighted by the number of hours each platform-worker worked in that month and municipality). I define the per hour wage on the platform for each worker as the total earnings they made in a month divided by 1.15 times the total hours they worked on the platform that month. That is, I allow for an additional 15% of time to be idle/non-productive time—the mid point of what has been used in other studies in Brazil following the same type of workers (Callil and Picanço, 2023). To recover a per adopting restaurant impact on platform worker earnings, I then multiply the average per hour wage in each restaurants corresponding municipality-month by the number of hours the restaurant utilized delivery platforms in that period. I finally take an average across all the adopting restaurant estimates.

D5 Imputation of informality: restaurants

I start by estimating the share of informal workers in the restaurant sector at each municipality using the 2010 Brazilian census (latest census available). I then impute to each restaurant in my sample the share of informal workers in the restaurant sector at the municipality level. This yields a per restaurant share of informal workers. I estimate that the average restaurant in my sample has 25 percent of informal workers.

To recover the pre-treatment earnings of informal workers per adopting restaurant, I use the Brazilian household survey (PNAD-C) which is representative at the state level. For each state and quarter I calculate the ratio of informal wages to formal wages in the restaurant sector. I estimate that informal restaurant workers in my sample earn on average 70 percent of the wages of formal workers. Finally, I assume that the earnings effect (in percentage of their pre-event earnings) on informal workers is the same as for formal workers. This imputed effect, in combination with the

number of informal workers calculated with the census and the earnings for informal workers calculated with PNAD-C, yields a per-restaurant earnings effect for informal workers.

To assess the sensitivity of the results to the assumption that informal workers experience the same proportional earnings effect as formal workers, Figure A26 plots the total impact of platform adoption on worker earnings as a function of the imputed earnings effect for informal workers, expressed as a multiple of the earnings effect for formal workers. The figure highlights three scenarios. First, if informal workers experience no earnings effect following platform adoption, the implied surplus in worker earnings is 10.7% of the pre-adoption wage bill. Second, under the baseline assumption that informal workers experience the same earnings effect as formal workers, the implied surplus is 5.7% of the pre-adoption wage bill of adopting restaurants. Third, the break-even point occurs when informal workers experience a negative earnings effect equal to 2.13 times the estimated effect for formal workers, in which case the implied worker-earnings surplus is zero.

D6 AKM Estimation Details

The AKM model builds on Abowd et al. (1999) to estimate the worker effects and firm effects in the data. To estimate the AKM model, I start by constructing an yearly panel of workers where log earnings reflect the average monthly earnings at the dominant employer in the year. That is, each worker is assigned to one employer in a year that reflects the employer that paid the largest total wages in the year to the worker. The model assumes that the log monthly wage paid to worker i, at firm j in year t is given by:

$$\ln y_{ijt} = \alpha_i + \psi_{j(i,t)} + X'_{it}\,\beta + \epsilon_{it} \tag{D60}$$

Where α_i is the worker fixed effect that is fully portable to every job the worker has, $\psi_{j(i,t)}$ is the firm wage premium which is specific to each firm (where $J(i,t)$ is an index indicating the workplace for worker i at year t), X_{it} is a vector of time-varying controls including a polynomial of age and year fixed effects, and ϵ_{it} is the error term.

If we assume that the conditional expectation of the error term ϵ_{it} in equation (1) is independent of the worker's job history (the "exogenous mobility" assumption), then estimating equation (1) using ordinary least squares (OLS) will provide unbiased estimates of establishment wage premiums. Several specification tests introduced by Card et al. (2013) and later applied in studies such as Card et al. (2016); Macis and Schivardi (2016); Song et al. (2019), indicate that wage changes among job movers in countries like Germany, Italy, Portugal, and the United States generally align with the exogenous mobility assumption. Furthermore, Gerard et al. (2021) test these specifications in Brazil using RAIS and find that the exogenous mobility assumption is also likely to

hold in this setting.

To estimate the AKM model, I follow the leave-out procedure outlined by Kline et al. (2020). As AKM highlights, in a two-way fixed effects model, establishment effects are only identifiable across "connected sets" of workplaces that are tied together by the movement of workers between them. Therefore, I restrict my analysis to the largest connected set in my sample to ensure the identification of these effects. I estimate the AKM model for the period 2012-2018.

Finally, if a worker did not work in the formal sector during my estimation period, I impute their worker effects when possible. To input the worker effects, I leverage equation D60 and compute the worker effects at each year as:

$$\alpha_{it} = \ln y_{ijt} - \psi_{j(i,t)} - X'_{it}\,\beta$$

And then I average the worker effects across all the years in which the worker was employed in the formal sector such that:

$$\hat{\alpha}_i = \frac{\sum_{t=2012-2021} \alpha_{it}}{T}$$

where T is the number of calendar yeras in whcih worker i is observed with a formal job.

D7 Estimation of Firm Values V_j^e using PageRank

In this section I present a summary of the algorithm utilized to estimate the firm valuation V_j^e developed by Sorkin (2018). The starting point is to define the utility that a worker i receives from working at firm j as $u_{ij} = V_j^e + \varepsilon_{ij}$ where V_j^e is a firm specific value that is common to all workers and ε_{ij} is a worker specific idiosyncratic shock. I further assume that ε_{ij} is distributed as a standard type-I extreme value. In a market with 2 firms, the likelihood that a worker favors firm j over firm k is given by: $\frac{\exp(V_j^e)}{\exp(V_j^e)+\exp(V_k^e)}$. When allowing for N workers and defining M_{jk} as the number of workers selecting firm j over firm k, the following relation holds between employment choices and firm specific employment valuations: $\frac{M_{kj}}{M_{jk}} = \frac{\exp(V_k^e)}{\exp(V_j^e)}$.

When accounting for multiple firms $j \in J$, the condition described above imposes the following condition for each pair of firms:

$$M_{kj}\exp(V_j^e) = M_{jk}\exp(V_k^e),\ \forall\ j \in\ J \tag{D61}$$

Summing (D61) over all firms in the market, one arrives to the following expression:

$$\frac{\overbrace{\sum_{j \in J} M_{kj} \exp(V_j^e)}^{\text{value-weighted entry}}}{\underbrace{\sum_{j \in J} M_{jk}}_{\text{exits}}} = \underbrace{\exp(V_k^e)}_{value} \tag{D62}$$

which imposes a linear restriction for each firm j. As explained by Sorkin (2018), it is possible to solve for V_j^e as a fixed point of a linear system and a unique solution exists for the set of strongly connected firms. Intuitively, equation (D62) follows the same logic as Google's PageRank algorithm in website searches. While in Google's case a good website is linked to other good webpages, in the labor market a good employer is defined as one that receives workers from other good employers while loses few employees to other firms. To estimate the value of platform work as a job destination (and the rest of the establishments in Brazil), I restrict to employer-to-employer transitions in the formal sector (or delivery platform) between quarters.[52]. This implies that I only measure the valuation of the delivery platform for workers who transitioned to (from) gig work from (to) the formal sector and earn their main income through the platform. The tradeoff is that while this is a restrictive group of transitions, it represents a group of workers for whom the gig job is somewhat comparable to a formal job.

To keep the analysis consistent, I consider the delivery platform in each microregion as a different establishment. Furthermore, similar to Sorkin (2018), I adjust $\exp(V_j^e)$ by the hours hired and by the share of unemployment-to-employment transitions that the establishment has. The adjustment is done to account for the fact that larger establishments will have more separations, while the share of unemployment-to-employment transitions adjusts for the job offer rates. [53]

D8 Geolocation details

To geolocate restaurants in the data, I leverage the address of the restaurants and the name of the establishments available in RAIS. Importantly, the address is only available until 2020, and so I assume that restaurants do not change addresses when open in 2021. Specifically, the steps I take to geolocate the restaurants are the following:

1. I use the Google Maps API to obtain the latitude and longitude of the address of each restau-

[52] A worker is considered to work for establishment j in quarter t if the main source of income during t was from j.

[53] In contrast to Sorkin (2018), I use hours instead of firm size to make it comparable between the delivery platform and the rest of the establishments.

rant.

2. For restaurants that met any of the following criteria:

    - More than one observation in the same latitude and longitude, with only a partial match for the address found in Google Maps,
    - Latitude and longitude were missing after the first geolocation attempt,
    - The geolocation type in Google Maps was approximate or corresponded to a geometric center.

3. For these cases, I attempted a second round of geolocation. This second attempt involved:

    - Geocoding the postal code and municipality
    - Running the places autocomplete API from Google Maps, using the original addresses but restricting the search to being within a radius of 20 kilometers of the geometric center of the postal code.
    - Using the predicted address to geolocate the restaurant again with the Google Maps API.

4. For those that have non-missing address after step 2 (90% of the observations), I compare the postal codes found in step 2 with the original postal code from RAIS. I drop the cases when these two postal codes do not match.

5. I finally combine the restaurants that geo-coded in step 1 with the restaurants that I was able to geocode in step 3, and that forms my final sample of geocoded restaurants.

Table D10 shows the share of restaurants in RAIS that I am able to find geocoded in each year using the procedure described above.

Table D10: Share of Restaurants that Geocode in Each Year

	Share of Restaurants that are Geocoded
2016	90%
2017	91%
2018	88%
2019	88%
2020	88%

Notes: Restaurants are defined as those that have a CNAE code equal to 56.